Testing a Re-employment Incentive for Displaced Workers:
The Earnings Supplement Project

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Preface

This is one of a series of reports produced in connection with the Earnings Supplement Project (ESP). Three important themes are brought together in the project.

First is the search for innovative approaches to help people to become re-employed. The principal purpose of unemployment benefits is to provide temporary earnings replacement while people search for work. It may be possible, however, to devise more effective ways of providing financial assistance — ways that can help unemployed people overcome the barriers they face to becoming re-employed. This is recognized, for example, in the 1996 *Employment Insurance Act*, which includes provisions for "active measures" in the form of program supports for the unemployed who are eligible for Employment Insurance (EI).

Second, in a world where the pace of economic change seems to be accelerating, economic adjustments leading to job losses are all too prevalent. While the argument can be made that the majority of Canadians benefit from economic adjustments, because they lead to more efficient allocation of resources and lower-cost products, the costs of such adjustments are disproportionately borne by those who experience a loss of employment — often long-term, well-paid employment. Thus, there is considerable policy interest in finding ways to cushion the effects on those who suffer the most.

Finally, because governments have limited resources with which to implement new programs, it is important that any new spending be directed to initiatives that have demonstrated their potential to be effective in meeting the objectives set for them. Policy based on evidence should produce a more efficient use of program resources and better results than policy solely based on intuition and hope.

In light of these considerations, ESP was designed to test an innovative use of EI funds — one that offered displaced workers a guarantee against extreme earnings losses for up to two years if they went back to work quickly and experienced a reduction in earnings in doing so. The new program was tested on a relatively small scale in five communities using the most reliable method of evaluation available — a random assignment experiment. Although the ESP program model did not produce large impacts on the re-employment experiences of those who took part, this finding is itself an important result. It can help policy-makers avoid a potentially costly mistake and steer program development toward more effective forms of intervention.

John Greenwood
Executive Director

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The Earnings Supplement Project (ESP) would not have been possible without the collaboration of many people and organizations who brought a combination of funding, research, data management, and operational experience to the project.

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Finally, we would like to express our appreciation to all the participants in the ESP study. They answered our questions, spent time with us discussing their experiences, and made this study possible.

The Authors

Executive Summary

The Earnings Supplement Project (ESP) was a multi-site test of an innovative financial incentive designed to stimulate rapid re-employment for two groups: displaced workers and repeat users of unemployment benefits. ESP was conceived and funded by Human Resources Development Canada (HRDC),[1] and was conducted by the Social Research and Demonstration Corporation (SRDC), a Canadian non-profit social policy research organization.

The project was implemented in nine Human Resource Centres of Canada (HRCCs) located in seven provinces.[2] It comprised two separate studies:

- a study in five HRCCs of displaced workers, who had lost jobs permanently due to changing economic conditions; and
- a study in four HRCCs of repeat users of unemployment benefits, who regularly combined periods of work with spells of benefits.

The financial incentive tested was an offer to supplement or "top up" the earnings of selected unemployment benefit claimants if they found a new full-time job quickly and their new job paid less than their old one. Supplement payments could make up 75 percent of the difference for up to two years, up to $250 weekly.

The primary goal of the supplement for displaced workers was to shorten their often long and painful re-employment process and to provide them with a source of income in a form that promoted employment. It was hoped that doing so would help to compensate displaced workers for the losses they incurred due to economic change. In addition, it was hoped that, by encouraging re-employment, the supplement would reduce the cost of unemployment benefits.

The goal of the supplement for repeat users of unemployment benefits was to stimulate off-season employment and promote a shift toward year-round jobs. It was hoped that doing so would reduce their long-term dependence on unemployment benefits.

A randomized experiment, the best way known to measure program impacts, was used to measure the effects of ESP on full-time employment, earnings, and unemployment benefit payments. Eligible sample members were randomly assigned either to a supplement group that was offered the supplement program, or to a control group that was not. Both groups could receive all unemployment benefits to which they were entitled normally. Choosing the groups in this way helped to ensure that the supplement offer was their only initial systematic difference. Thus, comparisons of their future experiences provided valid estimates of the impacts of ESP on these experiences.

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[1] At the time, HRDC was called Employment and Immigration Canada.

[2] At the time ESP began and the sites were selected, local HRDC offices were called Canada Employment Centres (CECs). Part way through the project they were redesignated as HRCCs, so this term is used throughout the present report to be consistent with current usage.

This report, one in a series about ESP, presents findings for displaced workers.[3] The report reviews the rationale, design, and implementation of the displaced worker study. It assesses how well participants understood the terms and conditions of the supplement offer. It documents the pattern of supplement receipt. It presents estimates of ESP impacts. It outlines the financial implications of these findings for displaced workers and the Canadian government. It explores variations in findings across the five study sites. It concludes by summarizing what has been learned to date about re-employment incentives for displaced workers, and considering the policy implications of these findings.

THE DISPLACED WORKER STUDY IN BRIEF

ESP was conducted as part of the unemployment benefit claims process for displaced workers in five cities: Granby, Oshawa, Toronto, Winnipeg, and Saskatoon. When individuals applied for regular unemployment benefits, local HRCC staff used a brief screening form to determine whether they met the criteria established for identifying displaced workers. Claimants who met these criteria were encouraged to apply for ESP. When the research team received notice that an ESP applicant's first unemployment benefit cheque had been issued, the applicant was then randomly assigned by computer to the supplement group or the control group.[4] Supplement group members were notified of their selection by letter and sent further information about the program. Control group members were notified by letter that they had not been selected.

If supplement group members found a new full-time job (with 30 or more hours of work per week) within 26 weeks, and if that job paid less than their previous weekly insurable earnings, they could receive supplement payments equal to 75 percent of the difference for up to two years after random assignment, up to a maximum of $250 per week. In addition, supplement group members who found full-time employment within 26 weeks that paid as much or more than they used to make could initiate a supplement that would protect them for up to two years against a future re-employment earnings loss. It was hoped that this offer of supplement payments for some plus "earnings insurance" for others would provide a strong inducement to return quickly to full-time work.

Sample members' attitudes, behaviour, and experiences were documented through multiple data sources. In addition, a field study was conducted to examine how the program was implemented.

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[3]The first ESP report (Bloom et al., 1997) describes the overall project and assessed its implementation. The other report in the series (Tattrie, 1999) presents findings for repeat users of unemployment benefits.

[4]To focus the study only on unemployment benefit recipients, as opposed to all claimants, ESP applicants were not randomly assigned until the research team received notice of claimants' first benefit cheque. Claimants with severance payments were an exception to this rule. Because their unemployment benefits could not start until after the allocation of their severance payments ran out (which could take months), these claimants were randomly assigned as soon as their unemployment benefit claim was approved. On average, eight weeks elapsed between ESP application and random assignment, although this period was shorter for claimants with severance than for others.

THE FINDINGS IN BRIEF

This section summarizes the main findings from the ESP research for displaced workers.

It was possible to recruit a large and diverse sample and implement a rigorous study as part of the local unemployment benefit program in five sites.

Within roughly one year, local HRCC staff screened and enrolled 5,912 sample members who met the definition of displaced workers used for this report.[5] Half of the sample was randomly assigned to the supplement group and half to the control group. These two groups were virtually identical at random assignment. Thus, they provide a valid basis for measuring the impacts of ESP. Furthermore, they represent a variety of displaced workers and labour market conditions. Hence, ESP findings for displaced workers are broadly generalizable, even though they are not based on a representative national sample.

Supplement group members understood the terms and conditions of the supplement offer well enough for it to provide a "fair test."

Supplement group members received detailed information about ESP from brochures, personal letters, telephone orientation sessions, and a toll-free information line. HRCC staff who were surveyed felt that the information provided about ESP was quite effective and that most displaced workers had knowledge about it. Supplement group members who were surveyed felt that ESP information was easy to understand and quite helpful. Furthermore, their answers to survey questions about the supplement indicated that they clearly understood its basic provisions, and many were knowledgeable about its specific details. In addition, most supplement group members who participated in focus groups had a good basic understanding of ESP. Almost all knew what it was about, and more than two out of three understood its specific details. On balance, ESP appeared to be a fair test of the supplement program that it examined.

Two out of ten supplement group members received supplement payments, which were substantial for those who received them. Only 1.5 percent of all supplement group members initiated earnings insurance and very few of them ever used this insurance.

Roughly 3 out of 10 supplement group members *qualified* to receive supplement payments by quickly finding a new full-time job that paid less than their previous one. Seven out of ten of these supplement qualifiers received supplement payments, and those with the highest expected payments were the most likely to apply for and receive them. Therefore, 2 out of 10 supplement group members received supplement payments. On average, supplement recipients were paid $8,705 for 64 weeks of full-time employment during the two-year supplement receipt period. About 44 percent of all recipients were still receiving payments at the end of two years.

About 12 percent of the supplement group members qualified for earnings insurance by quickly finding a new full-time job that paid as much or more than their previous job; and only one out of eight of these qualifiers bothered to initiate (register for) their earnings insurance. Thus, only 1.5 percent of all supplement group members initiated earnings insurance. Of this very small group, only seven people ever used their earnings insurance to

[5] As explained later, a total of 8,144 individuals were enrolled in ESP and randomly assigned. However, the sample for this report includes only the 5,912 who did not expect to return to their former employer.

obtain supplement payments. Thus, it appears that displaced workers had very little interest in earnings insurance and made very little use of it.

ESP had a small positive effect on rapid re-employment.

ESP produced a modest increase in full-time employment toward the end of the six-month job-search period. Thus, the program increased the percentage of displaced workers who became re-employed full time during this period by 4.4 percentage points.[6] This reflected, in roughly equal parts, a shift from part-time to full-time employment, and an increase in overall employment.

ESP's effect on hastening re-employment may have caused a few displaced workers to take jobs that paid less than the ones they would have taken otherwise.

The program may have caused average earnings during the 15-month follow-up period to be $682, or 4.6 percent lower than they would have been otherwise, presumably by inducing a few supplement group members to take lower-paying jobs.[7] Hourly wage rates were $0.33 or 2.5 percent less than they would have been otherwise.[8]

ESP had virtually no effect on the amount or duration of unemployment benefits received by supplement group members.

The estimated program impact on average weeks of unemployment benefit payments during the first 15 months after random assignment was 0.2 weeks or 0.9 percent. The estimated impact on total benefits received was $90 or 1.4 percent.[9]

ESP produced a modest transfer of resources from the Canadian government (taxpayers) to the 2 out of 10 displaced workers who received supplement payments. This helped to compensate recipients for the losses they incurred in a way that did not inhibit re-employment.

On average, supplement group members experienced a small financial gain of $569 during the first 15 months after random assignment. This was because the supplement payments they received exceeded the earnings loss they incurred and their unemployment benefit payments were virtually unchanged. However, this small average gain did not reduce the overall level of financial hardship experienced. Nevertheless, the large supplement payments made to the small fraction of displaced workers who received them were an important source of temporary income for this subgroup.

ESP produced a net financial cost for the Canadian government of $1,340 per supplement group member during the first 15 months after random assignment. This occurred because supplements paid did not reduce unemployment benefits paid.

Therefore, on balance, the program produced a transfer of resources from the government to individual displaced workers. This helped to compensate recipients for the losses they incurred in a way that did not inhibit re-employment.

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[6]This estimate was statistically significant (p-value < 0.01).

[7]This estimate was not statistically significant (p-value = 0.12).

[8]This estimate was statistically significant (p-value = 0.09).

[9]This estimate was not statistically significant (p-value = 0.50).

ESP was most successful in Saskatoon. Even there, however, its impacts were modest, and its net effect was a transfer of resources from the government to supplement recipients.

The supplement receipt rate and the impact of ESP were greater in Saskatoon than in any other site. Roughly 33.8 percent of the supplement group members from Saskatoon received supplement payments in the job-search period, and ESP increased the six-month full-time re-employment rate by 7.3 percentage points. In addition, the program appeared to reduce average unemployment benefit payments by $384 or 1.3 benefit weeks during the first 15 months after random assignment.[10] The net financial gain to displaced workers during the 15-month follow-up period was $1,679 per supplement group member and the net cost to the government was $1,753.

BACKGROUND OF THE STUDY

Displaced workers have lost stable, long-term, and often well-paying jobs due to changing technology, increased international competition, or shifting market demand. Worker displacement is a permanent feature of the Canadian economy that exists in good times and bad. Thus, more than one million Canadians were laid off permanently each year between 1981 and 1991.[11]

When faced with the loss of a long-term job, many displaced workers go through an initial period of shock and disbelief. Consequently, their job search is delayed by false hopes of returning to their old employer. Their lack of knowledge about how to find a new job further compounds this problem. When they do start looking for work, displaced workers often have unrealistic expectations about the wages and benefits that they can command. This is especially true for those with seniority gained by working many years for the same employer.

Hence, although some displaced workers find new jobs quickly, many remain unemployed for a long time and often exhaust their unemployment benefits. Some leave the labour force altogether. Many who become re-employed earn much lower wages than they had previously. In short, displaced workers often experience considerable economic and personal hardship.

Unemployment insurance, now referred to as Employment Insurance (EI), is the largest government program that provides assistance to displaced workers. This program pays most claimants 55 percent of their average insurable earnings in the last 20 weeks of employment, for up to 50 weeks.[12] Much has been written, however, about the potential for unemployment benefits to prolong joblessness.[13] Because of this, and because of the high costs of benefit payments,[14] Canadian policy-makers have been exploring more active re-employment

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[10]Neither of these estimates was statistically significant (p-value equals 0.17 for the impact on total benefit dollars, and 0.18 for benefit weeks.

[11]Lauzon, 1995, p. 4.

[12]The actual duration of benefit entitlement depends on how long a claimant has worked prior to applying for benefits and the unemployment rate in the region where the claimant lives.

[13]For discussions about the work disincentive effects of unemployment benefits, see Atkinson and Micklewright, 1991; Devine and Kiefer, 1991; and Christofides and McKenna, 1996.

[14]During fiscal year 1997–1998, Canadians received $10 billion in unemployment benefits (HRDC, 1998).

strategies for displaced workers. This shift in policy mirrors a similar shift in Europe and the United States.[15]

Studies in the United States have used randomized experiments to test combinations of job-search assistance, occupational skills training, and relocation assistance for displaced workers. These studies have found job-search assistance to be a cost-effective way to promote re-employment. In contrast, they have not found occupational skills training to be cost-effective.[16]

Other randomized experiments in the United States have tested re-employment bonuses for displaced workers who found new jobs quickly and ceased to need unemployment benefits.[17] The first of these studies, conducted in Illinois, found that a $500 bonus paid to displaced workers who became re-employed within 11 weeks reduced unemployment benefit payments by more than the cost of the bonuses paid. The next three studies, conducted in New Jersey, Pennsylvania, and Washington State, tested bonus amounts ranging from several hundred to several thousand dollars, with job-search periods ranging from 3 to 13 weeks.

These studies focused mainly on the ability of re-employment bonuses to reduce the costs of unemployment benefit payments.[18] But only the Illinois study found the approach to be cost-effective in this regard.[19] The fact that half of the sample members who qualified for bonuses (by finding a new job in time) did not receive them suggests that this finding would have been less favourable if more eligible claimants had received the bonus.

From another perspective, others have argued that getting displaced workers back to work quickly can improve their future labour market prospects by reducing the "scarring" that can occur from lengthy unemployment.[20] This argument suggests that prolonged unemployment can erode job skills and reduce the perceived value of individuals in the eyes of employers. Therefore, policies that reduce lengthy unemployment can, in theory, increase the future welfare of displaced workers, regardless of whether they reduce unemployment benefit payments.

Furthermore, some have argued that displaced workers should be compensated for the losses they incur because they bear the cost of change that is necessary for others to benefit from economic growth.[21] It is further argued that providing this compensation can reduce the political resistance often associated with proposals for economic change. This argument has particular relevance to the vigorous response by many Canadians to concerns about potential

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[15]See OECD, 1990, for a discussion of these issues in Europe. See Ross and Smith, 1993, for a discussion of these issues in the United States.

[16]Results of these experiments in Texas are reported by Bloom, 1990; in Buffalo, New York by Corson, Long, and Maynard, 1985; and in New Jersey by Corson et al., 1989.

[17]See Meyer, 1995, for a review of these experiments; Woodbury and Spiegelman, 1987, for a discussion of the Illinois experiment; Corson et al., 1989, for a discussion of the New Jersey experiment; Corson et al., 1991, for a discussion of the Pennsylvania experiment; and Spiegelman, O'Leary, and Kline, 1992, for a discussion of the Washington State experiment. The results presented in this section are drawn from these studies.

[18]They also sought to determine whether the bonus caused displaced workers to lower their future earnings by inducing them to choose a new job prematurely, and thereby producing a "sub-optimal" job match.

[19]The most generous bonuses with the longest allowable job-search periods produced the largest impacts, but their costs exceeded the unemployment benefit savings they produced (Meyer, 1995).

[20]Ruhm, 1991.

[21]See Baily, Burtless, and Litan, 1993.

job loss from the Canada–U.S. Free Trade Agreement and, subsequently, the North American Free Trade Agreement (NAFTA).[22]

To address these social, economic, and political issues, and to assess the potential for cost savings in unemployment benefits, HRDC asked SRDC in 1994 to conduct a demonstration project that would test a new re-employment incentive — an earnings supplement. This supplement was designed to make up part of the loss incurred by displaced workers who found a new job quickly and took a pay cut in order to do so. The demonstration was named the Earnings Supplement Project, or ESP.

Two years later, new federal legislation was passed to reform the Canadian unemployment insurance system. The resulting *Employment Insurance Act* of 1996 changed the distribution, size, duration, and eligibility conditions for unemployment benefits. It also authorized funds for active re-employment programs, including temporary earnings supplements such as that tested by ESP. Thus, findings from the project provide valuable insights into future programs that could be funded under the EI legislation.

SITES, SAMPLES, AND DATA SOURCES

HRDC determined the number and location of ESP sites, or HRCCs. The number of HRCCs reflected a trade-off between the need for adequate sample size and generalizability versus the cost of operating a project in multiple locations. The specific HRCCs chosen reflected a desire to include a mix of labour market types plus a desire to represent specific geographic areas. It also reflected HRDC judgements about the willingness of HRCCs to participate, their ability to conduct the demonstration, and their likelihood of generating an adequate sample size.

Because the displaced worker sites are not a probability sample of HRCCs, findings from ESP cannot be generalized to all Canadian displaced workers. Nevertheless, these findings represent a wide variety of people and places.

Local staff at each HRCC used a brief screening interview (which was all that was possible) to identify which unemployment benefit claimants were displaced workers (that is, had been employed continuously by one or more employers for an extended period, but had lost their job permanently). However, they included claimants who expected to be recalled by their previous employer because it was not clear that these expectations would prove to be accurate. Using these procedures, 8,144 persons were enrolled in ESP during roughly a one-year period.

Later, when data on supplement receipt were available, it became clear that very few supplement group members who expected to be recalled used the supplement, whereas those

[22]Several authors have argued that free trade agreements have hurt certain Canadian workers. Jackson (forthcoming) argues that such agreements have created downward pressure on wages, as well as eliminating "policy levers" that might have helped workers. Even Gaston and Trefler, 1997, who favour free trade agreements, report that up to 15 percent of job losses between 1989 and 1993 can be attributed to them. Doern and Tomlin, 1991, argue explicitly that free trade might benefit Canadians as a whole even if it hurts those affected by plant closures. They then argue that government has a role to play in compensating those hurt by such agreements.

who did not expect a recall were several times as likely to do so.[23] Thus, both for conceptual reasons (most persons who expect a recall probably will have no interest in a supplement and, if their recall expectation is accurate, they will have no need for one) and for empirical reasons (very few persons with recall expectations used the supplement), the samples used for this report do not include persons who expected to be recalled.[24]

The baseline sample (the sample for whom baseline data were available) for this report therefore contains 5,912 displaced workers. Half of this sample was randomly assigned to the supplement group and half to the control group. The distribution of the sample across sites was as follows: Granby (347 persons), Oshawa (2,261 persons), Toronto (1,455 persons), Winnipeg (870 persons), and Saskatoon (979 persons).

Table ES.1 compares characteristics of the supplement group and the control group in the baseline sample. These two groups are quite similar, as would be expected from random assignment.[25] In addition, they represent displaced workers who were about roughly split between men and women; spanned a wide range of ages; were relatively well-educated; had been relatively well-paid in their previous job; had worked a long time for their previous employer; typically were not union members (although some were); typically had not received severance (although many had); and were entitled to many weeks of unemployment benefits.

Table ES.1: Characteristics of the Baseline Sample

	Supplement Group (%)	Control Group (%)
ESP site		
Granby	5.9	5.8
Oshawa	38.4	38.0
Toronto	24.4	24.8
Winnipeg	14.3	15.1
Saskatoon	17.0	16.2
Gender		
Male	45.9	45.6
Female	54.1	54.4
Age		
30 years or younger	23.2	22.1
31–44 years	44.3	45.7
45–54 years	20.4	21.1
55 years or older	12.2	11.2

[23]Only 5.7 percent of the supplement group members who had a specific recall date received a supplement, and only 6.8 percent of those who expected a recall but had no specific date received a supplement. In contrast, 20.5 percent of the other supplement group members received a supplement.

[24]The New Jersey, Pennsylvania, and Illinois bonus experiments excluded persons with a specific recall date.

[25]A statistical test was conducted to compare *all* of the characteristics of the supplement group with those of the control group. This test indicated no statistically significant differences. The test was based on a linear probability model, with a dummy variable indicating membership in the supplement group or the control group as its dependent variable, and indicators for each of the categories of each of the baseline characteristics as its independent variables. The joint F-test for the overall statistical significance of the explanatory power of all independent variables provides a direct test of the null hypothesis that the baseline characteristics were the same for the populations represented by the two groups. The p-value for this F-test was 0.78, which indicates that the baseline characteristics for the two groups were not statistically significantly different.

Table ES.1: Characteristics of the Baseline Sample (cont'd)

	Supplement Group (%)	Control Group (%)
Highest education credential		
Less than high school	20.6	19.5
High school	45.7	45.8
College	16.9	17.5
University	16.8	17.2
Number of years worked for last employer		
Less than 1 year	13.7	14.3
1–2 years	13.9	13.9
3–5 years	27.7	27.2
6–9 years	21.9	23.2
10 years or more	22.8	21.4
Average prior weekly insurable earnings		
Less than $200 per week	2.8	2.9
$200–$399 per week	19.4	20.0
$400–$599 per week	30.6	31.0
$600–$799 per week	28.3	27.6
$800 per week or more	18.8	18.4
Received severance upon layoff		
No	58.8	59.3
Yes	41.2	40.7
Union member in last job		
No	82.5	83.1
Yes	17.5	16.9
Weeks of UI benefit entitlement		
38 weeks or less	25.7	25.2
39–42 weeks	52.7	53.6
43–50 weeks	21.6	21.2
Number of household members		
1 person	19.5	18.8
2 persons	31.3	32.4
3 persons or more	49.2	48.8
Number contributing to household income[+]		
1 adult	37.5	35.9
2 adults	54.9	57.7
3 adults or more	7.5	6.4

Note: The statistical significance of differences between the distributions for the supplement group and the control group was calculated using a chi-square test. Statistical significance levels are indicated as: $^{+}$ = 0.10; $^{++}$ = 0.05; and $^{+++}$ = 0.01.

Data for the study were obtained as follows:

- A ***Project Application and Informed Consent*** form was used to collect information about the background characteristics of all 5,912 members of the baseline sample.
- A **Program Management Information System** (PMIS) was used to document supplement receipt, duration, and amount for all 2,960 supplement group members in the baseline sample.

- A **follow-up survey** administered by telephone 15 months after random assignment was used to document job-search behaviour, employment, earnings, and other labour market outcomes for 4,479 members of the baseline sample.
- **Administrative records** from the unemployment insurance system were used to document the receipt, duration, and amount of unemployment benefits for 5,790 members of the baseline sample.
- A **special "mini-survey"** was used to assess how well a sample of 269 supplement group members understood the supplement offer.
- **Focus groups** were used to explore the in-depth personal experiences and reactions to the supplement of 48 supplement group members from Toronto and Oshawa.[26]
- **Interviews with 85 local HRCC staff members** were used to learn how ESP was implemented.
- **Records of ESP orientation sessions and calls to the ESP toll-free information line** were used to learn about how well supplement group members understood the supplement, problems they experienced, and concerns they had with it.
- **Site reports** by project staff members were used to assess how ESP was implemented.
- **In-depth telephone interviews** with 41 persons who had received large supplements that had expired when they reached their two-year time limit were used to explore how the supplement had affected them and how they had adapted to its expiration.

PARTICIPANTS' UNDERSTANDING OF THE PROGRAM

For ESP to provide a fair test, it was necessary for supplement group members to understand the terms and conditions of the supplement offer. They had to understand the financial trade-off between taking a new job and receiving the supplement versus staying unemployed and receiving unemployment benefits, and they had to know how to qualify for supplement payments. This information was provided through six points of contact:

- **ESP application**. HRCC staff briefly described ESP to unemployment benefit claimants who were judged eligible for the program based on their responses to a series of screening questions. Further information was provided in a consent form, which was part of the ESP application, and in a short brochure that was distributed to applicants. Applicants were told that ESP was a research project in which half of those who applied would be offered a chance to receive more money if they quickly found a new full-time job that paid less than their old one.
- **Random assignment notification letter** and **second program brochure**. Applicants who were randomly assigned to the supplement group were informed of this fact by letter roughly eight weeks after they applied to ESP, on average. This notification letter provided detailed information about the date by which they had to find a job to qualify

[26]Focus groups were conducted in Toronto and Oshawa because they were the two largest sites and were located within easy driving distance of each other. Focus groups were not conducted in the other sites because of the additional costs required to do so.

for a supplement, their prior earnings (which would serve as the basis for determining supplement amounts), and the end of the two-year period for which they could receive supplement payments. Enclosed with this letter was a second ESP brochure that explained in more detail how the program worked.

- **ESP orientation**. About four weeks after random assignment, staff in the central ESP office called supplement group members for a brief orientation.[27] More than 7 out of 10 supplement group members received this orientation, almost always within three months of random assignment. Thus, there were at least three months left (and usually more) to act on what had been learned. The orientation usually took two to three minutes, and was guided by a script. Time for questions also was provided.
- **ESP reminder letter** and **third program brochure**. Roughly eight weeks after random assignment, supplement group members who had not yet initiated a supplement received a letter reminding them of the last date by which they could do so. Enclosed with this letter was a third ESP brochure containing answers to the 12 questions most frequently asked about the supplement.
- **Supplement initiation**. Supplement group members who found a job that qualified for a supplement could initiate one by telephoning the ESP payment office. During this call, ESP staff reviewed the supplement requirements and mailed an initiation package. When these packages were returned, ESP staff examined them thoroughly and contacted initiators about any mistakes that had been made (these did not occur often and usually were minor). If an initiation package was not returned, ESP staff tried to call the individual involved to find out what had happened and to clarify any issues that remained.[28]
- **ESP toll-free information line**. Staff in the central ESP office maintained a toll-free ESP information line. Many calls were received, during which questions about the program were answered and further information was provided.

Information from these sources was both widely and well received. Eight out of ten HRCC staff surveyed felt that ESP printed materials answered all relevant questions about the supplement program and 7 out of 10 felt that ESP applicants had knowledge about the program. Nine out of ten supplement group members who were part of the mini-survey remembered receiving an ESP brochure; almost all who remembered felt that the brochure was easy to understand, had enough information about ESP, and succeeded in explaining the program.

Supplement group members who participated in focus groups also had a good basic understanding of ESP. Almost all knew what ESP was about, and more than two out of three understood its specific details.

A similar impression was conveyed by callers to the ESP toll-free information line. Most callers understood the basic provisions of ESP, although some were confused about, or unaware of, specific details.

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[27] Initial plans called for local staff to conduct in-person orientation sessions at each HRCC. Because very few people attended these sessions, however, a centralized telephone orientation system was implemented instead.

[28] Most persons who did not return their initiation package did not meet the minimum 30-hour weekly work requirement, did not experience a re-employment earnings loss, or qualified for a supplement that they felt was too small to warrant submitting the paperwork.

Quantitative evidence about participants' understanding of the supplement offer was obtained from responses to the mini-survey by 269 supplement group members. These responses, summarized in Table ES.2, clearly indicate that displaced workers who were offered a supplement understood its basic terms and conditions. Knowledge of specific details was less widespread, but nevertheless was substantial.

Table ES.2: Participants' Understanding of ESP

Understanding	(%)
Basic provisions	
Percent who knew:	
The month by which they had to find a job	91
They had to work a minimum number of hours per week	88
They could only receive a supplement for a limited time	90
Percent who knew:	
All of the above	76
2 or more of the above	96
1 or more of the above	99
None of the above	1
Specific details of basic provisions	
Percent who knew:	
How many weeks were in their job-search period	56
They had to work full time to receive a supplement	68
The supplement could be received only for two years	60
Percent who knew:	
All of the above	32
2 or more of the above	65
1 or more of the above	88
None of the above	12
Other provisions	
Percent who knew:	
ESP could provide extra money but not job-search help	89
ESP paid only for a re-employment earnings loss	84
ESP would make up only part of this loss	81
ESP would not cover new jobs with a previous employer	73
Percent who knew:	
All of the above	49
3 or more of the above	84
2 or more of the above	96
1 or more of the above	99
None of the above	1

On balance, the weight of evidence from many different sources suggests that members of the ESP supplement group understood the basic terms and conditions of the supplement program. Hence, the ESP evaluation was a fair test of the program in this regard.

RECEIPT OF THE SUPPLEMENT

About 2 out of 10 supplement group members (20.5 percent) received supplement payments. These payments were made for 64 weeks, on average, and totalled $8,705 per recipient. Thus, they provided a substantial source of temporary income for those who received them. About 44 percent of all supplement recipients were still being paid when their supplement expired at the end of the eligibility period. This suggests that most displaced workers who used the supplement did so to maintain their income for as long as possible. They did not make a quick transition to a higher-paying job.

Given that the supplement was so generous, why didn't more persons use it? The main reason was that most supplement group members (57.6 percent) did not find a new full-time job in time to qualify. The remainder comprised mainly two groups: the 26.8 percent of all supplement group members who *qualified for supplement payments* by quickly finding a new full-time job that paid less than their previous one, and the 11.9 percent who *qualified for earnings insurance* by quickly finding a new full-time job that paid as much or more than their previous one. Members of this latter group could initiate a supplement (open a supplement account) to obtain earnings insurance.

Roughly 7 out of 10 displaced workers who qualified for supplement payments actually signed up and received them. Hence, most persons who were due immediate cash payments submitted the required paperwork. Supplement qualifiers with the largest expected payments were the most likely to ask for and submit their paperwork.

When asked why they did not receive supplement payments, about 50 percent of all non-recipients said they could not find a full-time job in time. Another 13 percent said that they had found a job that did not require them to accept lower pay. Another two percent said that they were self-employed, and two percent said that they had returned to their previous employer (neither of which qualified for supplement payments). Another three percent said that they had gone back to school. Very few non-recipients said that they were not interested in the supplement (two percent) and even fewer said that supplements involved too much paperwork (one percent). These findings suggest that the main barrier to supplement use was an inability to find suitable new full-time work in time.

Focus groups made it possible to probe beyond answers such as "I couldn't find a job" to examine what motivated displaced workers and to explore the role of the supplement in their job-search decisions.[29] Almost all focus group participants thought the ESP supplement was a good idea, although few felt that it was relevant to them. Many of these participants said that the supplement offer was "not very important" or "not at all important" while they were searching for jobs. A small fraction of the focus group participants who felt that the supplement was personally relevant often first began to think seriously about it as they approached the end of their six-month ESP job-search period. By this time they had begun to consider the possibility of a lower-paying job more seriously, and they recognized that their supplement "window of opportunity" was closing.

The story was quite different, however, for the earnings insurance feature of ESP. Basically, there was almost no interest in this option. Only one out of eight displaced workers

[29] Although focus groups were held only in Toronto and Oshawa, many themes that emerged were consistent with other findings for the sites not included in the focus groups.

who qualified for earnings insurance by finding a new higher-paying job in time bothered to sign up for it by initiating a supplement. Thus, only 1.5 percent of all supplement group members signed up and, of these, very few actually received a supplement payment.

On the one hand, the low take-up rate for earnings insurance is surprising, given the limited paperwork necessary to initiate a supplement. On the other hand, this phenomenon is a direct extension of the take-up pattern observed for supplement qualifiers — the smaller the expected payment, the lower the likelihood of signing up. In the case of earnings insurance, the immediate cash payment was zero and the take-up rate was very low.

Anecdotal evidence from focus groups, ESP orientation sessions, and calls to the ESP toll-free information line suggested that not everyone was fully aware of the earnings insurance provision of ESP. But even when this provision was explained, there was little interest in it. Some displaced workers who had found new good jobs did not expect to lose them or simply did not want to think about doing so. Therefore, the low take-up rate for earnings insurance appears mainly to reflect a lack of interest, not a lack of knowledge.

In summary, ESP supplement payments were a substantial source of temporary income for the small fraction of displaced workers who received them. In contrast, there was almost no interest in the program's earnings insurance feature.

IMPACTS OF THE PROGRAM

As noted above, ESP offered displaced workers a large financial incentive to find new full-time work as soon as possible. For the typical sample member, working full time with a supplement paid far more than staying unemployed and receiving unemployment benefits (see Table ES.3). And for most new jobs, earnings plus the supplement almost fully replaced past weekly earnings.

But did the supplement offer actually affect job-search behaviour? Did it cause displaced workers to find new full-time jobs more quickly? Did it induce them to take lower-paying jobs? And what effect did it have on the duration and amount of their unemployment benefits?

Partial answers to the first of these questions are provided by responses to the follow-up survey. When asked, 25.3 percent of supplement group members said that the supplement offer had prompted them to start looking for a job sooner than they would have otherwise; 28.5 percent said it had caused them to spend more time each week looking for a job once they had started to do so; 44.3 percent said that it had enabled them to look for a different kind of job; and 64.6 percent said that it had made them more willing to accept a lower-paying job.[30] Thus, few supplement group members felt that ESP had affected the timing or intensity of their job search, but many felt that it had caused them to broaden its scope.[31]

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[30]These answers were in response to yes/no questions about each potential effect on job search.

[31]Findings in Table 6.1 of Chapter 6 indicate that the only statistically significant difference between the job-search activities of supplement group members and control group members was that a few more supplement group members considered new types of jobs (p-value = 0.10).

Table ES.3: Weekly Unemployment Benefits Versus Weekly Earnings plus Supplement Payments for a Typical Supplement Group Member[a]

Re-employment Earnings ($)	Supplement Payment ($)	Earnings plus Supplement ($)	Unemployment Benefit Payment ($)
560	0	560	308
500	45	545	308
400	120	520	308
300	195	495	308
200	250[b]	450	308

[a]The average supplement group member had earned $560 per week in his or her prior job and thus was eligible to receive unemployment benefits of $308 per week.

[b]The $250 maximum weekly supplement payment would apply to weekly earnings below $225.

But these survey questions were asked 15 months after respondents had received their supplement offer and do not address how their attitudes and behaviour may have evolved over time with changes in their personal circumstances and expectations. Hence, they tell only part of the story.

Another part of the story comes from comments by focus group participants. As noted earlier, although most participants understood ESP, many did not pay close attention to it, especially at first. But as they approached the end of the ESP job-search period, some participants said that they began to consider the supplement more seriously.

Figure ES.1 illustrates how this story was reflected in monthly full-time employment rates.[32] During the first several months after random assignment, there was almost no difference between full-time employment rates for supplement group members and control group members.[33] Beginning in months 4 and 5, however, and especially in Month 6, the rate for supplement group members began to exceed that for control group members. This suggests that a few supplement group members may have broadened the range of jobs they would consider, or lowered their minimum acceptable wages, as the time allowed to qualify for a supplement began to run out.

By one year after random assignment, full-time employment rates for the supplement group and the control group were virtually the same once again. In other words, the control group had "caught up" with the supplement group. This pattern of impacts over time suggests that ESP did, in fact, promote rapid re-employment slightly.

One way to summarize this impact is in terms of the percentage of sample members who were employed full time at any point during the first six months after random assignment — 42.3 percent for the supplement group versus 37.9 percent for the control group. This 4.4 percentage point difference[34] was due equally to a shift from part-time to full-time jobs and an overall increase in employment.[35]

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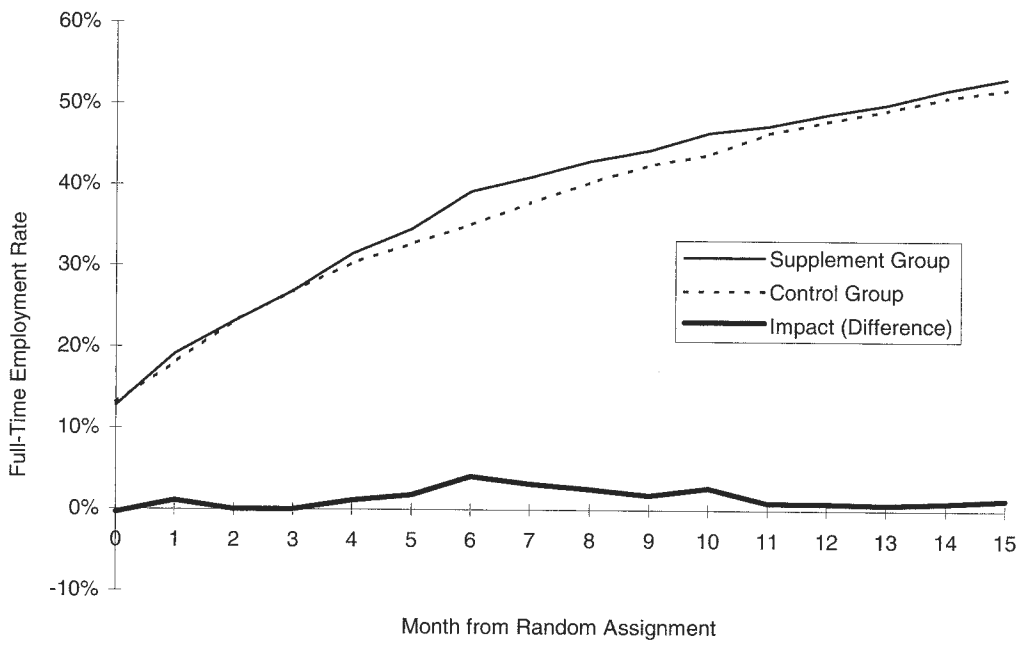
[32]This rate was defined as the percentage of sample members who were employed full time for at least one week during a month.

[33]Almost 20 percent of the supplement group members and the control group members were employed full time by follow-up Month 1 (in the figure) because random assignment occurred eight weeks after ESP application, on average.

[34]The significance level (p-value) for this difference was less than 0.01. It and all other impact estimates reported were obtained from a multiple regression model used to increase the statistical power of the estimates (see Appendix A).

[35]See Chapter 6 for a discussion of how this finding was obtained.

Figure ES.1: ESP Impact on Monthly Full-Time Employment Rates for Sample Members



Although a few supplement group members found new full-time jobs a little sooner than they would have otherwise, their average total earnings for the first 15 months after random assignment was slightly below that for control group members ($14,209 versus $14,891, for a $682 or 4.6 percent difference). Thus, the supplement may have caused some displaced workers to take lower-paying jobs. However, this effect was modest overall, and was not statistically significant.[36] Average hourly earnings for employed members of the supplement group were just below those for employed members of the control group ($12.67 versus $13.00, for a $0.33 or 2.5 percent difference).[37]

Because the modest employment effect of the supplement offer occurred near the end of the six-month ESP job-search period, and because this period typically began two months after ESP application, a total of eight months had elapsed before any effect on employment had materialized. Hence, there was limited time left to reduce unemployment benefits, which may explain why there was no reduction.

On average, supplement group members received $6,460 in benefits during their first 15 months after random assignment versus $6,370 for control group members. The small positive difference ($90 or 1.4 percent) was not statistically significant.[38] Likewise, there was virtually no difference in the duration of benefits paid to recipients (22.1 weeks for supplement group members versus 21.9 weeks for control group members).[39]

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[36]The significance level (p-value) for this difference was 0.12.

[37]The significance level (p-value) for this difference was 0.09. It was not possible to determine how much of the difference represented a supplement-induced shift in who became employed versus a supplement-induced reduction in wages for persons who would have become employed with or without ESP.

[38]For this difference, the significance level (p-value) was 0.50. Note that average benefits include zeros for sample members who received no benefits during the 15-month follow-up period.

[39]The significance level (p-value) for this difference was 0.77.

In summary, ESP may have caused a few displaced workers to broaden their job search as the supplement window of opportunity began to close. This, in turn, expedited full-time re-employment slightly and also may have reduced average wages slightly. However, the program did not reduce unemployment benefit payments.

FINANCIAL IMPLICATIONS OF THE PROGRAM

Supplement group members received $569 more than control group members from earnings, plus supplement payments, plus unemployment benefits during the first 15 months after random assignment.[40] But this $38 monthly difference did not affect the average level of financial hardship experienced. Table ES.4 illustrates this point by comparing supplement group and control group responses to follow-up survey questions about financial hardship.

Table ES.4: ESP Impacts on Financial Hardship

Impact	Supplement Group (%)	Control Group (%)	Impact (%)	Significance Level[b]
Percent who said that losing their job had made them:[a]				
Take in boarders	3.9	3.7	0.2	0.61
Sell stock or other possessions	26.1	25.5	0.6	0.65
Receive financial help from family or friends	29.2	28.9	0.3	0.78
Stop or delay paying bills	32.4	32.3	0.1	0.96
Borrow or take out a loan	40.9	44.1	-3.2**	0.03
Reduce their savings	79.3	81.0	-1.7	0.15
Percent who said they had to cut spending on some things	79.6	81.1	-1.5	0.19
Cut spending specifically on:				
Social activities (including eating out)	56.8	57.0	-0.2	0.89
Food and clothing	42.4	44.8	-2.4	0.10
Leisure (e.g., travelling)	24.3	24.7	-0.4	0.74
Luxuries or unnecessary items	23.6	24.6	-1.0	0.43
Transportation (e.g., gas, car)	13.0	13.7	-0.7	0.54
Housing	5.8	4.7	1.1*	0.10
Other Items	8.2	9.0	-0.8	0.32

[a]All questions refer to the first 15 months after random assignment

[b]The p-value for a two-tailed hypothesis test.

Note: Statistical significance levels for a two-tailed test are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

As can be seen, hardship was widespread and serious for both groups. For every 10 sample members, 8 had to reduce their savings or cut back on spending; 4 had to borrow money or take out a loan; and 3 had to stop or delay paying bills, get financial assistance from friends or family, or sell stock and other possessions. Forced spending cutbacks were most often for social activities and restaurants, food and clothing, leisure and travel, or luxuries and "unnecessary items."

[40]This finding reflects $1,161 in supplement payments, minus a $682 decline in earnings, plus a $90 increase in unemployment benefits. It does not account for taxes and is averaged across all supplement group members, including those with zero supplements, earnings, or unemployment benefits.

That ESP did not affect these outcomes is evidenced by the small differences between the responses of supplement group members and control group members. Only 2 out of 14 responses had statistically significant differences. These two differences suggested a slight overall reduction in financial hardship for supplement group members.

Consistent with these findings is the fact that the supplement group distribution of household income from all sources was not statistically significantly different from that for the control group (analysis not shown).[41]

Note, however, that only the 2 out of 10 supplement group members who received supplement payments benefited directly from them. Supplement recipients received $5,663 during their first 15 months after random assignment, or $378 per month.[42] They also received $21,176 in earnings and $3,630 in unemployment benefits. Thus, supplement payments were a major component of their personal income.

Further evidence of the importance of supplement payments *for recipients* was obtained from in-depth telephone interviews with 31 respondents who had received large supplement payments until they reached their two-year time limit and their supplements had expired.[43] These interviews were conducted three months after the supplement had expired for each respondent.

Most respondents felt that ESP had played a major role in their job decisions, had made a big difference to their finances, and had been quite important for their personal well-being. Furthermore, when asked about losing the supplement, two thirds said that it was "somewhat difficult" and another 13 percent said that it was "very difficult." Many respondents said that losing the supplement had caused them to cut back on "extras."

On balance, it appears that the ESP supplement was very important to the 2 out of 10 displaced workers who received payments and typically relied on them heavily for as long as possible. It did not appear to have a major effect on the re-employment experiences of this group, however.[44]

Since ESP did not reduce unemployment benefits, it did not produce cost savings for the Canadian government. Thus, the supplement program was a net financial cost to the government. Almost all of this cost was due to the direct expense of supplement payments, which averaged $1,161 per supplement group member for the full two-year period, or $8,705 per supplement recipient. A second related government cost was the $89 per supplement group member needed to administer the supplement program.[45]

The supplement payments themselves represent a transfer of resources from taxpayers in general to specific displaced workers because their cost to the government is directly offset by a corresponding benefit to displaced workers. However, the modest expense of

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[41] A chi-square test was used to compare the distribution of reported household income for the supplement group and the control group. The difference was not significant at the 0.10 level.

[42] They were paid a total of $8,705 for the two-year maximum supplement receipt period, or about $363 per month.

[43] This was a sample of 31 people who had started with weekly supplement payments of $100 or more and had exhausted these payments.

[44] The small estimated impact for the overall supplement group suggests that most supplements were received for jobs that would have been taken without the supplement program.

[45] This amount did not include research costs because they would not be part of an ongoing program. It also did not account for the negligible amount of taxes not received by the government because of the small ESP earnings reduction.

administering the supplement program represents a true social cost because the resources used for this purpose were no longer available for other uses.

Therefore, on balance, ESP produced a transfer of resources from the Canadian government to the 2 out of 10 displaced workers who received supplement payments. This transfer helped to compensate supplement recipients for part of the losses they incurred in a way that promoted full-time employment slightly. It did not, however, reduce the costs of unemployment benefits.

HRCC DIFFERENCES IN FINDINGS

ESP was one program with one set of rules and procedures. However, it was operated at five HRCCs in four provinces. The main role of HRCC staff was to recruit, screen, and enrol sample members for the study. Although this process was standardized and fairly simple, there was room for some local variation in how it was implemented. Also, the types of displaced workers at each HRCC and the labour market conditions they faced varied substantially. Hence, responses to the program might have varied as well.

On the other hand, program operations other than sample intake (informing participants about ESP, processing requests for supplement payments, computing payment amounts, and distributing supplement cheques) were conducted centrally by the ESP Payment Centre in Halifax. Hence, there was little room at this stage for HRCC differences.

Table ES.5 illustrates that responses to ESP did indeed vary somewhat by HRCC.[46] The largest response to the program was in Saskatoon and the smallest response was in Toronto. Responses in other HRCCs generally were between these two extremes. For example, the supplement receipt rate was 34.5 percent in Saskatoon versus 15.2 percent in Toronto. Correspondingly, the average supplement payment was $2,048 per supplement group member in Saskatoon versus $878 in Toronto. In both regards, the other HRCCs were between these two extremes.

Estimated supplement impacts varied in a similar way. For example, Saskatoon had the largest impact on the percentage of sample members who became re-employed full time within six months. The 7.3 percentage point increase for Saskatoon was statistically significant. In contrast, Toronto had virtually no impact on early re-employment.

ESP reduced average unemployment benefit payments by $384 per supplement group member in Saskatoon, and the average number of weeks for which unemployment benefit payments were received by 1.3 weeks. These differences, however, were not statistically significant.

[46] An F-test of the differences in HRCC impact estimates indicates that they were not statistically significantly different from each other. However, this test had limited statistical power because of the large sampling error in the HRCC-level impact estimates.

Table ES.5: Selected ESP Findings by HRCC

	Saskatoon	Granby	Winnipeg	Oshawa	Toronto
Percent who received supplement payments (%)[a]	34.5***	30.9***	25.9***	19.1*	15.2
Average total 15-month supplement per supplement group member ($)	2,048	1,756	1,749	943	878
Impact on percent re-employed full time within six months[b] (%)	7.3**	1.5	4.6	4.5**	-0.6
Impact on average total 15-month unemployment benefits ($)	-384	-134	-415	217	386
Impact on average total 15-month unemployment benefit weeks[c]	-1.3	-1.4	-1.4	0.6	1.1
Impact on average total 15-month earnings ($)	15	-1,438	-1,244	-380	-887
Net 15-month financial benefit to displaced workers in the supplement group[d] ($)	1,679	184	90	780	377
Net 15-month government cost per supplement group member[e] ($)	-1,753	-1,711	-1,423	-1,249	-1,353

[a]These rates include supplements received by all supplement group members, including the seven who first initiated "earnings insurance" and later received supplement payments.

[b]Impact on the percentage who became re-employed full time for at least one week during the first six months after the month of random assignment.

[c]Impact on the total number of weeks for which unemployment benefits were received during the first 15 months after random assignment.

[d]The sum of total 15-month supplement payments per supplement group member, plus the impact on average total 15-month earnings, plus the impact on average total 15-month unemployment benefits.

[e]The sum of total 15-month supplement payments per supplement group member, plus the impact on average total 15-month unemployment benefits, plus the average administrative cost of the supplement program.

Note: Statistical significance levels for a two-tailed test are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

Estimates of ESP impacts on average earnings during the 15-month follow-up period exhibited a similar pattern, although none were statistically significant. In all HRCCs except Saskatoon, ESP appears to have reduced earnings somewhat, perhaps because of a tendency for supplement group members to take lower-paying jobs without a countervailing tendency for them to start work sooner. However, ESP had virtually no impact on earnings in Saskatoon.

Overall, ESP produced a net financial benefit of $1,679 per displaced worker in the supplement group at Saskatoon during the 15-month follow-up period. This net benefit was the result of a large average supplement payment, plus a modest reduction in average unemployment benefits, plus no change in average earnings. The net financial benefit of the program was much smaller for displaced workers at the other HRCCs.

Even in Saskatoon, however, ESP did not produce a net cost savings for the government. Because the cost of supplement payments exceeded the program-induced reduction in unemployment benefits, ESP produced a net cost to the government of $1,753 per supplement group member. Net government costs of a similar magnitude were experienced in the other HRCCs.

In summary then, it appears that displaced workers from Saskatoon were most likely to have received supplement payments, experienced the largest program-induced increase in full-time employment, experienced the second largest reduction in unemployment benefits,

and received the largest net positive benefit from ESP. Displaced workers from Toronto were at the opposite extreme, and those from the other HRCCs were generally in the middle. At no HRCC, however, did the program reduce unemployment benefits by enough to cover the costs of supplement payments. Hence, at all sites, the program produced a transfer of resources from the government to supplement recipients.

WHAT HAS BEEN LEARNED ABOUT RE-EMPLOYMENT INCENTIVES FOR DISPLACED WORKERS?

As noted earlier, ESP grew out of research on re-employment bonuses tested in Illinois, New Jersey, Pennsylvania, and Washington State. These studies examined bonuses that typically ranged from $500 to $1,650 for finding a new full-time job within 3 to 13 weeks and holding the new job for four months.

Findings for the first experiment in Illinois were the most encouraging. Its $500 bonus for re-employment within 11 weeks reduced average unemployment benefit payments by roughly one week, or about $200, on average.[47] This exceeded the cost of bonuses paid. The fact that half of the sample members who qualified for bonuses (by finding a new job in time) did not receive them suggests that this finding could have been much less favourable if more eligible claimants had received the bonus.[48]

The later experiments tested different combinations of bonus amounts and job-search periods to help fine-tune the bonus program.[49] Unfortunately, they found very small impacts that did not offset the costs of bonuses paid.[50]

Although these experiments focused mainly on reducing the costs of unemployment benefits, they also examined impacts on earnings because of concern that, by reducing the time spent looking for new jobs, bonuses might cause displaced workers to accept "sub-optimal" jobs. Estimated earnings effects ranged from positive to negative, most were quite small, and few were statistically significant. Hence, there was little evidence of an earnings loss caused by the bonus.

Because ESP was more generous and less restrictive than the re-employment bonuses in several ways, it was hoped that this new incentive program would generate larger impacts. ESP supplement payments could begin immediately upon re-employment within the job-search period and continue for up to two years after random assignment, whereas re-employment bonuses usually were paid only after four months of continual employment. In addition, ESP provided far more time to find a new job — 26 weeks instead of 3 to 13 weeks for the bonuses. And, most importantly, ESP provided much larger payments, totalling $8,705 per supplement recipient instead of $500 to $1,650 per bonus recipient. On the other hand, ESP provided supplements only for jobs that paid less than the ones that were lost. So, in this regard, it was more restrictive than the re-employment bonuses.

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[47]Findings reported in this paragraph are from Meyer, 1996.

[48]Roughly 25 percent of the Illinois program group members found a job in time to qualify for a bonus, but less than 14 percent actually collected a bonus (see Meyer, 1995).

[49]The New Jersey experiment was the only one to include job-search assistance with bonuses (see Corson et al., 1989).

[50]Estimated impacts for the most generous bonus amount and longest job-search period reached the one-week benefit reduction mark achieved in Illinois, but bonus payment costs were higher than in Illinois. Estimated impacts for the other bonus plans were generally much smaller (see Meyer, 1995).

Another important difference between ESP and the bonus experiments was ESP's primary focus on improving the welfare of displaced workers, both by helping them to make successful labour market transitions and by compensating them for the losses they incurred in a way that promoted employment. It was hoped that ESP would reduce the costs of unemployment benefits, but it was acknowledged from the outset that it might not be possible for reductions in unemployment benefits to fully offset the large supplements offered.

The overall effect of ESP was quite small, however. Two out of ten displaced workers who were offered the supplement program actually received supplement payments. The effect of the program was a small increase in early full-time re-employment, with a small countervailing reduction in wages, and no decrease in unemployment benefits. The net financial benefits of the supplement were modestly positive for displaced workers overall, although it was a major source of temporary income for the small fraction who received supplement payments. Furthermore, the supplement was a net cost to the Canadian government.

In summary, both the re-employment bonus experiments and ESP suggest that even a generous re-employment incentive will not make a big difference for large numbers of displaced workers. For whatever reasons — whether due to an inability to find acceptable new jobs, insufficient interest in the incentive, or interest that occurs too late — this approach has to date neither markedly improved labour market outcomes nor substantially reduced unemployment benefits. However, in the words of one ESP supplement recipient, receiving a supplement "was the difference between worrying all the time and not." Thus, supplements can help to cushion the shock as displaced workers make a transition from their past to their future. But supplements are not likely to change this future. Therefore, although re-employment incentives can provide partial compensation for the losses incurred by some displaced workers in a way that does not inhibit their re-employment, these incentives by themselves do not appear to be an adequate policy response to the serious and widespread problems caused by worker displacement.

Chapter 1:
Introduction

The Earnings Supplement Project (ESP) was a multi-site test of an innovative financial incentive designed to stimulate the rapid re-employment of two groups: displaced workers and repeat users of unemployment benefits. ESP was conceived and funded by Human Resources Development Canada (HRDC),[1] and was conducted by the Social Research and Demonstration Corporation (SRDC), a Canadian non-profit social policy research organization.

The project was implemented in nine Human Resource Centres of Canada (HRCCs) located in seven provinces.[2] It comprised two separate studies:

- a study in five HRCCs of displaced workers; and
- a study in four HRCCs of repeat users of unemployment benefits.

The financial incentive tested was an offer to supplement or "top-up" earnings for selected unemployment benefit claimants if they quickly found new full-time jobs that paid less than their old ones. Supplement payments could make up 75 percent of this earnings difference for up to two years, up to a maximum of $250 per week.

The primary goals of the supplement for displaced workers were to shorten their often long and painful re-employment process, and provide them with a temporary source of income in a form that promoted employment. It was hoped that this would help to compensate displaced workers for the losses they incurred due to economic change. In addition, it was hoped that, by encouraging rapid re-employment, the supplement would reduce their use of unemployment benefits.

The primary goal of the supplement for repeat users of unemployment benefits was to stimulate "off-season" employment and promote a shift toward year-round jobs. It was hoped that this would reduce their long-term use of unemployment benefits.

A randomized experiment, the best way known for evaluating program impacts, was used to measure the effects of ESP on full-time employment, earnings, and unemployment benefit payments. Eligible sample members were randomly assigned either to a "supplement group" that was offered the supplement program or a "control group" that was not. Both groups could receive all unemployment benefits and services to which they were entitled normally. Choosing the groups in this way ensured that the supplement offer would be their only initial systematic difference. Thus, comparisons of their future experiences would provide valid estimates of ESP impacts.

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[1] At the time, HRDC was called Employment and Immigration Canada.

[2] At the time ESP began and the sites were selected, local HRDC offices were called Canada Employment Centres (CECs). Part way through the project they were redesignated as HRCCs, so this term is used throughout the present report to be consistent with current usage.

This report, one in a series about ESP, presents findings for displaced workers.[3] It provides the following information:

- a review of the rationale, design, and implementation of the displaced worker study;
- an assessment of participants' understanding of the terms and conditions of the supplement offer;
- documentation of participants' pattern of supplement receipt;
- estimates of ESP impacts;
- an assessment of the financial implications of ESP for displaced workers and the Canadian government; and
- an assessment of what has been learned to date from ESP and related studies.

THE PROBLEM

Displaced workers are individuals who have lost stable, long-term, and often well-paying jobs due to changing technology, increased international competition, or shifting market demand. Worker displacement is a permanent feature of the Canadian economy — one that exists in good times and bad. For example, more than one million Canadians were laid off permanently each year between 1981 and 1991.[4]

When faced with the loss of a long-term job, many displaced workers go through an initial period of shock and disbelief. Consequently, their job search is delayed by false hopes of returning to their old employer. Their lack of knowledge about how to find a new job further compounds this problem. When they do start looking for work, displaced workers often have unrealistic expectations about the wages and benefits they can command. This is especially likely for those with seniority gained by working many years for the same employer.

Hence, although some displaced workers find new jobs quickly, many remain unemployed for a long time and frequently exhaust their unemployment benefits. Some leave the labour force altogether. Many who do become re-employed earn much lower wages than they had earned previously. Therefore, displaced workers often experience considerable economic and personal hardship.

Unemployment insurance, now referred to as Employment Insurance (EI), is the largest source of government assistance for displaced workers. This program pays most claimants 55 percent of their average insurable earnings in the last 20 weeks, for up to 50 weeks.[5] Much has been written, however, about the potential for unemployment benefits to prolong joblessness.[6] Because of this, and because of the high costs of benefit payments,[7] Canadian

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[3]The first ESP report (Bloom et al., 1997) describes the background of the project and examined its implementation. The other report in the series (Tattrie, 1999) presents findings for repeat users of unemployment benefits.

[4]Lauzon, 1995, p. 4.

[5]The actual duration of benefit entitlement depends on how long a claimant has worked prior to applying for benefits and the unemployment rate in the region where the claimant lives.

[6]See Atkinson and Micklewright, 1991; Devine and Kiefer, 1991; and Christofides and McKenna, 1996, for discussions about the work disincentive effects of unemployment benefits.

[7]For example, in fiscal year 1997–98, Canadians received $10 billion in unemployment benefits (HRDC, 1998).

policy-makers have been exploring more active re-employment strategies. This shift in Canadian policy mirrors a similar shift in Europe and the United States.[8]

Studies in the United States have used randomized experiments to test combinations of job-search assistance, occupational skills training, and relocation assistance for displaced workers. These studies have shown that job-search assistance can be a cost-effective way to promote re-employment. These studies have not found occupational skills training to be cost-effective.[9]

Other randomized experiments in the United States have tested re-employment bonuses for displaced workers who found new jobs quickly and stopped receiving unemployment benefits.[10] The first of these studies was conducted in Illinois. It found that a $500 bonus, paid to displaced workers who became re-employed within 11 weeks, reduced unemployment benefit payments by more than the cost of the bonuses paid. The next three studies were conducted in New Jersey, Pennsylvania, and Washington State. These tested bonus amounts ranging from several hundred to several thousand dollars, with job-search periods ranging from 3 to 13 weeks.

These studies focused mainly on the ability of bonuses to reduce unemployment benefit costs.[11] However, only the Illinois study found bonuses to be cost-effective in this regard.[12] The fact that half of the sample members who qualified for bonuses (by finding a new job in time) did not receive them suggests that this finding could have been much less favourable if more eligible claimants had received the bonus.

From another perspective, others have argued that getting displaced workers back to work quickly can improve their future labour market prospects by reducing the "scarring" that can occur from lengthy unemployment.[13] This argument suggests that prolonged unemployment can erode job skills and reduce the perceived value of individuals in the eyes of employers. Therefore, policies that reduce the duration of unemployment can, in theory, increase the future welfare of displaced workers, regardless of whether they reduce unemployment benefit payments.

Furthermore, some have argued that displaced workers should be compensated for the losses they incur because they disproportionately bear the costs of economic adjustment.[14] It is further argued that providing this compensation can reduce the political resistance often associated with proposals for economic change. This argument has particular relevance to the

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[8]For a discussion of these issues in Europe, see OECD, 1990. For a discussion of these issues in the United States, see Ross and Smith, 1993.

[9]Results of these experiments in Texas are reported by Bloom, 1990; in Buffalo, New York by Corson, Long, and Maynard, 1985; and in New Jersey by Corson et al., 1989.

[10]See Meyer, 1995, for a review of these experiments; Woodbury and Spiegelman, 1987, for a discussion of the Illinois experiment; Corson et al., 1989, for a discussion of the New Jersey experiment; Corson et al., 1991, for a discussion of the Pennsylvania experiment; and Spiegelman, O'Leary, and Kline, 1992, for a discussion of the Washington State experiment. The results presented in this section were drawn from these studies.

[11]They also sought to determine whether the bonus caused displaced workers to lower their future earnings by inducing them to choose a job prematurely, and thereby produce a "sub-optimal" job match.

[12]The most generous bonuses with the longest allowable job-search periods produced the largest impacts, but their costs exceeded their unemployment benefit savings (Meyer, 1995).

[13]Ruhm, 1991.

[14]Baily, Burtless, and Litan, 1993, propose that "earnings insurance" be provided to displaced workers because they bear the costs of economic change, the benefits of which accrue to others.

vigorous negative response by many Canadians to the Canada–U.S. Free Trade Agreement and, subsequently, the North American Free Trade Agreement (NAFTA).[15]

To address these social, economic, and political issues, and to assess the potential for cost savings in unemployment benefits, HRDC asked SRDC in 1994 to conduct a demonstration project that would test a new re-employment incentive — an earnings supplement. This supplement was designed to make up part of the loss incurred by displaced workers who quickly found a new job that paid less than their previous one. The demonstration was named the Earnings Supplement Project, or ESP.

Two years later, new federal legislation was passed to reform the Canadian unemployment insurance system. The resulting *Employment Insurance Act* of 1996 changed the distribution, size, duration, and eligibility conditions for unemployment benefits. It also authorized funds for active re-employment programs, including a temporary earnings supplement such as that being tested by ESP. Thus, findings from the project provide valuable information about programs that could be funded under the EI legislation.

THE PROJECT

Displaced workers were enrolled in ESP as part of the regular unemployment benefit claims process at five HRCCs, one in each of the following cities: Granby, Oshawa, Toronto, Winnipeg, and Saskatoon. When individuals applied for regular unemployment benefits at these HRCCs, local staff used a brief screening form to determine whether they met the criteria set for identifying displaced workers. Claimants who met these criteria were encouraged to apply for ESP. When the research team received notice that an ESP applicant's first unemployment benefit cheque had been issued, the applicant was then randomly assigned by computer to the supplement group or the control group.[16] Supplement group members were notified of their selection by letter and sent further information about the project. Control group members were notified by letter that they had not been selected for the program and received no further information about ESP.

Supplement group members who, within 26 weeks, found a new full-time job[17] (with 30 or more hours of work per week), which paid less than their previous weekly insurable earnings, could receive supplement payments equal to 75 percent of the earnings difference. The maximum weekly supplement was $250 and supplement payments could be received for

[15]Several authors have argued that free trade agreements have hurt certain Canadian workers. Jackson (forthcoming) argues that such agreements have created downward pressure on wages, as well as eliminating "policy levers" that might have helped workers. Even Gaston and Trefler, 1997 who favour free trade agreements, report that up to 15 percent of job losses between 1989 and 1993 can be attributed to them. Doern and Tomlin, 1991, argue explicitly that free trade might benefit Canadians as a whole even if it hurts those affected by plant closures. They then argue that government has a role to play in compensating those hurt by such agreements.

[16]To focus the study only on unemployment benefit recipients, as opposed to all claimants, ESP applicants were not randomly assigned until the research team received notice of their first benefit cheque. Claimants with severance payments were an exception to this rule. Because their unemployment benefits could not start until after the allocation of their severance payments ran out (which could take months), these claimants were randomly assigned as soon as their unemployment benefit claim was approved. On average, eight weeks elapsed between ESP application and random assignment, although this period was shorter for claimants with severance than for others.

[17]A new job with their most recent employer at their previous work location was not eligible for supplementation.

up to two years after random assignment.[18] In addition, supplement group members who found full-time employment within 26 weeks, which paid as much or more than they used to make, could initiate a supplement that would "insure" them against a future re-employment earnings loss for up to two years after random assignment. It was hoped that this offer of immediate supplement payments for some, plus earnings insurance for others, would provide a strong inducement to return quickly to full-time work. Furthermore, it was hoped that, by expediting re-employment, ESP would reduce the need for unemployment benefit payments.

Table 1.1 illustrates the magnitude of the ESP re-employment incentive for a typical member of the displaced worker sample. A typical sample member is one who was laid off from a job that had paid $560 per week and was therefore eligible for roughly 38 weeks of unemployment benefits at $308 per week. If, for example, this sample member quickly found a new full-time job that paid $400 per week, he or she would qualify for a $120 weekly supplement payment. Thus, total weekly earnings plus the supplement payment would be $520 or 93 percent of previous earnings. This is considerably more than the $308 in available unemployment benefits.

Table 1.1: Weekly Unemployment Benefits Versus Weekly Earnings plus Supplement Payments for a Typical Sample Member[a]

Re-employment Earnings ($)	Supplement Payment ($)	Earnings Plus Supplement ($)	Unemployment Benefit Payment ($)
560	0	560	308
500	45	545	308
400	120	520	308
300	195	495	308
200	250[b]	450	308

[a]The average displaced worker in the sample had earned $560 per week in his or her prior job, and thus was eligible to receive unemployment benefits of $308 per week.

[b]The $250 maximum weekly supplement payment took effect for weekly earnings below $225.

Note: Earnings, supplement payments, and unemployment benefits are all subject to income taxes.

THE POTENTIAL IMPACT

Table 1.1 clearly illustrates that, for almost any weekly earnings amount, the supplement plus earnings would replace most of a displaced worker's prior weekly wages and greatly exceed weekly unemployment benefits. Furthermore, findings in Chapter 5 indicate that ESP supplement payments totalled $8,705 per recipient — a substantial amount.

Because ESP was in most ways more generous and less restrictive than the re-employment bonuses tested previously, it was hoped that this new incentive would generate larger impacts. ESP supplement payments could begin immediately upon re-employment and continue for up to two years after random assignment, whereas most of the bonuses tested could be paid only after four months of continual employment. ESP also provided far more time to find a new job, 26 weeks instead of 3 to 13 weeks for the bonuses. Most importantly, ESP provided much larger payments, averaging $8,705 per supplement recipient instead of $500 to $1,664 per bonus recipient. On the other hand, ESP provided supplements only for

[18]In calculating re-employment earnings losses, previous earnings were capped at the maximum level covered by unemployment benefits, which was $42,380 per year or $815 per week when ESP began in 1995.

jobs that paid less than the ones that were lost. So, in this important regard, it was more restrictive than the re-employment bonuses.

Another important difference between ESP and the bonus experiments was ESP's primary focus on improving the welfare of displaced workers, both by helping them to make successful labour market transitions and compensating them for the losses they incurred in a way that promoted re-employment. It was hoped that ESP would reduce unemployment benefit payments, but it was acknowledged from the outset that it probably could not reduce these payments by enough to fully offset the costs of supplements paid.

While there were reasons to believe that ESP might provide an effective way to encourage displaced workers to become re-employed quickly, there also were reasons for approaching this idea with caution before implementing it as part of a national program. The first reason was the potentially high costs of making supplement payments, especially for payments made to displaced workers who would have taken new lower-paying jobs quickly, even without a supplement. Thus, it was important to determine the overall costs of supplement payments as well as the impact of ESP on full-time employment.

A second important reason for testing the supplement was the possibility that it might cause displaced workers to cut their job search "too short." If this happened, they might miss out on better jobs that they would have found by searching longer. For example, some might prematurely accept low-paying, dead-end jobs that would not provide opportunities for advancement. Furthermore, being employed in these jobs might carry a stigma that would reduce their probability of being hired for better jobs in the future.

DATA SOURCES

The randomized experiment used to evaluate ESP produced an analysis sample of 5,912 displaced workers from the five HRCCs in the study. Half of the sample was randomly assigned to the supplement group and half to the control group. By comparing the experiences of these two groups during the first 15 months after random assignment, it was possible to estimate the impacts of ESP on these experiences. Several additional components of the evaluation were used to probe more deeply into the attitudes, behaviours, and situations that may have influenced how displaced workers coped with the problems they faced, and how they were affected by ESP. The following data sources were used for the analyses in this report:[19]

- A ***Project Application and Informed Consent*** form was used to collect information about the background characteristics of all sample members.
- A **Program Management Information System** (PMIS) was used to document which supplement group members received supplement payments, how long they received these payments, and how much they received.
- A **follow-up survey** administered by telephone 15 months after random assignment was used to document job-search behaviour, employment, earnings, and other labour market outcomes for members of both the supplement group and the control group.

[19]Statistics Canada collected the survey data and obtained the data from administrative records. SHL Systemhouse developed and maintained the PMIS.

- **Administrative records** from the unemployment insurance system were used to document the duration and amount of unemployment benefits received by members of the supplement group and the control group.
- A **special "mini-survey"** of 269 supplement group members was used to document how well they understood the terms and conditions of the supplement offer.
- **Focus groups** were used to explore the personal experiences and reactions to ESP of 48 supplement group members from Toronto and Oshawa.[20]
- **Interviews with 85 local HRCC staff members** were used to learn about the process of implementing ESP.
- **Records of ESP orientation sessions and calls to the ESP toll-free information line** were used to learn how well supplement group members understood the program, and about problems they experienced and concerns they had with it.
- **Site reports** by project staff members were used to document the process of implementing ESP.
- **In-depth telephone interviews** with 41 persons who had received large supplements that had ended when they reached their two-year time limit were used to explore how the supplement had affected them and how they had adapted to its expiration.

THIS REPORT

This report provides a detailed account of ESP's effects on displaced workers. Chapter 2 describes how the project was operated and Chapter 3 describes its evaluation. Chapter 4 presents an analysis of how well displaced workers understood the terms and conditions of ESP to determine whether the project was a "fair test." Chapter 5 examines the percentage of supplement group members who received supplement payments and the percentage who initiated a supplement (opened a supplement account) in order to receive earnings insurance. It also examines the duration and amount of supplements paid. Chapter 6 presents estimates of ESP impacts on job-search behaviour, employment, earnings, wage rates, and unemployment benefits. Chapter 7 examines the financial implications of these findings for displaced workers and the Canadian government. Chapter 8 examines how ESP findings varied across the five HRCCs in the study. Chapter 9 concludes by summarizing what has been learned to date about re-employment incentives for displaced workers.

[20]Focus groups were conducted in Toronto and Oshawa because they were the two largest sites and were located close to each other.

Chapter 2:
The Program

This chapter describes the ESP program for displaced workers. First it describes how Human Resource Centres of Canada (HRCCs) were selected for the project and how displaced workers were identified. It then describes how the sample of displaced workers was enrolled at each HRCC and how sample members were randomly assigned to the supplement group or control group. Next it describes how information about ESP was provided to supplement group members. Finally, it outlines the steps that were required to initiate a supplement and receive supplement payments.

CHOOSING THE HRCCS

Human Resources Development Canada (HRDC) determined the number and location of HRCCs that participated in the project. This number reflected a balance between the need for adequate total sample size and generalizability versus the cost of operating a project in multiple locations. The specific HRCCs chosen reflected a desire to include a mix of labour market types plus a desire to represent specific geographic areas. It also reflected HRDC's judgments about the HRCCs' willingness to participate, their ability to conduct the demonstration, and the likelihood of their generating an adequate sample size.

Table 2.1 identifies the HRCCs that were selected. Because HRCCs were selected purposively, ESP findings cannot be generalized directly to all Canadian displaced workers. Nevertheless, the HRCCs chosen represented very different people and places, and thus the findings from the study should be broadly representative.

Table 2.1: Displaced Worker Sites (HRCCs)

Granby, Quebec
Oshawa, Ontario (including branch offices in Whitby and Pickering)
Toronto, Ontario (the Toronto Centre HRCC and the Dufferin Street branch office)
Winnipeg, Manitoba (the combined Winnipeg West and Winnipeg North HRCCs)
Saskatoon, Saskatchewan (including a branch office in Humboldt)

DEFINING THE POPULATION AND ENROLLING THE SAMPLE

Displaced workers are typically defined as individuals who have been employed continuously by one or more employers for an extended period, but have lost their jobs permanently. Although this definition is simple in principle, it was difficult to implement in practice. Thus, while the project was underway, it was necessary to revise the applicant

selection criteria.[1] These criteria were used in the course of a brief interview conducted by local HRCC staff to screen unemployment benefit claimants for ESP eligibility. The final set of criteria included only claimants who:

- had been employed continuously during the past 36 months by one or more employers; and
- had not received regular unemployment benefits during the preceding 36 months, or had received benefits only for temporary layoffs, after which they had returned to their previous employer and did not now expect to be able to return to their most recent employer.

Individuals who met these criteria were encouraged by HRCC staff to apply for ESP as well as for unemployment benefits. All others applied only for unemployment benefits.

Those who met the criteria were asked to complete a project application form and given a project brochure to take home. The application form contained 24 questions that asked about applicants' most recent job, educational background, household composition, and demographic characteristics. The form also asked applicants to give signed consent for their participation in the ESP research and for the release of administrative data about them for the research. The brochure provided basic information about the project to help interest applicants in participating.

During the early part of the sample intake process, individuals who expected to return to their previous employer were allowed to apply for ESP because it was not clear that their expectations would prove to be accurate. Including these individuals, a total of 8,144 persons were enrolled in the study during the roughly one-year sample intake period. All enrollees were randomly assigned to either the supplement group or the control group.

Later, when data on supplement receipt were available, it became clear that very few supplement group members who expected to return to their previous employer used the supplement, whereas those who did not expect a recall were several times more likely to do so.[2] Thus, for both conceptual reasons (most persons who expect a recall probably will have no interest in a supplement and, if their recall expectation is accurate, will not need one) and empirical reasons (few persons with recall expectations used the supplement), the samples used for this report include only the 5,912 ESP enrollees who did not expect to return to their previous employer. Appendix B presents selected findings for the remaining 2,232 enrollees who did expect a recall.

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[1]Originally, eligibility for ESP required having not received regular unemployment benefits during the past three years because this was thought to be a good proxy for continuous employment. However, this criterion was a poor measure for workers who typically received benefits when their plants shut down for retooling because these individuals maintained an employer-employee relationship and, thus, stable employment, even while they were receiving unemployment benefits. Therefore, the second version of the screening criteria accepted workers if they had received unemployment benefits for temporary layoffs and had subsequently returned to their same job. However, HRCC staff objected to the resulting increase in their workload and were concerned that this criterion would open ESP to seasonal workers. Thus, the criteria were revised a final time to accept workers who had previously received regular unemployment benefits for temporary layoffs only if they did not expect to return to their most recent employer. This probably best emulates how an ongoing program might select displaced workers.

[2]Only 5.7 percent of the supplement group members who had a specific recall date received a supplement, and only 6.8 percent of those who expected a recall but had no specific date received a supplement. In contrast, 20.5 percent of the supplement group members who did not expect to be recalled received a supplement (see Chapter 5).

CONDUCTING RANDOM ASSIGNMENT

Applicants were not fully eligible for ESP until their unemployment claims were finalized and their first benefit cheques had been issued. Hence, they were not randomly assigned until Statistics Canada received notification from automated data sent by the HRDC regional computer centres that their benefit claim had been approved and a cheque had been issued. Table 2.2 summarizes the amount of time this took. As can be seen, an average of eight weeks elapsed between application and random assignment, although there was considerable variation across sample members.

Table 2.2: Number of Weeks between ESP Application and Random Assignment

Number of Weeks	Percentage of Sample
Less than 3 weeks	0.1
3–5 weeks	32.5
6–8 weeks	41.4
9–11 weeks	14.0
12–14 weeks	5.3
15 weeks or more	6.7

There were several reasons for the lag between ESP application and random assignment. Some applicants received vacation pay when they were laid off, and this delayed the start of their unemployment benefit payments. Others found temporary part-time jobs that made them ineligible to receive benefit payments for awhile. In addition, during busy periods of application for unemployment benefits, the processing of claims took longer than usual. Furthermore, some benefit claims, such as those for individuals who were fired, took a long time to resolve because they were contentious.

Applicants for unemployment benefits who receive severance payments from their previous employer are subject to an "allocation of earnings on separation." The amount of severance is divided by average weekly earnings and the claimant is deemed to be still earning during the period of severance allocation. Unemployment benefits are not paid until this allocation period ends, which can take many months. Hence it was decided that ESP applicants who were receiving severance (41 percent of the baseline sample) would not have to receive an unemployment benefit cheque to become eligible for ESP. Instead, random assignment was conducted as soon as their unemployment benefit claim was approved.

PROVIDING INFORMATION TO PARTICIPANTS

For ESP to be a "fair test," it was necessary for supplement group members to understand its terms and conditions. In particular, they had to understand the financial trade-off between taking a new job and receiving a supplement, and staying unemployed and receiving unemployment benefits. They also had to know how to qualify for and receive supplement payments. This information was provided mainly through six points of contact:

- **ESP Application**. As discussed above, HRCC staff briefly described ESP to unemployment benefit claimants who were potentially eligible. Further information was provided through the ESP application package and a short brochure. The brochure

explained that ESP was a research project in which half of the enrollees would be offered an opportunity to receive extra money if they found a new full-time job that paid less than their previous one.[3] The brochure also listed a toll-free number to call for information. A conscious effort was made to limit the information provided at this stage, however, to minimize the disappointment of applicants who subsequently would be randomly assigned to the control group.

- **Random Assignment Notification Letter and Second Program Brochure**. On average, about eight weeks after applying for ESP, individuals were randomly assigned to either the supplement group or the control group. Those assigned to the supplement group were informed of this by letter. The letter provided detailed information about the deadline (a specific date) for finding a job to qualify for a supplement, their prior earnings amount (which would serve as the basis for determining supplement amounts), and the date when the two-year period during which they could receive supplement payments would end. Enclosed with this letter was a second ESP brochure that provided a more detailed explanation of how the supplement worked.

- **ESP Orientation**. About four weeks after random assignment, staff in the central ESP office called supplement group members for a brief orientation session.[4] More than 7 out of 10 supplement group members received this orientation, almost always within three months of random assignment. Thus, there were at least three months left (and usually more) to act on what had been learned. The orientation took two to three minutes, and was guided by a script that covered key points about the supplement. Time also was provided for answering questions.

- **ESP Reminder Letter and Third Program Brochure**. Roughly eight weeks after random assignment, supplement group members who had not yet initiated a supplement received a letter reminding them of the last date by which they could do so. Enclosed with this letter was a third ESP brochure containing answers to the 12 questions most frequently asked about the supplement.

- **Supplement Initiation**. When supplement group members found a job and telephoned to initiate a supplement (as described in the next section), ESP staff reviewed the supplement requirements to confirm eligibility and then mailed out an initiation package.[5] If a package was not returned, ESP staff tried to call the individual to find out what had happened and clarify any remaining issues.[6]

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[3]The brochure also noted that, as part of the research, Statistics Canada would interview all enrollees and obtain information from HRDC about their unemployment benefit histories.

[4]Initial plans required local staff to conduct in-person orientation sessions at each HRCC. However, since very few applicants attended these sessions, a centralized telephone orientation system was implemented.

[5]The initiation package contained an *Employment Registration Form* (to record information about the eligible job), a *Project Participation Agreement* (to certify that the participant understood the rules governing supplement receipt and to allow data sharing with HRDC and Revenue Canada), an initial voucher (to indicate the participant's hours of work and gross wages earned in the supplementable pay period), a Revenue Canada *TD1 Form* (to collect information for deducting the correct amount of income tax from the supplement), a *Direct Deposit Request Form*, and an *Instruction Guide*.

[6]Most individuals who did not return their initiation package did not meet the 30-hour weekly work requirement, did not experience a re-employment earnings loss, or qualified for a supplement that they felt was too small to warrant submitting the paperwork.

- **The ESP Toll-Free Information Line**. Staff in the central ESP office maintained a toll-free information line. Many calls to this line were received, during which questions about ESP were answered and further information about the program was provided.

PAYING THE SUPPLEMENT

Members of the supplement group could initiate a supplement by telephoning the ESP payment office in Halifax. After verifying eligibility, ESP staff mailed out an initiation package. Packages were generally returned within three weeks.[7] ESP staff examined them thoroughly and contacted individuals about any mistakes (these were infrequent and typically minor). If participants who initiated a supplement subsequently changed jobs or experienced breaks in their employment, they were required to complete a new initiation package.

Supplement payments could begin only after all required data had been entered into the ESP payment system. Once participants had successfully qualified for a supplement, they could be paid for all subsequent periods in which they were eligible. Earnings and employment were self-reported, but this information had to be substantiated by pay stubs from employers. Recipients were sent a six-month supply of vouchers with pre-printed information about their employer. To make a claim for payment, they simply had to complete a voucher for each pay stub and send it to the ESP payment office with a copy of the pay stub. The voucher contained information necessary to verify that program rules had been met and to calculate the supplement amount. This information, such as paid hours of work and gross earnings, was usually copied by participants directly from their pay stubs. ESP staff then used the pay stub to verify the information reported on the voucher before entering it into the payment system.

The payment system calculated a supplement payment for each voucher. The system first checked that the participant had been paid at least the minimum wage and had worked at least 30 hours a week. The minimum wage verification was straightforward;[8] but verifying the minimum 30-hour work week was not as simple.

A two-stage process was used to check hours of work. The first stage checked whether 60 or more hours had been worked during the two-week accounting period; if so, the requirement was met for both weeks. If less than 60 hours were reported for this two-week period, the system conducted a week-by-week analysis to identify whether the 30-hour requirement had been met in either week.[9]

Once the payment system determined that all program requirements had been met, it calculated the supplement amount for the eligible period (either two weeks or one week). This was done by subtracting total gross earnings in the new job from average insurable earnings in the old job. The gross supplement amount was then computed as 75 percent of this difference, up to a maximum of $250 for one week or $500 for two weeks. Because the supplement payment was subject to income tax, the amount of the tax was subtracted from

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[7]This is an estimate by ESP payment staff.

[8]The system calculated the hourly wage rate (gross earnings divided by total hours) and determined whether it met the minimum wage for the province of employment.

[9]Consider, for example, a participant who had worked 15 hours in the first week and 35 hours in the second. This participant would fail the first system check because total hours worked was less than 60. However, the second check would indicate eligibility for one week of supplement payments.

the gross supplement amount to yield the net supplement payment. The tax rate used for this purpose was based on information obtained from supplement recipients. The supplement payment was not, however, considered part of earnings for the purpose of determining future unemployment benefit amounts.

Supplement cheques were issued monthly during the first year of program operation. This was then changed to twice a month so that recipients who were late submitting their vouchers did not have to wait a full month to receive cheques. Information that explained how the payment was determined was included with each cheque.

IMPLEMENTING THE PROGRAM

Although in concept ESP was fairly simple, in practice it involved many steps and required co-operation from many people. For example, because sample intake was implemented separately by local HRCCs, there was room at this stage for variation in the quality of the process. Nevertheless, an assessment of ESP implementation found that all five HRCCs were able to conduct sample intake successfully even though they experienced major administrative changes and staff reductions while the project was underway.[10]

Other components of the program were implemented centrally by ESP project staff. Hence, they were easier to control and monitor, although in some ways they were more complicated than sample intake. The assessment of program implementation found that these steps also were executed successfully, even though problems arose that required attention.[11]

[10]Bloom et al., 1997.
[11]Ibid.

Chapter 3:
The Evaluation

This chapter introduces the analytic components of the ESP evaluation.[1] These components focus on participants' understanding of the supplement offer; their patterns of supplement receipt; the impacts of the supplement program on their job search, employment, earnings, and unemployment benefit payments; and the financial implications of the program. For each analytic component, the chapter briefly describes the questions posed, the data sources and samples used, and relevant methodological issues. It also describes several special studies that explored the personal experiences of displaced workers and the role that ESP played for them. The chapter concludes with a summary of data sources and samples used for the ESP evaluation.

ANALYSIS OF PARTICIPANT KNOWLEDGE

The extent to which displaced workers understood ESP is examined in Chapter 4. The analyses described there are designed to determine whether members of the supplement group understood ESP's terms and conditions well enough for it to provide a "fair test" of the supplement concept. Findings are based mainly on responses to a special "mini-survey" administered to a small sample of supplement group members about seven weeks after random assignment.[2]

Questions Addressed

Part 1 of the mini-survey asked respondents about their reactions to the ESP informational brochures. Part 2 focussed on respondents' ability to assess the benefits of ESP and their understanding of how to qualify for supplement payments. Part 3 focussed on how well they understood their unemployment benefits. Part 4 asked how ESP had affected respondents' job search, and whether they expected to use a supplement.

The Sample

The mini-survey attempted to interview all displaced workers who were randomly assigned to the supplement group between December 4, 1995 and January 5, 1996. The sample was limited to one monthly cohort to minimize the time and cost of fielding the survey. The mini-survey sample used for this report comprised 269 respondents from a total of 308 survey attempts, for an overall response rate of 87.3 percent.

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[1] As noted earlier, Bloom et al., 1997, provides a detailed analysis of ESP's implementation. Thus an implementation analysis is not included in this report, although key implementation findings are presented where relevant.

[2] Bloom et al., 1997, presents a similar analysis of ESP knowledge, but their sample included individuals who expected a recall. The sample used for the present report does not include such individuals. However, the results for both samples were quite similar.

Table 3.1 compares characteristics of the mini-survey sample with those of the full supplement group. In most regards, the two samples look quite similar. Thus, it seems reasonable to generalize findings from the mini-survey for the full supplement group. Therefore, these findings probably yield a good indication of how well the average supplement group member understood ESP.

Table 3.1: Characteristics of the Mini-Survey Sample and the Supplement Group in the Baseline Sample

Characteristics	Mini-Survey Sample (%)	Supplement Group (%)
ESP site		
Granby	6.7	5.9
Oshawa	40.9	38.4
Toronto	20.4	24.4
Winnipeg	13.4	14.3
Saskatoon	18.6	17.0
Gender		
Male	39.8	45.9
Female	60.2	54.1
Age		
30 years or less	21.6	23.2
31–44 years	43.5	44.3
45–54 years	21.9	20.4
55 years or older	13.0	12.2
Highest education credential		
Less than high school	21.9	20.6
High school	44.6	45.7
College	15.6	16.9
University	17.8	16.8
Number of years worked for last employer		
Less than 1 year	16.2	13.7
1–2 years	15.4	13.9
3–5 years	24.4	27.7
6–9 years	22.6	21.9
10 years or more	21.4	22.8
Average prior weekly insurable earnings		
Less than $200 per week	3.7	2.8
$200–$399 per week	20.1	19.4
$400–$599 per week	29.4	30.6
$600–$799 per week	28.6	28.3
$800 per week or more	18.2	18.8
Received severance upon layoff		
No	59.1	58.8
Yes	40.9	41.2
Union member in last job		
No	86.2	82.5
Yes	13.8	17.5
Weeks of UI benefit entitlement		
38 weeks or less	26.5	25.7
39–42 weeks	50.8	52.7
43–50 weeks	22.7	21.6

Table 3.1: Characteristics of the Mini-Survey Sample and the Supplement Group in the Baseline Sample (cont'd)

Characteristics	Mini-Survey Sample (%)	Supplement Group (%)
Number of household members		
1 person	16.0	19.5
2 persons	31.3	31.3
3 persons or more	52.6	49.2
Number contributing to household income		
1 adult	32.4	37.5
2 adults	56.1	54.9
3 adults or more	11.5	7.5

Note: Due to rounding, percentages may not add up to exactly 100 percent.

ANALYSIS OF SUPPLEMENT RECEIPT

This analysis describes how many supplement group members received supplement payments, how long they received these payments, and how much they received. Chapter 5 presents these findings.

Questions Addressed

The analysis of supplement receipt addressed questions such as:

- What percentage of supplement group members received payments? What factors were related to whether they received these payments? What percentage of supplement group members initiated a supplement just to get earnings insurance?
- How long were supplement payments typically received? Did recipients use these payments for a short period, as a temporary source of income until they found new higher-paying jobs, or did they rely on these payments for a long time? What percentage continued to receive payments until they reached their two-year time limit?
- How much in total was the typical supplement recipient paid per week?

Data and Samples

Data for the analysis of supplement receipt were obtained from the ESP Program Management Information System (PMIS) maintained by SHL Systemhouse. This system was designed to make sure that: only eligible supplement group members were paid; these individuals were contacted on schedule and received all necessary information; all documentation for supplement payments was provided; and payment amounts were computed appropriately.

PMIS data for this report were obtained for all 2,960 supplement group members in the baseline sample. These data were used to measure the percentage of supplement group members who received supplement payments and the percentage who initiated a supplement in order to receive earnings insurance. They were also used to measure the duration and amount of supplement payments.

ANALYSIS OF PROGRAM IMPACTS

The impacts of ESP (examined in detail in Chapter 6) represent the changes in the behaviour and experiences of displaced workers that were caused by the program and, thus, would not have occurred without it. To estimate these impacts, the ESP evaluation was conducted as a randomized experiment, which is the best way known to obtain valid program impact estimates. To understand why, consider the following explanation.

Some displaced workers will find new jobs on their own and stop receiving unemployment benefits. Others will gain access to existing services that will help them to find new jobs. Thus, measuring the impacts of ESP required being able to compare what displaced workers achieved under the new program with what they would have achieved by themselves, or through help from existing programs. The best way to do this was to compare the future experiences of the two groups randomly selected from ESP applicants: a supplement group that was offered the program and a control group that was not. This evaluation design is referred to as a randomized experiment.

By randomly assigning eligible displaced workers to either a supplement group or a control group, the only systematic difference between the two groups will be that one receives the ESP offer and the other does not.[3] Because of this, the subsequent experiences of control group members will represent what supplement group members would have experienced without ESP. Therefore the *difference* between these experiences will provide a valid estimate of the difference *caused* by ESP — its *impacts*.

Consider the following example. Assume that 50 percent of the supplement group members became re-employed within six months of random assignment. This represents an *outcome* of ESP. It indicates what happened to supplement group members, but does not indicate what happened to them *because of* ESP. For example, if 40 percent of the control group members had become re-employed within six months of random assignment, 40 percent of the supplement group members, on average, would have become re-employed without ESP.[4] The impact of ESP in this case would therefore be an increase in the re-employment rate of 10 percent — not the full 50 percent observed for the supplement group. Thus, if the experience of control group members were not taken into account, the impact of ESP would be greatly overstated.

Researchers have tried many "quasi-experimental" alternatives to random assignment for constructing comparison groups to estimate program impacts. But no alternatives have been as successful as random assignment. The basic problem with these alternatives is "selection bias" created by pre-existing differences between the program group and the comparison group. It is possible to use statistical matching and modelling to reduce some of the differences that can be measured well. But it is not possible to eliminate, with confidence, differences that cannot be measured well or at all, such as motivation or state of mind. In fact,

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[3]Strictly speaking, even though the *expected values* of the means for all pre-existing characteristics of the supplement group and the control group are the same, their *actual values* may differ, especially in small samples. Given the large samples used for this analysis, however, the actual pre-existing characteristics of the supplement group and the control group did not differ appreciably.

[4]What would have happened to program group members in the absence of a program is often referred to as the "counterfactual."

without random assignment, it is not possible to know how many of these differences have been removed.[5]

Randomly assigning the supplement program to eligible ESP applicants also made it possible to estimate program impacts for any subgroup of the study sample that could be defined in terms of baseline characteristics. For example, it was possible to obtain valid impact estimates for subgroups defined in terms of their age, educational background, and so on.

Not only was random assignment a methodologically sound way to measure the impacts of ESP, but it was also a fair way to allocate the limited number of ESP supplement offers. All eligible ESP applicants had the same probability of receiving the offer and only chance determined who did so.

Questions Addressed

The ESP impact analysis addressed the following questions:

- Did ESP expedite full-time re-employment for displaced workers? If such an effect occurred, to what extent was it due to a shift from part-time to full-time jobs versus an increase in overall employment?
- How, if at all, did ESP affect the earnings and wage rates of displaced workers? Did the program cause them to take lower-paying jobs? If so, did this cause them to earn less than they would have otherwise during the follow-up period, or did starting work sooner offset this potential loss of earnings?
- Did ESP reduce displaced workers' reliance on unemployment benefits? Did it reduce the amount of time they received benefits or the total amount they received?
- If impacts were experienced, to what extent did they vary by site and by subgroup of displaced workers?

The Baseline Sample

The baseline sample for this report (the sample for which baseline data were available) comprised 5,912 displaced workers. There were 347 sample members from Granby, 2,261 from Oshawa, 1,455 from Toronto, 870 from Winnipeg, and 979 from Saskatoon. Half of the sample members were randomly assigned to the supplement group and half to the control group.

Table 3.2 presents selected characteristics of the supplement group and the control group. For each characteristic (e.g., Age or Highest educational credential) the distribution of supplement group members was compared with the distribution of control group members. As can be seen, the differences were very small in all cases, and were statistically significant in only one (Number contributing to household income). This one significant difference was itself quite small, however, and could have occurred by chance given the numerous

[5]A special committee of the U.S. National Academy of Sciences concluded that "control groups created by random assignment yield research findings about employment and training programs that are far less biased than results based on any other method . . . 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 . . ." (Betsey, Hollister, and Papageorgiou, 1995, p. 18).

significance tests reported in the table. Thus, a single joint test was conducted of the overall supplement and control group difference for all baseline characteristics. This test indicated that there was no statistically significant difference between the two groups.[6]

Table 3.2: Characteristics of the Baseline Sample

Characteristics	Supplement Group (%)	Control Group (%)
ESP site		
Granby	5.9	5.8
Oshawa	38.4	38.0
Toronto	24.4	24.8
Winnipeg	14.3	15.1
Saskatoon	17.0	16.2
Gender		
Male	45.9	45.6
Female	54.1	54.4
Age		
30 years or less	23.2	22.1
31–44 years	44.3	45.7
45–54 years	20.4	21.1
55 years or older	12.2	11.2
Highest education credential		
Less than high school	20.6	19.5
High school	45.7	45.8
College	16.9	17.5
University	16.8	17.2
Number of years worked for last employer		
Less than 1 year	13.7	14.3
1–2 years	13.9	13.9
3–5 years	27.7	27.2
6–9 years	21.9	23.2
10 years or more	22.8	21.4
Average prior weekly insurable earnings		
Less than $200 per week	2.8	2.9
$200–$399 per week	19.4	20.0
$400–$599 per week	30.6	31.0
$600–$799 per week	28.3	27.6
$800 per week or more	18.8	18.4
Received severance upon layoff		
No	58.8	59.3
Yes	41.2	40.7

[6]This test was based on a linear probability model, with a dummy variable indicating membership in the supplement group or the control group as its dependent variable and indicators for each of the categories of each of the baseline characteristics as its independent variables. The joint F-test for the overall statistical significance of the explanatory power of all independent variables provides a direct test of the null hypothesis that the baseline characteristics were the same for the populations represented by the two groups. The p-value for this F-test was 0.78, which indicates that the baseline characteristics for the two groups were not statistically significantly different.

Table 3.2: Characteristics of the Baseline Sample (cont'd)

Characteristics	Supplement Group (%)	Control Group (%)
Union member in last job		
No	82.5	83.1
Yes	17.5	16.9
Weeks of UI benefit entitlement		
38 weeks or less	25.7	25.2
39–42 weeks	52.7	53.6
43–50 weeks	21.6	21.2
Number of household members		
1 person	19.5	18.8
2 persons	31.3	32.4
3 persons or more	49.2	48.8
Number contributing to household income^{+}		
1 adult	37.5	35.9
2 adults	54.9	57.7
3 adults or more	7.5	6.4

Notes: Due to rounding, percentages may not add up to exactly 100 percent.

A chi-square test of equal distribution for the supplement group and control group was used. Statistical significance levels are indicated as: $^{+}$ = 0.10; $^{++}$ = 0.05; and $^{+++}$ = 0.01.

Thus, it appears that random assignment of the baseline sample produced a supplement group and a control group that were the same in all regards, except for the fact that ESP was offered to supplement group members but not to control group members. Therefore, comparisons of subsequent labour market experiences, unemployment benefit receipt, and other outcomes for the two groups provide valid estimates of the impacts of ESP on these outcomes.[7]

Table 3.2 also indicates that the baseline sample represents a broad range of different types of displaced workers. Thus, estimates of ESP impacts can be generalized to many different types of displaced workers.[8]

Lastly, Table 3.2 indicates that the baseline sample represents displaced workers who were roughly split between men and women, spanned a wide range of ages, were relatively well educated, had been relatively well paid in their previous job, had worked a long time for their previous employer, typically were not union members (although some were), typically had not received severance (although many had), and were entitled to many weeks of unemployment benefits.

Estimating Program Impacts

Because ESP was conducted as a randomized experiment, estimating program impacts was straightforward. It was only necessary to compare the average outcome for supplement group members with its counterpart for control group members. For example, the impacts on average earnings during the first 15 months after random assignment could be estimated by

[7]This refers to the "internal validity" of the impact estimates, or their validity for the specific situation that was observed.

[8]This refers to the "external validity" of the impact estimates.

obtaining the difference between average earnings for supplement group members and control group members. Or the impact of ESP on the percentage of displaced workers who found new full-time jobs within six months could be estimated by obtaining the difference between this percentage for supplement group members and for control group members. Such differences of means or differences of percentages are referred to as "unadjusted impact estimates."

However, standard practice for most randomized experiments is to increase the statistical power of impact estimates (reduce their standard errors) by statistically controlling for individual differences in baseline characteristics.[9] For continuous outcome measures, such as earnings, this control is accomplished through ordinary least squares (OLS) regression. Hence, impact estimates from this procedure are often referred to as "regression-adjusted impact estimates." In most cases, these "adjusted" estimates are close to their "unadjusted" counterparts, but the standard errors are smaller. For discrete outcomes (e.g., employed full time or not) that are measured as proportions or percentages, statistical adjustment can also be achieved through OLS regression with the dependent variables set equal to zero or one for each sample member. This procedure is referred to as a "linear probability model."

Appendix A outlines how these estimates were obtained and indicates that they were quite similar to their unadjusted counterparts. Thus, the validity of the impact estimates presented in this report relies on the validity of the randomized experiment, not on the validity of the statistical adjustment procedure used.

Sources of Follow-Up Data

Follow-up data for the ESP impact analysis were obtained from a telephone survey and administrative records maintained by HRDC.

Telephone Survey

A follow-up telephone survey was conducted by Statistics Canada 15 months after sample members were randomly assigned.[10] The survey was administered through computer-assisted telephone interviews (CATI) conducted from Sturgeon Falls, Ontario. The average interview took about 25 minutes and the overall completion rate was 88.8 percent. Completed surveys were forwarded to the head office of Statistics Canada for data processing, quality control, and construction of analysis files.

The main purpose of the survey was to collect detailed information about all spells of employment since random assignment. The survey also asked respondents about their job-search activities, financial situation, household composition, educational background, response to the supplement offer, and how losing their job had affected them. In addition, the survey measured levels of depression using a scale developed by the Center for Epidemiologic Studies.[11]

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[9]For example see Bloom et al., 1997; Riccio, Friedlander, and Freedman, 1994; Woodbury and Spiegelman, 1987; Corson et al., 1989; Corson et al., 1991; and Spiegelman, O'Leary, and Kline, 1992.

[10]Because random assignment was conducted from July 1995 to October 1996, the survey was in the field from December 1996 through to February 1998. Each month a new survey cohort was forwarded to the interviewers. To maximize the response rate, cohorts were left in the field for up to three months.

[11]The Center for Epidemiologic Studies Depression (CES-D) Scale was developed for epidemiologic surveys of depression. It provides an index of cognitive and behavioural depression based on how often specific symptoms occurred during a particular week. The CES-D Scale is a 20-item scale administered either by respondents themselves or *(continued)*

Although 88.8 percent of the baseline sample members responded to the survey, 12.9 percent of these respondents did not include complete information about their employment or earnings. Most often this was because of missing job-start or job-end dates (needed to identify when respondents had been employed), or missing wage rates or hours worked (needed to compute their earnings). Thus, estimates of ESP impacts on employment and earnings were based on data for 4,479 persons, or 75.8 percent of the baseline sample. This group is referred to as the "15-month survey" sample.[12] Statistical tests described in Appendix A suggested that impact estimates obtained for this sample were affected only slightly by survey non-response bias.

Administrative Records

Administrative records from HRDC were used to document the unemployment benefits received by sample members during the first 15 months after their random assignment. Complete data were available for 5,790 persons, or 97.9 percent of the baseline sample. This sample is referred to as the "15-month benefit" sample; all estimates of impacts on the duration and amount of unemployment benefits were obtained from this sample.[13]

These data were obtained from the Status Vector File maintained by HRDC for research purposes. This file provides detailed information on unemployment benefits by week, and is considered to be a reliable source of information. HRDC indicated that the Status Vector File might not report unemployment benefits for as many as two percent of the sample, but this did not affect the present analysis.[14]

SPECIAL ANALYSES

During the study, questions arose that required direct responses from participants and these could not be addressed through traditional quantitative methods — the "why" and "how" kinds of questions that help promote a better understanding of the behaviour behind the numbers. What motivated some, but not others, to take up the supplement offer? How important was the supplement offer in encouraging participants' early re-employment? What factors influenced the offer's importance for participants? How did the supplement influence the employment decisions of those who received it? And how did recipients deal with the loss of their supplement when they reached its two-year time limit? These issues were explored using focus groups and in-depth telephone interviews.

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through an interview. Scores are summed to provide an overall scale ranging from 0 to 60. Higher scores indicate greater depression and scores of 16 or more are commonly taken to indicate depression when all 20 items are administered (Radloff, L.S., 1977). Only 19 of the 20 items were included in the ESP follow-up survey so the resulting scores were multiplied by 20/19 to make them roughly comparable with findings based on all 20 items.

[12]In addition, 122 sample members were selected to pre-test the 15-month survey, and thus were excluded from the 15-month survey sample for estimating program impacts.

[13]Fourteen sample members who were randomly assigned in October 1996 were also excluded from the 15-month benefit sample because they had only 14 months of follow-up data.

[14]Two percent is a very small margin of error. Furthermore, this error probably affected the supplement group and the control group similarly. Thus, its effect on impact estimates was probably negligible.

The Focus Groups

Focus groups are increasingly considered to be a valuable tool for evaluation research.[15] By gathering people who experienced the same phenomena — in this case, job loss and ESP — in an informal and non-threatening venue, and guiding them through an open-ended discussion, researchers often can gain valuable insights into complex behaviour. Furthermore, by comparing the responses of one group that shared a particular experience, such as those who received supplements, with responses from another group that experienced something different, such as those who did not receive supplements, further insights into the phenomena can be gained.

The focus groups used for this report were held in Toronto and Oshawa in November 1996. They were the second of two sets of focus groups that explored the reactions and experiences of ESP supplement group members.[16] These focus groups took place one month after the ESP job-search deadline had passed for participants. This made it possible to recruit separate groups of supplement recipients and non-recipients. A total of 48 individuals — 18 supplement recipients and 30 non-recipients — participated in six focus groups, one in each city for recipients and two in each city for non-recipients. Findings from these conversations were used to help deepen and enrich this report.

The In-Depth Interviews

To complement what was learned from focus groups, in-depth interviews were conducted with 41 supplement group members three months after their two-year time limit had expired. The goal of these interviews was to speak at length with supplement recipients who had experienced a large earnings loss and had received supplement payments for a long time. Supplement group members who met these criteria and whose supplements had expired between October 1997 and January 1998 were chosen for the interviews. All interviews were conducted by telephone.

Some of the types of questions asked included:

- What kinds of jobs were supplemented?
- Did the ability to have wages supplemented affect participants' employment decisions?
- Would they have taken the jobs they did without the supplement offer?
- How were those who were still receiving large payments when the supplement expired affected by the loss of payments?

Although many of the questions asked were qualitative, there were also questions that required a more systematic and close-ended approach. Often these two types of questions were combined. For instance, respondents were asked to rate, on a scale from 1 to 10, the importance of finding a better-paying job, followed by a request to explain their rating. Although surveys often include a few such "open-ended" questions, the protocol used for the in-depth interviews relied extensively on this approach, and thus might best be characterized as a "qualitative survey."

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[15]For a discussion of focus groups in evaluation research, see Morgan, 1993.

[16]The first set of focus groups, also held in Toronto and Oshawa, took place halfway through the six-month ESP job-search period for participants. Their findings were reported by Bloom et al., 1997.

SAMPLES BY DATA SOURCE

As described above, the ESP evaluation drew on many types of data from many data sources. Table 3.3 provides a brief guide to these data sources. It lists each source with the name of its sample, plus the size of the sample and a description of the data obtained. This table provides a useful reference for the analyses presented in subsequent chapters.

Table 3.3: Samples by Data Source

Data Source	Sample Name	Sample Size	Data Obtained
ESP baseline application form	Baseline sample	5,912	Individual background characteristics
HRDC administrative records on unemployment benefits paid	15-month benefit sample	5,790	Weekly unemployment benefit payment amounts for the first 15 months after random assignment
ESP follow-up survey conducted 15 months after random assignment	15-month survey sample	4,479	Employment, earnings, job-search, and other outcomes for the first 15 months after random assignment
ESP Program Management Information System (PMIS)[a]	PMIS sample	2,960	Weekly supplement payment amounts for the 24 months after random assignment
ESP "mini-survey"[a]	Mini-survey sample	269	Information about how well participants understood their supplement offer
Second round of ESP focus groups[a]	Focus group sample	48	Detailed information about personal experiences and reactions to losing their jobs and to ESP
In-depth telephone interviews[a]	Interview sample	41	Detailed information about personal experiences and reactions to losing their jobs, to ESP, and to losing their supplement payments when they expired

[a]Supplement group members only.

Chapter 4:
Participants' Knowledge of the Program

A "fair test" of a re-employment earnings supplement requires that participants be adequately informed. They must know how the supplement can benefit them and what they must do to receive it. In particular, they must know enough to make an informed choice between trying to find work that qualifies for supplement payments, and remaining unemployed and continuing to collect unemployment benefits.

To assess whether ESP met these conditions, this chapter examines how well supplement group members understood the program. The main source of data for this analysis was the mini-survey of 269 supplement group members described in Chapter 3. This survey was conducted about two weeks after the ESP orientation. This orientation took place about five weeks after random assignment, which in turn occurred about eight weeks after application to ESP (see Figure 4.1).

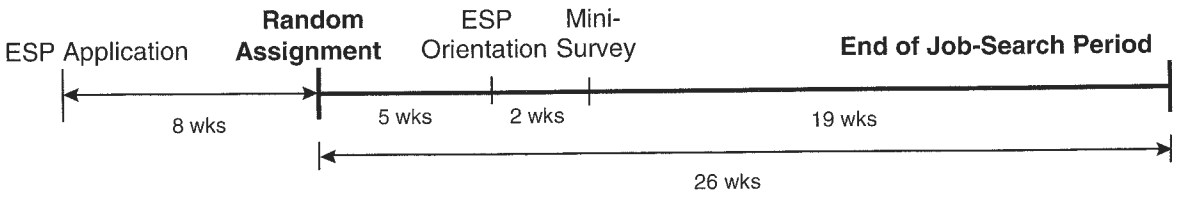
Additional information about participants' knowledge was obtained from focus groups with supplement group members, interviews with Human Resource Centre of Canada (HRCC) staff members, and records of calls to the ESP toll-free information line.

KEY FINDINGS

Findings suggest that ESP was a fair test because:

- Information was received by virtually all displaced workers who were offered the program.
- They found this information easy to understand and quite helpful.
- They were well informed about both ESP and their unemployment benefits.

Figure 4.1: Average Timing of the ESP Mini-Survey for Displaced Workers



EXPOSURE TO ESP INFORMATION

Table 4.1 describes the information about ESP that respondents were exposed to before the mini-survey. This information was the basis for their knowledge about the program when they were interviewed. Note that almost 9 out of 10 respondents recalled getting an ESP brochure,[1] and more than 8 out of 10 received an ESP orientation.[2] Seven out of ten were exposed to both of these information sources, and almost all were exposed to at least one. Only 1 out of 10 respondents had obtained further information by initiating a supplement. Thus, most respondents had been exposed to a program brochure or an orientation session, but very few had been exposed directly to the program by initiating a supplement.

Table 4.1: Displaced Workers' Exposure to ESP Information Sources

	Displaced Workers (%)
Percent who received:	
A brochure	87
An orientation	82
An initiation[a]	10
A brochure and orientation	72
A brochure and initiation	9
An orientation and initiation	6
All of the above	5
2 or more of the above	77
1 or more of the above	98
None of the above	2
Percent of brochure recipients who thought it:	
Was easy to understand	95
Had enough information about ESP	92
Succeeded in explaining ESP	92
All of the above	90
2 or more of the above	94
1 or more of the above	96
None of the above	4

[a]Includes only respondents who initiated a supplement *before* the mini-survey.

Table 4.1 also indicates that most mini-survey respondents were highly satisfied with the information in the ESP brochures. Over 9 out of 10 respondents who recalled getting a brochure felt that it was easy to understand. A similar proportion felt that it contained enough information about ESP and succeeded in explaining the program.[3] Nine out of

[1]This information was obtained from responses to the mini-survey.

[2]This information was obtained from the ESP Program Management Information System (PMIS). Note that 75.3 percent of all supplement group members in the baseline sample received an ESP orientation, which was slightly below the rate for mini-survey respondents. Thus, the mini-survey may have slightly overstated the level of ESP knowledge for all supplement group members. Nevertheless, the orientation session was only one source of information about ESP; it lasted only several minutes, and there was not much difference between the orientation rate for the mini-survey sample and that for the full supplement group.

[3]The mini-survey did not ask these questions about the ESP orientation.

ten respondents agreed with all of these statements, and less than 1 out of 10 did not agree with any.

Overall, it appears that information about ESP was widely and well received by displaced workers who were offered the supplement program.

KNOWLEDGE ABOUT ESP

Table 4.2 summarizes responses to mini-survey questions about key features of ESP. These findings indicate that mini-survey respondents had a good working knowledge of the program. For example, 9 out of 10 knew the month by which they had to find a new job in order to qualify for supplement payments. Almost 9 out of 10 respondents knew that they had to work at least a minimum number of hours per week to receive supplement payments, and that they could receive these payments only for a limited time. Furthermore, about 8 out of 10 respondents knew all three basic ESP provisions; and more than 9 out of 10 knew at least two of them.

Mini-survey respondents knew somewhat less about specific ESP details, which was to be expected. Nevertheless, they generally knew quite a bit about these details. More than 5 out of 10 respondents knew their exact job-search period. Slightly more knew that they had to work at least 30 hours per week to receive supplement payments, and that they could receive these payments for up to two years. About 6 out of 10 knew at least two of these details, and only 1 out of 10 did not know any of them.

Table 4.2: Knowledge about ESP

	Displaced Workers (%)
Basic provisions	
Percent who knew:	
The month by which they had to find a job	91
They had to work a minimum number of hours per week	88
They could receive a supplement for only a limited time	90
Percent who knew:	
All of the above	76
2 or more of the above	96
1 or more of the above	99
None of the above	1
Specific details of basic provisions	
Percent who knew:	
How many weeks were in their job-search period	56
They had to work full time to receive a supplement	68
The supplement could be received for only two years	60
Percent who knew:	
All of the above	32
2 or more of the above	65
1 or more of the above	88
None of the above	12

Table 4.2: Knowledge about ESP (cont'd)

	Displaced Workers (%)
Other provisions	
Percent who knew:	
ESP could provide extra money but not job-search help	89
ESP paid only for a re-employment earnings loss	84
ESP would make up only part of this loss	81
ESP would not cover new jobs with the previous employer	73
Percent who knew:	
All of the above	49
3 or more of the above	84
2 or more of the above	96
1 or more of the above	99
None of the above	1

The bottom panel of Table 4.2 provides further evidence that mini-survey respondents had a good working knowledge of ESP. Nine out of ten knew that it provided extra money for finding a new job, but did not provide job-search assistance. Eight out of ten also knew that they could receive supplement payments only for jobs that paid less than their previous one, and that the supplement was designed to help offset a re-employment earnings loss. Seven out of ten knew that they could not receive supplement payments for returning to their previous employer. Almost everyone knew at least two of these additional ESP provisions.

KNOWLEDGE ABOUT UNEMPLOYMENT BENEFITS

To make an informed choice between trying to find a job that might qualify for supplement payments or continuing to receive unemployment benefits, displaced workers also had to know how their unemployment benefits worked. Several questions were included in the mini-survey to assess this knowledge. Table 4.3 summarizes responses to these questions.

These findings suggest that mini-survey respondents had a good understanding of the unemployment benefits to which they were entitled. Almost all knew that there was a limit to how long they could receive these benefits. In addition, more than 5 out of 10 knew how long they could receive benefits, within a margin of 10 percent. Hence, they were aware of how long they could count on these benefits.

Respondents also were aware of the size of their benefit payments. About 5 out of 10 knew their weekly benefit amount within 10 percent, although fewer knew the ratio of this amount to their previous wage (their wage replacement rate).

Table 4.3: Displaced Workers' Knowledge of Unemployment Benefits

Percent who knew:	
That unemployment benefits have a time limit	99
The length of their time limit, within 10 percent	56
Their weekly unemployment benefit payment, within 10 percent	53
Their unemployment benefit wage replacement rate, within 5 percent	24
That they could earn some money while receiving unemployment benefits	78
Percent who knew:	
All of the above	11
4 or more of the above	40
3 or more of the above	67
2 or more of the above	93
1 or more of the above	100
None of the above	0

Respondents were asked whether it was possible to work and receive unemployment benefits at the same time (this can be done by certain part-time workers). Eight out of ten answered this question correctly.

On balance, all respondents answered at least one question about their unemployment benefits correctly, almost all answered at least two questions correctly, and 7 out of 10 answered at least three questions correctly.

OTHER INDICATORS OF PARTICIPANTS' KNOWLEDGE

Although the mini-survey was the primary and most systematic source of data about participants' knowledge, several other sources of information helped to inform the assessment of this factor.

First, a survey of 85 local HRCC staff members about the implementation of ESP suggested that the information provided about the program was quite effective. About 8 out of 10 staff members felt that the ESP printed materials answered all relevant questions, and 7 out of 10 felt that applicants had knowledge about the program. Second, supplement group members who participated in focus groups had a good basic understanding of ESP. Almost all knew what ESP was about, and more than two out of three understood its specific details. Third, most callers to the ESP toll-free information line understood the basic provisions of ESP, although some were confused about or unaware of certain details.

On balance, the weight of the evidence acquired from multiple sources suggests that displaced workers who were offered the ESP supplement program understood its terms and conditions. Therefore, ESP was a fair test of the program in this regard.

Chapter 5:
Patterns of Supplement Receipt

This chapter describes the patterns of supplement receipt experienced by members of the ESP supplement group. It indicates how many supplement group members became recipients, how long they received payments, and how much they received.

Data for the analysis came from several sources. The ESP Program Management Information System (PMIS) provided information about all supplement payments made. The 15-month ESP follow-up survey provided information about who qualified for supplement payments by finding a suitable job in time, and why others did not receive supplement payments. As well, focus groups provided additional insight into why some displaced workers received supplements and others did not.

As noted in previous chapters, participants had to find a new full-time job within 26 weeks of random assignment to qualify for an ESP supplement. If they found a new job that paid less than their previous one, they could receive immediate supplement payments. If their new job paid as much or more than their previous one, they could register for ESP "earnings insurance" that would provide supplement payments if their earnings declined in the future.

It was anticipated that many supplement group members would not qualify for supplement payments. Some would not be able to find new full-time work in time. Others would find full-time work that paid as much or more than their previous job. Hence, only a minority of supplement group members were expected to receive supplement payments.

KEY FINDINGS

An analysis of the supplement receipt data revealed the following:

- **Supplement Receipt Rate**. Just over one-fifth of all supplement group members received supplement payments. Only 1.5 percent registered for a supplement to obtain earnings insurance. The cumulative supplement receipt rate rose steadily over time, with only a slight acceleration at the end of the six-month job-search period.
- **Supplement Amounts**. On average, supplement recipients were paid $8,705 during the two-year supplement receipt period. The average weekly supplement payment was $127. By design, recipients who experienced the largest re-employment earnings losses received the largest supplement payments. The 20 percent who received the largest total amounts received 44 percent of all supplement payments made.
- **Supplement Duration**. The number of weeks for which supplements could have been paid ranged from 78 to 104, depending on how quickly supplement group members found new full-time jobs. On average, supplement recipients received payments for 64 weeks, or roughly 69 percent of the weeks for which this was possible.

- **Supplement Spells**. Supplement recipients typically received payments for one or two separate time periods, or spells. The average length of the longest spell was 53 weeks. About 14 percent of recipients were paid for all weeks between their first payment and the end of their two-year eligibility period. About 44 percent of recipients were still being paid when their supplement expired at the end of the eligibility period.[1]
- **Supplement Qualifiers**. Roughly 7 out of 10 individuals who qualified for supplement payments actually received them. The main determinant of supplement receipt, among those who qualified, was the size of the potential supplement payment. Individuals who qualified for small payments were the least likely to register for them.

CUMULATIVE AND WEEKLY RECEIPT RATES

This section summarizes findings for cumulative and weekly supplement receipt rates.

The Cumulative Rate

In all, 20.3 percent of supplement group members received supplement payments within the six-month job-search period. Another 1.5 percent — 44 individuals — initiated a supplement to obtain earnings insurance. Of these 44 "insurers," seven later received supplement payments. This raised the cumulative supplement receipt rate to 20.5 percent.

The 20.5 percent receipt rate compares favourably with corresponding rates for the unemployment insurance bonus experiments discussed in Chapter 1. Typically, about 15 percent of unemployment benefit claimants in the four bonus experiments received a bonus payment.

As discussed later in this chapter, during the six-month ESP job-search period, about 12 percent of supplement group members found a new full-time job that paid as much or more than their previous job. Hence, they qualified for earnings insurance. But only about one out of eight of these qualifiers — the 1.5 percent of all supplement group members mentioned above — registered for the supplement to obtain their insurance. This suggests that displaced workers had very little interest in the earnings insurance feature of ESP.

Figure 5.1 illustrates that the cumulative supplement receipt rate was about four percent at the beginning of the ESP job-search period, then rose steadily by roughly one percentage point every two weeks until it reached 20.3 percent at the end of six months.[2] The four percent of supplement group members who received supplement payments for the first week of their job-search period were either already working full time at random assignment or found a new full-time job immediately thereafter. Figure 5.1 also illustrates that there was a very slight acceleration in supplement receipt as the job-search period came to a close.

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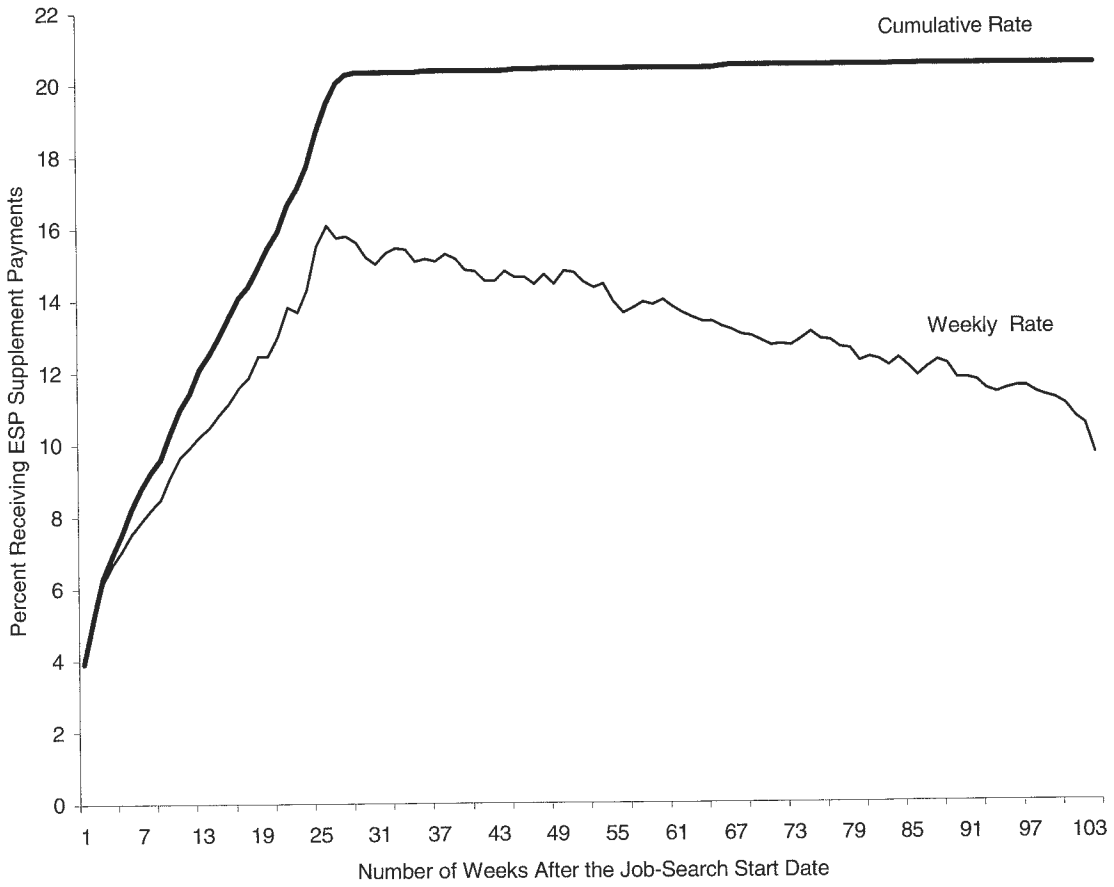
[1]These recipients received a supplement payment for every week in the last four weeks of their eligibility period.

[2]The cumulative supplement receipt rate was 20.3 percent at the end of week 28, which is two weeks after the end of the job-search period. Although supplement group members had only 26 weeks to qualify for the supplement, those who could document full-time job offers made before the end of the job-search period, or who had worked less than a week before the end of the job-search period, were given a brief grace period to meet the 30-hour work requirement.

Figure 5.1: ESP Supplement Receipt Rates



The Weekly Rate

Almost 14 percent of all supplement recipients were paid continuously from their first payment until the end of their supplement eligibility period. The remaining recipients had one or more weeks for which they received no payments. For some, these "gaps" occurred because an increase in earnings made them ineligible for supplement payments. Others experienced a job loss or a change in job that made them ineligible, either permanently or temporarily.

Thus, not all supplement recipients were paid in any given week. Consequently, the percentage of supplement group members who were paid in any one week was always less than the 20.5 percent who were *ever* paid.

Figure 5.1 illustrates the weekly supplement receipt rate during the two-year eligibility period. Note that it peaked at 16.1 percent in the 26th week after random assignment, then fell gradually to about 10 percent by the end of the supplement eligibility period.

AMOUNT AND DURATION OF SUPPLEMENT PAYMENTS

In both theory and practice, ESP was a generous program, at least for supplement recipients. On average, they received payments for 64 weeks and the total amount of supplements paid was $8,705 per recipient. This section explores these findings in more detail.

Duration of Payments

There are several ways to measure the duration of supplement payments:

- the total number of weeks for which supplements were paid;
- the length, in weeks, of the longest continuous period of supplement payment;
- the ratio between the number of weeks for which supplements were paid and the total number of weeks for which they could have been paid; or
- the percentage of supplement recipients who were still being paid at the end of their two-year eligibility period.

The median number of weeks for which supplements were paid was 73 weeks and the mean was 64 weeks. As might be expected, however, there was considerable variation in this outcome. For example, 15.3 percent of all supplement recipients were paid for less than 26 weeks (or half of a year), while 41.2 percent were paid for more than 78 weeks (or one-and-a-half years). Table 5.1 summarizes these findings.

Table 5.1: Number of Weeks for which Supplements Were Paid

Number of Weeks	Percentage of Recipients (%)
Less than 27	15.3
27–52	17.8
53–78	25.7
More than 78	41.2
Total	100.0

Most supplement recipients experienced one or two long payment spells. About half of all recipients had only one spell of supplement payments, while 26 percent had two spells. The median length of the longest spell was 48 weeks and the mean was 53 weeks.[3] On balance, supplements typically were paid continuously for long spells.

For each supplement recipient, it was possible to compare the number of weeks for which supplements *actually* were paid with the number of weeks for which they *could* have been paid, based on when his or her new job had started. The ratio of these two outcomes, expressed as a percentage, provides an alternative measure of the continuity of supplement payments. The median value of this percentage was 81 percent and its mean was 69 percent. (Table 5.2 presents the corresponding distribution.) Hence, supplements typically were paid

[3]Single weeks of non-receipt between two periods of receipt were ignored and the two adjoining periods were combined as a single continuous spell of supplement receipt. This was done because supplement amounts were not paid weekly, but rather by each recipient's relevant payment period. To create weekly supplement amounts, it was necessary to pro-rate actual payment periods into calendar weeks. This process may have created some apparent "gaps" in a truly continuous supplement receipt process.

for most of the period between re-employment and supplement expiration. Thus, it appears that most recipients continued to work full time in jobs that paid less than their previous ones. In fact, 44 percent of all recipients were still receiving payments when their supplements expired.

Table 5.2: Percentage of Potential Weeks for which Supplements Were Paid

Percentage of Potential Weeks for which Supplements Were Paid (%)	Percent of Recipients (%)
Less than 25	13.8
25–50	15.2
51–75	16.1
76–99	40.9
100	14.0
Total	100.0

As noted earlier, supplement payments were set to equal 75 percent of the difference between weekly earnings in recipients' new job and weekly earnings in their previous job. This could range from a minimum of $1 to a maximum of $250 per week. The actual mean weekly payment was $127, the median was $117, and the range was $1 to $250. Table 5.3 indicates that these payment amounts were distributed fairly evenly around their mean.

Table 5.3: Weekly Supplement Payments

Weekly Payment Amount	Percent of Recipients (%)
$0–50	17.8
$51–100	25.0
$101–150	18.5
$151–200	15.8
$201–250	22.9
Total	100.0

Table 5.4 presents the distribution of the total supplement amount paid to each recipient. As can be seen, the amounts were substantial in most cases. The median was $7,196 and the mean was $8,705. About 39 percent of recipients were paid more than $10,000.

Table 5.4: Total Supplement Payments

Total Payment Amount	Percent of Recipients (%)
$0–2,500	22.6
$2,501–5,000	15.6
$5,001–7,500	13.7
$7,501–10,000	9.5
$10,001–12,500	9.3
$12,501–15,000	8.0
$15,001–17,500	8.3
$17,501–20,000	6.2
More than $20,000	6.8
Total	100.0

Another way to examine how total payment amounts were distributed among recipients is to rank the recipients from lowest to highest according to their total payment, divide them into five equally sized groups, or quintiles, and then compute the percentage of total payments received by each quintile. As can be seen in Table 5.5, the 20 percent of recipients for whom ESP was most generous — the highest quintile — received 44 percent of total supplement payments. In contrast, the 20 percent for whom ESP was least generous — the lowest quintile — received only two percent of total supplement payments. Thus, recipients who were paid the most received a disproportionate share of total payments. Note, however, that this was because they experienced a disproportionate share of re-employment earnings losses.

Table 5.5: Percentage of all Supplement Payments Received by Quintile

Quintile	Percent of Total Supplement Payments (%)
Lowest	2.3
Second	8.3
Third	16.8
Fourth	28.7
Highest	43.9

DETERMINANTS OF SUPPLEMENT RECEIPT

What about the 80 percent of supplement group members who did not receive supplement payments? Two data sources — the follow-up survey and focus groups — were used to help determine why the supplement offer did not lead to supplement receipt for these sample members.

When asked on the follow-up survey whether they had received supplement payments, 23 percent of the supplement group members answered "yes."[4] Those who answered "no" were then asked why they had not received a payment. The most frequent response to this question was that it was not possible to find a job that qualified. About 42 percent said they "couldn't find a job in time" and 8 percent said they "couldn't find a job working the required number of hours." Table 5.6 lists the frequency of these and other responses.

Note, however, that the answer "couldn't find full-time work" might mask important differences in responses to the supplement offer. For some individuals, the supplement offer might have produced an intensive effort to find full-time work. For them, "couldn't find full-time work" might have meant not being able to find any type of work despite their best efforts. For others who were less concerned about finding a new job quickly and were more concerned about finding a suitable job, "couldn't find full-time work" might have meant an inability to find a particular kind of job.

[4]These percentages do not exactly match those obtained from the PMIS data reported above because the samples involved were different and the survey data probably contain some recall errors.

Table 5.6: Main Reason for Not Receiving a Supplement Payment

Reason	Percent of Supplement Group Members (%)
Couldn't find a job in time	41.7
Couldn't find a job working 30 hours/full time	7.8
Accepted a job with no salary/wage decrease	12.6
Self-employed	1.9
Did not know	15.6
Other reasons[a]	8.3
Did not recall ESP	4.6
Went to school	2.9
Planned to return to previous employer[b]	1.8
Not interested	2.1
Too much trouble or paperwork	0.7

[a]Respondents were asked to specify "other" reasons. Any answers that occurred frequently were included in the table.

[b]Although the sample includes only those with no recall expectations at baseline, some individuals may have returned to their previous employer anyway.

Fortunately, it was possible to explore these findings in more detail using results from focus groups conducted with participants one month after their ESP job-search period had ended. These focus groups were conducted separately with 18 supplement recipients and 30 non-recipients.

One of the most striking findings from the non-recipient groups was the limited perceived relevance of the supplement offer, even though the majority of the participants had planned to stay in the labour force when they were laid off. Many of these participants said that the supplement offer was "not very important" or "not at all important" while they were searching for jobs. In the words of one participant:

> *It* [the supplement] *was secondary. It was not a priority. The priority was to get a job. I would like a good fit considering my background and my education so I wasn't willing to settle. It wasn't a money issue really.*

When focus group participants were asked why they were not employed, some indicated that they had other sources of income (for example, a working spouse, severance payments, or a pension) that reduced the pressure on them to become re-employed quickly. Others reported that they faced major barriers to re-employment because they were older or lacked marketable skills.

Surprisingly, the supplement offer seemed irrelevant to job-search decisions even for supplement recipients. Only one out of six recipients said that the offer had played a "very important" role in their job search.[5]

> *I basically decided I would go back to work because I was running out of money in my chequing account. And I decided if my chequing account gets below* [omitted] *I'm just going to take a job and start working again. If the supplement turns up, that's fine. If it doesn't, I mean I'm going to have to basically make it on my own with what I've got.*

[5]Most of these focus group participants said that the offer had been "somewhat or not very important" in their job search. Recall, however, that only 18 supplement recipients took part in these focus groups.

The supplement recipients seemed to have been more flexible in terms of the jobs they would accept and seemed to have started their job search earlier than did non-recipients. Thus, it may have been the flexibility and early start of their job search, rather than the promise of an earnings supplement, that led them to quickly find a new lower-paying job that qualified for a supplement.

It is impossible to make generalizations about the full supplement group based on focus groups conducted with a small number of participants. And even if these results properly characterized the full supplement group, it is possible that some of its members were affected by the supplement offer, even though most were not.

To explore this possibility, an analysis was conducted to examine the relationship between the background characteristics of supplement group members and whether they received supplement payments.

Categorizing Supplement Group Members

The first step in this analysis was to divide supplement group members into four categories based on follow-up survey data about their spells of employment.[6] The four categories were made up of those who, during the ESP job-search period:

- did not find a full-time job and thus did not qualify for a supplement;
- found a full-time job that paid less than their previous job, but the job was not eligible for a supplement (for example, because it involved self-employment or a return to a previous job);
- found a new full-time eligible job that paid as much or more than their previous job, and thus qualified for earnings insurance; or
- found a new full-time eligible job that paid less than their previous job and thus qualified for supplement payments.

Table 5.7 lists the percentage of supplement group members in each of these groups. As can be seen, 57.6 percent did not find a full-time job during the six-month ESP job-search period and thus did not qualify for a supplement. Another 3.7 percent found a full-time job, but did not qualify for a supplement because of the nature of their job (usually because it involved self-employment or a return to a previous job). About 12 percent of supplement group members qualified for earnings insurance by finding a new full-time job that paid as much or more than their previous job. Lastly, 26.8 percent qualified for supplement payments by finding a new full-time job that paid less than their previous job.

[6]Because of inevitable errors in the survey data, the findings discussed in this section are only approximations. Nevertheless, they provide an adequate account of the early re-employment experiences of supplement group members.

Table 5.7: Six Month Re-employment Experience of Supplement Group Members

	Number	Percent
Did not find a full-time job in time	1303	57.6
Found a full-time job in time	961	42.4
Found a full-time job in time that:		
Did not qualify for a supplement	84	3.7
Paid as much or more than the previous job	270	11.9
Paid less than the previous job	607	26.8

Source: 15-month follow-up survey.

Background Characteristics and the Likelihood of Becoming Re-employed

The second step in this analysis was to estimate a logistic regression model of the relationship between individual background characteristics and the likelihood of becoming re-employed full time during the ESP job-search period. This model was estimated separately for men and women.[7] Independent variables were selected for the model based on what had been learned from ESP focus groups and from previous research about the determinants of re-employment for displaced workers. These variables included, among others, age, education, duration of prior employment, marital status, prior earnings, and union membership, plus variables to indicate the presence of alternative income sources.

Table C.1 in Appendix C presents the results of the model. These findings indicate that age was one of the most important determinants of re-employment. Each additional year of age reduced the probability of finding a full-time job by about one percent. This finding is consistent with comments by several focus group participants that age was a major barrier to re-employment. A second related finding was that individuals who had been with their previous employer for a long time were less likely than others to become re-employed quickly. A third significant finding was that individuals with higher earnings in their previous job were much more likely than others to become re-employed quickly. All three of these findings apply to both men and women.

Contrary to expectations, however, individuals' access to additional sources of household income was not correlated with their re-employment success. For example, individuals who had received severance payments upon layoff were no more or less likely to become re-employed quickly. The same was true for those who had additional adults contributing to their household income.

Supplement Receipt Among Supplement Qualifiers

The third step in the analysis was to estimate, from those who qualified to receive supplement payments, the percentage of individuals who received them during the six-month ESP job-search period. The total number of individuals who received supplement payments,

[7]These models were estimated separately for men and women because this is standard procedure for labour market research. However, because ESP findings were similar for men and women (see tables 6.10 and 6.11), the findings are usually presented for the sample as a whole.

508, was readily determined from PMIS data.[8] It was more difficult, however, to estimate the total number of individuals who qualified for these payments.

This required combining PMIS data with that from the follow-up survey. Because of the extensive data checks conducted by ESP program staff, everyone who received supplement payments could be identified through PMIS data and could be assumed to have qualified for them. However, PMIS data could not identify individuals who qualified for supplement payments but did not receive them. Thus, the follow-up survey was used for this purpose. Because of survey errors, however, especially those involving wrongly reported start dates for jobs and starting wages, the survey-based identification of supplement qualifiers was subject to measurement error. Nevertheless, it was the best way possible to make this determination.

Individuals who, during the six-month ESP job-search period, reported a new full-time job that paid less than their previous job, were classified as supplement qualifiers, as were all supplement recipients.[9] A total of 705 supplement group members met this condition. Thus, 705 out of 2,264 supplement group members appeared to have qualified for supplement payments. Given that 508 individuals received supplement payments, 508/705 or 72.1 percent of supplement qualifiers received payments in the six-month job-search period.

Table 5.8: Proportion of Eligible Supplement Group Members Who Received Supplement Payments

	Number	Percent
Total	2,264	100.0
Worked full time within the job-search period	1037	45.8
Worked full time in an eligible job within the job-search period	960	42.4
Worked full time in an eligible job within the job-search period, and earned less than in their previous job (*or* received a supplement payment)	705	31.1
Received a supplement payment	508	22.4
Eligible participants who received payments	508 of 705	72.1

Source: 15-month follow-up survey and PMIS.

Regression Analysis

The fourth and final step in the analysis was to estimate a logistic regression model of the factors related to whether supplement qualifiers became supplement recipients. Table C.2 of Appendix C presents the results of this analysis. It indicates that the most important determinant of supplement receipt among supplement qualifiers was the size of the potential

[8]The number of supplement recipients (508) is not the same number that was discussed in earlier sections (where it was 607) because this analysis required employment data and thus used the 15-month survey sample, and not the PMIS sample.

[9]The accuracy of the survey information depends on how well survey respondents can recall the exact details of their work experience. If the data are not perfectly accurate, some supplement group members may not be correctly classified. In principle, all supplement group members will fall into one of six categories. Of those *classified as qualifying for supplement payments* according to the 15-month survey, members have (1) qualified and received supplement payments, (2) qualified and not received supplement payments, or (3) not qualified and not received supplement payments. Members who are *classified as not qualifying for supplement payments* according to the 15-month follow-up survey have (4) qualified and received supplement payments, (5) qualified and not received supplement payments, or (6) were ineligible and not received supplement payments. In practice, only the first and fourth groups can be identified. It is not possible to distinguish between groups 2 and 3 or between groups 5 and 6. Therefore, only rough estimates of the number of supplement group members that qualified for supplement payments can be provided.

payment — the larger the payment, the higher the probability of receipt.[10] Based on the estimated model, a $100 increase in the weekly supplement payment would lead to a 19 percentage point increase in the probability that a supplement qualifier would become a supplement recipient.

Supplement receipt rates did not vary much with respect to other characteristics, such as age, gender, and so on, included in the model. Therefore, it seems likely that most supplement qualifiers were well-informed about the terms and conditions of their supplement offer.

REGISTERING FOR EARNINGS INSURANCE

As noted earlier, ESP offered earnings insurance to supplement group members who quickly found new full-time work that paid as much or more than their previous job. These workers could register for earnings insurance by contacting ESP program staff and completing some simple forms. This would enable them to receive supplement payments if their earnings fell below those of their previous job at any time during the remainder of their two-year eligibility period.

Overall, 12 percent of supplement group members qualified for earnings insurance by finding a new full-time job. But only one out of eight of these qualifiers registered. Consequently only 1.5 percent of all supplement group members registered for earnings insurance. Of this very small group (44 individuals), only seven actually received supplement payments.

The clear lack of interest in the earnings insurance offered by ESP seems to be a direct extension of the pattern observed among those eligible for immediate supplement payments — the smaller the expected payment, the lower the probability of signing up. In the case of earnings insurance, the immediate cash payment was zero and very few eligible supplement group members registered.

[10]For those who actually received supplement payments, the "supplement amount" variable was defined as the average payment over the first six weeks after eligibility was established. For those who did not receive a supplement payment, the potential supplement amount was computed using the reported pre-layoff insurable earnings and the reported starting weekly wage of the full-time job that began during the job-search period.

Chapter 6:
Program Impacts

Chapter 5 describes patterns of ESP supplement receipt. However, because supplement receipt does not necessarily represent a change in behaviour caused by ESP, these findings do not provide measures of ESP impacts. The purpose of this chapter is to present estimates of these impacts on job-search behaviour, employment, earnings, wage rates, and unemployment benefits. These estimates were based on comparisons of outcomes for the supplement group and the control group.

Two main data sources were used for the analysis. The 15-month follow-up survey provided information on job-search behaviour, employment, earnings, and wage rates; unemployment benefit administrative records provided information on the duration and amount of unemployment benefit payments.[1] Additional information from focus groups, in-depth telephone interviews, and records of calls to the ESP toll-free information line was used to help interpret the impact findings.

KEY FINDINGS

The analysis of ESP data revealed the following:

- **Impacts on job-search behaviour**. Supplement group members and control group members exhibited very similar job-search behaviour. Both groups started looking for new jobs soon after being laid off, tried hard to find them, and used similar approaches to do so. The only observed but very important difference was that supplement group members were slightly more likely to consider new types of jobs, including those that paid less than their previous job.
- **Impacts on employment**. ESP caused a few displaced workers to find full-time employment slightly sooner than they would have otherwise. This re-employment effect was first observed toward the end of the six-month ESP job-search period. It was due, in roughly equal parts, to a shift from part-time to full-time employment and an increase in overall employment.
- **Impacts on earnings and wage rates**. ESP may have reduced wage rates and earnings slightly, but these findings were not statistically significant.
- **Impacts on unemployment benefit payments**. ESP did not reduce the duration or total amount of unemployment benefit payments.
- **Impacts for subgroups**. ESP expedited full-time re-employment somewhat for many subgroups of displaced workers but produced large impacts for none. Thus, the modest average impact observed for the full sample represents modest impacts for many

[1]Information for some sample members was not included in both data sets. However, the analysis did not exclude persons from one sample if they were missing from the other, in order to maximize the size of each sample. Sensitivity tests described in Appendix A indicate that the sample differences did not produce important differences in impact estimates.

different subgroups, not large impacts for a few. The small gains in early full-time employment did not reduce unemployment benefit payments for any subgroup for reasons discussed later in this chapter.

IMPACTS ON JOB-SEARCH BEHAVIOUR

ESP offered displaced workers a large financial incentive to find new full-time work as soon as possible. For most sample members, working full time with a supplement would pay more than staying unemployed and receiving unemployment benefits. Indeed, earnings plus the supplement could almost fully replace past weekly earnings. This suggested that ESP might have a major influence on job-search decisions. On the other hand, since the only way to receive supplement payments was to take a lower-paying job, it was not clear how many displaced workers would consider the supplement offer a major inducement.

When asked about specific ways that ESP might have affected their job search: 25.3 percent of the supplement group members who responded to the follow-up survey said it had prompted them to start looking for a new job sooner than they would have otherwise; 28.5 percent said it had caused them to spend more time each week looking for a job once they had begun to look; 44.3 percent said it had enabled them to look for a different kind of job; and 64.6 percent said it had made them more willing to accept a lower-paying job. Thus, few supplement group members felt that ESP had affected the timing or intensity of their job search, but many felt that it had caused them to broaden its scope.

Further information was obtained from in-depth discussions with 48 displaced workers from Toronto and Oshawa who participated in focus groups one month after their ESP job-search period had ended.[2] Most of these participants felt that ESP was a good idea but that it did not apply to them. Overall, 4 out of 10 focus group participants felt that ESP had affected their job-search decisions — mainly by causing them to consider jobs that they would not have considered otherwise. Those who felt that ESP had influenced their job search often did not begin a more pro-active search for several months until the need to take a lower-paying job had begun to seem important. This, plus the growing recognition that their ESP "window of opportunity" would soon close, prompted them to consider a broader range of job options.

Although many supplement group members felt that ESP had influenced their job-search behaviour, it had almost no observable impact on their *actual* job-search activities. This can be seen in the comparison in Table 6.1 of job-search activities reported on the follow-up survey by supplement group members and control group members. The top panel of the table presents responses from all supplement group members and control group members. The bottom panel presents responses from the 94 percent of supplement group members and control group members who had looked for a new job.[3]

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[2]As noted earlier, focus groups were conducted in Toronto and Oshawa because they were the two largest sites and were located close to each other.

[3]Because almost all sample members looked for a new job, there was very little margin for selection bias to affect comparisons of outcomes for supplement group members and control group members who looked for new jobs.

Table 6.1: ESP Impacts on Job-Search Activities

	Supplement Group	Control Group	Impact	Significance Level[c]
All sample members				
Percent who:				
Started looking for a job before layoff	26.0	27.2	-1.2	0.35
Started looking for a job after layoff	67.7	66.6	1.1	0.44
Did not look for a job	6.2	6.1	0.1	0.85
Percent who looked for:				
Full-time job only	45.9	47.7	-1.8	0.24
Part-time job only	4.2	4.6	-0.4	0.50
Full-time or part-time job	43.7	41.6	2.1	0.17
No job	6.2	6.1	0.1	0.85
Percent who contacted:				
Employers directly[a]	88.8	87.7	1.1	0.25
Friends and relatives	80.9	82.6	-1.7	0.15
Companies who had placed job ads	78.8	79.1	-0.3	0.81
A government employment agency	75.0	74.0	1.0	0.46
A private employment agency	40.1	39.3	0.8	0.60
A union	5.1	5.3	-0.2	0.82
Percent who:				
Considered a new type of job	78.0	75.9	2.1*	0.10
Turned down a full-time job offer	8.9	8.0	0.9	0.28
Turned down a part-time job offer	6.1	5.7	0.4	0.57
Percent who:				
Did not change residence	75.1	74.4	0.7	0.60
Moved less than 150 km	18.1	19.2	-1.1	0.35
Moved more than 150 km	6.8	6.4	0.4	0.58
Sample members who tried to find a new job				
Mean weeks after layoff before starting to look for a job[b]	1.5	1.6	-0.1	0.73
During a typical week spent looking for a job:				
Mean hours spent looking for work	18.3	18.0	0.3	0.43
Mean employer contacts in person	6.0	5.7	0.3	0.27
Mean employer contacts by phone	7.5	7.4	0.1	0.90
Mean employer contacts by mail	8.1	7.9	0.2	0.58

[a]By phone, by mail, or in person.

[b]Includes zero values for sample members who started looking for a job *before* or *at* layoff. Corresponding means for sample members who started looking for a job *after* layoff were 2.1 weeks for supplement group members and 2.2 weeks for control group members.

[c]The p-value for a two-tailed hypothesis test.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

Results in the table suggest that ESP had almost no effect on job-search behaviour. The largest estimated impact, and the only one that was statistically significant, was for "considering a new type of job." Thus, ESP may have caused a few displaced workers to expand the range of jobs they considered. This finding is consistent with reports by

supplement group members about how ESP had affected their job-search behaviour. In addition, it is consistent with how ESP would be expected to influence job-search behaviour.

But in no other observable way did the job-search behaviour of supplement group members differ from that of control group members. For example, the timing of their job search was virtually identical. More than one-quarter of each group had begun to look for a new job before they were laid off, and two-thirds began after they were laid off (six percent did not look for a new job). Those who began their search after layoff typically did so within two weeks.[4]

The intensity with which sample members looked for work was the same for the supplement group and the control group. During a typical week, members of both groups spent 18 hours looking for work, on average, and contacted six employers in person, seven by phone, and eight by mail.

Furthermore, the mix of approaches used to find new jobs was the same for supplement group members and control group members. They contacted employers directly, asked friends and relatives about job leads, answered job advertisements from newspapers, and contacted a government employment agency (probably their local HRCC). However, less than half used a private employment agency and very few looked for work through their union.

Even the mix of part-time versus full-time jobs was the same for both groups. Very few members of either group (about four percent) looked only for part-time jobs: half looked only for full-time jobs, and the other half looked for both part-time and full-time jobs. Very few persons turned down a part-time or a full-time job offer.

Lastly, very few members of either group moved a long distance to take a new job (less than seven percent did so), although 2 out of 10 moved a short distance during the follow-up period.

In summary, both supplement group members and control group members started looking for new jobs soon, tried hard to find them, and used similar approaches to do so. The only observed difference was that supplement group members were slightly more likely to consider new job types.

IMPACTS ON EMPLOYMENT

Because ESP was designed to stimulate re-employment in full-time jobs, its impact on full-time employment was a central focus of the evaluation. Figure 6.1 illustrates this impact by plotting the full-time employment rates for the supplement group and the control group, starting with the month of random assignment (Month 0 in the figure) and continuing for the next 15 months.[5] The full-time employment rate was defined as the percentage of a group's members who were employed for a minimum of 30 hours during at least one week in a

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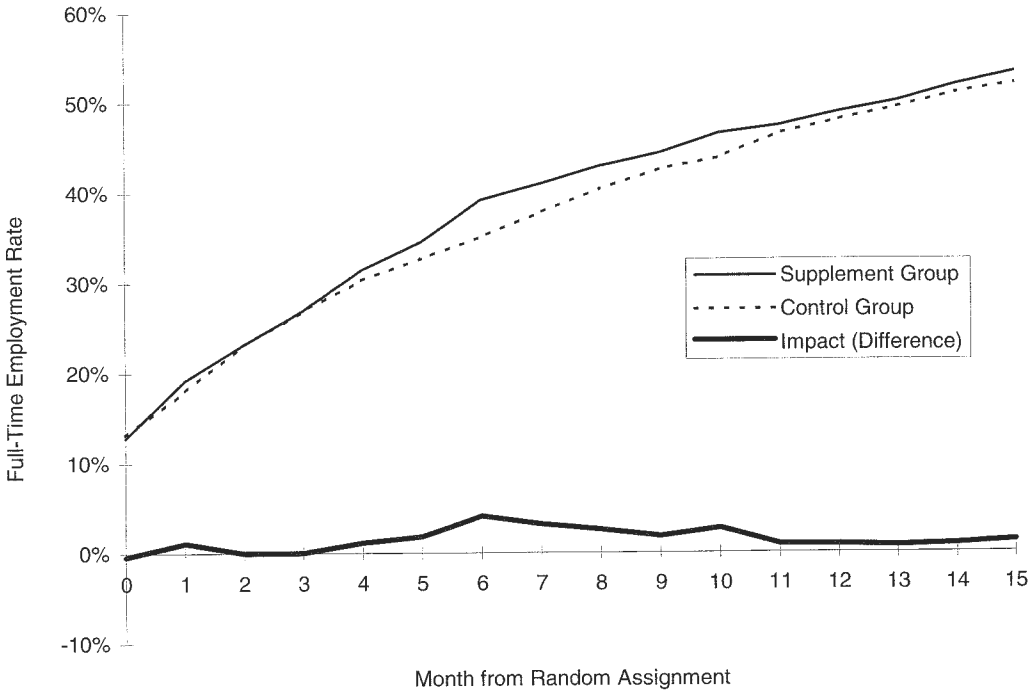
[4]Because random assignment typically occurred eight weeks after application to ESP, most sample members had begun their job search before they knew which study group they were in. Thus, the mean starting date for the two groups was expected to be the same. And since most sample members started looking for jobs soon after layoff (thus, before random assignment), one would expect the mean starting date for the two groups to be the same. However, there was still time for ESP to influence other features of the job-search process.

[5]The first 15 months after random assignment comprised the follow-up period for the ESP impact analysis.

particular month. The figure also plots the monthly *difference* in the full-time employment rates for the two groups. This difference is a measure of the impact of ESP.

Figure 6.1: ESP Impact on Monthly Full-Time Employment Rates for Sample Members



Outcomes and impacts in the figure, as well as all others in this chapter, are based on regression-adjusted comparisons of means or percentages (as discussed in Chapter 3). Appendix A describes how these estimates were obtained.[6]

Consider the findings, starting with those for the control group. A few members of this group (roughly 13 percent) were re-employed during the month in which they were randomly assigned. This was possible because random assignment typically occurred eight weeks after application to ESP.[7] Hence, there was time to find a new job before random assignment.

As each additional month passed, the percentage of control group members who held full-time jobs gradually increased, reaching 52 percent by follow-up Month 15. This finding illustrates two important points. First, many displaced workers found new full-time jobs without special incentives. It was crucial, therefore, to document this experience as a point of comparison for estimating ESP impacts. The opposite side of the same finding indicates, however, that even 15 months after random assignment, or more than 17 months after layoff,

[6]Analyses presented in Appendix A, Table A.4 indicate that the regression adjustments had little effect on the magnitudes of the impact estimates, although they somewhat reduced their standard errors. Hence, the internal validity of the impact estimates derives from the experimental design, not from the regression models.

[7]In addition, part of the month of random assignment came after the date of random assignment for most sample members.

half of the displaced workers in the control group were not re-employed in a full-time job — a clear sign of the serious labour market problems they experienced.

Full-time re-employment rates for the supplement group were virtually identical to those for the control group at random assignment and for several months thereafter. But starting in Month 5, and especially by Month 6, as their ESP "job-search window of opportunity" was about to close, more supplement group members than control group members had taken new full-time jobs. The difference (about four percentage points at its peak) gradually diminished during the next several months as the control group "caught-up." Roughly one year after random assignment, full-time employment rates for the two groups were basically the same and remained the same thereafter.

This pattern is consistent with the focus group findings, suggesting that the small fraction of supplement group members who found ESP to be relevant personally began to take it more seriously toward the end of their six-month job-search period.

Table 6.2 presents the numeric findings illustrated by Figure 6.1. The first two columns in the table list monthly full-time employment rates for the supplement group and the control group. The third column lists the difference between the first and second columns. This difference provides a direct estimate of the impact of ESP. The fourth column in the table indicates the statistical significance level (or p-value) for each monthly impact estimate. The division between the upper and lower panels in the table approximates the end of the ESP job-search period.[8]

Table 6.2: ESP Impacts on Monthly Full-Time Employment Rates

Month after Random Assignment	Percent Employed Full Time[a]		Impact (%)	Significance Level[b]
	Supplement Group (%)	Control Group (%)		
Month 0 (random assignment)	12.8	13.2	-0.4	0.63
Month 1	19.1	18.0	1.1	0.26
Month 2	23.1	23.1	0.0	0.98
Month 3	26.9	26.9	0.0	0.97
Month 4	31.4	30.3	1.1	0.37
Month 5	34.5	32.7	1.8	0.16
Month 6	39.2	35.1	4.1***	0.00
Month 7	41.0	37.8	3.2**	0.02
Month 8	43.0	40.4	2.6*	0.06
Month 9	44.3	42.5	1.8	0.18
Month 10	46.5	43.8	2.7**	0.05
Month 11	47.3	46.4	0.9	0.50
Month 12	48.8	47.9	0.9	0.52
Month 13	50.0	49.3	0.7	0.60
Month 14	51.8	50.9	0.9	0.49
Month 15	53.2	51.9	1.3	0.35

[a]Percent employed for at least 30 hours during at least one week in the month.

[b]The p-value for a two-tailed hypothesis test.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

[8]The exact ESP job-search period was 26 weeks from the *week* of random assignment, not six months from the *month* of random assignment.

As can be seen, full-time employment rates for the supplement group and the control group were virtually the same until follow-up months 5 and 6, when the supplement group rate began to exceed that of the control group. The biggest difference, which was statistically significant, was 4.1 percentage points in Month 6. This difference remained significant for three of the next four months, after which the control group caught up with the supplement group.

It is useful to consider the extent to which this impact was due to a shift from part-time to full-time jobs versus an overall increase in early re-employment. The next three tables help to clarify this issue. Table 6.3 presents estimates of ESP impacts on monthly part-time employment rates. Table 6.4 presents estimates of ESP impacts on monthly overall employment rates. Table 6.5 summarizes these impacts for the six-month ESP job-search period.[9]

Table 6.3: ESP Impacts on Monthly Part-Time Employment Rates

Month after Random Assignment	Percent Employed Part Time[a]			
	Supplement Group (%)	Control Group (%)	Impact (%)	Significance Level[b]
Month 0 (random assignment)	4.8	5.6	-0.8	0.24
Month 1	5.4	7.5	-2.1***	0.00
Month 2	6.3	8.3	-2.0***	0.01
Month 3	7.5	9.2	-1.7**	0.03
Month 4	7.7	9.5	-1.8**	0.03
Month 5	8.7	10.3	-1.6*	0.07
Month 6	8.6	10.6	-2.0**	0.02
Month 7	8.6	10.8	-2.2***	0.01
Month 8	9.2	10.9	-1.7*	0.05
Month 9	9.9	11.3	-1.4	0.11
Month 10	10.5	11.5	-1.0	0.27
Month 11	10.7	11.8	-1.1	0.25
Month 12	11.3	12.2	-0.9	0.32
Month 13	11.8	12.3	-0.5	0.59
Month 14	11.5	11.9	-0.4	0.65
Month 15	10.9	12.0	-1.1	0.24

[a]Percent employed for at least one hour during at least one week in the month, but not employed for more than 29 hours during any week in the month.

[b]The p-value for a two-tailed hypothesis test.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

First, consider the findings for part-time employment in Table 6.3. The outcome in this case was defined as the percentage of a group's members who were employed part time (less than 30 hours a week) for at least one week during a month *and* were not employed full time during any other week that month. Hence, the part-time employment rate in Table 6.3 and the full-time employment rate in Table 6.2 were defined to be mutually exclusive.

[9]Although findings in Table 6.5 are for months 1 through 6 after random assignment (to keep the presentation consistent with other findings for monthly employment), they are virtually identical to those for the exact 26-week ESP job-search period (for weeks 1 through 26 after the week of random assignment).

Few sample members were employed exclusively in part-time jobs. For the control group, the part-time employment rate started at 5.6 percent in the month of random assignment and slowly increased to 12.0 percent by follow-up Month 15. For the supplement group, the rate was about the same as for the control group in the month of random assignment. However, it did not increase as quickly as its control group counterpart and remained about two percentage points lower for the next year. This difference was statistically significant through Month 8.[10] Thus, it appears that ESP caused a few supplement group members to continue looking for full-time jobs instead of taking part-time jobs soon after layoff. This led to some members finding full-time jobs several months later, as reflected by the increase in full-time employment at that time.

But the two percentage-point reduction in part-time employment represented only half of the four percentage-point increase in full-time employment experienced by supplement group members in follow-up Month 6. Hence, the other half must have resulted from an increase in overall employment.

Table 6.4 supports this conjecture, although the evidence is uncertain because of its limited statistical significance. This table lists the monthly percentage of supplement group members and control group members who were employed either part time or full time, or both. This overall employment rate equals the sum of the part-time rate in Table 6.3 plus the full-time rate in Table 6.2.

Table 6.4: ESP Impacts on Monthly Overall Employment Rates

Month after Random Assignment	Percent Employed Any Time[a]		Impact (%)	Significance Level[b]
	Supplement Group (%)	Control Group (%)		
Month 0 (random assignment)	17.7	18.8	-1.1	0.23
Month 1	24.5	25.5	-1.0	0.39
Month 2	29.4	31.4	-2.0	0.12
Month 3	34.3	36.0	-1.7	0.20
Month 4	39.1	39.8	-0.7	0.63
Month 5	43.2	43.0	0.2	0.88
Month 6	47.8	45.7	2.1	0.13
Month 7	49.5	48.6	0.9	0.50
Month 8	52.2	51.3	0.9	0.54
Month 9	54.2	53.8	0.4	0.79
Month 10	57.0	55.3	1.7	0.22
Month 11	58.1	58.3	-0.2	0.91
Month 12	60.1	60.2	-0.1	0.97
Month 13	61.8	61.6	0.2	0.89
Month 14	63.3	62.8	0.5	0.70
Month 15	64.1	63.9	0.2	0.90

[a]Percent employed for at least one hour during at least one week in the month.

[b]The p-value for a two-tailed hypothesis test.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

[10]Because the underlying part-time employment rates were so low, a difference of two percentage points was statistically significant.

As can be seen, 18.8 percent of control group members were employed in the month of random assignment. This rate increased gradually to 63.9 percent by follow-up Month 15. The corresponding monthly rate for supplement group members was almost the same, except for follow-up Month 6, when it was two percentage points higher. This two-point difference represents about half of the observed supplement-induced increase in full-time employment for Month 6 reported earlier.

Table 6.5 reinterprets the preceding findings in a way that makes them easier to see. The first line in the table reports impacts on the percentage of supplement group members who were employed full time for at least one week during their *six-month ESP job-search period.* As can be seen, 42.3 percent of the supplement group had found a full-time job during this time, whereas only 37.9 percent of the control group had done so. The 4.4 percentage-point difference was statistically significant and summarizes the impact of ESP on full-time employment.

Table 6.5: ESP Impacts on the Percentage of Displaced Workers Who Were Employed Full Time, Part Time, or Any Time During the First Six Months after Random Assignment

	Supplement Group (%)	Control Group (%)	Impact (%)	Significance Level[d]
Employed full time[a]	42.3	37.9	4.4***	0.00
Employed only part time[b]	8.4	10.5	-2.1**	0.02
Employed any time[c]	50.7	48.4	2.3*	0.09

[a]Employed 30 or more hours for at least one week.

[b]Employed 1–29 hours for at least one week but never employed 30 or more hours.

[c]Employed one or more hours for at least one week.

[d]The p-value for a two-tailed hypothesis test.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

The second line in the table indicates that 8.4 percent of the supplement group members versus 10.5 percent of the control group members were employed part time for at least one week during the ESP job-search period but were never employed full time. The difference of -2.1 percentage points was statistically significant and summarizes the effect of ESP on part-time employment.

The third and final line in the table indicates that 50.7 percent of supplement group members versus 48.4 percent of control group members were employed either part time or full time, or both, for at least one week during the ESP job-search period. The difference of 2.3 percentage points was statistically significant and summarizes the effect of ESP on overall employment.

Therefore, the 4.4 percentage-point increase in full-time employment produced by ESP can be seen as a 2.1 percentage-point reduction in part-time employment and a 2.3 percentage-point increase in overall employment.

In summary, ESP caused a few displaced workers to take new full-time jobs sooner than they would have otherwise. This effect occurred toward the end of the six-month ESP job-search period and reflected, in roughly equal parts, a shift from part-time to full-time employment and an increase in overall employment.

IMPACTS ON EARNINGS AND WAGE RATES

Although ESP was designed to stimulate full-time employment for displaced workers, there was concern that it might reduce their earnings. In theory, this could occur for two different but related reasons. First, by reducing the job-search period, the supplement might result in sub-optimal job matches, with wages that were lower than those that would have been earned had the job search run its normal course. Second, by subsidizing jobs that paid less than the ones lost, the supplement might induce displaced workers to take jobs that paid less than the ones they would have taken otherwise.

Findings in Table 6.6 suggest that ESP did reduce earnings slightly during the first 15 months after random assignment. On average, supplement group members earned a total of $14,209, whereas control group members earned $14,891.[11] The -$682 or -4.6 percent difference was not statistically significant, however.

Examining this effect through hourly earnings (wage rates) instead of through total earnings produced a similar conclusion. On average, supplement group members *who were employed* during their 15-month follow-up period earned $12.67 per hour, whereas control group members *who were employed* earned $13.00 an hour.[12] The difference of -$0.33 per hour or -2.5 percent was statistically significant.[13]

Table 6.6: ESP Impacts on Average Total Earnings and Average Hourly Earnings

	Mean		Impact		
	Supplement Group	Control Group	Dollars	Percent	Significance Level[a]
15-month total earnings	$14,209	$14,891	-$682	-4.6%	0.12
15-month hourly earnings	$12.67	$13.00	-$0.33*	-2.5%	0.09

[a]The p-value for a two-tailed hypothesis test.

Notes: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

Table 6.7 provides a more detailed analysis of ESP's impact on wages by comparing the full distribution of wage rates for the supplement group and the control group. There are some differences between the two distributions, but these differences are modest. For example, supplement group members were two percentage points more likely than control group members to be low earners — with wage rates of less than $8 per hour. Conversely, they were two percentage points less likely to be high earners — with wage rates of $20 per hour or more. But these differences were partly offset by other smaller differences in the

[11]Zero amounts for sample members who were not employed at all during this period are included in the calculation of mean earnings.

[12]In calculating these averages, all employed sample members were considered equally, regardless of how many hours they worked during the period. Hence, mean hourly earnings in the table represent *the wage rate for the average employed sample member* not the wage for the average hour worked.

[13]About 72 percent of supplement group members and, surprisingly, the same percent of control group members were employed at some time during the 15-month follow-up period. Thus, differences in their hourly earnings reflect an unknown mix of two potential causal explanations: (1) *a selection effect* — a supplement-induced shift in who was employed; and (2) *a wage effect* — a supplement-induced change in the wage rates for persons who would have been employed with or without the supplement. Given the small effect of ESP on employment, however, it is unlikely that the small *observed* wage difference is masking a large *true* wage effect.

opposite direction. Thus, overall, the two distributions were not statistically significantly different.

In summary, it appears that ESP may have reduced wage rates and earnings slightly, but these reductions were not statistically significant.

Table 6.7: ESP Impacts on the Distribution of Average Hourly Earnings

15-Month Hourly Earnings	Supplement Group (%)	Control Group (%)	Difference (%)
Less than $5.00	2.9	2.7	0.2
$5.00–7.99	20.2	18.3	1.9
$8.00–10.99	25.3	25.3	0.0
$11.00–13.99	20.0	20.4	-0.4
$14.00–16.99	11.9	12.9	-1.0
$17.00–19.99	8.6	7.5	1.1
$20.00 or more	11.1	12.8	-1.7

Notes: The p-value was 0.47 for the chi-square test of the difference between the two distributions. Hence, they were not statistically significantly different.

IMPACTS ON UNEMPLOYMENT BENEFIT PAYMENTS

It was hoped that, by stimulating rapid full-time employment, ESP would reduce the duration and amount of unemployment benefits paid to displaced workers. However, ESP had only a modest re-employment effect, and this effect occurred six months after random assignment (or eight months after ESP application). By this time, many sample members probably had received most of the benefits to which they were entitled and, hence, there was little scope for an ESP impact on unemployment benefit payments.

Figure 6.2 illustrates that indeed there was no impact. First, consider the experience of control group members. Of this group, 67.4 percent were paid unemployment benefits for the month in which they were randomly assigned.[14] This percentage declined gradually until follow-up Month 8, when it dropped precipitously as recipients began to exhaust their benefit entitlements. By Month 12, the percentage had levelled out and thereafter it declined gradually. At the end of the 15-month follow-up period, only 11.1 percent of control group members were receiving unemployment benefit payments.[15]

The pattern for supplement group members was almost exactly the same. Hence, the difference between the experiences of the two groups, represented by the line at the bottom of the figure, was near zero throughout the follow-up period.

[14]All analyses in this report identify unemployment benefits according to the week for which they were paid, not the week in which they were received. The majority of sample members who were not paid unemployment benefits for the month in which they were randomly assigned had received severance from their last employer.

[15]Since claimants can collect benefits for a maximum of 50 weeks, these sample members were either receiving benefits that began a significant period of time after random assignment (for example, after the end of an allocation of severance pay) or receiving benefits under a new claim.

Figure 6.2: Percentage of Sample Members Who Received Unemployment Benefits each Month

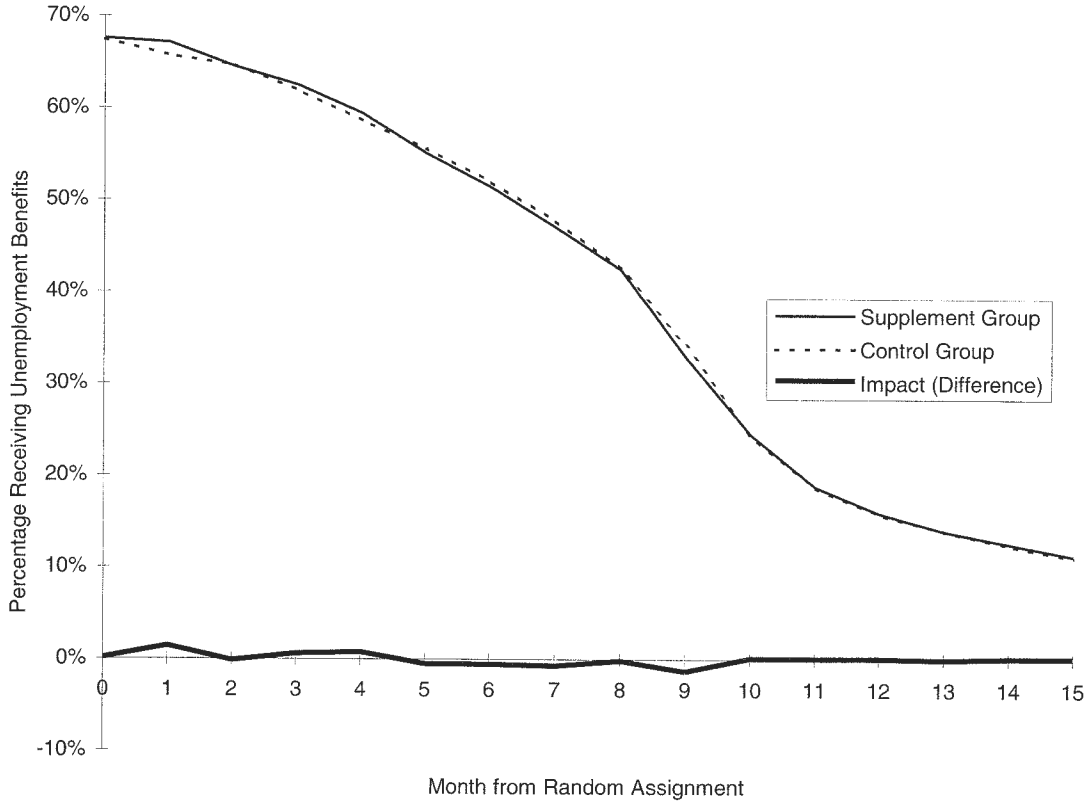


Table 6.8 is a numeric representation of the information illustrated by Figure 6.2. As can be seen, the impact of ESP on the monthly percentage of displaced workers who were paid unemployment benefits was uniformly near zero and not statistically significant.

Table 6.8: ESP Impacts on the Percentage Receiving Unemployment Benefits, by Month

Month after Random Assignment	Percent Who Received Unemployment Benefits		Impact (%)	Significance Level[a]
	Supplement Group (%)	Control Group (%)		
Month 0 (random assignment)	67.5	67.4	0.1	0.87
Month 1	67.1	65.7	1.4	0.16
Month 2	64.6	64.7	-0.1	0.91
Month 3	62.5	61.9	0.6	0.61
Month 4	59.5	58.7	0.8	0.52
Month 5	55.1	55.6	-0.5	0.69
Month 6	51.5	52.0	-0.5	0.67
Month 7	47.0	47.7	-0.7	0.57
Month 8	42.5	42.6	-0.1	0.91
Month 9	33.0	34.3	-1.3	0.27
Month 10	24.5	24.4	0.1	0.91

Table 6.8: ESP Impacts on the Percentage Receiving Unemployment Benefits, by Month (cont'd)

Month after Random Assignment	Percent Who Received Unemployment Benefits		Impact (%)	Significance Level[a]
	Supplement Group (%)	Control Group (%)		
Month 11	18.8	18.7	0.1	0.92
Month 12	15.9	15.8	0.1	0.87
Month 13	13.9	13.9	0.0	0.99
Month 14	12.7	12.5	0.2	0.84
Month 15	11.3	11.1	0.2	0.84

[a]The p-value for a two-tailed hypothesis test.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

Table 6.9 summarizes these findings in two ways. The first line in the table presents estimates of the impact of ESP on the average number of weeks for which unemployment benefit payments were received (the number of benefit weeks) during the 15-month follow-up period. On average, supplement group members received benefits for 22.1 weeks and control group members received them for 21.9 weeks. The difference of 0.2 weeks or 0.9 percent was not statistically significant.

Table 6.9: ESP Impacts on Mean Weeks and Mean Dollars of Unemployment Benefits

Unemployment Benefits	Mean		Impact		Significance Level[b]
	Supplement Group	Control Group	Weeks or Dollars	Percent	
Mean total weeks[a]	22.1	21.9	0.2	0.9%	0.77
Mean total dollars[a]	$6,460	$6,370	$90	1.4%	0.50

[a]For the first 15 months after the month of random assignment.

[b]The p-value for a two-tailed hypothesis test.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

The second line in the table presents estimates of the impact of ESP on the average total dollar amount of unemployment benefits received during the follow-up period. Supplement group members received $6,460, on average, and control group members received $6,370. The difference of $90 or 1.4 percent was not statistically significant.

Thus, ESP did not reduce either the duration or the total amount of unemployment benefits paid to displaced workers.

IMPACTS BY SUBGROUP

The preceding impact findings were for the full sample of displaced workers and thus represent an average of the impacts for different subgroups within the sample. The next logical step was to explore how these impacts varied by subgroup. This information could be used to help target future supplement programs.

It is possible that, because of their personal circumstances, some types of displaced workers were less interested than others in the supplement and considered it a less important part of their job search, resulting in smaller impacts. For example, workers displaced from very low-paying jobs had little prospect of finding new jobs that would qualify for large supplement payments. Consistent with this hypothesis is the finding in Chapter 5 that workers displaced from low-paying jobs were less likely to register for a supplement payment, even if they qualified for one (see Table C.2 in Appendix C).

Likewise, it is possible that displaced workers who confronted the greatest obstacles to finding new jobs were the least able to use a supplement and, consequently, experienced the smallest impacts from it. For example, past research has documented that older displaced workers face especially serious labour market problems and are very unlikely to make a quick transition to a new job.[16] Indeed, as shown in Chapter 5, older workers in the ESP supplement group were far less likely than others to find new full-time work. Hence, interest in the supplement might have been especially low among older displaced workers.

But, as mentioned earlier, supplement receipt is not a measure of supplement impacts because displaced workers can receive supplement payments for jobs that they would have taken anyway. Thus, to test hypotheses about different ESP impacts for different types of displaced workers, tables 6.10 and 6.11 present impact estimates for key subgroups of the study sample. Table 6.10 presents subgroup estimates of ESP impacts on the percentage of supplement group members who became re-employed full time during the six-month ESP job-search period. Table 6.11 presents subgroup estimates of ESP impacts on the average number of weekly unemployment benefit payments received during the 15-month follow-up period. All impacts were estimated separately for each subgroup.[17] Note that separate impact estimates are not presented for each ESP site because these findings are the focus of Chapter 8.

Table 6.10: ESP Impacts on the Percent Employed Full Time for at Least One Week During the First Six Months after Random Assignment, by Sample Subgroup

Subgroup	Percent Employed Full Time Within Six Months		Impact (%)	Significance Level[a]
	Supplement Group (%)	Control Group (%)		
Gender				
Male	50.5	44.8	5.7***	0.01
Female	36.1	32.2	3.9**	0.02
Age				
30 years or less	52.2	49.4	2.8	0.37
31–44 years	48.7	42.1	6.6***	0.00
45–54 years	35.4	31.9	3.5	0.24
55 years or older	15.0	12.1	2.9	0.31

[16]See, Lauzon, 1995, for a discussion of these issues for Canadian displaced workers; see Ross and Smith, 1993, for a discussion of these issues for displaced workers in the United States.

[17]This is referred to as the "split-sample" approach to estimating subgroup impacts.

Table 6.10: ESP Impacts on the Percent Employed Full Time for at Least One Week During the First Six Months after Random Assignment, by Sample Subgroup (cont'd)

Subgroup	Percent Employed Full Time Within Six Months: Supplement Group (%)	Percent Employed Full Time Within Six Months: Control Group (%)	Impact (%)	Significance Level[a]
Highest education credential				
University	50.7	47.5	3.2	0.39
College	42.8	40.7	2.1	0.54
High school	42.4	37.2	5.2***	0.01
Less than high school	34.6	30.1	4.5	0.11
Number of years worked for last employer				
Less than 1 year	59.0	56.3	2.7	0.52
1–2 years	47.0	42.6	4.4	0.26
3–5 years	44.7	39.0	5.6**	0.04
6–9 years	36.9	33.0	3.9	0.16
10 years or more	32.0	27.4	4.6*	0.07
Average prior weekly insurable earnings				
Less than $200 per week	36.6	28.7	7.9	0.52
$200–$399 per week	33.6	34.0	-0.4	0.90
$400–$599 per week	42.3	35.7	6.6***	0.01
$600–$799 per week	44.8	42.3	2.5	0.33
$800 per week or more	48.7	40.4	8.3**	0.01
Received severance upon layoff				
No	42.0	38.6	3.4**	0.05
Yes	42.7	36.8	5.9***	0.00
Union member in last job				
No	43.9	38.8	5.1***	0.00
Yes	34.9	33.6	1.3	0.69
Weeks of unemployment benefit entitlement				
38 weeks or less	42.7	36.2	6.5**	0.02
39–42 weeks	40.5	36.9	3.6*	0.05
43–50 weeks	41.8	37.1	4.7	0.10
Number of household members				
1 person	39.4	37.3	2.1	0.52
2 persons	38.1	36.3	1.8	0.43
3 persons or more	45.8	39.2	6.6***	0.00
Number contributing to household income				
1 adult	41.5	38.1	3.4	0.14
2 adults	43.3	38.0	5.3***	0.00
3 adults or more	39.0	36.0	3.0	0.61

[a]The p-value for a two-tailed hypothesis test.

Notes: The statistical significance of subgroup impact estimates was calculated using a two-tailed test. Statistical significance levels for subgroup impact estimates are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

Statistical significance of *differences* in subgroup impacts is indicated as: $^{+}$ = 0.10; $^{++}$ = 0.05; and $^{+++}$ = 0.01.

Table 6.11: ESP Impacts on Mean Weeks of Unemployment Benefits Received During the First 15 Months after Random Assignment, by Sample Subgroup

Subgroup	Mean Weeks of Unemployment Benefits		Impact		
	Supplement Group (%)	Control Group (%)	Weeks	Percent	Significance Level[a]
Gender					
Male	19.9	20.5	-0.6	-3.0	0.31
Female	23.9	23.2	0.7	3.0	0.19
Age					
30 years or less	18.9	18.3	0.6	3.3	0.47
31–44 years	21.8	22.3	-0.5	-2.3	0.40
45–54 years	23.7	23.1	0.6	2.8	0.49
55 years or older	25.6	25.9	-0.3	-1.3	0.76
Highest education credential					
University	23.3	24.4	-1.1	-4.5	0.22
College	22.1	21.8	0.3	1.4	0.62
High school	22.3	22.0	0.3	1.5	0.74
Less than high school	20.1	19.5	0.6	3.2	0.54
Number of years worked for last employer					
Less than 1 year	18.4	17.2	1.2	7.2	0.24
1–2 years	20.1	20.5	-0.4	-1.7	0.75
3–5 years	22.9	22.6	0.3	1.5	0.66
6–9 years	24.7	25.4	-0.7	-2.7	0.44
10 years or more	21.7	21.5	0.2	1.0	0.81
Average prior weekly insurable earnings					
Less than $200 per week	18.0	15.7	2.3	14.3	0.29
$200–$399 per week	22.4	21.8	0.6	2.6	0.48
$400–$599 per week	23.7	24.0	-0.3	-1.1	0.72
$600–$799 per week	22.0	22.1	-0.1	-0.3	0.92
$800 per week or more	19.6	19.3	0.3	1.4	0.79
Received severance upon layoff[++]					
No	22.4	21.6	0.8	3.8*	0.08
Yes	21.5	22.4	-0.9	-3.8	0.23
Union member in last job					
No	22.2	21.8	0.4	1.9	0.35
Yes	21.2	22.5	-1.3	-5.9	0.19
Weeks of unemployment benefit entitlement					
38 weeks or less	18.1	18.2	-0.1	-0.5	0.83
39–42 weeks	24.4	24.2	0.2	0.8	0.76
43–50 weeks	25.0	25.2	-0.2	-0.8	0.80
Number of household members					
1 person	22.6	22.2	0.4	1.9	0.66
2 persons	22.2	22.2	0.0	0.0	0.98
3 persons or more	21.7	21.8	-0.1	-0.6	0.81

Table 6.11: ESP Impacts on Mean Weeks of Unemployment Benefits Received During the First 15 Months after Random Assignment, by Sample Subgroup (cont'd)

Subgroup	Mean Weeks of Unemployment Benefits		Impact		Significance Level[a]
	Supplement Group (%)	Control Group (%)	Weeks	Percent	
Number contributing to household income					
1 adult	23.0	22.1	0.9	4.0	0.20
2 adults	21.5	22.0	-0.5	-2.4	0.33
3 adults or more	22.4	20.8	1.6	7.5	0.34

[a]The p-value for a two-tailed hypothesis test.

Notes: The statistical significance of subgroup impact estimates was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

Statistical significance of *differences* in subgroup impacts is indicated as $^{+}$ = 0.10; $^{++}$ = 0.05; and $^{+++}$ = 0.01.

Statistical significance is presented in two different ways in the tables. First, the significance of *each impact estimate* is indicated by a system of asterisks located adjacent to estimates that were significant. These asterisks indicate, for example, that ESP impacts on full-time employment were statistically significant both for men and for women.

Second, the statistical significance of *differences* among subgroup impact estimates is indicated by a system of plus signs located adjacent to the header for subgroup categories with significantly different subgroup impacts.[18] The absence of plus signs in Table 6.10 indicates that no subgroup categories had impact estimates that differed significantly. So, for example, the significant impact estimates for men and for women were not significantly different from each other.

The only subgroup estimates that were significantly different from each other were those in Table 6.11 for impacts on the number of weeks of unemployment benefits received by displaced workers with severance upon layoff and those without severance. As can be seen, these estimates were opposite in sign and equal in magnitude; neither was large, however.

Overall, the findings in Table 6.10 suggest that ESP had modest and statistically significant impacts on full-time re-employment rates for many subgroups, and there were no subgroups with large and significant impacts.[19] Hence, the small average impact observed for the full sample of displaced workers reflects a small impact experienced by many subgroups, not a large impact experienced by a few.

The findings in Table 6.11 suggest that ESP did not reduce unemployment benefits for any subgroup of displaced workers. Estimated impacts ranged from positive to negative, their magnitudes were small, and the only one that was statistically significant was positive, not negative.[20]

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[18]Appendix A describes how the statistical significance of subgroup impact differences was determined.

[19]The large estimated impacts on full-time employment rates for displaced workers with previous weekly earnings of less than $200 were based on a very small sample (127 persons) and thus were not statistically significant.

[20]Given the large number of subgroup impact estimates presented in Table 6.11, there is a good chance that the one finding that appeared to be statistically significant was, in fact, due to random sampling error.

In summary, ESP increased full-time re-employment rates for many subgroups of displaced workers, but produced large impacts for none. Hence, the modest average impact for the full sample represents modest impacts for many subgroups, not large impacts for a few. However, gains in full-time employment did not reduce the duration or amount of unemployment benefits received by any subgroup of displaced workers.

Having examined the impacts of ESP in detail, the next chapter assesses their financial implications for displaced workers and for the Canadian government.

Chapter 7:
Financial Implications

The initial research design for ESP called for a benefit-cost analysis. A traditional benefit-cost analysis would have placed dollar values on ESP's main impacts and compared them with its principal costs. This analysis would have been conducted from three perspectives: ESP participants, government budgets, and society as a whole.

However, the previous chapter established that ESP had no large or lasting impacts. Thus, a complete benefit-cost analysis was not warranted.[1] Nevertheless, this chapter examines the financial implications of ESP for individual displaced workers and the Canadian government budget by focussing on selected financial benefits and costs.[2] It determines whether ESP made individual displaced workers better off in terms of the *net* change in their income from three sources: earnings, unemployment benefits, and supplement payments. It also determines whether ESP produced a net financial gain for the government in terms of the cost of administering the program, the cost of supplement payments, and the net change in unemployment benefit payments produced by the program.

In addition, this chapter uses responses to selected questions from the 15-month follow-up survey and responses to in-depth interviews conducted with a small sample of supplement recipients to explore their perceptions about ESP's effects on the financial and personal hardship they experienced.

KEY FINDINGS

Based on the experiences of sample members, findings presented in this chapter suggest implications for the following:

- **Displaced workers**. On average, supplement group members experienced a modest net financial gain because the supplement payments they received exceeded the earnings losses they incurred, while their unemployment benefit payments remained virtually unchanged. This small increase in income did not affect their overall level of financial and personal hardship.

  Nevertheless, supplement payments were an important source of temporary income for the small fraction of displaced workers who received substantial supplement payments and thus helped to offset the re-employment earnings losses they experienced.

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[1]Kemper, Long, and Thornton, 1984, argue that good candidates for a benefit-cost analysis should be programs that are promising but unproven.

[2]The present analysis of ESP financial implications does not address three important issues: job displacement, societal benefits and costs, and intangible benefits and costs to participants. Displacement occurs when increased employment and earnings for one group are offset by corresponding losses for another. However, it is virtually impossible to determine how much displacement occurs because of a program. Hence, this issue has yet to be dealt with effectively by any benefit-cost analysis. A proper measure of ESP's benefits and costs to society would have required measuring its long-term impacts, which was well beyond the scope of the present report. Lastly, the present analysis does not try to assign dollar values to intangible benefits or costs associated with participation in ESP, such as increased self-esteem or foregone leisure time.

- **The Canadian government**. ESP produced a moderate net cost to the Canadian government because supplement payments were not offset by reductions in unemployment benefit payments or increases in taxes.

Therefore, ESP produced a transfer of resources from the Canadian government (i.e., taxpayers) to supplement recipients. This transfer was provided in a way that promoted full-time re-employment to a small degree and helped to compensate recipients for the losses they incurred due to involuntary unemployment.

OVERALL IMPLICATIONS FOR DISPLACED WORKERS

How did ESP affect the economic well-being of displaced workers during their first 15 months after random assignment?[3] The key determinant of well-being from this perspective was the program's effect on personal income. Hence, this section compares average total income from earnings, supplement payments, and unemployment benefit payments for members of the supplement group with those of control group members.

Table 7.1 summarizes this information. The bottom line of the table indicates that the net personal income of supplement group members from the three sources was $569 higher than that for control group members. In other words, ESP produced a net financial gain for displaced workers of $569 per supplement group member during the first 15 months after random assignment, or $38 per month. This reflected the fact that supplement group members received $1,161 in supplement payments, on average; their earnings were $682 lower than that of control group members, on average; and their unemployment benefit payments were $90 higher.[4]

Table 7.1: Implications for Displaced Workers — Net Change in Income per Supplement Group Member in the First 15 Months after Random Assignment

Source of Income	Average Change in Income ($)
Supplement payments	1,161
Earnings	-682
Unemployment benefits	90
Total	569

Sources: The net changes in earnings and unemployment benefits were taken from tables 6.6 and 6.9 in Chapter 6. The average supplement amount comes from supplement receipt data in the Program Management Information System (PMIS).

It is possible, but not likely, that this small financial gain reduced the overall level of financial hardship experienced by displaced workers. To explore this possibility, answers to questions on the 15-month follow-up survey about financial hardships were analyzed. These questions included, among others, items from the Center for Epidemiologic Studies

[3]The 15-month follow-up period was used to report this outcome in order to make it comparable with estimates of ESP impacts.

[4]It was hoped that ESP would *reduce* the duration and amount of unemployment benefits paid. Thus, the $90 increase was the opposite of what was expected. Nevertheless, it was not statistically significant and was quite small (representing only a 1.4 percent change).

Depression (CES-D) Scale, which is commonly used to assess the emotional state of respondents.[5]

Table 7.2 compares the survey responses of supplement group members with those of control group members. Control group responses indicate that, without ESP, most displaced workers experienced considerable financial hardship. For every 10 control group members, 8 had to reduce their savings or cut back on spending, 4 had to borrow money or take out a loan, 3 had to stop or delay paying bills or get financial help from family or friends, 6 had to cut back spending on social activities (including eating out), and 4 had to cut back on basic necessities such as food or clothing.

Table 7.2: ESP Impacts on Indicators of Financial Hardship

	Supplement Group (%)	Control Group (%)	Impact (%)	Significance Level[a]
Percent who said losing their job had made them:				
Reduce their savings	79.3	81.0	-1.7	0.15
Borrow money or take out a loan	40.9	44.1	-3.2**	0.03
Stop or delay paying bills	32.4	32.3	0.1	0.96
Receive financial help from family or friends	29.2	28.9	0.3	0.78
Sell stock or other possessions	26.1	25.5	0.6	0.65
Take in boarders	3.9	3.7	0.2	0.61
Percent who said they had to cut spending on some things	79.6	81.1	-1.5	0.19
Specifically on:				
Social activities (including eating out)	56.8	57.0	-0.2	0.89
Food or clothing	42.4	44.8	-2.4	0.10
Leisure (e.g., travelling)	24.3	24.7	-0.4	0.74
Luxuries or unnecessary items	23.6	24.6	-1.0	0.43
Transportation (e.g., gas, car)	13.0	13.7	-0.7	0.54
Other items	8.2	9.0	-0.8	0.32
Housing	5.8	4.7	1.1*	0.10
Mean score on CES-D Scale	14.4	14.2	0.2	0.36

[a]The p-value for a two-tailed hypothesis test.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

On the other hand, the mean score on the CES-D Scale for control group members was 14.2 points, which is below the benchmark of 16 points commonly used as a threshold for indicating depression. Thus, although displaced workers experienced substantial economic hardship, they did not, on average, appear to be suffering depression.

[5]The CES-D Scale was developed for epidemiologic surveys of depression. It provides an index of cognitive and behavioural depression based on how often specific symptoms occurred during a particular week. The CES-D Scale is a 20-item scale administered either by respondents themselves or through an interview. Scores are summed to provide an overall scale ranging from 0 to 60. Higher scores indicate greater depression and scores of 16 or more are commonly taken to indicate depression when all 20 items are administered (Radloff, L.S., 1977). Only 19 of the 20 items were included in the ESP follow-up survey so the resulting scores were multiplied by 20/19 to make them roughly comparable with findings based on all 20 items.

The third column in Table 7.2 presents the differences in specific hardship indicators reported by supplement group members and control group members. These differences were consistently small, and were only statistically significant for two items: "had to borrow money or take out a loan" and "had to cut spending on housing." Thus, although members of both the supplement group and the control group experienced major financial hardships, there was little observed difference between these two groups. Therefore, the $569 net financial gain from ESP did not appreciably reduce the level of financial hardship experienced by supplement group members.

The small change in personal income is also consistent with the fact that ESP had very little observed effect on the distribution of household income. Table 7.3 illustrates this finding. The first column in the table shows the distribution of household income for supplement group members and the second column lists the corresponding distribution for control group members. Note that there were very few differences between these two distributions and these differences were not statistically significant.[6]

Table 7.3: Household Income Distribution of the Supplement Group and Control Group

Household Income	Supplement Group (%)	Control Group (%)
Less than $10,000	4.8	5.2
$10,001–$19,999	15.8	15.0
$20,000–$29,999	18.9	16.7
$30,000–$39,999	15.1	14.1
$40,000–$49,999	13.1	12.2
$50,000–$59,999	9.4	10.8
$60,000–$69,999	8.4	9.0
$70,000–$79,999	5.0	5.7
$80,000–$89,999	3.5	3.8
$90,000–$99,999	1.4	1.5
$100,000–$109,999	2.1	2.5
$110,000–$119,999	1.0	1.4
$120,000 or more	1.5	2.1

Source: The 15-month follow-up survey.

Note: The 15-month follow-up survey asked a series of questions about household income to determine the respondent's income category. Respondents were first asked for a best estimate of their total household income in the past 12 months. Sixty-six percent of all survey respondents provided an estimate and 29 percent did not know their income. For those who did not provide an estimate, the interviewer asked questions used to estimate their income with broader income categories (e.g., "Would you say your household income was less than $50,000, between $50,000 and $80,000, or more than $80,000?"). Statistics Canada then combined answers from these questions to derive the above income categories and reported valid responses for 86 percent of the survey sample.

A chi-square test was used to compare the household income distributions of the supplement group and the control group. The p-value was 0.41.

IMPLICATIONS FOR SUPPLEMENT RECIPIENTS

When interpreting the preceding results, it is important to note that only 2 out of 10 supplement group members actually received supplement payments. Thus, only this small group could have experienced the direct financial benefits of the supplement. Nevertheless,

[6]It is possible that, in households with two or more earners, ESP could have affected spouses' employment decisions. However, the similarity in the distribution of household incomes for the supplement group and the control group suggests that ESP had no impact on spouses' labour market behaviour.

supplement payments might have been an important source of personal income for this group.

The total amount of supplements paid to recipients during the two-year period for which they were available ranged from a minimum of $1 to a maximum of $25,750. The average amount paid to recipients was $8,705. Thus, total payments were typically quite large. In light of this, it was important to consider two questions: how did these payments affect the financial and personal well-being of recipients and how did recipients cope when these payments expired?

To address these questions, non-experimental information was obtained from in-depth interviews.[7] These interviews were conducted with 31 supplement recipients who had experienced a large re-employment earnings loss and had received large supplement payments until they reached the end of their two-year supplement eligibility period.[8] Each respondent was interviewed three months after his or her supplement had expired.

Findings from the interviews suggested that ESP was indeed an important source of temporary income for these supplement recipients. Almost every recipient interviewed said that the supplement had made a difference to their financial well-being — 71 percent said that it had made a big difference and 23 percent said that it had made a fair bit of difference. When asked how the supplement had affected them financially, most respondents spoke about it helping them to meet substantial financial obligations, such as paying bills and debts, and making mortgage payments.

It is not surprising then that 8 out of 10 respondents indicated that losing the supplement was at least somewhat difficult. Although most respondents were aware of the temporary nature of the supplement, it still represented a considerable amount of income for them. Often it was considered to be a major resource for meeting monthly expenses. Many respondents adjusted to the loss of the supplement by making further cuts to their budgets.

Findings from these in-depth interviews also suggested that supplement payments had indirect benefits for the personal well-being of recipients. The reported effect was striking — 55 percent of the interview respondents said that the supplement had made a big difference to their personal well-being and 36 percent said that it made a fair bit of difference. Many respondents shared feelings similar to those expressed in the following two quotes: *"It was the difference between worrying all the time and not"* and "[It] *Took a lot of pressure off because of being able to make mortgage payments."*

IMPLICATIONS FOR THE CANADIAN GOVERNMENT

One goal of ESP was to reduce costs for the Canadian government. However, because supplement payments could be substantial and could be received for a long time, it was acknowledged from the outset of the project that any reductions in unemployment benefit payments produced would probably not be sufficient to fully offset the costs of supplements paid. Therefore, the most relevant question from the government's perspective was how much would it cost to compensate displaced workers for the losses they incurred in a way that promoted — or at least did not inhibit — their rapid re-employment?

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[7]See Chapter 3 for a description of these interviews.

[8]Findings in this section are for the 31 out of 41 individuals interviewed who relied on the supplement until it expired.

To address this question, Table 7.4 summarizes the net financial implications of ESP for the Canadian government budget. As can be seen, the program cost the government $1,340 per supplement group member during the 15-month follow-up period. Almost all of this cost was due to the direct expense of supplement payments. These payments were a transfer of resources from the Canadian government to supplement recipients. A small fraction of the overall net costs was due to the $89 per supplement group member that was required to administer the supplement program. This amount represents a true social cost — as opposed to a transfer from Canadian taxpayers to supplement group members — because the resources used were diverted from other purposes.

The preceding costs were not offset by savings in unemployment benefit payments — which actually increased slightly, although not by a statistically significant amount. Therefore, the supplement produced a net transfer of resources from taxpayers in general to displaced workers in particular.

Table 7.4: Implications for the Government Budget in the 15 Months after Random Assignment

Program Cost	Change in Government Expenditures ($)
Unemployment benefits	-90
ESP program	
Supplement payments	-1,161
Administrative costs	-89
Total	-1,340

Note: Since ESP had no impacts on unemployment benefits (see Chapter 6), unemployment benefit administrative costs have been excluded from the above analysis. While the analysis would have been more complete by obtaining information to estimate the unemployment benefit administrative cost per month of unemployment benefit receipt for sample members, the operating cost per claim (initial and renewal) processed from application to adjudication is only $70. This suggests that the ongoing processing costs are small. In addition, the information is not readily available.

Sources: The net change in unemployment benefits was taken from Table 6.9 in Chapter 6. The ESP program costs were obtained from supplement administrative records and fiscal reports.

In summary, ESP produced a modest net financial gain for supplement group members overall. This gain was too small to reduce their average level of financial hardship, however, even though the supplement was an important source of temporary income for the subset of the supplement group that actually received significant payments. Furthermore, this income support was provided in a way that promoted full-time re-employment slightly. It did, however, represent a moderate net cost to Canadian taxpayers.

Chapter 8:
Differences in Findings by HRCC

ESP was one program with one set of rules and procedures. However, it was operated at five different Human Resource Centres of Canada (HRCCs) located in four provinces. The main role of HRCC staff was to recruit, screen, and enrol sample members for the study. Although this process was standardized and fairly simple, there was room for some local variation in how it was implemented. Also, the types of displaced workers at each HRCC and the labour market conditions they faced varied substantially. Hence, responses to the program might have varied as well.

On the other hand, most program operations, such as informing participants about ESP, processing requests for supplement payments, computing payment amounts, and distributing supplement cheques, were conducted centrally by the ESP Payment Centre in Halifax, leaving little room for site-to-site differences. Thus, it was not clear whether ESP findings would vary by HRCC.

The present chapter addresses this issue by comparing HRCC findings for ESP knowledge, supplement receipt, program impacts, and the financial implications of ESP. The analysis draws on most of the data sources used for the present report.

KEY FINDINGS

An analysis of data from the various HRCCs revealed the following:

- **Participants' knowledge**. Supplement group members from Saskatoon and Granby knew more about ESP, on average, than did their counterparts from Toronto.
- **Supplement receipt**. Supplement receipt rates were much higher in Saskatoon and Granby than in Oshawa or Toronto. Because of this, the average total payment per supplement group member was about twice as large in Saskatoon and Granby.
- **Program impacts**. In Saskatoon, ESP expedited full-time re-employment somewhat and thereby reduced the total amount of unemployment benefits received during the 15-month follow-up period. At the opposite extreme, in Toronto, ESP had no effect on re-employment and did not reduce unemployment benefit payments. Impacts for the other HRCCs were between these two extremes, but were mainly small and not statistically significant.
- **Financial implications**. Displaced workers from Saskatoon experienced a roughly $1,600 increase in personal income per supplement group member during the 15-month follow-up period. This increase was much larger than those at other HRCCs and was due almost entirely to supplement payments. At no HRCC did government cost savings from reduced unemployment benefit payments fully offset the increased cost of supplement payments.

In conclusion, ESP was most successful for displaced workers from Saskatoon. But even for this site, the primary effect of the program was to transfer resources from the government to supplement recipients.

DIFFERENCES IN PROGRAM KNOWLEDGE

As discussed in Chapter 4, a special mini-survey was used to measure participants' knowledge about ESP. A program knowledge scale was then constructed based on responses to selected questions from this survey.[1] The maximum scale value for an individual was 14 points, indicating that all questions included in the scale had been answered correctly. The minimum value was zero points, indicating that none of the questions had been answered correctly. The mean scale value for the 269 displaced workers in the mini-survey sample was 10.5 points; the standard deviation was 2.6 points.

Table 8.1 presents each HRCC's mean knowledge score. As can be seen, the level of ESP knowledge was slightly higher in Saskatoon, Granby, and Winnipeg than in Oshawa and Toronto — especially Toronto. In addition, the mean scores for Saskatoon and Granby were statistically significantly higher than that for Toronto.[2]

Table 8.1: ESP Knowledge, by HRCC

HRCC	Mean ESP Knowledge Score
Saskatoon	10.9**
Granby	11.3**
Winnipeg	10.8
Oshawa	10.5
Toronto	9.9

Note: A two-tailed test was used to determine whether the scores were statistically significantly different from those of the Toronto group. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

DIFFERENCES IN SUPPLEMENT RECEIPT

Table 8.2 presents supplement receipt rates for each HRCC. Column 1 of the table lists the percentage of all supplement group members who received supplement payments. As can be seen, supplement group members from Saskatoon and Granby (with receipt rates of 33.8 and 30.9 percent) were almost *twice* as likely to receive payments than were those from Toronto and Oshawa (with receipt rates of 15.2 and 18.9 percent).

[1]This scale was created by Marc Lachance of Statistics Canada. A description of these scales can be obtained from SRDC upon request.

[2]Because ESP was least successful in Toronto in virtually all respects, the findings for Toronto were used as a basis for comparing the findings of other HRCCs. Thus, tests were conducted to determine the statistical significance of *differences* between the findings for Toronto and those for each of the other HRCCs.

Table 8.2: Supplement Receipt Rates, by HRCC

HRCC	All Supplement Group Members		Supplement Qualifiers Only
	Percent Who Received a Supplement[a]	Percent Who Qualified for a Supplement	Percent Who Received a Supplement
Saskatoon	33.8***	43.2***	78.2**
Granby	30.9***	41.2***	75.0
Winnipeg	25.9***	36.0***	71.9
Oshawa	18.9*	27.1*	69.8
Toronto	15.2	23.0	66.3

[a]These rates do not include supplements received by the seven persons who first initiated "earnings insurance" and then later received supplement payments. With these persons included, the rates are 34.5 percent for Saskatoon, 30.9 percent for Granby, 25.9 percent for Winnipeg, 19.1 percent for Oshawa, and 15.2 percent for Toronto.

Note: A two-tailed test was used to determine whether the findings were statistically significantly different from those of the Toronto group. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

One possible reason for these differences is the corresponding differences in the percentage of supplement group members who qualified for payments by quickly finding new, lower-paying, full-time jobs. Column 2 of the table indicates that, in fact, such differences existed. In Saskatoon and Granby, the percentage of supplement group members who qualified for supplement payments was 43.2 and 41.2 percent versus 23.0 and 27.1 percent for Toronto and Oshawa.

However, this does not fully account for the HRCC differences in supplement receipt rates because, even among individuals who qualified for payments (supplement qualifiers), those from Saskatoon and Granby were more likely to receive payments than were those from Toronto and Oshawa — 78.2 and 75.0 percent versus 66.3 and 69.8 percent.[3]

Thus, in HRCCs where supplement group members understood ESP best, they were most likely to qualify for supplement payments *and* to receive payments if they qualified.[4] In HRCCs where supplement group members understood ESP least well, they were least likely to qualify for supplement payments and to receive payments if they qualified. This finding does not necessarily mean that learning more about ESP caused an increase in the likelihood of receiving supplement payments. It could mean instead that persons who were more interested in the supplement to begin with (perhaps because they expected to take a lower-paying job) might have bothered to learn more about it because they were more likely to qualify for payments.

HRCC differences in supplement receipt rates produced corresponding differences in total payments per supplement group member during the 15-month follow-up period (see Table 8.3) because average payments *per recipient* did not vary much by HRCC. In Saskatoon and Granby, average payments per supplement group member were about twice as much as those in Oshawa and Toronto — $2,048 and $1,756 versus $943 and $878.[5]

[3]To see how the percentages in column 3 were computed, consider the following example: If 43.2 percent of supplement group members in Saskatoon qualified for supplement payments and 33.8 percent received them, then 33.8 out of 43.2 supplement qualifiers (or 78.2 percent) received the payments for which they qualified.

[4]This finding is unlikely to reflect knowledge about ESP gained from actually receiving supplement payments because the mini-survey was conducted well before most supplement recipients had initiated their supplement payments.

[5]Average total payments per supplement group member included zero amounts for non-recipients.

Table 8.3: Total 15-Month Supplement Payments, by HRCC

HRCC	15-Month Payments per Supplement Group Member ($)
Saskatoon	2,048***
Granby	1,756***
Winnipeg	1,749***
Oshawa	943
Toronto	878

Note: A two-tailed test was used to determine whether the findings were statistically significantly different from those of the Toronto group. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

DIFFERENCES IN PROGRAM IMPACTS

Having established that knowledge about ESP correlated somewhat with supplement receipt across HRCCs, it was useful to see whether ESP impacts followed a similar pattern. Tables 8.4 and 8.5 suggest that, although this was not true in general, it was true for the two extreme cases: Saskatoon and Toronto.

Table 8.4: ESP Impacts on Full-Time Employment and Earnings, by HRCC

	ESP Impact on		
HRCC	Percent Re-employed Full Time Within Six Months[a] (%)	Average Total 15-Month Earnings[b] ($)	Average Hourly 15-Month Earnings[c] ($)
Saskatoon	7.3** ++	15	-0.26
Granby	1.5	-1,438	-0.12
Winnipeg	4.6	-1,244	-0.64
Oshawa	4.5**	-380	-0.09
Toronto	-0.6	-887	-0.28

[a]Impact on the percentage who became re-employed full time for at least one week during the first six months after the month of random assignment.

[b]Impact on average total earnings during the first 15 months after the month of random assignment.

[c]Impact on average hourly earnings during the first 15 months after the month of random assignment for sample members who were employed during this period.

Notes: The statistical significance of impact estimates was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

A two-tailed test was used to determine whether the findings were statistically *significantly different* from those of the Toronto group. Statistical significance levels are indicated as: + = 0.10; ++ = 0.05; and +++ = 0.01.

In Saskatoon, ESP appeared to make progress toward its two main goals — to expedite re-employment and reduce unemployment benefit payments. With respect to the first goal, ESP appeared to increase the six-month full-time re-employment rate by 7.3 percentage points (see Table 8.4).[6] This finding was moderate in magnitude and statistically significant. In addition, this increase occurred without any reduction in average earnings during the 15-month follow-up period and with only a small and statistically insignificant reduction in average hourly wages.[7] Furthermore, the re-employment gain experienced by displaced

[6]This reflected an estimated increase from 44.0 percent for the control group to 51.3 percent for the supplement group, using the regression adjustment procedure described in Appendix A.

[7]The estimated $ 0.26 reduction in average hourly earnings was only 2.5 percent of the estimated $10.38 average for control group members.

workers in Saskatoon appeared to reduce their average unemployment benefit payments by $384 or 1.3 benefit weeks during the 15-month follow-up period (see Table 8.5).

Table 8.5: ESP Impacts on Unemployment Benefits, by HRCC

HRCC	ESP Impact on	
	Average Total 15-Month Unemployment Benefit Weeks[a]	Average Total 15-Month Unemployment Benefit Amount[b] ($)
Saskatoon	-1.3	-384
Granby	-1.4	-134
Winnipeg	-1.4	-415
Oshawa	0.6	217
Toronto	1.1	386

[a]Impact on the total number of weeks for which unemployment benefits were received during the first 15 months after the month of random assignment.

[b]Impact on the total amount of unemployment benefits received during the first 15 months after the month of random assignment.

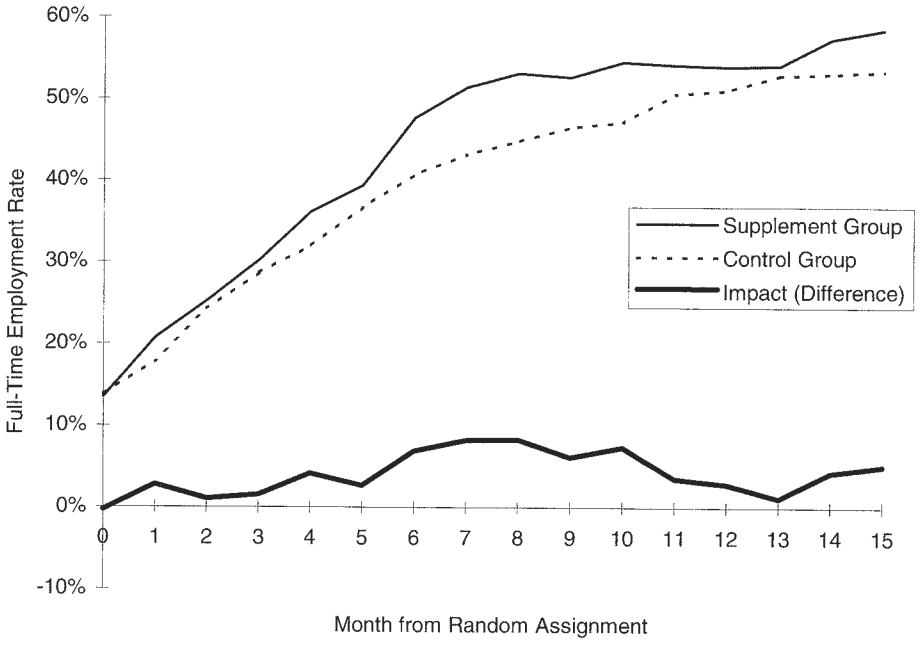
Notes: Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

No HRCC findings were statistically *significantly different* from their counterpart for Toronto.

Figure 8.1 illustrates how the impact of ESP on full-time employment in Saskatoon changed over time. It compares monthly full-time employment rates for the supplement group and control group from this site. During the first several months after random assignment, there was very little difference between the full-time employment rates of the two groups. But, toward the end of the six-month ESP job-search period, there was a noticeable increase in full-time employment for supplement group members, and thus a noticeable difference between the supplement group and control group. This difference became pronounced and statistically significant in the final month of the ESP job-search period (Month 6 after random assignment). However, four months later, the difference was much smaller and was no longer statistically significant because the control group had caught up with the supplement group. There was very little difference between the two groups thereafter. Thus, ESP caused some displaced workers in Saskatoon to find full-time employment a little sooner than they would have otherwise, but it did not increase the percentage who ultimately became re-employed full time.

The ESP experience of displaced workers in Toronto was at the opposite extreme. Supplement group members from this site experienced virtually no impact on re-employment and no reduction in unemployment benefit payments. The program at this site was associated with a negligible 0.6 percentage points estimated *reduction* in the six-month full-time re-employment rate, which was not statistically significant. Furthermore, unemployment benefit payments appeared to *increase* by $386 or 1.1 weeks, although these estimates were not statistically significant either.

Figure 8.1: Monthly Full-Time Employment Rates for Sample Members in Saskatoon



Impact estimates for the other HRCCs were generally between the two extremes represented by Saskatoon and Toronto. There was some evidence that ESP might have increased full-time employment slightly in Winnipeg and Oshawa, but only the estimate for Oshawa was statistically significant.[8] Also, there was some evidence that ESP might have reduced unemployment benefit payments in Winnipeg, but not in Oshawa, although none of these estimates was statistically significant. Thus, Saskatoon produced the only consistent evidence of a program impact.

To further explore HRCC differences, Table 8.6 provides some non-experimental evidence about program impacts on job-search behaviour. This evidence was based on answers to follow-up survey questions that asked supplement group members whether they felt that ESP had caused them to look for jobs sooner, spend more time looking for work each week (look harder), consider new types of jobs (broaden their search), or consider lower-paying jobs (which also broadened their search).[9]

Supplement group members generally felt that ESP was far more likely to cause them to broaden the scope of their job search than to increase its intensity or start it sooner. This is consistent with the nature of ESP's financial incentive.

[8]The significance of the estimate for Oshawa reflects, in part, the large size of its sample.

[9]Chapter 6 presents a summary of answers to these questions for all supplement group members in the survey sample.

Table 8.6: Perceived ESP Effect on Job Search, by HRCC

HRCC	Percentage of Supplement Group Members Who Said ESP Had Caused Them To: Look for Jobs Sooner (%)	Spend More Time Looking each Week (%)	Consider New Types of Jobs (%)	Consider Lower-Paying Jobs (%)
Saskatoon	28.4*	32.7***	47.4***	69.2***
Granby	29.3	36.6***	53.7***	68.6*
Winnipeg	25.4	30.0*	46.4**	67.0**
Oshawa	25.4	28.4	45.5***	66.8**
Toronto	22.9	24.4	37.3	60.0

Note: A two-tailed test was used to determine whether the findings were statistically significantly different from those of the Toronto group. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

But the most relevant fact is that, in all four ways, supplement group members from Saskatoon were more likely to feel that ESP had influenced their job search than supplement group members from Toronto. For example, 47.4 percent of supplement group members from Saskatoon felt that ESP had caused them to consider new types of jobs, whereas only 37.3 percent from Toronto felt this way. Also, 69.2 percent of supplement group members from Saskatoon felt that ESP had caused them to consider lower-paying jobs, whereas only 60.0 percent from Toronto felt this way.[10]

In summary, it appears that ESP expedited full-time re-employment and reduced unemployment benefit payments somewhat for displaced workers in Saskatoon. This might have been because it caused a *few* supplement group members to broaden the scope of their job search. In Toronto, these impacts were not observed. In the other three HRCCs, there was some evidence of small impacts, but the evidence was not very strong or consistent.

DIFFERENCES IN FINANCIAL IMPLICATIONS

Table 8.7 lists, by HRCC, the net financial implications of ESP for displaced workers (column 1) and the Canadian government (column 2). These findings represent the experiences of sample members during their first 15 months after random assignment and all findings are expressed as averages per supplement group member. Hence, they pool the results for both supplement recipients and non-recipients in the supplement group.

The net financial implication of ESP for displaced workers is defined in Chapter 7 as the sum of total supplement payments per supplement group member,[11] the impact on average earnings, and the impact on average unemployment benefits. At most HRCCs, supplement payments were the largest component of this measure and impacts on unemployment benefits were the smallest component.

[10]Note, however, that findings in Chapter 6 indicate that even though supplement group members felt that ESP had influenced their job-search behaviour, there was no observable difference between the job-search activities of supplement group members and control group members.

[11]The 15-month follow-up period, not the full two-year maximum supplement receipt period, was used to report this outcome in order to make it comparable with estimates of ESP impacts on earnings and unemployment benefits.

Table 8.7: ESP Financial Implications, by HRCC

HRCC	Net Personal 15-Month Financial Benefit per Supplement Group Member[a] ($)	Net 15-Month Government Cost per Supplement Group Member[b] ($)
Saskatoon	1,679	-1,753
Granby	184	-1,711
Winnipeg	90	-1,423
Oshawa	780	-1,249
Toronto	377	-1,353

[a]The sum of total 15-month supplement payments per supplement group member, the ESP impact on average total 15-month earnings, and the ESP impact on average total 15-month unemployment benefits.

[b]The sum of total 15-month supplement payments per supplement group member, the ESP impact on average total 15-month unemployment benefits, and an $89 cost per supplement group member to administer the supplement program.

Note: It was not possible to test for the statistical significance of the difference between the financial implications of ESP in Toronto and those in the other HRCCs.

Displaced workers from Saskatoon experienced the largest net financial gain — an average of $1,679 or roughly $110 per month. This increase in personal income was due entirely to supplement payments, but was offset slightly by reduced unemployment benefit payments. There was no program-induced change in earnings. The estimated net financial gains for displaced workers from the other HRCCs were much smaller.

The net financial implication of ESP for the Canadian government is defined in Chapter 7 as the sum of the reduction in unemployment benefits minus total supplement payments per supplement group member minus the average cost of administering the supplement program per supplement group member. This outcome was negative at all five HRCCs, ranging from -$1,249 to -$1,753. Thus, ESP was a net cost to the government in all cases.

THE BOTTOM LINE: WHAT DO THE FINDINGS MEAN?

The preceding findings suggest that ESP was most successful in Saskatoon. Displaced workers at this site knew more about ESP, were more likely to receive supplement payments, were more likely to feel that ESP had influenced their job search, experienced the largest program-induced increase in the rate of return to full-time employment, experienced a reduction in unemployment benefit payments, and realized the largest net program benefit.

But even in Saskatoon, ESP had only a moderate effect on how soon displaced workers found new full-time jobs, whereas the costs to the Canadian government of making supplement payments were substantial. Thus, the main result of the program in Saskatoon was a transfer of government resources to the one out of three displaced workers who received supplement payments. Findings for the other HRCCs — especially those for Toronto — were less favourable.

Chapter 9:
What Has Been Learned About Re-employment Incentives for Displaced Workers

This chapter summarizes what has been learned about re-employment incentives for displaced workers based on findings from ESP and findings from the four re-employment bonus experiments conducted in the United States during the 1980s. The first section of the chapter compares the bonus experiments with ESP. The next section presents key findings from these studies. The final section considers the policy implications of the findings.

THE BONUS EXPERIMENTS AND ESP

As noted in Chapter 1, four randomized experiments were conducted in the United States to test bonuses for unemployment benefit claimants who found new full-time jobs quickly and stopped receiving unemployment benefit payments.[1] The primary goal of these experiments was to reduce the government costs of unemployment benefits without reducing claimants' future earnings by causing them to take new jobs prematurely. The experiments focused on unemployment benefit claimants in general and on displaced workers in particular.

The first and most successful bonus experiment was conducted during 1984 in Illinois. It found that paying a $500 cash bonus to unemployment benefit claimants who became re-employed full time within 11 weeks, and remained employed for at least four months, reduced total benefit payments by more than the cost of the bonus, on average. The next three experiments were conducted from 1986 to 1989 in New Jersey, Pennsylvania, and Washington State to test variations of the bonus approach in order to fine-tune it. The results of these experiments were disappointing, however. All three found that bonuses did not produce impacts that were large enough to "pay for themselves."

As discussed earlier, ESP took a different approach. Its primary focus was to improve the welfare of displaced workers, both by helping them to make successful labour market transitions and by compensating them for the losses they incurred in a way that promoted their re-employment. Thus, ESP focused on rapid re-employment in new full-time jobs by offsetting re-employment earnings losses. It was hoped that the relatively large incentive offered by the program would make a big difference in how fast displaced workers returned to work. It was also hoped that this would reduce the costs of their unemployment benefits appreciably. But reductions in unemployment benefits were not expected to fully offset the costs of incentives paid.

Instead of offering a lump-sum bonus payment to everyone who found a new job quickly, ESP offered to supplement the earnings of displaced workers who took a new job that paid less than their old one. As discussed earlier, this supplement would equal 75 percent of the

[1] Although the U.S. bonus experiments are described briefly in Chapter 1 of this report, some of this information is repeated to compare the experiments with ESP.

difference between weekly earnings in the new job and weekly earnings in the old job.[2] And it would be paid for up to two years.

Table 9.1 compares key features of the bonus experiments and ESP. Information for the bonus experiments was extracted from Meyer's 1995 review of them. First note that ESP was the most generous financial incentive tested. Its average total supplement payment was $8,705 per recipient versus $500 to $562 for three of the bonus experiments and $1,644 for the fourth. It was hoped that ESP's larger financial incentive would produce correspondingly larger impacts.

ESP was also less restrictive than the bonuses tested in two important ways. First, it gave displaced workers 26 weeks to find new full-time jobs instead of 3 to 13 weeks for the bonus experiments. It was hoped that relaxing this restriction would make ESP more attractive and thereby help it to produce larger impacts.

Second, ESP did not require participants to remain employed for a specified time in order to begin receiving supplement payments, whereas all of the bonus experiments did so. The Illinois, Pennsylvania, and Washington State experiments had a four-month minimum re-employment period, after which the full bonus amount was paid. The New Jersey experiment paid 60 percent of the bonus amount after four weeks of re-employment and the remainder after 12 weeks. ESP began paying supplements after the first week of re-employment and continued making payments as long as recipients stayed employed. Thus the five experiments reflected a continuum of immediacy and continuity of payments. It was hoped that having ESP supplements begin immediately upon re-employment would increase their attractiveness and thereby increase their impacts.

However, ESP was *more restrictive* than the bonus experiments in one very important way — it provided supplement payments only to displaced workers who experienced re-employment earnings losses. In part this was done to help compensate individuals who paid especially high costs due to economic change. And in part this was done to reduce the chance that adding a supplement to unemployment benefits would make the total package so attractive that it would increase the number of persons who claimed benefits.[3] However, there was concern that adding this restriction might limit the impacts of ESP and perhaps cause some displaced workers to take lower-paying jobs.

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[2]In calculating re-employment earnings losses, previous earnings from the layoff job were capped at the maximum level covered by unemployment benefits, which was $42,380 per year or $815 per week month when ESP began in 1995.

[3]Meyer, 1995, argues that such an "entry effect" could be a serious problem if re-employment bonuses became part of a national program. First, because not everyone who qualifies for unemployment benefits claims them; a greater percentage of qualifiers might claim their benefits if they became sufficiently more attractive. Second, enriching the package of unemployment benefits would make losing a job for short periods less "costly." This, in turn, might increase the willingness of employers to lay off workers or increase the willingness of laid-off workers to remain unemployed long enough to claim unemployment benefits.

Table 9.1: Key Features of the Re-employment Bonus Experiments and ESP

	Bonus Experiment				
	Illinois[a]	**New Jersey**	**Pennsylvania**	**Washington State**	**ESP**
Enrolment period	7/84 to 11/84	7/86 to 6/87	7/88 to 10/89	2/88 to 11/88	5/95 to 6/96
Bonus amount per recipient	$500	$1,644 on average	$500 on average	$562 on average	$8,705 on average[c]
When bonus offer was made	At job service registration after unemployment benefits claim was filed	Roughly 5 weeks after unemployment benefits claim was filed	After first unemployment benefits payment or waiting week	When unemployment benefits claim was filed	8 weeks on average after unemployment benefits claim was filed
Job-search period to qualify for bonus	11 weeks	10 weeks	6 weeks or 12 weeks	3 to 13 weeks[b]	26 weeks
Re-employment period	4 months	60% of bonus after 4 weeks and the rest after 12 weeks	16 weeks	4 months	None
Combinations of bonus amounts and job-search periods ("treatments")	1 combination	1 combination	5 combinations	6 combinations	1 combination
Study population	unemployment benefit claimants 20–55 years old without a definite recall date and not hired through a union hall	unemployment benefit claimants over 24 years old with 3 or more years of job tenure but without a definite recall date and not hired through a union hall	unemployment benefit claimants without a definite recall date of less than 60 days and not hired through a union hall	unemployment benefit claimants	unemployment benefit claimants with 3 years of continuous past employment but with no recall expectation and not hired through a union hall
Sample size	8,149 in two program groups plus 3,952 in the control group	8,675 in six program groups plus 2,385 in the control group	10,694 in six program groups plus 3,392 in the control group	12,452 in six program groups plus 3,082 in the control group	2,960 in the program group plus 2,952 in the control group

Source: Meyer, 1995, Table 1, pp. 94–95.

[a]Information is for the "employee experiment" only.

[b]The number of weeks was based on the maximum duration of individual unemployment benefit claims according to one of the following two formulas: 0.2 times the duration plus one week *or* 0.4 times the duration plus one week.

[c]The average total supplement payment per recipient for the two-year maximum receipt period.

There were several notable differences in the structure of the incentive programs tested. The Illinois program was the simplest — a flat $500 bonus with one job-search period and one re-employment period for everyone. The Pennsylvania and Washington State experiments tested several combinations of bonus amounts and job-search periods. Each combination was a separate treatment group to which sample members were randomly assigned. Within each treatment group, the bonus amount was set as a multiple of claimants' weekly unemployment benefits. Low bonus levels and high bonus levels were set in terms of lower or higher percentages of weekly benefits. The New Jersey bonus was set as a percentage of claimants' total remaining unemployment benefits. To further increase the incentive for rapid re-employment, this percentage declined over time. In addition, the New Jersey experiment tested a combination of bonuses with special job-search assistance versus job-search assistance only. ESP tested only one program — a two-year 75 percent supplement for re-employment earnings losses.

With one exception, the study populations of ESP and the bonus experiments were defined using the same criteria. All five experiments focused on unemployment benefit claimants. And all but the Washington State experiment excluded claimants hired through union halls plus those with definite recall dates.[4] ESP and the New Jersey experiment had a more explicit focus on displaced workers. The New Jersey experiment included only persons with three or more years of job tenure with a single employer, and ESP included only persons with three or more years of continuous employment with one or more employers. All of the experiments had large samples.

KEY IMPACT ESTIMATES FROM THE BONUS EXPERIMENTS AND ESP

All five experiments reported estimates of program impacts on the number of weeks of unemployment benefit payments received (benefit weeks). This measure was the central focus of the bonus experiments and a key finding for ESP. (This finding is reported in Table 9.2 for the first year after random assignment for the four bonus experiments.[5]) To place these findings in perspective, note that control group members averaged 15 to 20 weeks of benefit payments during this period.[6] The corresponding impact estimate for ESP was reported on an annualized basis for the first 15 months after random assignment.

As can be seen, Illinois had the largest and most statistically significant impact — an average reduction of 1.15 benefit weeks. This was equivalent to roughly $200 per program group member, which was not large in total dollars or as a percentage of total benefits. However, it was large enough to exceed bonus costs because less than 14 percent of all program group members were paid bonuses.[7]

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[4]Although ESP did not exclude from random assignment persons who expected to be recalled (either with or without a specific date), this report excludes them from the analysis for reasons discussed in Chapter 2.

[5]Findings for the bonus experiments were extracted from Meyer, 1995.

[6]Meyer, 1995, p. 100.

[7]About 25 percent of the Illinois program group members qualified to receive bonus payments, but less than 14 percent actually received them (Meyer, 1995).

Table 9.2: Key Findings from the Re-employment Bonus Experiments and ESP

	Bonus Experiment				
	Illinois[a]	New Jersey[b]	Pennsylvania	Washington State	ESP
Impact on average annual weeks of unemployment benefits[c]	-1.15***	-0.40	-0.19 to -0.92***[d]	-0.06 to -0.73**[e]	0.2
Impact on average annual earnings[f]	$366*	-$77[g]	-$289 to $171[h]	-$226 to $300[i]	-$546

[a]Findings are for the "employee experiment" only.

[b]Findings represent the *difference* between estimated impacts for the bonus *plus* job-search assistance and the estimated impact of job-search assistance *only*.

[c]Findings for the bonus experiments are for the first year after filing for unemployment benefits. Those for ESP were annualized from data for the first 15 months after random assignment.

[d]Findings are for five combinations of bonus amounts and job-search periods. Only the high bonus and long qualification period treatments produced significant impact estimates.

[e]Findings are for six combinations of bonus amounts and job-search periods. Impact estimates for the high bonus and short qualification period and the low bonus and long qualification period were significant at the 0.10 level; that for the high bonus and long qualification period was significant at the 0.05 level.

[f]Findings for Illinois, Pennsylvania, and Washington State are for the first year after random assignment. Findings for New Jersey were annualized from data for the first 2.5 years after random assignment and those for ESP were annualized from data for the first 15 months after random assignment.

[g]Standard error was not available to determine significance level.

[h]Findings are for four combinations of bonus amounts and job-search periods.

[i]Findings are for six combinations of bonus amounts and job-search periods.

Source: Meyer, 1995, Table 2, pp. 98–99.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

The impact of the bonus in New Jersey was estimated as the difference between the impact for the bonus plus job-search assistance and that for job-search assistance only.[8] This difference implied a 0.40-week reduction in unemployment benefit payments.[9] The impact of the bonus in Pennsylvania and Washington State varied across combinations of bonus amounts and job-search periods. The smallest impact (for a small bonus and short job-search period) was a 0.06-week reduction and the largest impact (for a larger bonus and longer job-search period) was a reduction of 0.92 weeks. Although impacts increased somewhat as bonus amounts and job-search periods increased, these relationships were not strong. And in no case other than Illinois did bonuses produce meaningful government cost savings; they most often increased costs.[10] Corresponding impacts for ESP were negligible — a 0.2-week increase in benefits — and were not statistically significant.

Table 9.2 also presents estimates of program impacts on average annual earnings. This was a key result for the bonus experiments because of concerns that, if bonuses "worked" as intended, and prompted recipients to take new jobs sooner, this might reduce their job search below its "optimal" level, which, in turn, might reduce their earnings. In principle, this problem was further compounded for ESP because it directly subsidized new jobs that paid less than former ones.

Findings in the table for Illinois, Pennsylvania, and Washington State were based on earnings during the first year after random assignment. Findings for New Jersey were annualized from results for the first two-and-a-half years after random assignment. Findings for ESP were annualized from results for the first 15 months after random assignment.

The only statistically significant earnings impact was for Illinois, where program group members earned $366 *more* per year due to the bonus, or about $31 more per month. By inducing them to start work sooner, the Illinois bonus caused program group members to earn more, not less, in the short run. While not large, this was the most favourable earnings impact observed.

In New Jersey, Pennsylvania, and Washington State, earnings impacts fluctuated from negative to positive, although none were large or statistically significant. ESP reduced annual earnings by $546, or $46 per month. This was less than a five-percent reduction and was not statistically significant.

Findings about the impact of bonuses on employment were more difficult to summarize in a table because the different studies estimated and reported employment impacts in different ways.[11] Hence, they are not included in Table 9.2. Nevertheless, these findings are consistent with those reported for impacts on earnings and unemployment benefits.

Thus, none of the experiments indicated that re-employment bonuses would produce large effects and only the Illinois experiment suggested that such bonuses could be cost-

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[8]This follows the procedure used by Meyer, 1995.

[9]The impact of the bonus *plus* job-search assistance was only a 0.90-week reduction in unemployment benefits, however.

[10]See Meyer, 1995, Table 3, p. 103.

[11]The Illinois and Washington State studies reported impacts in terms of weeks of "insured unemployment," which is basically the same as the number of weeks for which unemployment benefits were received (benefit weeks) discussed above (see Woodbury and Spiegelman, 1987; and Spiegelman, O'Leary, and Kline, 1992, respectively). The New Jersey and Pennsylvania experiments reported employment impact estimates obtained from survey data and from earnings data reported by employers to each state unemployment insurance agency (see Corson et al., 1989; and Corson et al., 1991, respectively).

effective. In addition, ESP findings did not indicate that supplementing re-employment earnings losses would have a big effect.

Before reaching a final conclusion about ESP, however, it is important to consider a broader range of its findings.

FURTHER ESP FINDINGS

Table 9.3 summarizes the main findings presented for ESP in previous chapters of this report. This summary, plus the preceding summary of findings for the bonus experiments, helps to provide a basis for drawing conclusions about the effectiveness of financial re-employment incentives for displaced workers.

Table 9.3: Summary of Findings for ESP

	Supplement Group	Standard Group	Impact
Supplement receipt			
Percent who *qualified for* supplement payments[a] (%)	26.8	na	na
Percent who ever *received* supplement payments any time during the supplement period (%)	20.5	na	na
Average 24-month supplement payments per recipient ($)	8,705	na	na
Average 15-month supplement payments per supplement group member ($)	1,161	na	na
Percent who *qualified for* earnings insurance (%)	11.9	na	na
Percent who *initiated* earnings insurance (%)	1.5	na	na
Percent who *received* supplement payments after initiating earnings insurance (%)	0.2	na	na
Impacts			
Percent employed full time within six months of random assignment (%)	42.3	37.9	4.4*** (0.00)
Average total earnings during the first 15 months after random assignment ($)	14,209	14,891	-682 (0.12)
Average weeks of unemployment benefits during the first 15 months after random assignment	22.1	21.9	0.2 (0.77)
Average total amount of unemployment benefits during the first 15 months after random assignment ($)	6,460	6,370	90 (0.50)
Financial implications			
Net financial *benefits* per supplement group member during the first 15 months after random assignment ($)	569	na	na
Net government *costs* per supplement group member during the first 15 months after random assignment ($)	1,340	na	na

[a]This is an estimate of the percentage of supplement group members who, within six months, found a new full-time job that paid less than their former job.

Note: Statistical significance was calculated using a two-tailed test. Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01. P-values are reported in parentheses.

Supplement Receipt

Chapter 5 reported that 26.8 percent of the ESP supplement group members qualified for supplement payments by starting a new, lower-paying, full-time job within six months after random assignment. It also reported that 20.3 percent of all supplement group members actually received supplement payments for re-employment during this period. Thus, 72.1 percent of the supplement qualifiers applied for and received the payments to which they were entitled. This suggests that displaced workers who qualified for supplement payments were aware of and interested in them.

Furthermore, the benefits to supplement recipients were substantial. About two-thirds received payments for 53 weeks or longer, and the average total payment was $8,705 per recipient.

Given the large total payment per recipient and the fact that 2 out of 10 supplement group members became recipients, the average total payment per supplement group member was $1,161 during the 15-month follow-up period for the ESP impact analysis.[12]

Chapter 5 reported that 11.9 percent of the supplement group members qualified for ESP "earnings insurance" by finding a new full-time job within six months that paid as much or more than their previous job. Thus many supplement group members could have initiated this option. However, only 1.5 percent (44 persons) did so. Thus, only about one out of eight supplement group members who qualified for earnings insurance bothered to initiate it. Furthermore, only seven of the initiators subsequently used this insurance to help offset a future earnings loss.

Overall, most displaced workers who qualified for supplement payments were interested enough to apply for and receive them. Furthermore, those who did so benefited substantially. But very few displaced workers who qualified for earnings insurance bothered to initiate this option and only a small fraction of these individuals used their insurance to cover a future earnings loss.

Program Impacts

Because ESP was designed to stimulate rapid re-employment in full-time jobs and because its supplement qualification period was six months, the central outcome measure for the ESP evaluation was the percentage of sample members who took new full-time jobs within six months. Table 9.3 indicates that 42.3 percent of the supplement group members and 37.9 percent of the control group members did so, for a program-induced increase of 4.4 percentage points. Thus, it appears that ESP took a small step toward its goal of expediting full-time re-employment. This modest but statistically significant increase was the single most positive finding obtained for the overall study sample.

As discussed earlier, expectations for ESP impacts on earnings were unclear. On the one hand, it was hoped that the program would increase earnings in the short run by causing displaced workers to start new jobs sooner. On the other hand, it was feared this might end their job search prematurely, resulting in "sub-optimal" job matches that would reduce earnings relative to what they would have been in the absence of ESP. In addition, there was

[12]This finding is reported because it was comparable with estimates of ESP impacts on earnings and unemployment benefits that were computed per supplement group member for the 15-month follow-up period.

concern that ESP would further reduce earnings by directly inducing some displaced workers to take lower-paying jobs.

Table 9.3 indicates that, during the first 15 months after random assignment, supplement group members earned $14,209, on average, and control group members earned $14,891. This implies a program-induced earnings reduction of $682 over 15 months, or $45 per month. This modest five-percent decrease was not statistically significant.

Table 9.3 also indicates that ESP did not reduce the duration or amount of unemployment benefits received. During the first 15 months after random assignment, supplement group members received benefits for 22.1 weeks, on average, and control group members received them for 21.9 weeks. This represented total payments of $6,460 and $6,370, respectively. The difference between supplement group members and control group members was negligible and not statistically significant.

Thus, on balance, ESP expedited full-time employment slightly for displaced workers but had little or no impact on their earnings or unemployment benefit payments.

Financial Implications

Table 9.3 indicates that ESP's net financial benefit for displaced workers was $569 per supplement group member during the first 15 months after random assignment, or $38 per month. This comprised $1,161 in supplement payments (the largest component), minus $682 in reduced earnings (which was not statistically significant), plus $90 in increased unemployment benefits (which was negligible and not significant).

To place the $569 net benefit in perspective, note that, for control group members, average earnings were $14,891 during this period and average unemployment benefit payments were $6,370, for a total of $21,261. Thus, even including supplement payments, ESP increased total personal income by only 2.7 percent (569/21,261).

However this small average benefit for all supplement group members masks a much larger average benefit for the 2 out of 10 supplement group members who received supplement payments. During the first 15 months after random assignment, these recipients averaged $5,663 in supplement payments, plus $21,176 in earnings, plus $3,630 in unemployment benefit payments, for a total of $30,469. Thus, supplement payments comprised 18.6 percent of their personal income.

Because ESP had only a modest effect on re-employment, most supplement recipients received payments for jobs that they would have taken anyway. In other words, they experienced "windfall gains." However, because *all of them* had incurred an earnings loss (which often was quite large) due to labour market displacement, the supplement payments they received helped to compensate for the losses they had incurred.

The government costs of ESP totalled $1,340 per supplement group member for the first 15 months after random assignment. This was the net result of $1,161 in supplement payments, plus $89 in costs to administer the supplement program, plus $90 in increased unemployment benefit payments. The $1,161 in supplement payments and $90 in increased unemployment benefits were a *transfer of resources* from the Canadian government (or taxpayers) to displaced workers. The $89 to administer the supplement program was a true opportunity cost because these resources could not have been used for other purposes.

On balance, ESP produced a transfer of resources from the Canadian government (taxpayers) to the 2 out of 10 supplement group members who received supplement payments. These resources helped to compensate recipients for the losses they incurred due to labour market displacement.

Best-Case Findings for an HRCC

The preceding findings represent the average response to ESP by displaced workers from all five HRCCs in the study. However, the findings reported in Chapter 8 suggest that ESP was more successful in Saskatoon than elsewhere. Nevertheless, these results do not change the program's financial implications.

In Saskatoon, ESP increased full-time re-employment rates for the first six months after random assignment by 7.3 percentage points. However, this produced virtually no impact on total earnings during the first 15 months after random assignment, and it reduced unemployment benefits during the period. Total earnings were increased by only $15. Total unemployment benefits were reduced by $384 or 7.4 percent, and the duration of these benefits was reduced by 1.3 weeks or 6.3 percent. These findings represent a "best-case" scenario for ESP.

From the perspective of displaced workers, these findings imply an increase of $1,521 in personal income per supplement group member during the 15-month follow-up period. This was the net result of $1,890 in supplement payments, plus $15 in increased earnings, minus $384 in reduced unemployment benefit payments. Since almost all of this benefit was from supplement payments, it was experienced by only the 2 out of 10 supplement group members who received these payments.

From the government perspective, the findings for Saskatoon imply a total cost of $1,595 per supplement group member. This was the net result of $1,890 in supplement payments, plus $89 for administering the supplement program, minus the $384 savings from reduced unemployment benefit payments.

Thus, the main result of ESP in Saskatoon was the same as that for the overall sample — it transferred government resources to supplement recipients as partial compensation for their losses.

Policy Implications

Policy decisions about using re-employment bonuses or earnings supplements for displaced workers should reflect answers to the following three questions:

- Can bonuses or supplements reduce unemployment benefit payments by enough to justify their use?
- Can they expedite re-employment without reducing earnings?
- Can they help to compensate displaced workers in a way that neither inhibits re-employment nor increases the number of persons who claim unemployment benefits?

The emphasis placed on each question will depend on the value attached to the issue it addresses. Thus, policy decisions must ultimately rest both on political judgments and programmatic considerations.

Findings to date suggest that the answer to the first question is "no." Re-employment bonuses or supplements have not been shown to reduce unemployment benefit payments substantially — certainly not by enough to justify their use on this basis alone. Only the Illinois bonus experiment suggested otherwise. And this finding would have been less favourable if more persons who qualified for bonuses had received them.

Findings to date suggest that the answer to the second question is "not by much." The most direct evidence of this comes from ESP, where a very large earnings supplement produced only a modest effect on re-employment.

The answer to the third question is "no" for bonuses but "perhaps yes" for supplements. Many have argued that, because displaced workers bear the direct costs of economic change, which benefits the public in general, it is only fair for government to compensate them for their losses.[13] If this argument is accepted, the next logical question is "How should this be done?" There are three basic approaches: unemployment benefits, re-employment services, and re-employment incentives.

Unemployment benefits provide temporary income support to individuals while they try to find new jobs. But these payments can create incentives that may prolong unemployment or increase the number of persons who become unemployed. This issue has been the subject of widespread debate and considerable research for many years.[14] Although the research is not definitive, the balance of evidence suggests and the weight of current professional opinion concludes that unemployment benefit payments do increase unemployment to some extent. Thus, both in North America and in Europe, there has been increased emphasis on "active" labour market policies to promote re-employment.[15]

Two categories of such active policies exist: direct services and financial incentives. Both have been tested extensively using randomized experiments. Most of the evidence from these studies indicates that services that help people to find new jobs can be effective and often can pay for themselves by reducing unemployment benefit payments.[16] Hence, this approach appears to be a promising way to compensate some displaced workers by helping them to help themselves. Because it is not clear, however, "which of these services work best for whom," further research on the effectiveness of services is warranted.

Of the two types of re-employment incentives tested — bonuses and supplements — neither approach has demonstrated much effect on employment, earnings, or unemployment benefits, so they are both fairly "neutral" in this regard. However, adding re-employment bonuses to a national program of unemployment benefits might increase the attractiveness of the overall package and thereby increase the number of persons who claim these benefits. This probably would be much less of a problem for supplements designed to offset re-employment earnings losses.

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[13]Baily, Burtless, and Litan, 1993, propose that "earnings insurance" be provided to displaced workers because they bear the costs of economic change, the benefits of which accrue to others.

[14]See Atkinson and Micklewright, 1991; Devine and Kiefer, 1991; and Christofides and McKenna, 1996, for discussions about the work disincentive effects of unemployment benefits.

[15]For a discussion of these issues in Europe, see OECD, 1990. For a discussion of these issues in the United States, see Ross and Smith, 1993.

[16]Meyer's review of research on job-search assistance (1995) provides considerable evidence that the economic benefits of these services outweigh their costs. However, research on job training for displaced workers suggests that its benefits may not justify its much higher costs (for example, see Bloom, 1990).

Bonuses might increase the number of persons who claim unemployment benefits in several ways.[17] First, because not all persons who qualify for unemployment benefits actually claim them, bonuses could increase the number who do so. This is especially true for persons who expect to become re-employed quickly and thus would have little need for unemployment benefits but could expect to qualify for a bonus. Second, persons who change jobs could delay taking their new jobs to qualify for unemployment benefits and receive a bonus. Third, adding bonuses to unemployment benefits reduces the cost to workers of short layoffs. This would subsidize unstable industries where short layoffs are prevalent and thereby could increase the number of such layoffs. This, in turn, could lead to increased claims for unemployment benefits.

Supplements for re-employment earnings losses are arguably less susceptible to such "program entry effects" because they help only persons who take new jobs that pay less than their old ones. It does not seem plausible that workers would consciously wait longer between jobs just to insure themselves against the possibility of earning less in their next job. It seems even less plausible that employers would change their layoff practices to account for this new incentive. It does not even seem likely that unemployed persons who otherwise would not bother to claim unemployment benefits would do so if a supplement of this type were added. Direct evidence on this last point is provided by the fact that very few displaced workers who qualified for ESP earnings insurance bothered to complete the simple paperwork required to initiate this option.

CONCLUSION

On July 1, 1996 the *Employment Insurance Act* came into effect in Canada. This new legislation retained the basic system of financing unemployment benefits that had been in effect, but changed the distribution and size of benefits paid. Among other changes that were initiated, the new law increased benefits for low-income families, decreased benefits for repeat users of unemployment benefits, and reduced benefits for highly paid workers. The legislation also made it possible to use some of the funding available for active employment programs, including re-employment earnings supplements like that tested by ESP.

The findings from ESP indicate that supplements probably will not improve the labour market prospects of displaced workers markedly or reduce their unemployment benefit payments substantially. Implementing an earnings supplement program might be appropriate, however, if the goal is to provide direct financial compensation to displaced workers who must take lower-paying jobs in the future and thus will pay a disproportionately higher share of the costs of economic progress. In the words of one ESP supplement recipient, receiving a supplement "was the difference between worrying all the time and not." Therefore, supplements can help to cushion the shock as displaced workers make a transition from their past to their future. But supplements are not likely to change this future.

[17]The following argument was put forth by Meyer, 1995.

Appendix A:
Estimating the Impacts of ESP

This appendix explains how the impacts of ESP were estimated. It first describes the data sources and samples involved, then outlines the methods used to obtain regression-adjusted impact estimates for the full sample and for subgroups of the sample.

DATA SOURCES AND SAMPLES

Two data sources were used to estimate ESP impacts. Data from the 15-month follow-up survey were used to estimate impacts on job search, employment, earnings, and wage rates. Data from HRDC administrative records were used to estimate impacts on the duration and amount of unemployment benefit payments. Complete data on unemployment benefit payments were available for 5,790 of the 5,912 displaced workers in the baseline sample. Complete follow-up survey data were available for 4,479 of the displaced workers in the baseline sample. The difference between the two samples was mostly due to the fact that 11 percent of the displaced workers in the baseline sample did not respond to the survey and 13 percent of the survey respondents did not have usable survey data on employment and earnings.

Table A.1 compares the characteristics of the 15-month survey sample with those of the 15-month benefit sample. Almost all of these characteristics were distributed similarly in two samples, suggesting that they represent basically the same group.

Table A.1: Characteristics of the 15-Month Survey Sample and the 15-Month Benefit Sample

Baseline Characteristics	15-Month Survey Sample (%)	15-Month Benefit Sample (%)
ESP site		
Granby	6.1	5.9
Oshawa	40.4	38.3
Toronto	21.9	24.7
Winnipeg	14.5	14.7
Saskatoon	17.1	16.4
Gender		
Male	43.8	45.9
Female	56.2	54.1
Age		
30 years or younger	21.9	22.5
31–44 years	44.6	45.1
45–54 years	20.8	20.8
55 years or older	12.6	11.7

Table A.1: Characteristics of the 15-Month Survey Sample and the 15-Month Benefit Sample (cont'd)

Baseline Characteristics	15-Month Survey Sample (%)	15-Month Benefit Sample (%)
Highest education credential		
Less than high school	20.5	20.2
High school	46.1	45.8
College	17.4	17.1
University	16.0	17.0
Number of years worked for last employer		
Less than 1 year	13.2	14.0
1–2 years	13.7	13.9
3–5 years	26.7	27.7
6–9 years	23.1	22.5
10 years or more	23.3	21.8
Average prior weekly insurable earnings		
Less than $200 per week	2.8	2.9
$200–$399 per week	19.6	19.7
$400–$599 per week	31.2	30.8
$600–$799 per week	28.0	28.1
$800 per week or more	18.3	18.5
Received severance upon layoff		
No	58.6	59.2
Yes	41.4	40.8
Union member in last job		
No	82.4	82.8
Yes	17.6	17.2
Weeks of unemployment benefit entitlement		
38 weeks or less	24.9	25.6
39–42 weeks	52.7	53.0
43–50 weeks	22.5	21.4
Number of household members		
1 person	17.8	19.2
2 persons	31.7	31.7
3 persons or more	50.4	49.2
Number contributing to household income		
1 adult	35.4	36.7
2 adults	57.8	56.4
3 adults or more	6.8	6.9

To further explore this issue, a sensitivity analysis was conducted by estimating ESP impacts on unemployment benefit payments for each sample. Table A.2 presents the results of this analysis. As can be seen, the impact estimates for the two samples were quite similar, although those for the larger unemployment benefit sample were slightly more efficient statistically (that is, they had lower p-values).

Table A.2: Sensitivity Analysis — ESP Impacts on Unemployment Benefits, by Sample

Unemployment Benefit Outcome	Sample	Supplement Group	Control Group	Impact	Significance Level (p-value)
15-month mean weeks of unemployment benefits	Unemployment benefit sample	22.1	21.9	0.2	0.77
	Survey sample	22.1	22.2	-0.1	0.92
15-month mean dollars of unemployment benefits ($)	Unemployment benefit sample	6,460	6,370	90	0.50
	Survey sample	6,462	6,435	27	0.86

Therefore it appears that using two different samples — the 15-month survey sample and the 15-month benefit sample — to estimate ESP impacts on different outcomes did not affect the comparability of these findings, and made it possible to maximize the sample size for each estimate.

IMPACT ESTIMATION

In a randomized experiment, unbiased estimates of program impacts can be obtained by comparing mean outcomes for the program group and the control group. However, using multiple regression to make this comparison can increase the precision or statistical power of the impact estimates. Hence, this approach was used for the present report.

If random assignment was successful, there should be little difference between point estimates of impacts obtained from simple differences of means and those obtained from regression-adjusted differences of means. Only the standard errors of these impact estimates should differ and they should be smaller for the regression-adjusted estimates.

Table A.3 illustrates that this was in fact the case for ESP impact estimates. The point estimates obtained from the two methods were quite similar and estimates from the regression approach were slightly more statistically significant (their p-values were smaller) in most cases.

Table A.3: Regression-Adjusted Versus Unadjusted Impact Estimates

Outcome	Estimation Method	Supplement Group	Control Group	Impact	Significance Level (p-value)
Percentage employed full time during the first six months (%)	Adjusted	42.3	37.9	4.4	0.00
	Unadjusted	42.4	37.7	4.7	0.00
15-month mean earnings ($)	Adjusted	14,209	14,891	-682	0.12
	Unadjusted	14,235	14,865	-630	0.21
15-month mean weeks of unemployment benefits	Adjusted	22.1	21.9	0.2	0.77
	Unadjusted	22.0	22.0	0.0	0.98
15-month mean dollars of unemployment benefits ($)	Adjusted	6,460	6,370	90	0.50
	Unadjusted	6,460	6,370	90	0.54

The independent variables (covariates) for the regression models were individual characteristics of sample members plus a dummy variable to indicate membership in the supplement group. Table A.4 lists each covariate included.

Table A.4: Covariates in the ESP Impact Regression Models

Covariate Name	Covariate Definition
PROGRAM	Belongs to supplement group, or not (1/0)
RAYM9509	Random assignment month is September 1995, or not (1/0)
RAYM9510	Random assignment month is October 1995, or not (1/0)
RAYM9511	Random assignment month is November 1995, or not (1/0)
RAYM9512	Random assignment month is December 1995, or not (1/0)
RAYM9601	Random assignment month is January 1996, or not (1/0)
RAYM9602	Random assignment month is February 1996, or not (1/0)
RAYM9603	Random assignment month is March 1996, or not (1/0)
RAYM9604	Random assignment month is April 1996, or not (1/0)
RAYM9605	Random assignment month is May 1996, or not (1/0)
RAYM9606	Random assignment month is June 1996, or not (1/0)
RAYM9607	Random assignment month is July 1996, or not (1/0)
RAYM9608	Random assignment month is August 1996, or not (1/0)
RAYM9609	Random assignment month is September 1996, or not (1/0)
SVRNC	Received severance payment upon layoff, or not (1/0)
HOMEOWN	Own home at baseline, or not (1/0)
HOMEMISS	Dummy for missing data on information about home ownership (1/0)
AGE1	Age of sample member is less than or equal to 30, or not (1/0)
AGE2	Age of sample member is between 31 and 44, or not (1/0)
AGE3	Age of sample member is between 45 and 54, or not (1/0)
GENDER	Male, or not (1/0)
INEA1	Average prior weekly insurable earnings are less than $200, or not (1/0)
INEA2	Average prior weekly insurable earnings are $200–$399, or not (1/0)
INEA3	Average prior weekly insurable earnings are $400–$599, or not (1/0)
INEA4	Average prior weekly insurable earnings are $600–$799, or not (1/0)
TORONTO	ESP site is Toronto, or not (1/0)
OSHAWA	ESP site is Oshawa, or not (1/0)
WINNIPEG	ESP site is Winnipeg, or not (1/0)
SASKATN	ESP site is Saskatoon, or not (1/0)
EMPSP1	Number of years worked for last employer is less than 1 year, or not (1/0)
EMPSP2	Number of years worked for last employer is 1–2 years, or not (1/0)
EMPSP3	Number of years worked for last employer is 3–5 years, or not (1/0)
EMPSP4	Number of years worked for last employer is 6–9 years, or not (1/0)
EMPSP5	Number of years worked for last employer is 10 or more years, or not (1/0)
UNIV	Highest education credential is university, or not (1/0)
COLL	Highest education credential is college, or not (1/0)
HIGH	Highest education credential is high school, or not (1/0)
UNION	Union member in last job, or not (1/0)
UNIMISS	Dummy for missing data on information about union membership (1/0)
INSWKS1	Number of insurable unemployment benefit weeks is less than 39, or not (1/0)
INSWKS2	Number of insurable unemployment benefit weeks is 39–42, or not (1/0)
RPY1	Total unemployment benefit amount in the 1st year before random assignment ($)
RPY2	Total unemployment benefit amount in the 2nd year before random assignment ($)
RPY3	Total unemployment benefit amount in the 3rd year before random assignment ($)

Table A.4: Covariates in the ESP Impact Regression Models (cont'd)

Covariate Name	Covariate Definition
RPYMISS	Dummy for missing data on information about unemployment benefit amount in the three years before random assignment (1/0)
UIPM1	Received unemployment benefit payments in the 1st month before random assignment (1/0)
UIPM2	Received unemployment benefit payments in the 2nd month before random assignment (1/0)
RA_LAG	Lag between random assignment and UI application date (weeks)
FL2_1	Employed full time at least 1 week in the 1st month before random assignment (1/0)
FL2_2	Employed full time at least 1 week in the 2nd month before random assignment (1/0)
F2_1MSS	Dummy for missing data on full-time employment in the two months before random assignment
NHHI1	Number contributing to household income is 1 adult, or not (1/0)
NHHI2	Number contributing to household income is 2 adults, or not (1/0)
NHHI3	Number contributing to household income is 3 or more adults, or not (1/0)
NPHH1	Number in household is 1 person, or not (1/0)
NPHH2	Number in household is 2 persons, or not (1/0)
NPHH3	Number in household is 3 or more persons, or not (1/0)
INDAGR	Industry category of last job is agriculture, or not (1/0)
INDPRI	Industry category of last job is other primary, or not (1/0)
INDMNF	Industry category of last job is manufacturing, or not (1/0)
INDCON	Industry category of last job is construction, or not (1/0)
INDTRN	Industry category of last job is transportation, or not (1/0)
INDCOM	Industry category of last job is communications, or not (1/0)
INDUTL	Industry category of last job is utilities, or not (1/0)
INDTRD	Industry category of last job is trade, or not (1/0)
INDFIN	Industry category of last job is finance, etc., or not (1/0)
INDCSV	Industry category of last job is community service, or not (1/0)
INDBUS	Industry category of last job is business and personal services, or not (1/0)
INDMIC	Industry category of last job is miscellaneous services, or not (1/0)
INDPUB	Industry category of last job is public administration, or not (1/0)
PINSLF	Pineo occupation code is self employed professional, or not (1/0)
PINPRF	Pineo occupation code is employed professional, or not (1/0)
PINMNG	Pineo occupation code is high level management, or not (1/0)
PINSPR	Pineo occupation code is semi-professional, or not (1/0)
PINTCH	Pineo occupation code is technician, or not (1/0)
PINMDM	Pineo occupation code is middle manager, or not (1/0)
PINSPV	Pineo occupation code is supervisor, or not (1/0)
PINFOR	Pineo occupation code is foreperson, or not (1/0)
PINCS1	Pineo occupation code is skilled clerical/sales/service, or not (1/0)
PINCT1	Pineo occupation code is skilled crafts and trade, or not (1/0)
PINCS2	Pineo occupation code is semi-skilled clerical/sales/service, or not (1/0)
PINMN1	Pineo occupation code is semi-skilled manual, or not (1/0)
PINCS3	Pineo occupation code is unskilled clerical/sales/service, or not (1/0)
PINMN2	Pineo occupation code is unskilled manual, or not (1/0)
PINFRM	Pineo occupation code is farm labour, or not (1/0)

Data for some covariates were missing for some sample members. To avoid dropping these sample members from the analysis, "missing data indicators" were added to the regression model. For each binary covariate with missing data (for example, home-ownership

status) a missing variable indicator was added to the model and set equal to one for sample members with missing data for the covariate and zero for all others. The value of the covariate itself was then set equal to zero for sample members with data missing for the covariate.[1] If the covariate was multi-categorical (for example, age), the "left-out" category was used to indicate missing data. Values for dummy variables for all categories of the variable were then set equal to zero for sample members who were missing data for the variable.

Impacts on Continuous Outcomes and Binary Outcomes

Ordinary Least Squares (OLS) regression was used to estimate ESP impacts both on continuous outcomes (such as mean earnings, mean weeks of unemployment benefit payments, and mean dollars of unemployment benefit payments) and binary outcomes[2] (such as the percentage of sample members employed full time and the percentage receiving unemployment benefits).

ESP impacts for continuous outcomes and binary outcomes were estimated as the coefficient for the supplement group dummy variable. A two-tailed t-test of this coefficient was used to assess its statistical significance and the resulting significance level (p-value) was reported.

In addition to reporting regression-adjusted impact estimates, it was also useful in some cases to report regression-adjusted means for the supplement group and the control group. The regression-adjusted mean for the control group was obtained by substituting the mean value for the full sample of each covariate into the estimated regression model and setting the supplement group dummy variable to zero. The regression adjusted mean for the supplement group was obtained by adding the ESP impact to the control group mean.

Impacts by Subgroup

Subgroups were defined using baseline data. When sample members were missing data needed to define a subgroup, they were excluded from that subgroup analysis. For example, if data on age were missing for an individual, he or she would be excluded from the impact analysis by age group.

Impacts for each subgroup were estimated by splitting the sample into subgroups and estimating a separate impact regression for each. This is often referred to as the "split-sample" method. The regression procedures for estimating each subgroup impact were the same as those described above for the full sample.

A partial F-test (Chow test) was used to assess the statistical significance of differences in subgroup impact estimates. This was accomplished by estimating two OLS regression models for each set of subgroup estimates — a restricted model and an unrestricted model — from data for the full sample. The restricted model contained one supplement group dummy variable, which thus allowed only one estimate of the impact of ESP. Hence, the model

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[1]The following example shows how missing data indicators were specified and how covariates were defined accordingly:

- HOMEOWN = 1 if a sample member owned a home at layoff, 0 if not, and 0 if data on home ownership were missing.
- HOMEMISS = 1 for sample members with missing data on home-ownership status and 0 for all others.

[2]An OLS model with a binary dependent variable is called a "linear probability model."

restricted the impacts for all subgroups to be the same. The unrestricted model specified interactions between the subgroups and the supplement group dummy variable, which allowed impacts to vary by subgroup; it did not restrict them to be the same.

To assess the statistical significance of subgroup impact differences, an F-statistic was computed by comparing the Sum of Squared Errors (SSE) for the restricted and unrestricted models as follows:

$$F = \frac{[SSE_{restricted} - SSE_{unrestricted}] / J}{SSE_{unrestricted} / (T - K - 1)}$$

where

J = the difference between the number of parameters in the unrestricted model and the number of parameters in the restricted model.

T = the total sample size.

K = the number of covariates.

Appendix B:
Findings for Sample Members Who Expected To Return to their Previous Employer When They Applied for ESP

As explained in Chapter 2, unemployment benefit claimants who met all of the eligibility requirements for ESP but expected to return to their previous employer were enrolled in the project and randomly assigned to the supplement group or the control group. This was done because it was not clear at the time whether these individuals would in fact return to their previous employer (and thus have no need for a supplement), or not return to their previous employer (and thereby become a displaced worker who might qualify for a supplement).

However, these sample members were not included in the analysis for the present report because it was determined subsequently that ESP was not relevant for them. They had little or no interest in a program that offered to subsidize a new, lower-paying job, and did not reward returning to a previous job. Only 6.5 percent received supplement payments — as opposed to 20.5 percent of supplement group members who did not expect to return to their previous employer. To complete the analysis, Table B.1 presents findings for the 2,202 sample members who expected to return to their previous employer.[1]

Table B.1: Key Findings for All Sample Members Who Expected To Return to their Previous Employer

	Supplement Group	Control Group	Impact	Significance Level (p-value)
Supplement receipt[a]				
Percent who received supplement payments (%)	6.5	na	na	na
Mean weeks of supplement payments per recipient	43.5	na	na	na
Mean total supplement payments per recipient ($)	5,811	na	na	na
Impacts				
Percent employed full time within six months of random assignment (%)	62.5	58.8	3.7	0.09
Average total earnings during the first 15 months after random assignment ($)	19,152	19,118	35	0.96
Average weeks of unemployment benefit payments during the first 15 months after random assignment	18.9	19.0	-0.1	0.82
Average total amount of unemployment benefit payments during the first 15 months after random assignment ($)	5,269	5,193	76	0.65

[a]Findings are for supplements received any time during the two-year supplement receipt period.

Note: The sample size for the data sets used in this table are: 2,202 in the baseline sample; 2,181 in the 15-month benefit sample; 1,668 in the 15-month survey sample; and 1,103 (supplement group members only) in the PMIS sample.

[1]Of these sample members, 511 expected to return to their previous employer by a specific date; and 1,691 expected to return, but not by a specific date. Thirty additional sample members were excluded from the analysis due to missing information.

Appendix C:
Multivariate Analysis of Determinants of Supplement Receipt

Part of the analysis of supplement receipt in Chapter 5 involved an effort to understand why some of those who qualified for supplements did not receive them. As discussed in Chapter 5, this effort revealed that the most important determinant of supplement receipt among "supplement qualifiers" was the size of the potential supplement payment. Specifically, the probability that a supplement qualifier would apply for and receive supplement payments increased markedly as the size of his or her potential supplement increased.

This conclusion was reached using a two-step process. First, supplement group members were divided into those who became re-employed full time within the ESP job-search period and those who did not. Using this categorization, a logistic regression model of the relationship between re-employment and individual background characteristics was estimated. Results for this model are shown in Table C.1.

In the second step, supplement qualifiers — those who had found eligible jobs paying less than their pre-layoff jobs — were distinguished from all others who became re-employed within the job-search period. Supplement qualifiers were then divided into those that applied for and received a supplement payment and those who did not.

Using this categorization, a logistic regression model of the relationship between supplement receipt and individual background characteristics was estimated for supplement qualifiers. Results for this model are shown in Table C.2. As can be seen, the size of the potential supplement payment is the most important determinant of supplement receipt among supplement qualifiers. The coefficient for this variable is both statistically significant and substantively large.

Table C.1: Multivariate Model of whether Supplement Group Members Found Eligible Full-Time Jobs within the 26-Week Job-Search Period, by Gender

	Partial Derivative	
	Male	**Female**
Age (in years)	-0.013***	-0.011***
HRCC		
Granby	0.232***	0.155**
Oshawa	0.019	0.019
Toronto (omitted)	—	—
Winnipeg	0.139**	0.034
Saskatoon	0.206***	0.171***
Highest education credential		
University	0.020	-0.004
College	0.018	-0.087
High school	0.052	-0.028
Less than high school (omitted)	—	—
Marital status		
Married/common-law	0.115*	-0.078
Never married	-0.138*	-0.054
Divorced, separated, or widowed (omitted)	—	—
Only contributor to household income	-0.002	0.021
Home owner	0.042	0.037
Pre-layoff employment characteristics		
Average pre-layoff insurable earnings ($000)	0.322**	0.484***
Years working a pre-layoff job	-0.007**	-0.019***
Working in an unskilled occupation	-0.065	-0.062
Union member	-0.093*	-0.080*
Worked at a firm with less than 100 employees	-0.005	0.004
Number of insurable weeks	-0.003	-0.002
Received severance payments	-0.021	0.035
Sample size	941	1,191

Note: Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

The values reported are estimates of the effect of a one-unit change in the independent variable on the probability of finding a full-time job within the job-search period. These values are calculated as ß(y)(1-y), where ß is the estimated logit coefficient and y is the mean of the dependent variable.

Table C.2: Multivariate Model of Supplement Receipt among Eligible Supplement Group Members

	Partial Derivative
Supplement amount ($100)	0.194***
Gender (Male=1)	-0.019
Age (in years)	0.001
HRCC	
Granby	0.233**
Oshawa	0.071
Toronto (omitted)	—
Winnipeg	0.138*
Saskatoon	0.238***
Highest education credential	
University	-0.060
College	-0.130*
High school	-0.083
Less than high school (omitted)	—
Marital status	
Married/common-law	-0.083
Never married	-0.126
Divorced, separated, widowed (omitted)	—
Only contributor to household income	0.007
Home owner	-0.010
Pre-layoff employment characteristics	
Average pre-layoff insurable earnings ($00)	0.062***
Years working in pre-layoff job	0.005
Working in an unskilled occupation	0.073
Union member	-0.095*
Worked at a firm with less than 100 employees	-0.031
Number of insurable weeks	0.004
Received severance payments	0.072*
Sample size	678

Note: Statistical significance levels are indicated as: * = 0.10; ** = 0.05; and *** = 0.01.

The values reported are estimates of the effect of a one-unit change in the independent variable on the probability of receiving a supplement. These values are calculated as ß(y)(1-y), where ß is the estimated logit coefficient and y is the mean of the dependent variable.

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