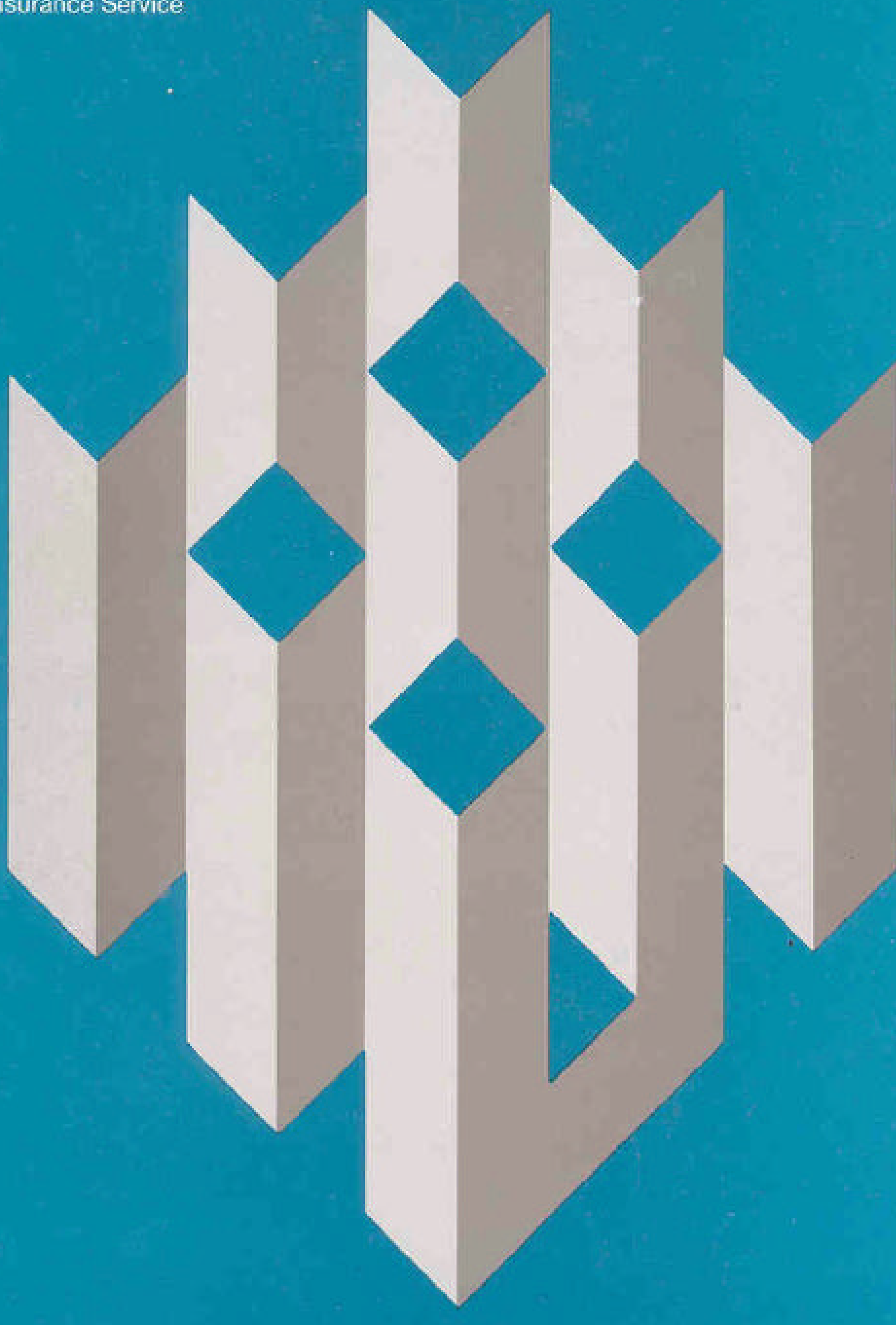
The Washington Reemployment Bonus Experiment Final Report

Unemployment Insurance
Occasional Paper 92-6

U.S. Department of Labor
Employment and Training Administration
Unemployment Insurance Service



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U.S. Department of Labor
Lynn Martin, Secretary

Employment and Training Administration
Roberts T. Jones
Assistant Secretary for
Employment and Training

Unemployment Insurance Service
Mary Ann Wyrsch, Director

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INTRODUCTION

The principal objective of unemployment insurance (UI) is to reduce hardship by providing labor force members with partial wage replacement during periods of involuntary unemployment. In performing this income maintenance function, UI has the potential of prolonging spells of unemployment. Indeed, leading economists began publishing research findings in the 1970s strongly suggesting that UI tends to lengthen jobless spells beyond that which would occur without UI payments. The 1980s saw several state and federal experiments, testing initiatives designed to reduce work disincentives while retaining the income maintenance functions of UI. A new program, offering bonus payments to UI claimants for speedy return to work, was tested in experiments run in Illinois in 1984-85 and in New Jersey in 1986-87. The apparent success of these experiments in reducing insured unemployment led the U.S. Department of Labor (USDOL) to launch expanded versions of these bonus offer experiments in Washington and Pennsylvania in 1988.

The purpose of the Washington Reemployment Bonus (WREB) experiment was to validate results of the previous experiments, test a new range of reemployment bonus plans, and identify the most cost-effective plan. WREB was designed by the W.E. Upjohn Institute for Employment Research in collaboration with the Washington State Employment Security Department (WSESD) and the USDOL.

EXPERIMENTAL DESIGN

A reemployment bonus plan has three parts:

(1) a bonus amount--in WREB, the bonus amount equaled a multiple of a claimant's weekly benefit amount (WBA);

(2) a qualification period, i.e., the period of unemployment over which the bonus offer is open--in WREB, the qualification period was specified as a fraction of the claimant's entitled duration of benefits, plus one week to account for the waiting week; and

(3) a reemployment period, i.e., the length of time the participant must remain employed full time to receive a bonus--in WREB, the reemployment period was fixed at four months.

The WREB experimental design had six treatments--three bonus levels and two alternative qualification periods, as shown in the following table:

Treatment Arrangement

	Qualification Period	
Bonus Amount	.2 x duration + 1 (short)	.4 x duration + 1 (long)
2xWBA (low)	Treatment 1	Treatment 4
4xWBA (mid)	Treatment 2	Treatment 5
6xWBA (high)	Treatment 3	Treatment 6

OPERATIONS

Enrollment into the experiment took place between February and November, 1988, in 21 of the State's 31 Job Service Centers (JSCs), handling 85 percent of the state's claimant population. Claimants, filing for a new benefit year, were randomly selected on the basis of their Social Security Numbers, and made bonus offers by regular claimstakers. To be eligible to participate in the experiment, a UI claimant must have established a benefit year based on Washington wages. In total, 12,451 eligible claimants were enrolled into the six treatments, and an additional 3,083 were assigned as controls.

To qualify for a bonus:

- An enrolled claimant had to submit a Notice of Hire (NOH) to the WSESD central office upon becoming reemployed at a full time job; recalls to the previous job and union hiring hall placements did not qualify, but self-employment was acceptable.

- After being reemployed continuously for four months, the claimant submitted a voucher for payment of the bonus; after verification that the bonus conditions had been met, WSESD authorized payment of the bonus.

This design was followed closely in WREB. Through both computer checks and personal visits, operations were carefully monitored. The error rates were very low, and the program appeared to have functioned as designed.

RESULTS

The goals of the experimental program were to reduce unemployment and to reduce costs to the UI trust fund. Thus, differences between control and treatment group members in weeks of insured unemployment and amount of UI compensation received were the measures of experimental effect. The measurements of greatest policy interest were those over the full benefit year.

The following table shows the overall average effects on benefit-year compensation received and weeks of insured unemployment for each of the six experimental treatments, each bonus level, and all treatments combined[1]:

Differences Between Experimental and Control Group Means Over the Benefit Year (standard errors in parentheses)

	UI Compensation		Weeks of Insured Unemployment	
	Parameter Estimate	Standard Error	Parameter Estimate	Standard Error
T1	18.66	45.74	-0.04	0.293
T2	-40.70	45.16	-0.27	0.289
T3	-106.92**	50.98	-0.70**	0.326
T4	-117.15**	44.95	-0.62**	0.287
T5	-39.79	45.14	-0.26	0.289
T6	-140.53**	51.52	-0.75**	0.329
T1,4	-51.32	38.33	-0.34	0.245
T2,5	-40.23	38.22	-0.26	0.244
T3,6	-123.45**	41.89	-0.73**	0.268
All T's	-65.18**	33.18	-0.41*	0.212

Source: Table 5-4.

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

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[1]These results were derived from regressions that used control variables to eliminate the effects attributable to differences in pre-experimental characteristics among the six treatments and the control group. Mean comparisons between treatment and control groups without use of control variables understated the differences in compensation received, because the control group included a sample of claimants with lower than average WBAs.

The following is a summary of the principal findings:

• The high bonus level treatments (six times the WBA) caused substantial, and statistically significant, reductions in UI compensation and weeks of insured unemployment.

• Treatment 6, the high bonus and long qualification period, had the largest effects; i.e., a reduction of $140 in compensation and 0.75 weeks of unemployment.

• Three of the four low and middle level bonus treatments failed to produce statistically significant effects.

• Across the six treatments, the mean response was a $65 reduction in compensation and a 0.41 week reduction in the duration of UI payments.

It was a goal of WREB to use the six experimental treatments to select the most cost-effective combination of bonus level and qualification period for a bonus offer program. The effects of varying the bonus amount, holding the qualification period constant, and varying the qualification period, holding the bonus amount constant (see Table 5-6), were as follows:

• Shifting from a low (2xWBA) to a medium (4xWBA) bonus level had no effect;

• Shifting from a medium to a high (6xWBA) bonus level had a statistically significant impact, reducing compensation by $83, and weeks of insured unemployment by one-half week; and

• Shifting from a short to a long qualification period somewhat reduced compensation.

In the same vein, we estimated the effect of each dollar of additional bonus payment and each week of qualification period, with the following results:

• While the estimates were computed with a large margin of error (Table 5-7), the estimated effect was a reduction in compensation in the benefit year of $6.51 for each $100 increase in bonus amount offered, and $5.48 for each additional week in the qualification period.

We also investigated the timing of treatment impacts. If the bonus offer is effective, treatment assigned claimants should leave UI sooner than control assigned claimants. Following are the findings regarding the impact of the experiment on the timing of UI benefit termination:

- Through week 7--the longest qualification period for Treatments 1, 2 and 3--claimants assigned to T3 left UI at a rate 3.0 percent greater than control subjects;

- Through week 13--the longest qualification period for Treatments 4, 5 and 6--claimants assigned to T6 left UI at a rate 4.7 percent greater than control subjects.

- By the time the maximum entitled duration of benefits in Washington elapsed, 0.7 percent more treatment assigned claimants than control claimants had left UI, confirming the overall finding of a permanent effect of the bonus offer on insured unemployment.

IMPACTS ON SELECTED SUBGROUPS

Treatment effects were also computed for population subgroups defined on the following characteristics: dislocated worker status, gender, race/ethnicity, age, base period earnings, and characteristics of local areas.

Experimental effects were examined for dislocated workers categorized under three successively more inclusive definitions of dislocation. Only for the broadest definition of worker dislocation was there a treatment effect on UI compensation which was significantly different from that for nondislocated workers. Claimants who were employed during each of the 12 calendar quarters prior to filing for unemployment reduced UI compensation by an average of $217 over the benefit year when offered a high bonus, and this response was statistically significantly greater than for nondislocated workers.

Males exhibited a larger response to bonus offers than females. However, the differences across gender were not statistically significant.

With regard to impacts on different racial/ethnic groups, a statistically significant treatment effect on compensation or unemployment was exhibited only by non-hispanic whites. There was no evidence that there were different impacts across racial/ethnic groups, but this may have been due to the relatively small size of the minority samples.

The estimated effects of the experiment were greater for older claimants (aged 45 and over) than for younger claimants, although the differences were not statistically significant.

In an analysis of subgroups defined by a combination of age, gender, and race/ethnicity, the average treatment response of younger black males was very different from the response of all other subgroups (see Table 6-7). Younger black males showed a statistically significant response opposite to expectations. The bonus offer apparently caused members of this group to increase the level of compensation they received and their weeks of insured unemployment.

Considering subgroups defined by their age and base period earnings (BPE), the following was found:

- The impact was very strong on UI compensation drawn by high earning/older workers, possibly indicating that discouraged workers were responsive to job search incentives;

- The impact was also strong on UI compensation drawn by low earning/young workers, who might not yet have been strongly attached to the work force and were encouraged to increase job search;

- High earning/young workers did not respond to the experimental treatment; these workers may have already been strongly attached to the work force and maximizing job search effort.

Some differences in response were found across geographic regions of the state and between areas experiencing different economic conditions. The differences, however, were not overwhelmingly strong.

- Claimants filing for benefits in western Washington (excluding the Seattle Metropolitan Area) responded less strongly than those in Seattle or in Eastern Washington.

- Impacts differed mildly across areas experiencing different total unemployment rates (TUR); claimants filing in areas where the TUR was particularly low (TUR below 5 percent) strongly responded to the experiment, whereas claimants in areas experiencing moderate or high TUR responded weakly or not at all.

- There were no differences across areas experiencing different employment growth rates.

SECONDARY EFFECTS OF THE BONUS OFFER

Secondary effects of the bonus offer of particular interest were effects on job quality, employer and union attachment, and use of the Employment Service.

An undesirable side effect of the experiment would have occurred if more rapid reemployment were achieved by acceptance of lower quality jobs. Measured by quarterly earnings (differences in which could result from taking jobs that paid lower hourly wage rates or provided fewer hours of work per quarter), no discernable, statistically significant, effects were found either for the total sample or for any of the major subgroups.

The design of the experiment gave rise to concerns that the experiment was (1) anti-union, because it precluded bonuses for placement through a union hiring hall, and (2) anti-employer, because it denied bonuses to claimants recalled to the separating job.

While the sample may be too small for reliability, data from 1,900 complete responses to the WREB follow-up survey contained no evidence that the experiment affected union membership adversely. Evidence regarding the effects on placement through the union hiring hall was inconclusive, with a relatively large estimated effect that was not statistically significant.

Since one purpose of the UI system is to maintain the employee-employer relationship by providing short-term benefits to workers on layoff, evidence that the experiment weakened the relationship would be troublesome. Results in this regard are mixed. Based on administrative data for the whole sample, we found that among claimants who return to work, the bonus offer did not affect the probability of returning to the previous employer. This was true also for the smaller group of claimants on "standby" and awaiting recall.

However, data for 1,900 respondents to a follow-up survey told a different story. Treatment assigned claimants who returned to work, returned to their previous employer (the separating employer or the main employer during the 5 years before filing for benefits) at a rate about 6 percent lower than reemployed control claimants who had about a 35 percent probability of returning to their previous employer. Therefore, we cannot dismiss the possibility that the experiment reduced employer attachment.

Finally, we found no evidence of increased use of the Employment Service, but there was evidence that job search intensity increased. In the experimental group the number of employer contacts averaged two per week, while in the control group there was an average of only 1.3 contacts per week; the difference between the treatment and control groups was statistically significant.

THE BENEFITS OF A BONUS OFFER PROGRAM

For purposes of policy making, the bottom line is whether or not a program's benefits outweigh its costs. If they do not, there is no reason to consider the program as a policy option. However, the net benefits of a program depend on the policy perspective.

The most striking overall finding is that from the perspective of society as a whole, a reemployment bonus program has large net benefits and an extremely high benefit/cost ratio. From the perspective of the UI system, the program is not appealing. However, for government as a whole, it is close to a break-even proposition.

Huge societal benefits derive from the high value of earnings gains and the very low administrative costs of the program--only $3 per eligible claimant. Negative net benefits calculated for the UI system are a result of too small an effect on compensation relative to the cost of paying bonuses. From the perspective of the government as a whole the program is somewhat more appealing, because added tax revenues derive from the increased earnings of claimants.

A bonus offer program for older workers looks like a good prospect, showing large gains to society and positive net benefits to the UI system and to government as a whole. For dislocated workers (defined as having been continuously employed for three years), middle and high WBA multiple bonus offers had large net societal benefits, but generated significant losses to the UI system. The possible earnings reductions for participants reduces the appeal of a bonus offer program for dislocated workers.

One caveat is that our estimate of societal benefits does not take into account changes in participation that might occur in a regular program. We have calculated that up to one-third of those assigned to the experiment did not collect bonuses to which they were entitled. Some portion of these probably would collect bonuses in a regular program, and this would lead to reduced societal benefits.

CONCLUSIONS

In conclusion, the WREB experiment was successful in that it operated as designed and generated reasonable results, consistent with those of the other bonus offer experiments. For society as a whole, the program appears to be beneficial as bonus offers may be a less expensive way to get people back to work than other alternatives, such as retraining. Unfortunately, a bonus offer program does not appear to generate net benefits to the unemployment insurance system specifically, or to the government in general. Except for a program aimed at older workers, some additional funds would be required to pay for a bonus offer program.

CHAPTER 1

INTRODUCTION

1.1 Policy Context of the Washington Reemployment Bonus (WREB) Experiment

The principal objective of unemployment insurance (UI) is to reduce hardship by providing labor force members with partial wage replacement during periods of involuntary unemployment. In performing this income maintenance function, the system has the potential of prolonging spells of unemployment. Indeed, in the 1970s, leading economists began to publish research findings which suggested that UI lengthens jobless spells beyond what would occur in the absence of such compensation--perhaps even beyond the time needed for efficient job search. To ensure continuing labor force attachment by beneficiaries and to guard against avoidable joblessness, work search requirements have been part of continuing eligibility rules since the inception of UI. Work search rules vary across the states, as does compliance with and enforcement of the rules.

In the 1980s, concern over the financial condition of the federal-state UI system, combined with efforts on the part of political leaders to restrain tax increases, led to the exploration of new means for dealing with work disincentive problems while retaining the income maintenance function of UI. A variety of new initiatives were tested as field experiments, with the UI reemployment bonus gaining considerable attention. By encouraging more timely and vigorous job search, the reemployment bonus experiment offered the prospect of shortening spells of insured unemployment while maintaining income and not worsening the quality of job matches. If effective, the bonus promised direct savings to UI trust funds through reduced benefit payouts, increased revenues to governmental treasuries through increased personal income, and the prospect of reduced UI work test compliance monitoring costs. The bonus also has the advantage of being a positive rather than a negative reinforcement for UI beneficiaries to return to work--a carrot rather than a stick.

The Illinois Reemployment Bonus Experiment, conducted in 1984-85, involved the first random trials to test whether offering reemployment bonuses to UI claimants would shorten their unemployment and reduce the amount of UI benefits they received. The large response and substantial net benefits estimated for the Illinois experiment, together with encouraging results from another bonus experiment conducted in New Jersey in 1986-87, led the U.S. Department of Labor (USDOL) to undertake further tests of this concept.

The Illinois and New Jersey experiments each tested a single bonus offer program. The Illinois experiment was the simpler of the two; it offered UI claimants $500 for returning to full-time employment within 11 weeks after filing for benefits and remaining fully employed for four months. In 1987, USDOL asked the W. E. Upjohn Institute for Employment Research to design an experiment that tested a range of bonus offers, so as to identify the structure of an optimal reemployment bonus offer. In the meantime, USDOL surveyed states about their interest in hosting such an experiment, and selected Washington and Pennsylvania as the locations for two new experiments. Washington became the site for testing the new Upjohn Institute design, and Mathematica Policy Research was selected to design and evaluate the experiment to be conducted in Pennsylvania.

1.2 Decisions Leading to the WREB Experimental Design

Late in 1987, after receiving a grant from the Sloan Foundation for work on the design and evaluation of the WREB experiment, the Upjohn Institute commenced work with the Washington State Employment Security Department (WSESD) to finalize the design and develop procedures for the experiment.

Three matters requiring immediate attention were selection of sites, composition of the sample, and length of the enrollment period. The resolution of these issues led to the experimental design described in Chapter 2. The decision as to the number of Job Service Centers (JSCs) within which to operate the experiment was essentially taken out of the hands of the designers of the experiment by the federal requirement to use a sample with characteristics representative of the population of the host state. This rule led to the selection of 21 of the 31 JSCs in Washington, comprising 85 percent of the state's UI claims load. Among the 10 JSCs omitted as enrollment sites, 8 were particularly small and remote, 1 handled mostly interstate claims since it served the labor market that included Portland, Oregon, and 1 (Tacoma) was host to another experiment.

In addition to being representative of the state UI claimant population, another consideration that dictated the composition of the WREB sample was the desire not to exclude groups of claimants whose behavior might be affected by a program that was modelled on the experimental treatments. Thus, our sample included almost all new claimants who were eligible for UI benefits, whether or not they actually received benefits. Other UI bonus experiments eliminated some groups of claimants included in WREB, e.g., claimants excluded from the UI work search requirement and awaiting recall to their previous employer and union hiring hall members.

Enrollment rates were specified at the 21 selected JSCs to achieve a balance of several competing concerns. To minimize seasonality effects, an enrollment period of close to a year was planned. To minimize displacement effects--the likelihood that the additional job search activity by claimants offered the bonus would measurably reduce job opportunities for control group members and thereby bias the impact estimates--the plan called for a relatively small proportion of the claims load at each JSC to be assigned to an experimental treatment. But to guarantee awareness and interest on the part of office personnel responsible for the experiment, a sufficient volume of treatment assignment was necessary. The decision was made to involve 20 percent of the eligible claims load in 20 of the 21 experimental sites, and 40 percent at the other (to obtain the proper racial balance for the sample). The selected enrollment rates permitted enrollment of the sample to take place over an eight-month period, creating little chance of displacement.

There were effectively two data bases for the experiment. An operational data base was designed by USDOL, utilizing Oracle relational data base management system software and called the Participant Tracking System (PTS). The PTS was updated weekly with administrative data. This system was used to monitor claimant flow and generate appropriate letters and forms to send to assigned claimants. The flow of data was so current that it allowed very precise prediction of the week to terminate enrollment so as to exactly exhaust the $1.2 million bonus budget. Enrollment ended in November of 1988. After the last bonus was paid in January of 1990, 99 percent of the bonus budget had been paid out.

Supplementary data were provided by the WSESD for use in evaluating the experiment. This data base was formed from several key administrative files, described in Chapter 4, and was provided by the state one year after completion of the benefit year for the last claimant enrolled into the experiment.

1.3 Chapter Outline

The remainder of this chapter sets forth the outline of the report, which comprises an additional nine chapters and appendices.

Chapter 2 describes the Experimental Design, consisting of eligibility conditions, treatment design, and sample design.

Chapter 3, Implementation and Operations, describes the procedures that took place at the local offices and in the central office. This chapter discusses the flow of claimants through the experimental system, and the paper flow that triggered agency response and eventually bonus payments.

Chapter 4 describes the Participant Tracking System used to operate the experiment, and the analytic data base built from a series of administrative files.

Chapter 5, Experimental Effects of WREB on UI Benefits and Unemployment, presents the results of the experiment. These results are first discussed as experimental-control differences across each of the six treatments, the three bonus levels, and the average across all the treatments. Weekly rates of exiting from insured unemployment and differences in impacts among the experimental treatments are also described.

Chapter 6 continues the discussion of results with the presentation of results for several subgroups. An attempt is made to identify the unique characteristics of groups that do and do not respond to the experiment.

Chapter 7 investigates several key potential effects on other economic variables, primarily the effect on quarterly earning rates, employer attachment, union membership, use of union hiring halls, use of Job Service Centers, and contribution to household income.

Chapter 8 discusses the issue of participation, and documents the large number of eligible claimants who do not take advantage of the bonus offer.

Chapter 9 lays out the benefits and costs of the bonus offer program, providing estimates of net benefits to society, the UI system, and government as a whole.

Chapter 10 summarizes the results, compares them with those from three other reemployment bonus experiments, and draws policy conclusions.

Several appendices accompany the report to document technical details of the WREB operations and evaluation.

CHAPTER 2
EXPERIMENTAL DESIGN

The experimental design consists of three parts: (1) eligibility conditions, delimiting the target population; (2) treatment design, detailing the components of the experimental bonus program; and (3) sample design, comprising determination of the appropriate sample size and selection of sites.

2.1 Eligibility Conditions

2.1.1 Requirements for Participation

Two objectives of the experiment guided the decision as to who would be eligible to participate. The first objective was to increase the job search efforts of UI claimants, and thereby to reduce the amount of unemployment. The second objective was to reduce the costs to the State's Unemployment Insurance (UI) Trust Fund. These goals dictated that UI claimants whose job search effort could not be increased by the bonus offer, and those claimants for whom the State would not incur cost, should be excluded. Therefore, eligibility for UI benefits was an obvious first condition for bonus eligibility.

A further restriction imposed was that the claimant must have been submitting an initial claim, i.e., a claim to start a new benefit year. Eligibility was restricted to claimants filing new claims in order to replicate a steady-state environment in which a reemployment bonus was part of the UI system, and only new claimants would be offered the opportunity to obtain a bonus. In the steady state, those claimants filing additional claims and those filing continuing claims will have received bonus offers at the time they filed initial claims. It is reasonable to assume that, in an actual program, claimants would be offered a reemployment bonus at the start of their benefit year. Restricting eligibility to those filing initial claims, therefore, simulates a realistic program.

In states with a waiting week, it was deemed unnecessary, if not undesirable, to limit eligibility for the bonus to those who actually received UI benefits. A claimant could become reemployed during the waiting week, or even before receiving waiting week credit. To encourage such claimants to intensify their job search immediately, and, more important, to avoid discouraging them from taking jobs before receiving benefits, such claimants became eligible for a bonus by taking a job prior to receiving a first payment, as long as they would have otherwise been eligible for UI benefits.[1]

An exception to this criterion was made in the case of claimants who filed a monetarily valid claim, but did not claim waiting week credit. These claimants were all declared eligible to participate and receive a bonus, even if there was an issue on the claim that might have prevented UI benefit payments. The rule that required eligibility for UI benefits as a condition for participation in the bonus offer program was modified for claimants not claiming a waiting week because there is no legal issue to adjudicate if a week is not claimed.

Claimants whose UI entitlement was not based on Washington State wage credits were excluded from participation in the experiment. This group included those claimants filing interstate claims, and those designated as UCFE (a recent federal employee) or UCX (a recently discharged veteran). These three categories would probably participate in a national program, but were excluded from the experiment because bonus offers based on UI entitlement could not be made at the time of filing. Claimstakers can identify Washington State wage credits at the time of filing in the computerized Benefits Automated System (BAS). Combined claims did not cause ineligibility for the bonus offer; however, the size of the bonus was governed by Washington State wage credits.

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[1] Making claimants eligible to participate in a bonus program immediately upon filing a valid claim opens the possibility of encouraging laid-off workers who expect to start new jobs within the waiting period to file claims they might not otherwise have filed. This may result in an increase in benefit payments that won't be captured in the experiment. However, these workers would only be doing what they are already entitled to do. This issue is addressed further in Section 5.6.

Eligibility for the bonus offer was further limited to those who had monetarily valid claims at the time of filing for benefits. Moreover, the amount of the bonus offer and the length of the qualification period were determined by the claimant's UI entitlement established at the time of filing. This procedure assured that all claimants offered the bonus had complete knowledge of the amount of the bonus and the qualification period from the date of filing. Claimants, such as state employees, whose monetary eligibility was not known at the date of filing, could not have been given the same information. This exclusion would probably not be applied in an actual reemployment bonus program.

In addition to having a monetarily valid claim, the claimant must not have been ineligible to receive benefits because of separation issues. A claimant who is discharged for cause or quits his/her job without good cause is denied UI benefits for the duration of that unemployment spell. Denial of benefits on a separation issue precluded eligibility for the bonus. On the other hand, there can be temporary denial of benefits for able-and-available issues. A claimant may be denied benefits in a particular week because he/she is not searching for work and is not available to accept employment (e.g., is sick, away on vacation, etc.). These issues are removed as soon as the claimant returns to job search, and were therefore not considered to be reasons to preclude the claimant from being eligible for a bonus. On the contrary, it was believed that the bonus could act as a stimulus for the claimant to return to work search and regain eligibility for UI benefits. For the same reason that the waiting week credit was not necessary to earn a bonus, the claimant with an able-and-available issue could obtain a bonus by accepting employment without previously having the stop removed.

2.1.2 Additional Requirements for Bonus Receipt

There are no other <u>ex ante</u> exclusions, although two groups of claimants were not paid bonuses. Bonuses were not paid to: (1) claimants recalled to their previous job by their terminating employer, or (2) claimants placed on a job through their union hiring hall. These two groups of claimants were denied bonuses on the grounds that the bonus offer would not affect their job search behavior, since their job acquisition was totally dependent upon the actions of the employer or the union.

Note, however, that these last mentioned exclusions were not <u>ex ante</u>, in that being on standby status awaiting recall, or being a member of a referral union, did not exclude the claimant from participation in the experiment. It was hoped that the bonus offer would encourage these claimants to seek jobs and thereby become employed more rapidly than if they simply waited for recall by their previous employer or placement by their union. Thus, a member of a referral union who obtained a job without union placement was eligible to receive a bonus. Likewise, a claimant on standby who obtained another job was eligible to receive a bonus. Such claimants would remain bonus-eligible if, after working at least one week on their new job, they returned to their previous job or accepted a union hiring hall placement.

Since the intent of the bonus offer was to encourage more aggressive job search, the bonus was not denied to a claimant who obtained what was clearly a new job with the previous employer, and not a recall to the previous job. To qualify for the bonus, the claimant must have been permanently separated from the employer, with the new job identified as a "new hire."

In summary, to be eligible to participate in the experiment and receive a WREB bonus offer, a UI claimant must:

1. Have a monetarily valid claim, with monetary eligibility determined at the date of filing;

2. Be filing a claim to establish a new benefit year;

3. Have at least one week during the qualification period in which there was no indefinite nonmonetary stop on the initial claim; and

4. Not be filing a totally interstate claim, or a UCFE or UCX claim.

In addition, to be eligible to receive a bonus, the participating claimant must:

1. Not have a separation issue on the initial claim that prevents UI benefit payments during the qualification period, or a separation issue associated with the previous job that is not removed prior to the end of the reemployment period (this condition prevents bonus payment only for claimants claiming a waiting week);

2. Not be recalled to the previous job by the separating employer;

3. Not be placed on the new job through a union hiring hall; and

4. Work full time (a total of at least 34 hours per week on all jobs), or have earnings sufficient to terminate UI benefit payments.

2.2 Treatment Design

The WREB experiment had three components: (1) the bonus amount; (2) the qualification period--the period of unemployment over which the bonus offer was open; and (3) the reemployment period--the length of time the participant must have remained employed full time to receive a bonus.

2.2.1 Bonus Amount

Since a major goal of the WREB experiment was to determine the most efficient bonus size, the experiment had three payment levels. This was considered to be the smallest number of options necessary to generate a range of bonus offers that both spanned the range of policy interest and provided sufficient variation to estimate the marginal effectiveness of different bonus sizes.

The bonus levels were specified in terms of a multiple of each individual's entitled weekly benefit amount (WBA). Therefore, bonus offers in a given treatment varied across claimants in dollar amount because of differing entitlement, but were constant in terms of opportunity cost of unemployment. That is, for a totally unemployed claimant, a reduction of one week in unemployment cost each individual one week of compensation, and the bonus was "priced" the same for each individual (in a given treatment group) in terms of this sacrifice. The bonus offer was determined on the basis of a monetary determination at the time the claim was filed. Additional wage credits, or other adjustments, were not taken into account in setting the bonus. In this way, all claimants had full information regarding their bonus at the date of filing, creating an important homogeneity condition for the experiment.

The three payment treatments were: two times the individual WBA; four times the individual WBA; and six times the individual WBA. The middle level is approximately that used in the Illinois experiment. The entitled WBA was the basis for the bonus. The formula used by the Washington State Employment Security Department (WSESD) to determine the entitled WBA used the highest two quarters of earnings in the base year, Q_1 and Q_2, subject to a minimum and maximum, as follows (see Revised Code of Washington 50.20.120):

$$\text{WBA} = \begin{cases} MIN, \text{ if } MIN > 1/25[(Q_1 + Q_2)/2] \\ MAX, \text{ if } MAX < 1/25[(Q_1 + Q_2)/2] \\ 1/25[(Q_1 + Q_2)/2], \text{ otherwise.} \end{cases}$$

Claimants drawing partial benefits, i.e., those filing a claim while employed, were also eligible to participate in the experimental program. Claimants receiving partial benefits were made the same bonus offer as claimants receiving the full weekly benefit amount. Based upon Washington's average entitled WBA of \$152 in 1988, the average bonus at the three treatment levels would have been \$304, \$608, and \$912 respectively. Since the WBA changes for some claimants between the date of filing and the date of first payment, some minor variation in the ratio of the bonus to the final WBA for individuals in the same treatment resulted.

2.2.2 Qualification Period

The qualification period is the maximum duration of insured unemployment that the participant could experience and still qualify for the bonus. The qualification period was set in terms of the individual's compensable duration of entitlement to UI benefits. In states with a fixed duration for all beneficiaries, the qualification period would be a fixed number of weeks for each treatment. In Washington, which has a variable compensable duration, the qualification period varied, since it was specified to be a fixed proportion of the individual's entitled duration of benefits. The qualification period was communicated to the claimant by setting a reemployment deadline, defined as the date by which the claimant must begin full-time employment and stop receiving UI benefits in order to qualify for the bonus.

It was believed that the length of the qualification period could impact the effectiveness of the program. If the qualification period was a high proportion of the compensable duration, then a high proportion of claimants would receive bonuses without needing to alter behavior, and a large deadweight loss to the UI program would have resulted. If the qualification period was a low proportion of the compensable duration, fewer claimants would qualify for a bonus, and the deadweight loss would be reduced. However, such a qualification period could have discouraged some prospective participants from increasing their job search effort. Thus, to determine the optimal qualification period, it is important to determine how claimant behavior varies with the length of the qualification period.

To determine the effects of different qualification periods, two qualification periods were used in the experiment. One was set at 20 percent of the individual's compensable duration, and the other at 40 percent. The qualification period was increased by one week to cover the required waiting week. Since the maximum entitled duration of benefits in Washington is 30 weeks and the effective minimum duration is 10 weeks, the qualification period in WREB ranged from 3 to 13 weeks. If the algorithm calculated a qualification period that included a partial week, the length of the period was rounded up to the next whole week. Although a week was added to the qualification period to cover the waiting week, it was not necessary to actually serve the waiting week to earn a bonus, as noted above in Section 2.2.1.

Initially, the qualification period was set to start on the Sunday before the Effective Date of Claim (EDC), which is the start of the claimant's benefit year. Two months into the experiment, the start date for the qualification period was changed to be the Sunday before the process date of the claim. The process date is the date that the claimant files the claim. The reasons for this and the implications of the change for the analysis are discussed in Section 3.3.2.

Some claimants file claims while still employed. Such claimants were also eligible to participate in the demonstration. However, the start of their qualification period coincided with their process date, not necessarily the date on which they started their unemployment spell. Nevertheless, to be eligible for the bonus, claimants who filed while still employed must actually have become unemployed under conditions that would have made them eligible for UI benefits before starting a new job.

2.2.3 Reemployment Period

The reemployment period, the length of time the claimant must remain continually employed in order to receive the bonus, was four consecutive months of full-time employment without any additional claim for UI benefits being filed. This period was deemed sufficiently long to avoid paying a bonus for seasonal work and to reduce to a reasonable minimum the tendency for a claimant to take a job strictly to obtain a bonus. The only job characteristic required was that the claimant work full time on one or more jobs, or be self-employed full time. It was not necessary that employment be with a single employer, as long as no UI benefits were claimed in the period and employment was essentially continuous.[2]

2.2.4 Summary of Treatment Design

In summary, the WREB Experiment had the following treatment design:

Bonus Payment: Three different levels, set at two, four, or six times the individual's WBA as determined on the filing date.

Qualification Period: Two different levels, set at 20 percent or 40 percent of the individual's compensable duration plus a week, with the qualification period starting on the Sunday before the process date.

Reemployment Period: Continuous full-time employment for four months after qualifying reemployment has begun.

Since eligibility conditions and the reemployment period did not vary, the experiment had six treatments. For reference, these treatments are arrayed and labeled by number (*Tn; n = 1, 2, ..., 6*) in Table 2-1.

[2] Gaps in employment of up to one week in duration, which resulted from job changes, were allowed.

Table 2-1

Treatment Arrangement

	Qualification Period	
Bonus Size	.2x duration + one week	.4x duration + one week
2xWBA	T1	T4
4xWBA	T2	T5
6xWBA	T3	T6

2.3 Sample Design

2.3.1 Randomization

Randomization is at the heart of experimentation. Because of randomization, a model-free approach to measurement of experimental results is feasible.[3] Randomization is a process of blind selection of a sample from a population. The key principle in randomization is that each member of a population has an equal chance of selection for the experiment and for assignment to any of its treatment cells. In this, as in many field experiments, randomization is accomplished by assigning to each individual in the population a unique number, and establishing a procedure to assure that each number has a specified probability of being selected into the experiment and assigned to a treatment or control cell.

In the case of WREB, randomization was accomplished by using the last two digits of each individual's Social Security Number (SSN) to assign eligible UI claimants

[3] Randomization is the basis of classical experiments which have been used extensively to measure the effects of social programs. A model-free measurement process involves a simple comparison of means of a dependent variable across treatments, without the use of control variables or other restrictions on the form of the response function.

to one of the six treatments or the control group. Since the last two digits of the SSN have been randomly assigned to individuals, the result of the assignment to treatments in the experiment should be that the average characteristics of individuals in each of the seven groups would be the same.

2.3.2 Determination of Sample Size

2.3.2.1 Statistical Significance and Type II Errors

When determining an appropriate sample size, standard statistical procedure focuses on reducing the level of Type I error--the significance level of the test. For WREB, we wanted to have confidence that if the treatment was effective, there was a high probability of not rejecting it. The power of the test expresses this interest. The null hypothesis of no experimental effect, is tested against the alternative hypothesis that an experimental effect exists. Type I error is the error of rejecting a true null hypotheses. Type II error is the error of accepting the null hypothesis when in fact the alternative hypothesis is correct. The confidence an experimenter may have that a treatment effect exists is called the power of the test, which is (1 - Prob (Type II error)). Thus, as the power of the test increases, the probability of rejecting the alternative hypothesis, when it is true, diminishes.

The appropriate size sample for WREB was estimated by reference to the duration of insured unemployment over the benefit year, the independent variable that was most directly affected by the bonus. In the absence of other information, sample size in each treatment cell was set to detect a reduction in the duration of insured unemployment as large as that found in the Illinois experiment, i.e., 1.15 weeks. Since we did not expect the bonus offer to cause an increase in the duration of unemployment, a one-tail test of significance was used.[4]

Table 2-2 shows the size of each treatment group implied by a range of power tests and significance levels for an effect size index (d) of 0.1.[5]

Table 2-2
Treatment Group Sample Sizes

	One-Tail Significance Level		
Power	.01	.05	.10
.5	1083	542	329
.6	1332	721	471
.7	1627	942	653
.8	2009	1237	902
.9	2605	1713	1314

Good statistical practice suggests testing main effects at a significance level of .01 with a power of .8. A power of .8 means that the chance of accepting the alternative hypothesis when it is true, i.e, there is an effect, or alternatively, rejecting the null hypotheses when it is false, is .8 (power = 1.0 - 0.2). To meet these two conditions requires treatment cells of at least 2,000 observations.

Using a total sample of 2,000 observations, treatment effects on four subgroups of equal size (e.g., two sexes and two races) of 500 observations each could be tested at a

[4] We eventually decided to use two-tail tests because of the possibility that the bonus offer, operating through an income effect, could cause an increase in the duration of unemployment.

[5] Adapted from Cohen (1977, p. 54). The table is based on an effect size index (d) of 0.1. This index is derived as follows: $d = [m(A)-m(B)]/s$, where $m(A) - m(B)$ is the experimental effect, and s is the standard deviation of either population. Using Illinois data, $d = (1.15/12)$, or about 0.1.

significance level of .05 and a power of .5. Note that a power of .5 means that if you accept the null hypothesis (that there is no effect) you have a 50-50 chance of being wrong.

2.3.2.2 Establishing the Bonus Budget

To establish a budget, it was necessary to estimate two other variables: the average bonus payment, B, and the expected take-up rate, r. The take-up rate is the proportion (or percentage) of those assigned to an experimental treatment who collect a bonus. It was hoped that the WREB bonus take-up rate would exceed the 14 percent experienced in Illinois. Based upon use of more effective procedures for encouraging participation and a value of the bonus (in 1984 dollars) somewhat higher than the $500 used in Illinois, we predicted a take-up rate of 20 percent.[6]

On the assumption that the average value of bonuses paid would be the same as that of bonuses offered, and the number of bonus offers in each treatment cell was the minimum required to yield tests with acceptable power (2,000 in each cell), the estimated cost of bonuses was determined as follows:

Total Cost of Bonuses $= B \times r \times n$

where,

B = The average dollar bonus offer

$B = \{(1/3 \times 2 \times \text{WBA}) + (1/3 \times 4 \times \text{WBA}) + (1/3 \times 6 \times \text{WBA})\}$

WBA = The average WBA in Washington in 2nd quarter 1988

$B = (12/3) \times \text{WBA} = 4 \times \$148 = \$592$

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[6] While the take-up rate in the Illinois experiment was .14, a somewhat higher rate of .20 is expected in the WREB demonstration because of improved information to claimants, more extensive follow-up procedures, and an average bonus somewhat higher in real value than that in Illinois. The average bonus offer in Washington of $575 in 1988 exceeded that in Illinois of $500 in 1984 by just $7 in constant 1983 dollars using the U.S. consumer price index for urban wage earners as the basis for comparison.

r = The take-up rate = .2

n = Total number of bonus offers made = 6 x 2,000 = 12,000.

Thus, $B \times r \times n$ = \$592 x .2 x 12,000 = \$1,420,000 was the estimated bonus cost.

2.3.2.3 Reallocation of the Sample to Treatments and Determination of a Bonus Budget

In considering the allocation of the sample to seven groups, statistical requirements do not dictate that each of the groups receive exactly one-seventh of the sample. This means that considerations of cost per observation and policy relevance could be taken into account in determining the proportion of the sample allocated to each group. From both of these perspectives, it was viewed as desirable to put a larger proportion of the sample in the less expensive treatments. The actual allocation of observations to treatments used in WREB is shown in Table 2-3.

Table 2-3
Proportionate Treatment Sample Size Distribution

	Qualification Period	
Bonus Size	.2x duration + one week	.4x duration + one week
2xWBA	.1875	.1875
4xWBA	.1875	.1875
6xWBA	.1250	.1250

This unequal assignment offered the prospect of reducing the bonus cost of the experiment below that of a scheme which would have specified equal assignment to each treatment cell. The expected cost reduction from this allocation is shown below:

let the proportion eligible for the low Bonus (\$296) = .375, let the proportion eligible for the mid Bonus ($592) = .375,

let the proportion eligible for the high Bonus ($888) = .25,

and let the expected take-up rate be .1875.[7] Then the expected total cost of bonuses was estimated to be:

= [(.375x$296) + (.375x$592) + (.25x$888)] x .1875 x sample size

= $555 x .1875 x sample size

= $555 x [the number of bonuses paid]

= $555 x [.1875 x 12,000]

= $555 x 2,250

= $1,248,750.

This allocation reduced the estimated bonus cost by over $170,000. The U. S. Department of Labor (USDOL) set $1.2 million as the bonus budget for the experiment, and this was the budget used in establishing enrollment goals for the experiment, as described below.

2.3.2.4 Determination of Enrollment Rates

The sample size of 12,000 was estimated to be the minimum size required to allow statistical tests with acceptable power. Given the budget of $1,200,000 provided by the USDOL for bonus payments, final determination of the sample size required use of the average size of the bonus expected to be paid and the take-up rate. The take-up rate--the proportion of enrolled claimants who receive a bonus--can be thought of as a

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[7] The take-up rate for the unequal distribution is assumed to be lower than the 20 percent rate predicted for the equal distribution, because it is reasonable to assume that the take-up rate will be positively correlated with the size of the bonus offer. Allocating fewer observations to the high bonus level reduces the predicted average size of the bonus from $592 to $555 = {(.375 x 2 x 148) + (.375 x 4 x 148) + (.25 x 6 x 148)}. Assuming that the take-up rate will decline proportionately yields an estimate of the take-up rate for the unequal allocation of .1875 = {.2 x ($555/$592)}.

joint probability written as the product of several conditional probabilities that can be estimated ex ante and then monitored for compliance.

Initial estimates of the parameters based on historical experience in the 21 Job Service Centers (JSCs) selected for the experiment provided a set of baseline statistics, described below, that led to the establishment of an enrollment rate of 16 percent in 20 offices and 32 percent in Rainier. The higher proportion was set for Rainier in order to increase the proportion of minority racial and ethnic groups in the sample. The enrollment rates were set with the intent of just exhausting the $1,200,000 bonus budget in 32 weeks.

Using the preliminary evidence that the average bonus paid would be $575, the following set of conditional probabilities were used to initiate the process, starting with an expected flow of claimants filing new claims in the 21 JSCs over a 32-33 week period starting in early March 1988:

New Washington Claims	89,797
times the proportion of claims monetarily valid at the time of filing	x .9
Monetarily Valid New Claims	81,816
times the average assignment rate (.16 at 20 JSCs and .32 in Rainier)	x .169
Treatment Assigned Monetarily Valid New Claimants	13,827
times the proportion eligible on nonmonetary criteria	x .9
Fully Enrolled New Claimants	12,444
times the proportion who are expected to obtain employment within their qualification period	x .409

Enrolled Claimants Obtaining Employment within their Qualification Period	5,087
times the proportion not expected to be eliminated for Recall, Union Hiring Hall Placement, and non-filing	x .5
Enrolled Claimants Filing Valid Notices of Hire	2,544
times the proportion expected to Complete 4 Months of continuous reemployment	x .85
Enrolled Claimants Expected to be Paid a Bonus	2,162

Note that if 2,162 bonuses averaged $575, the bonus cost would be $1,243,000. A monitoring system was established to determine if these parameters were being met, and if changes in the enrollment rate or the length of the enrollment period were needed to just meet the budgetary constraint. The details of the monitoring system and its use are described in Section 3.6.1.

2.3.2.5 The Control Group

For a treatment group of a given size, additional statistical power for hypothesis tests can be gained by having a control group which is larger. Given the expected treatment impact, the budget for bonuses, and the desired power it was decided to have a control group of 3,000. It was estimated that approximately this number would result if 20 percent of claimants determined to otherwise be eligible for a bonus in each enrollment JSC were assigned to the control group. Indeed the final analytic sample included 3,082 claimants in the control group.

2.3.3 Site Selection

In the State of Washington, all unemployment insurance and employment service offices are unified as Job Service Centers. The experiment was conducted in 21 of Washington's 31 JSCs. The ten offices eliminated from consideration included the seven smallest offices in the state, plus three offices excluded for specific reasons: Tacoma and Lakewood were excluded because of the presence of other experimental programs that could contaminate the results; and Vancouver was excluded because of its integration with the Portland, Oregon Metropolitan Area. The 21 offices included in the study had approximately 85 percent of the State's claims load.

One benefit of enrolling at a large number of offices is the protection afforded against the results being distorted by idiosyncratic behavior in one or a few offices. Averaging the results over a large number of offices gives confidence that the results are not due to particularly strong or weak performances in specific offices.

In selecting the sample, we made use of 20 percent of the available Social Security Numbers in all of the offices except Rainier. This meant that in 20 of the 21 offices, 16 percent of the claimants filing initial claims and eligible for UI benefits were made a bonus offer. In Rainier we used 40 percent of the Social Security Numbers, which meant that we enrolled 32 percent of the eligible population filing initial applications. Rainier was the JSC with the largest proportion of claimants who were black. The higher enrollment rate was used in Rainier to compensate for the absence from the sample of claimants using neighboring offices in Pierce County, which had racial breakdowns of the claimant population similar to Rainier's. Pierce County had been excluded because of the complexity caused by other demonstration programs being run in that county. As a result of enrolling at a higher rate in the Rainier JSC, the total sample had racial breakdowns very similar to that of the state as a whole.[8]

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[8] The racial breakdown in the WREB sample replicates that of the total state. Insured unemployment for the period March to November 1988 in the state had the following characteristics (Source: the 10 percent Continuous Wage Benefit History (CWBH) survey): white (non-Hispanic) 85 percent, black 4 percent, other 11 percent. This compares with the following breakdown for the WREB sample: white (non-Hispanic) 84 percent, black 4 percent, and other 12 percent. The differences were not significant at the 5 percent (two-tail) level.

Random assignment to control and treatment cells for each of the 21 JSCs resulted in the distribution of proportions in cells summarized in Table 2-4. The algorithm for random assignment was not intended to guarantee that each cell at each JSC received exactly the designed proportion of claimants. Rather, it was expected that the proportion assigned to each cell in individual sites would not differ significantly from the overall design proportions. The table also summarizes proportions assigned to cells by calendar quarter of 1988, no effort was made to equalize enrollment across quarters. Nonetheless, out of the 175 cells (7 groups in 21 JSCs and 4 quarters) only 7 or 4.0 percent are judged different from the designed proportion based on two-tail t-tests at the 95 percent confidence level.

Table 2-4

Proportions Assigned to Control and Treatment Groups by Job Service Center and Quarter of Enrollment

JSC	CONTROL	T1	T2	T3	T4	T5	T6	Sample Size
Aberdeen	0.196	0.154	0.133	0.103	0.172	0.154	0.088	377
Auburn	0.196	0.145	0.158	0.083*	0.166	0.149	0.103	974
Bellevue	0.183	0.152	0.163	0.106	0.142	0.161	0.092	991
Bellingham	0.214	0.158	0.138	0.134**	0.134	0.119	0.103	486
Bremerton	0.177	0.154	0.133	0.123	0.169	0.131	0.113	390
Cowlitz Co.	0.215	0.151	0.154	0.106	0.165	0.115*	0.095	358
Everett	0.194	0.132	0.150	0.107	0.160	0.158	0.099	952
Lewis Co.	0.158*	0.164	0.161	0.117	0.141	0.151	0.107	298
Lynnwood	0.173*	0.140	0.167	0.090	0.170	0.148	0.112	642
Moses Lake	0.245**	0.133	0.144	0.118	0.147	0.141	0.072*	347
Mt. Vernon	0.212	0.130	0.176*	0.102	0.139	0.130	0.111	561
N. Seattle	0.193	0.148	0.143	0.102	0.167	0.146	0.101	1,150
Olympia	0.239**	0.119	0.149	0.109	0.153	0.155	0.076*	503
Rainier	0.197	0.149	0.149	0.103	0.157	0.155	0.089*	2,391
Renton	0.216	0.139	0.141	0.102	0.146	0.163	0.093	821
Spokane	0.196	0.133*	0.148	0.110	0.155	0.160	0.099	1,222
Sunnyside	0.205	0.135	0.155	0.092	0.137	0.155	0.120	502
Tri-cities	0.172*	0.158	0.163	0.077*	0.144	0.188*	0.098	570
Walla Walla	0.223	0.137	0.165	0.065	0.144	0.129	0.137	139
Wenatchee	0.202	0.146	0.121**	0.108	0.158	0.143	0.123	595
Yakima	0.203	0.153	0.159	0.090	0.142	0.153	0.100	1,265
Q1-1988	0.184	0.148	0.167	0.084*	0.137	0.164	0.115	1,006
Q2-1988	0.206	0.140**	0.153	0.099	0.154	0.148	0.102	5,010
Q3-1988	0.201	0.149	0.148	0.105	0.156	0.150	0.091**	5,612
Q4-1988	0.189*	0.143	0.150	0.106	0.155	0.156	0.101	3,906
MEAN	0.198	0.144**	0.151	0.102	0.154	0.151	0.099	
DESIGN	0.200	0.150	0.150	0.100	0.150	0.150	0.100	

* Significantly different from design in a 10% two-tailed test.
** Significantly different from design in a 5% two-tailed test.

CHAPTER 3

IMPLEMENTATION AND OPERATIONS

3.1 Operations Design: Procedural Steps at the Local and Central Offices

While the experiment had a simple design, implementation was complicated by the need to conduct the experiment statewide in 21 different Job Service Centers (JSCs) and integrate it into local operating procedures.[1] Details of the operational design were specified in the WREB Procedures Manual, which was used in the local offices to insure that the experiment was implemented in each office according to design principles. Here, we will briefly describe how the participants flowed through the system.

The steps in the treatment process, which started when a UI claimant filed an initial claim and ended with the receipt of a bonus, are shown in Figures 3.1 and 3.2. The activities that took place in the local office at which the claimant filed his/her claim are shown in Figure 3.1, and those that occurred at the central office are shown in Figure 3.2a.

At the Job Service Center, the steps were as follows:

1. Claimant filed an initial claim for UI benefits.

2. Claimstaker searched the Computerized Claim Files to determine monetary eligibility, and ascertained that it was a new claim being filed.

[1] The 21 JSCs were: Aberdeen, Auburn, Bellevue, Bellingham, Bremerton, Cowlitz County, Everett, Lewis County, Lynnwood, Moses Lake, Mount Vernon, North Seattle, Olympia, Rainier, Renton, Spokane, Sunnyside, Tri-Cities, Walla Walla, Wenatchee, and Yakima.

Washington Reemployment Bonus Demonstration
Claimant Eligibility
Local Office

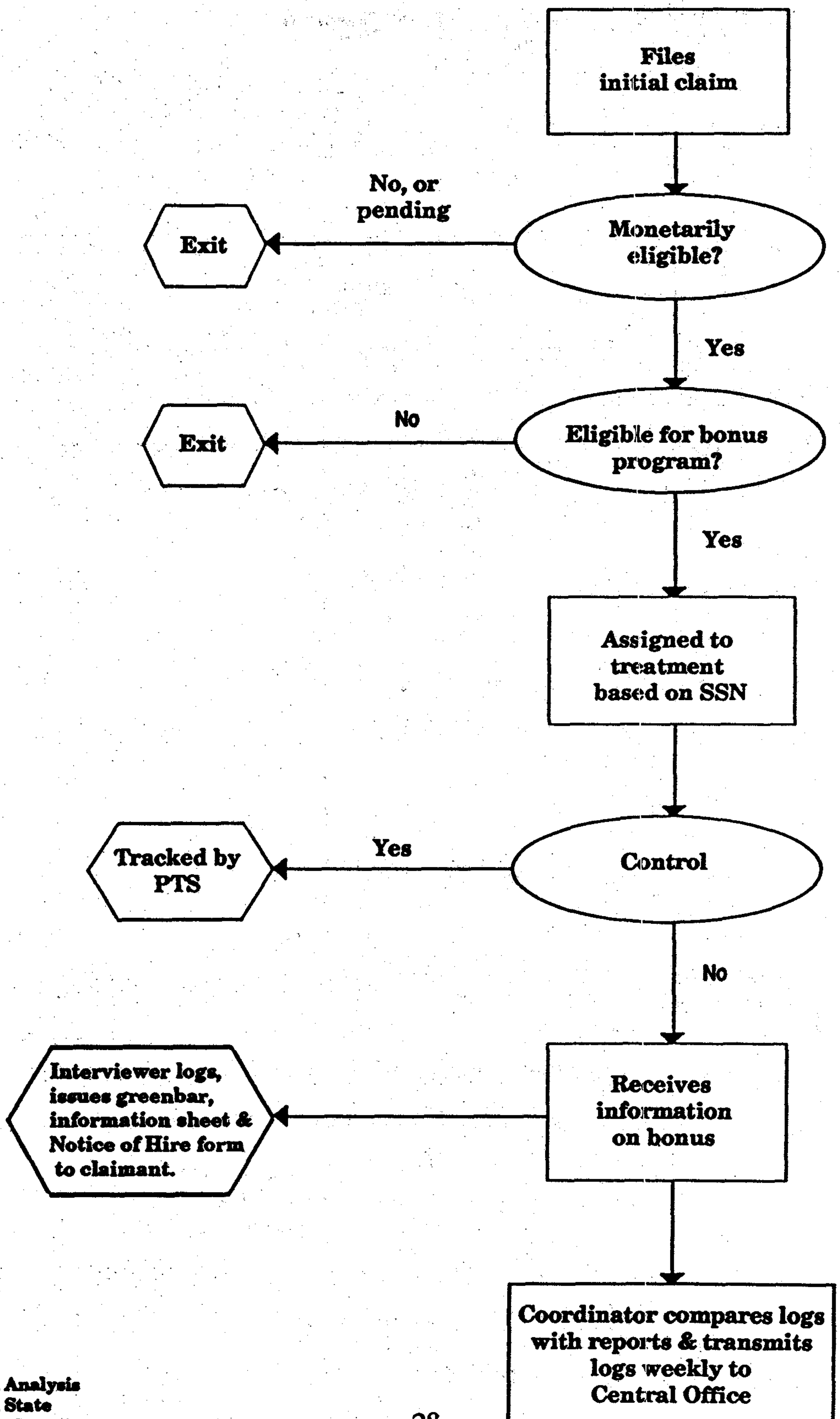
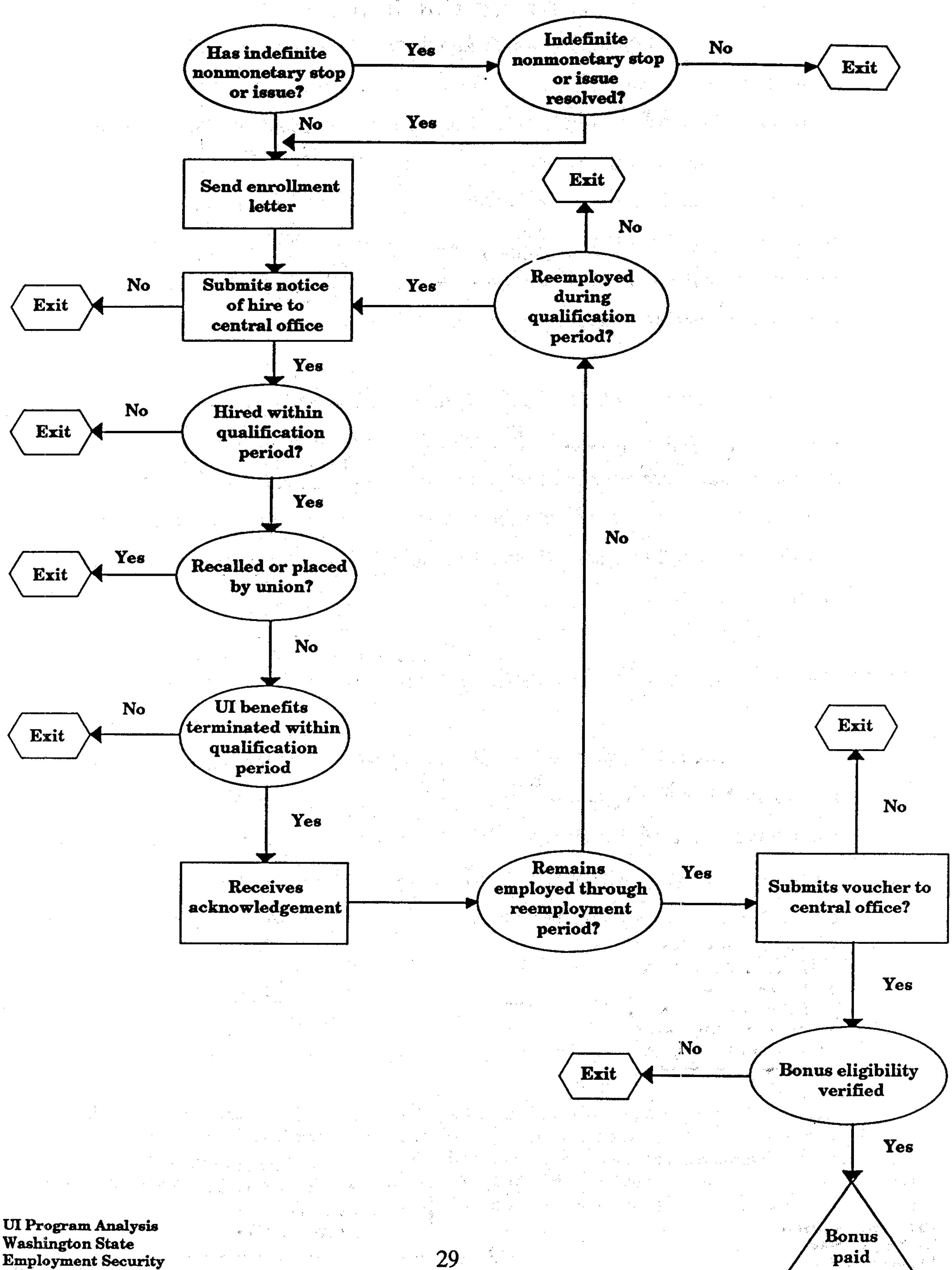


Figure 3.1

Claimant Eligibility Flow
Central Office



UI Program Analysis
Washington State
Employment Security
Department

Figure 3.2 a

3. For a randomly selected claimant, the Greenbar Monetary Determination printout (Figure 3.2b) contained a new entry that indicated assignment to a WREB treatment.[2] For an assigned claimant, the Greenbar provided the bonus award offer (based on the Weekly Benefit Amount), and the date by which full time reemployment had to start for the claimant to be eligible for a bonus. Claimants not treatment-assigned or ineligible to participate in the experiment had no entries in this special section, and received the standard interview for persons filing initial UI claims.

4. A claimant whose Greenbar displayed the bonus amount and qualification deadline was provided with an information sheet and verbally instructed in the procedures to be followed to qualify for a bonus. The enrollment interview ended with the claimstaker asking four predetermined questions designed to assess whether the claimant understood the steps necessary to qualify for a bonus.

<u>At the central office, the following activities occurred:</u>

5. Information on new WREB participants and updated claim information on existing WREB participants was downloaded from the Washington State Employment Security Department (WSESD) mainframe computer to the WREB Participant Tracking System (PTS). The PTS was PC-based and used the Oracle relational data base management system. The PTS was used to evaluate claimant eligibility.

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[2] A Greenbar Monetary Determination is a computer printout generated by a claimstaker for a claimant from the Washington State Employment Security Department's computerized Benefits Automated System. The printout lists information about a claimant's recent work history relevant to establishing a claim for UI benefits, e.g. employers and earnings in the base year. The printout is referred to as a "Greenbar" because it is printed on computer paper that has alternate green and white stripes.

WASHINGTON STATE EMPLOYMENT SECURITY DEPARTMENT M540B02
ON-LINE BENEFIT INQUIRY NETWORK
MONETARY DETERMINATION

SSA: 123-45-6716 2 JSC: PROCESSED: 01/07/88
JONES JENNIFER EFFECTIVE DATE: 01/03/88
1234 HER STREET NW BYS: 01/88 01/03/88
SOMEWHEREIN WA 98765-4321 BYE: 52/88 12/31/88
STOP: YES OVERPAYMENT: NO

QUARTERLY EARNINGS AND HOURS REPORTED FOR YOUR BASE YEAR:

NAME	EMPLOYER	ACCOUNT NO/CHK		QUARTER	WAGES	HOURS
JONES	QUICKTEST	33612373	8	3/87	192.00	24
JONES	FRUIT CORP	51425665	7	3/87	2793.75	372
JONES	QUICKTEST	33612373	8	2/87	1100.00	138
JONES	FRUIT CORP	51425665	7	2/87	1098.75	146
JONES	WILLIAMS CO	29514342	8	1/87	270.78	52
JONES	WILLIAMS CO	29514342	8	4/86	925.83	171

QUARTERLY TOTALS:

3/87		2/87		1/87		4/86	
WAGES	HOURS	WAGES	HOURS	WAGES	HOURS	WAGES	HOURS
2985.75	396	2198.75	284	270.78	52	925.83	171

TOTAL WAGES: 6389.11 TOTAL HOURS: 903 MBP: 2130 WBA: 103

412.00
FEBRUARY 27, 1988

PA1 = PAGE FWD PA2 = PAGE BACCMD: 01/07/88 11:15:32

Figure 3.2b

A. When it had been determined that there were no indefinite stops and that a valid UI claim had been established, an enrollment letter was sent to the claimant, officially confirming bonus eligibility, the size of the bonus offer and reemployment deadline. Accompanying the letter was a Notice of Hire (NOH) form to be submitted by the claimant upon starting a new full-time job.

B. No invalid claims were assigned to treatment status.

C. If a claimant had an issue stop, no enrollment letter was sent.

D. If a denial was determined, no further action was taken.

E. If an issue was adjudicated in favor of the claimant before the reemployment period expired, the claimant was eligible to participate and was mailed an enrollment letter.

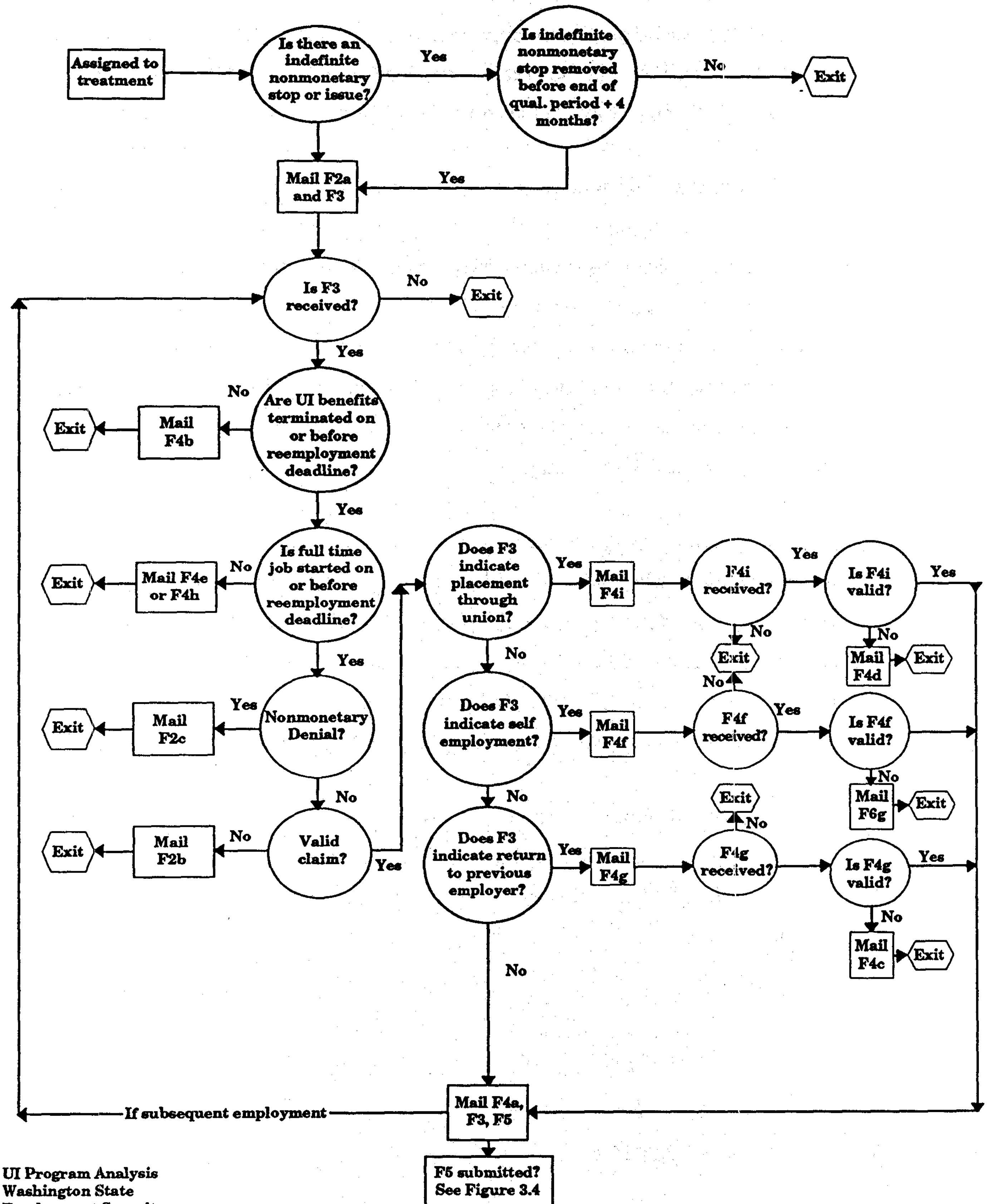
6. When a claimant obtained employment under conditions that established eligibility for the bonus, he/she was to complete the NOH and send it to the central office. When a Notice was received in the office, it was verified for completeness and checked against the PTS to assure that UI benefits had not been paid after the reemployment deadline. (See Figure 3.3.)

A. If benefits had been paid after the reemployment deadline, or the claimant had been placed on the new job through a union hiring hall, or the new job had not been full time, a rejection letter was sent to claimant.

B. If no benefits had been paid after the reemployment deadline, and the new employer was <u>not</u> the same as the prior employer, the participant was sent a valid Notice of Hire letter and a Bonus Voucher to be submitted upon completion of four months of full-time work.

C. If no benefits were paid after the reemployment deadline, and the new and prior employers were the same, or the claimant appeared to

Central Office Actions and Form Flow
Notice of Hire Process



UI Program Analysis
Washington State
Employment Security
Department

Figure 3.3

have been placed on the job through a union hiring hall, or the claimant became self-employed, an appropriate inquiry form was sent to determine if the conditions had been met for receipt of a bonus. After review of a returned inquiry form, either an enrollment or denial letter was sent.

7. Multiple Notice of Hire forms may have been received from a claimant obtaining subsequent jobs within the qualification or reemployment periods. Recall and union hiring hall placements were acceptable for subsequent employment. To maintain bonus eligibility, all other criteria had to be met. Upon receipt of a valid subsequent NOH, another NOH was sent out with instructions to send it in if a new job was acquired during the four months. If a subsequent NOH did not satisfy eligibility conditions, a denial letter was generated from the PTS and mailed.

8. Four months after the start of new full-time employment, treatment-assigned claimants who had remained employed for the entire period should have submitted Bonus Vouchers. (See Figure 3.4.) A weekly batch run of the PTS checked to determine if these conditions had been met. If vouchers had not been received at the central office, the PTS printed vouchers and letters to be mailed with the vouchers informing claimants of their likely eligibility for the bonus.

A. If benefits had been received after the reemployment deadline, the voucher was denied and a rejection letter was sent to the claimant.

B. If benefits had not been received after the reemployment deadline, the WREB Project Coordinator checked to assure that the employment conditions of the bonus offer had been met.[3]

(1) If employment could not be verified by the central office, the claimant was sent a letter requesting verification.

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[3] Ninety-eight percent of vouchers paid were audited; only reemployment with Boeing was not audited.

Central Office Actions and Form Flow
Bonus Voucher Process

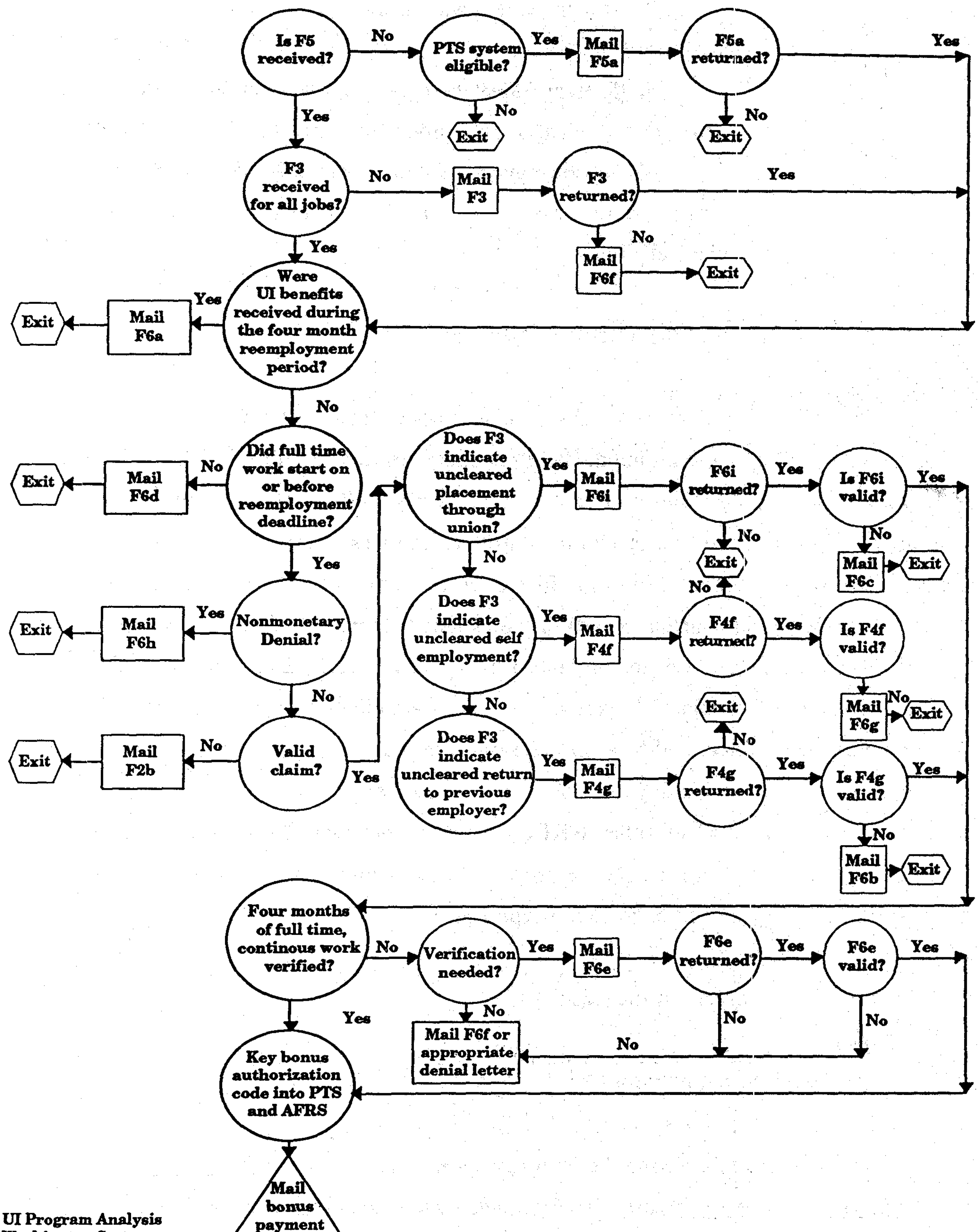

Figure 3.4

(2) If employment was verified, the Coordinator authorized payment of the bonus.

(3) If there was no response to a verification letter, or inadequate verification was produced, a denial letter was mailed.

(4) Review panels were held within the WREB central office in response to verbal or written requests for reconsideration of bonus eligibility.

3.2 Implementation of the WREB Experiment

As described in Section 3.1, the process starts with the claimant entering a local Job Service Center to file an initial claim for unemployment insurance. This section will describe in detail the operations in the local and central offices as shown in Figures 3.1 and 3.2a. The discussion starts with the enrollment process, describing how the WREB experiment was integrated into local office operations, and how the information about the bonus offer was imparted to each of the assigned claimants. The discussion then shifts to the role of the Washington State Employment Security Department (or central office) located in Olympia. We describe how claimant eligibility for the bonus is sequentially determined, and how various forms are used to acquire information needed from clients to make eligibility decisions. As summarized in Figure 3.2a, after a bonus offer was made in a JSC, all other WREB processes were carried out in the central office. This section also discusses the process of interaction between the central office and local JSCs designed to manage the experiment.

3.2.1 WREB Enrollment in the Local JSC

3.2.1.1 The Enrollment Interview

The best way to describe the interview is by reference to the Desk Aid for WREB Interview (reproduced as Figure 3.5), the single-page guide used by all JSC claimstakers

DESK AID FOR WREB INTERVIEW

1. **Check the greenbar.** If greenbar displays bonus amount and reemployment deadline, proceed with interview.

2. **Point out and state bonus amount and reemployment deadline** shown on the greenbar.

3. **Give** the claimant the **information sheet** and **read** it with him/her.

4. **Give** the claimant the **Notice of Hire** form and **tell** him/her:

   "If you get a job before the date on the greenbar:

   ✔ *Send us the Notice of Hire form,*

   ✔ *Give the name, address and phone number of the last employer prior to filing for benefits, and the first employer that leads to your not filing,*

   ✔ *Give the usual hours of work and wages per week on that job,*

   ✔ *Answer all questions."*

5. **If claimant is still employed** tell him/her: "You must either become unemployed or be eligible to draw unemployment insurance before getting a new job in order to qualify for the bonus."

6. **Please answer a few questions.** I want to be sure that I have covered all the information.

   ☛ What is the dollar amount of your bonus offer?

   ☛ By what date must you obtain full-time employment to be eligible for the bonus?

   ☛ How long must you hold a job, or jobs, in order to claim the bonus?

   ☛ How do you notify us that you started a job?

7. **Start looking for a job now!** You don't have to receive unemployment benefits or your enrollment letter to be eligible to receive a bonus.

If you have any questions about eligibility for the bonus, ask your WREB JSC Coordinator.

Figure 3.5

who gave WREB interviews. In the regular procedure, the WREB interview was conducted with randomly selected clients following the standard UI interview held with all persons filing initial claims for UI benefits. The steps in the WREB interview are described below.

The first step instructs the interviewer to check the Greenbar to see if a bonus amount and reemployment deadline are displayed, and to proceed with the interview if they are. This is an ideal situation in that treatment-assigned claimants are readily identified to interviewers, yet in such a manner as to provide no external evidence of their distinction from claimants who are not selected. Furthermore, this process guarantees that a claimstaker would not improperly enroll a claimant who was not randomly selected to be given a bonus offer. If there is no bonus entry, then there is no offer to make. On the other hand, if a claimant is selected, the entry is prominently displayed on the Greenbar so that it is extremely difficult for the claimstaker to fail to identify a WREB-assigned claimant. The Greenbar contains essential information about the claimant's benefits, and must be referenced by the claimstaker in the interview. There is evidence, however, that a few interviews were missed. As described below, there were multiple procedures followed by WREB local office and central office supervisors to identify and provide interviews to treatment-assigned claimants who had not been interviewed at the time of filing.

The WREB interview starts with the claimstaker pointing out the bonus amount and reemployment deadline displayed on the Greenbar and telling the claimant that these entries mean he/she has been randomly selected to take part in the experiment. The claimstaker fills in the claimant's name on an Information Sheet (F1 in Appendix A), and hands the sheet to the claimant. The claimstaker was trained to read the Information Sheet with the claimant, thereby assuring that the claimant has had both visual and auditory exposure to the material. The Information Sheet is a single page (in both English and Spanish) that introduces the experiment and informs the claimant about the steps necessary to qualify for a bonus.

Next, the Claimstaker hands the claimant a copy of the Notice of Hire (F3 in Appendix A), having filled out information about the last employer from the claimant's application. (Remember, the NOH is the form to be submitted by the claimant after obtaining full-time employment by the reemployment deadline.) Then, following the procedure outlined in the Desk Aid, the claimstaker gives instruction on how to file a NOH while pointing to relevant sections of the NOH during the explanation.

Step five in the Desk Aid is interposed only if the claimant is filing a claim while still employed, e.g., expecting to be laid off in the near future. The claimstaker identifies this situation from the UI application and then reads the appropriate statement. Step six is crucial, and somewhat unique. The claimstaker asks the claimant four questions to be sure that those four key pieces of information have been communicated to the claimant. If any of the questions are answered incorrectly, the claimstaker is to review that information again. Before ending the interview, the Claimstaker encourages the claimant to start looking for work immediately--a standard requirement for continuing UI eligibility--reminding him/her that obtaining employment before receiving any UI benefits does not preclude eligibility for a bonus.

Finally, a toll-free telephone number listed at the bottom of the Information Sheet is pointed out. Treatment-assigned claimants are told that if they have any questions about the WREB offer in the future, they should contact WREB headquarters in Olympia using this number. It was hoped that the toll-free telephone number would channel most questions about WREB to the central office, so that consistent information would be given. During the enrollment period, the central office received between 5 and 25 phone calls for information per day. The calls included several from nontreatment-assigned claimants inquiring as to why they were not selected to receive a bonus offer. The following are examples of questions from treatment-assigned claimants:

> My last claim was for a week of partial benefits in the week just after my reemployment deadline. Am I still eligible for the bonus?

I have filed an appeal on a nonmonetary denial. Will I be bonus- eligible if I win the appeal?

I started a part-time job before my reemployment deadline. How can I become eligible for the bonus?

I got a second new job since my reemployment deadline. What do I list as my prior job on the Notice of Hire form?

I landed a new job before the reemployment deadline, but I didn't start working on it until after the deadline. Am I still eligible for the bonus?

If I get a new job before the reemployment deadline and then lose that job, don't draw any UI benefits, and then go back to my pre-UI claim job, am I still eligible for the bonus?

My new job is with a temporary agency, which refers me to a different full-time job every week, is this full-time work for purposes of the bonus offer?

A variety of other questions about eligibility were received from union members, sales people who work for commissions, people receiving severance or vacation pay, and students. The questions indicate that people generally understood the program, but were concerned about the details of eligibility in special cases.

3.2.1.2 The WREB Interview Process

There are basically two alternative procedures for taking UI applications in the JSCs. They are either taken individually, in a one-on-one session between the claimant and a claimstaker, or they are taken in groups as large as 35 or 40. WREB did not require changes in these local office operating procedures; it did require that office managers and WREB coordinators work out systems to integrate WREB into these procedures. Certain requirements were imposed by the WSESD.

1. WREB interviews had to be held at the same time as the regular UI interview; WREB claimants would not be required to return to the JSC at another time for the WREB interview.

2. WREB claimants in group interviews had to receive all of the information imparted in the individual interview.

3. The WREB interview could be administered to all WREB participants in a group, but the four questions had to be answered by each participant separately.

These principles were followed without exception in all of the JSC offices.

For individual interviews, the WREB interview was added onto the end of the regular UI application process. For group interviews, special arrangements had to be made. In some cases, at the conclusion of the basic UI presentation and after special issues were handled on an individual basis, non-WREB-assigned claimants were excused and a group WREB interview was held for the treatment-assigned claimants. The four review questions from the Desk Aid were then asked of each claimant individually before he/she was allowed to leave the interview session. In other cases, the WREB applicants were assigned to claimstakers for individual WREB interviews after the group UI presentation.

A special kind of group interview, called a "mass application" or "mass ap" was sometimes conducted when there was a large layoff or plant closing. The mass ap was often conducted at the plant site, requiring special arrangements for the WREB interview.[4]

[4] In one case, the WREB interviews were forgone, because it was a clear case of temporary layoff in which the plant was to reopen in a specified period of time and recall the entire laid-off workforce. Applications for UI from this group were specially keyed from the central office to prevent assignment to WREB treatment or control status by the automated system.

In some JSCs, WREB interviews were given only by specially designated and trained staff; in other offices, all interviewers were trained to conduct WREB interviews. For example, in Rainier, all UI interviews were individual, and all interviewers were trained to conduct the WREB interviews. Thus, there was no change in procedure and the WREB interview (which averaged 5 to 10 minutes) was simply added at the end of the regular claim process (the entire claim process, including WREB, averaged about one-half hour in duration). At the other extreme was Wenatchee, in which all interviews, except special cases, were conducted as group interviews. Groups were scheduled sometime in the week that claimants came into the office to file a claim. Greenbars were printed when the claimants arrived for group interviews. Groups averaged about 35 in size, and 4 out of 15 specialists who took UI claims had been trained to give WREB interviews. WREB claimants in a group were identified and either asked to stay after the conclusion of the UI portion, or taken to a different room for the WREB interview.

In Spokane, WREB and non-WREB UI claimants were separated before the claims process was started. Two of the eight UI claimstakers and a supervisor were trained to give WREB interviews. WREB claimants were identified at the front desk when the Greenbar was printed. WREB claimants were then directed to one of the designated WREB claimstakers who conducted both the UI and WREB interviews. Non-WREB interviews were usually, though not always, group interviews.

Information was available for 19 of the 21 JSCs in which WREB was conducted that allowed a characterization of the office on three factors: (1) group versus individual interviews, (2) use of special WREB interviewers or use of all claimstakers to do the WREB interview, and (3) whether or not WREB represented a change in office procedures. The third factor proved not very interesting, because it was perfectly correlated with the second. That is, the principal way in which procedures were changed was to assign certain claimstakers as WREB interviewers. In Renton, a WREB specialist would come into the regular UI group session when it was finished and deliver the

WREB interview to WREB-assigned claimants who had been asked to remain after the others departed. As noted above, in Spokane, the WREB-assigned claimants bypassed the regular group session and were interviewed for both UI and WREB by the WREB specialist.

Of the 19 offices compared, WREB specialists were assigned in 5 (Everett, Renton, Spokane, Walla Walla, and Yakima), and all the claimstakers were trained in WREB in the other 14 offices. In nine offices, only individual interviews were conducted (Auburn, Bellingham, Cowlitz, Everett, Lewis, Mt. Vernon, Rainier, Spokane, and Sunnyside), while in seven offices (Bellevue, Lynnwood, Moses Lake, Olympia, Renton, Tri-Cities, and Wenatchee) only group interviews were conducted. Four of the sites had a mixture of individual and group interviews (Aberdeen, Bremerton, Walla Walla and Yakima). In Chapter 6, Section 4.1, we present results of an investigation to determine if any of these differences in office procedure affected experimental outcomes.

3.2.1.3 Instruments for Monitoring WREB Enrollment and Interview Processes

Although the sole task for the local office was to conduct enrollment interviews for UI claimants assigned to WREB treatments, two ancillary activities performed by WREB Coordinators were to assure the quality and consistency of interviews and to assure that all WREB-assigned claimants received WREB interviews. This second activity was shared by the central office.

Four instruments--two logs and two claimant flow reports--were used to ascertain whether or not a WREB assigned claimant received the WREB interview. These were:

1. The <u>Intake Log</u>, which was used in most JSCs to record names of all persons who filed UI claims;

2. The Interviewer's Weekly Log, used by WREB interviewers to record that a WREB interview was given to a WREB-assigned claimant;

3. The Status Report, a computer-generated report printed in each JSC three times a week, which provided cumulative reports for up to two weeks on all claimants for whom a Greenbar Monetary Determination had been printed; and

4. The weekly Bonus Offer Report, mailed from Olympia to each office listing WREB assignees for whom an initial claim had been filed in that week and a Greenbar had been printed.

The second, third, and fourth instruments were unique to WREB. The Intake Log, while not universally used, is part of standard procedure in many offices. JSC Coordinators were expected to check Intake Logs against the other three instruments to determine if any WREB-assigned individuals filed a claim without receiving a WREB interview. This was also checked in the central office. If either of these checks located a filed claim for a WREB-assigned claimant with no entry in an Interviewer's Weekly Log (unless the interviewer was sure that the interview had been given and the log entry inadvertently skipped), the JSC WREB Coordinator would contact the individual and mail all the forms and information sheets, after describing them on the phone. Furthermore, every claimant who was assigned to a treatment group was mailed an enrollment letter to confirm and further explain the bonus offer if there were no indefinite stops on the claim.

3.2.2 Central Office Procedures

3.2.2.1 Formal Enrollment into WREB

Once an application for UI benefits of a WREB-assigned claimant had been filed and the WREB interview given at the JSC, almost all further contact with the client about WREB was handled by the central office. The processing of records in the central office started with an attempt to confirm bonus eligibility so that an enrollment letter could be mailed, and ended with a determination as to whether the participating claimant was eligible to receive a bonus. Most of the process of handling WREB claimant files used the automated Participant Tracking System developed by the U.S. Department of Labor, with the assistance of the Upjohn Institute. The steps in this process, and the forms used to provide information to claimants, are described in Figures 3.3 and 3.4.

The applications of WREB-assigned claimants became part of the Benefits Automated System (BAS) system, the same as all applications for UI benefits. The files of WREB claimants were identified in the system by the Social Security Numbers and downloaded into the PC Oracle data base every Monday. The downloaded files included both initial claimant information on UI benefit eligibility and updated information on the status of claims. The information in the Oracle data base was organized by the PTS and used to query the data base and generate appropriate response letters.

An issue stop on a claim implied that a question had been raised regarding the claimant's eligibility for UI benefits. As long as the issue stop or a nonmonetary denial remained on the claim, no further action was taken by the central office. The PTS periodically checked to see if the stop or denial had been removed prior to the end of the period during which eligibility for the bonus could be established, i.e, the length of the qualification period plus four months; then the claimant would be sent an enrollment letter and be eligible to receive a bonus if all other conditions for the bonus were met. If a claimant failed to establish eligibility for UI (e.g., there was a separation issue that was adjudicated against the claimant with a denial being issued or a previous denial was not purged), then the claimant was ineligible to participate in WREB. However, no communication was necessary in this circumstance, since the claimant would have been informed upon enrollment that eligibility for a WREB bonus was conditional upon being eligible to receive UI benefits.

If there was no issue on the claim or an issue was removed through adjudication prior to the end of the bonus eligibility period, the claimant was sent an enrollment letter (F2a in Appendix A), which reaffirmed the bonus offer and provided instructions on how to file the Notice of Hire. A copy of the Notice of Hire (F3 in Appendix A) was included.

3.2.2.2 Processing the Notice of Hire

Further action on the WREB claim was undertaken by the central office only if a Notice of Hire was received, indicating that the claimant believed he/she had obtained employment under conditions that would qualify for a bonus.

To reiterate, these conditions were:

1. UI benefits were not paid after the reemployment deadline or the start date of qualifying reemployment, and full time reemployment was started before the reemployment deadline,
2. the claimant could not have been recalled to the job, the losing of which led to filing for benefits; and
3. the claimant was not placed on the job through a union hiring hall.

It was a task of the central office to verify that these conditions had been met.

The three sources of information used to determine qualification for WREB were: (1) the Notice of Hire form submitted by the claimant; (2) the Benefits Automated System, which contained "alpha indicators" as to standby or union status; and (3) the BAS system claim history records, which identified whether or not, and when, the claimant was paid UI benefits. This information was readily accessible in the PTS.

The Notice of Hire included the following relevant information: (1) the start date of work for the new job; (2) a description of the new job and the job held prior to filing for benefits, the losing of which led to the filing;[5] and (3) answers to three questions to identify multiple job-holders, self-employed individuals, and union members placed on the job by their union.

The steps in the central office started with a visual review of the NOH to determine recall, union hiring hall, self-employment, and full-time employment status. This review could have generated an inquiry, or a denial letter. The NOH was then keyed into the PTS. If, prior to the reemployment deadline full-time employment had not started, or after the deadline UI benefits had been drawn, or there was a stop on the claim, the PTS would alert the staff to the situation and display a recommended letter. The results of the visual and PTS reviews were combined and a decision made as to the appropriate letter to send treatment-assigned claimants. (Copies of the letters are presented in Appendix A, the purpose of each letter is identified in Figures 3.3 and 3.4.)

If benefit payments had been terminated in time, then the central office checked the other conditions for eligibility, as follows:

1. Full-Time Employment: If the hours were less than 34 or the average weekly earnings were not sufficient to prevent partial UI benefit payments, the claimant was sent a denial letter stating that full-time work conditions had not been met. As in the case of all of these inquiries, the claimant was asked to contact the central office if he/she believed that the reasons for denial were not valid. If the hours or wages were inadequate, but the claimant answered that he/she currently held more than one job, then the central office contacted the

[5] The prior job may or may not have been the long-term career job; i.e., the claimant could have held a job for 10 years, left or lost it, started another job for six months and lost it; the loss of the second job preceded benefit filing.

claimant by letter or telephone to obtain information about the other job(s) to determine if the sum of the hours or wages met the criteria.

2. Conditions of Employment: If a full time job was started on or before the reemployment deadline, the Notice of Hire was reviewed for compliance with the conditions of employment, which may be defined as a set of three questions:

A. Was placement through a union hiring hall? A letter of union inquiry (F4i in Appendix A) was sent any claimant who responded to the question on the Notice of Hire that he/she was a member of a full or partial referral union; or if there was an "alpha indicator" of "U" or "Q" in the PTS.[6]

B. Is the claimant self-employed? If the claimant answered "yes" to the question, "Do you own the business where you now work?" he/she was sent a self-employment inquiry asking for the name and address of the business, a Washington State or Federal business license number, and a copy of the latest quarterly business income tax form. Satisfaction with the response determined whether the designated self-employed was legitimate, in which case the claimant was eligible to receive a bonus, on the further condition that the claimant did not file for benefits for four months and remained employed full time.

C. Did the claimant return to his/her previous job? If the employers listed on the NOH for the new job and the prior job were the same, then a Job Change Inquiry was sent (F4g in Appendix A), asking for more details about the two jobs. Specifically, the job titles, pay rates,

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[6] "U" designates that the claimant is exempt from job search because he/she is a member of a full referral union, while "Q" means that the claimant is a member of a union that has a hiring hall, but it is not full referral, and thus the claimant is not completely exempt from the UI job search requirement.

geographic location, department or division, or job number (if applicable). Basically, if two of these categories differed, the statement that the job did not represent a recall by the claimant was accepted.

Table 3-1 provides the statistics on the various circumstances that could lead to a denial of eligibility for the bonus for those submitting Notices of Hire. The data show that of the 2,533 individuals submitting Notices of Hire, denials or inquiries were sent to 564, or 22 percent. Of these, 258, or 46 percent, responded successfully to the inquiry and their Notices of Hire were deemed valid. Only 306, i.e., 12 percent, of the submitted Notices of Hire resulted in either an explicit rejection or an implicit rejection resulting from the failure of the claimant to respond to the letter of inquiry.

Review of the Notice of Hire was repeated for any claimant changing jobs during the four-month reemployment period and submitting a subsequent Notice of Hire. A subsequent NOH was submitted by 594 claimants. However, most of the 2,533 claimants submitting NOHs, submitted only one.

3.2.2.3 The Bonus Payment System

The next stage in the process commenced when a claimant's four-month reemployment period ended. Sometime after that, a Bonus Voucher (F5 in Appendix A) should have been sent to the central office, providing notice that the claimant believed he/she had met all the conditions for receipt of a bonus. The following were steps taken by the staff in the central office to evaluate a Bonus Voucher: (1) all previous paperwork was pulled from files and reviewed, (2) the Bonus Voucher was keyed into the PTS and system edits were reviewed, (3) edits revealed were screened using the WSESD mainframe computer, the PTS, the Washington State Department of Revenue computer, and/or claimant calendars and earnings deduction charts; (4) the four months of work was verified by telephone or through the mail; (5) a bonus eligibility code was entered into the PTS to allow or deny the bonus; and finally (6) the PTS either authorized the

Table 3-1

Responses to Claimants Submitting Notices of Hire (NOH)

	Total Claimants
Total treatments	14,080
Total receiving enrollment letter	12,140
Total receiving no enrollment letter	1,941
Responses to Notices of Hire:	
Total individuals submitting NOH	2,533
Total individuals receiving F4a letter	2,255
Claimants denied or failed to return inquiry notices	278

Further Breakdown of Responses to NOH

	Response to NOH	Subsequent valid NOH	Invalid NOH
UI drawn after start/deadline - F4b	60	26	34
Job recall - F4c	37	2	35
Union hiring call - F4d	10	0	10
Job start after deadline - F4e	75	2	73
Self-employment notice - F4f	73	47	26
Job change inquiry - F4g	151	102	49
Job not full time - F4h	51	14	37
Union inquiry - F4i	107	65	42
Denials or inquiry notices[a]			
Multiple denials	564	258	306
Number of claimants denied			28
			278

[a] Total individuals receiving an F4a letter (2,255) includes claimants who receive a denial letter or inquiry notice and subsequently receive a valid NOH letter. Also the total of denials or failure to return notices (278) does not equal total denials or inquiry notices less subsequent valid NOH (564 - 258 = 306 < > 278). This occurs because some claimants, for example, receive both a Job Change Inquiry (F4g) and a Union Inquiry (F4i). These claimants appear in both rows. Still others were denied because of several reasons such as drawing benefits past the deadline (F4b) and being recalled (F4c). Since both types of letters were printed, the claimant appears in both rows.

State Vendor Payment System to make a bonus payment, or printed the appropriate denial letter.

If employment changed during the four-month period, a gap in employment not to exceed one week was allowed; longer gaps would disqualify the claimant from receipt of the bonus.[7] This policy led to some confusion; some claimants with longer spells of unemployment failed to file for UI benefits, even though the gap in employment was sufficiently long to qualify for UI benefits, on the expectation that simply not filing was sufficient to qualify for the bonus.[8] The central office engaged in extensive verification procedures.

If all checks indicated that the claimant was entitled to a bonus, a Bonus Voucher authorization was keyed into the PTS and a report was printed. From that report a payment authorization form, called Form A62, was completed and keyed into the WSESD computer system to generate a check to pay the bonus. These checks, prepared within two days of submittal, were mailed by the WREB coordinator to the claimant.

As shown in Table 3-2, 1,816 bonuses were paid, and 130 Vouchers were denied. The table shows that 205 inquiries were sent to claimants, and 72 were resolved in favor of the claimant (the difference of 133 exceeded the 130 denials because some Vouchers generated more than one response). The single most common question arose because of the inability of the central office to verify employment (88 cases). Most of these cases (69) ended in denial, because the claimants were not able to verify that they had four months of continuous full-time employment.

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[7] It should be noted that one week was also the minimum duration specified for bona fide reemployment in a new job before a recall or union placement would be acceptable as part of the four months of continuous reemployment for bonus eligibility.

[8] The central office assisted this group of claimants who became ineligible for a bonus to receive the UI benefits to which they were entitled.

Table 3-2

Responses to Claimants Submitting Bonus Vouchers

	Total claimants
Responses to bonus vouchers:	
Total bonus vouchers	1,946
Total bonus payments	1,816
Total denials	130

Breakdown of Responses to Vouchers

	Responses to Voucher	Subsequent Bonus Payment
UI drawn in 4-months - F6a	32	13
Job recall - F6b	13	3
Job not full time - F6d	21	5
Need additional information - F6e	30	23
Cannot verify employment - F6f	88	19
Cannot verify self-employment - F6g	10	8
Nonmon. denial at voucher - F6h	11	1
Denials or Inquiry[a]	205	72

[a] Total denials or inquiry less subsequent payments (133 = 205 - 72) does not equal total denials because some claimants were denied because of more than one reason. Since both types of letters were printed, the claimant appears in both rows.

The PTS generated a weekly mailing of letters reminding claimants who believed themselves to be eligible to submit Bonus Vouchers. At most, a single reminder was mailed to any one claimant. These letters were sent to any claimant who had submitted a valid Notice of Hire, had gone four months without filing for UI benefits, and had not submitted a Voucher. Such letters (Form F5a in Appendix A) were sent to 564 claimants, 482 of whom submitted Vouchers (which they may have eventually done without the letter to remind them), and 419 received bonus payments.

3.3 Assignment to Treatments

3.3.1 Procedures for Assigning Bonus Offers and Reemployment Deadlines

A bonus amount and reemployment deadline were established for each claimant assigned to the experiment, based upon the claimant's assigned treatment and UI entitlement (see Chapter 2, Sections 2 and 3). The bonus amount and reemployment deadline were printed on that claimant's Greenbar Monetary Determination. Among other things, the Greenbar provided the following information: Weekly Benefit Amount, Maximum Benefits Payable (MBP), Benefit Year Start (BYS), and quarterly earnings in the base year upon which the WBA and MBP had been calculated.

During the process of reviewing an initial claim for UI benefits, errors may have been discovered on the Greenbar based on information provided by the claimant. After any modification of the conditions of the claim based on verifiable information provided by the claimant, a new Greenbar was printed and the application keyed into the BAS data system. This last Greenbar presented information that matched the claim information regarding things like the Benefit Year Start date, the Maximum Benefits Payable, and the Weekly Benefit Amount. For WREB claimants, this information was also used to compute the bonus amount and reemployment deadline printed on the Greenbar. Once the Greenbar was printed and initial claim keyed, the bonus amount and reemployment deadline were permanently established. If another Greenbar was printed after that date, the bonus information did not appear on it.

3.3.2 Differences Between Design and Effective Reemployment Deadlines

The reemployment deadline is the date by which full time employment had to be started for the claimant to be eligible for the bonus. In theory, the time elapsed from filing the UI claim to the reemployment deadline was the length of time available to search for full-time employment, and this was specified in the experimental design to be a fixed proportion of the entitlement period (see Chapter 2, Section 2.2). However, the procedure adopted for setting the reemployment deadline introduced some unintended deviation from the design concept. This led to a change in the procedure for calculating the reemployment deadline partway through the experiment.

To determine the reemployment deadline, there were three dates of importance: (1) the Effective Date of Claim (EDC), which is the Sunday of the week that the benefit year starts; (2) the Printing Date, which is the date that the claimant files for benefits, and (3) the Initial Claim Process Date, which is the day on which the application is keyed into the computer system.

During the pilot study and for the first two months of the regular experiment, the EDC was used as the start date of the qualification period. However, this created a problem because of "backdating." A claim has been backdated if the EDC is set at a date prior to the Sunday of the week in which the claim was filed. If a claim was backdated, the beginning of the qualification period was also backdated, with the result that the available search time was shortened to less than the experimentally designed length. Indeed, under this procedure it was even possible for a reemployment deadline on a backdated claim to precede the Greenbar Printing Date, i.e., a negative qualification period was possible.

The procedure was changed during the week which started on May 9, 1988. Beginning that week, the Sunday before the Greenbar Printing Date was used as the starting date of the qualification period. This change was made so that the qualification period would have the same length for all claimants with the same entitled duration of benefits at filing. We believed that it was more important to assure this equality than to maintain the fixity of the ratio between the qualification period length and the number of weeks of compensation remaining after the Greenbar Printing Date.[9] Ex post the impact of the qualification period can be examined econometrically using a variety relative measures, but if a claimant is offered a bonus with a nonexistent or negative search period, no impact analysis is possible.

Table 3-3 shows the number of weeks between the EDC and the Initial Claim Process Date. Twenty-seven percent of treatment-assigned claimants and 29 percent of control-assigned claimants had at least one week difference between their EDC and Process Date. Most of these, about 85 percent for both control and experimental claimants, had differences of exactly one week. The majority were cases in which claims submitted on Friday were processed the following Monday. Even though the EDC is a week prior to the Process Date, this did not represent true backdating, nor would it indicate a search period for the bonus less than the designed length.[10] Thus, only 4 percent of both control- and treatment-assigned claimants truly backdated their claims, thereby creating a difference between the qualification period intended in the experimental design and their actual available search time.

[9] If a claimant backdates a claim, he/she may enjoy the same entitled duration of benefits at filing as someone who does not backdate his/her claim, but if the backdate is for more than one week (the length of the waiting period), the remaining compensable period is shorter for the claimant who backdated.

[10] There were several ways in which the EDC could differ from the Process Date. Although most applications are keyed into the computer system in the JSC during the week the claimant files their claim, it was not uncommon for a claim submitted late on Friday to be processed on the following Monday. However, this would not affect search time, since both the filing date and the EDC would be the Sunday prior to the filing date. These are not equivalent to backdated claims, since the EDC is not backdated.

Table 3-3

Weeks Between Effective Date of Claim and Initial Claim Process Date

Control Group Members

Weeks	Total Claimants	Enrolled before 05/09/88	Enrolled after 05/09/88
0	2,159	444	1,715
1	764	103	661
2	89	8	81
3	21	5	16
4	5	0	5
5	4	1	3
6	2	0	2
10	1	1	0
13	2	1	1
14	1	0	1
Totals	3,048	563	2,485
Average time (Weeks)	0.36	0.29	0.38
Share 1 week or more	0.29	0.21	0.30

Treatment Group Members

Weeks	Total Claimants	Enrolled before 05/09/88	Enrolled after 05/09/88
0	9,004	1,854	7,150
1	2,865	379	2,486
2	329	28	301
3	96	12	84
4	34	4	30
5	15	1	14
6	6	1	6
7	2	0	2
8	1	0	1
9	2	0	2
10	3	0	1
13	1	0	3
15	1	0	1
16	1	1	0
17	1	0	1
18	1	0	1
21	1	0	1
Totals	12,363	2,280	10,083
Average time (Weeks)	0.34	0.22	0.37
Share 1 week or more	0.27	0.19	0.29

According to data in Table 3-3, prior to May 9, 1988 there were 2.5 percent of control claimants who had differences between their Initial Claim Process Date and EDC of two weeks or more. In the same period, only 1.8 percent of treatment-assigned claimants had such a difference. Although this difference could have been experimentally caused, since experimental subjects seeking a bonus are disadvantaged by backdating a claim, the difference between these two proportions was not statistically significant. (See Appendix B for further analyses of backdating.) Therefore, even though the proportion of backdated claims increased after May 9, 1988, there is no reason to be concerned about the change in the procedure, since backdating the qualification start date did not have a significant effect on behavior, and the proportion of claimants involved was extremely small.

3.3.3 Differences Between Design and Effective Ratios of Bonus Offer to Weekly Benefit Amount

The bonus level is fixed in terms of the claimant's WBA at the time the application is filed. For a small number of claimants, the WBA changed because the base year earnings may not have been accurate or complete at the time of filing. For instance, so-called "stranger wages," i.e., wages that have been incorrectly assigned to the claimant, may be subtracted, or other earnings may be added, such as state and local government earnings, wages from another state, federal or military earnings, or wages that have not been appropriately credited to the claimant's account.

The decision was made not to have these additional wages affect the bonus offer because of the strong desire to be able to present potential participants with a fixed bonus offer at the time of the initial interview. It was felt that the inability to offer a fixed dollar bonus at the time of filing would weaken the claimant's response to the experiment. For those claimants whose WBA changed after the filing date, the ratio of the value of the bonus to their WBA would not be the same as that intended in the experimental design (see Chapter 2, Section 2.1).

Table 3-4 shows the distribution of bonuses as multiples of the final WBA for all assigned claimants. Over 95 percent of the bonuses were exactly the intended multiple of the WBA, and most of the remainder deviated to only a small degree. As a result, the mean values in each of the treatment cells deviated from the intended multiples only in the second decimal, and the variance was negligible. Thus, there was little or no distortion in the outcome as a result of using initial as against final WBAs to determine bonus multipliers. (This issue is discussed further in Appendix C.)

3.4 Training Program for Agency Personnel

The next section describes the training program and materials used to train those JSC staff who were responsible for carrying out the enrollment process. Emphasis in this section is on the efforts made to assure that the experimental design was correctly implemented in each of the JSC offices.

3.4.1 Train-the-Trainer

The training program, designed by the staff of the Upjohn Institute and WSESD, was conducted in two stages. The first stage was a day-long train-the-trainer session, and the second a three-hour session for the trainers to train the JSC staff members who would have contact with potential enrollees. Each office manager designated two trainers: one, called the JSC WREB Coordinator, would have responsibility in the JSC for supervising enrollment into WREB; the other would serve as a backup.

Upjohn Institute staff members and the WSESD WREB Project Coordinator conducted the train-the-trainer sessions. These sessions were conducted three times, once in eastern Washington and twice in western Washington. Each of the three sessions was attended by representatives from a different group of seven of the twenty-one JSCs involved in WREB.

Table 3-4

Distribution of Claimants by Ratio of Bonus Offer to WBA

Bonus multiple	Claimants in treatment group 1	Claimants in treatment group 4
< 1.5	29	21
1.5 through 1.9	67	52
2.0	2,118	2,260
2.1 through 2.5	9	10
> 2.5	4	10
Total claimants	2,227	2,353
Average multiple	2.0	2.0

Bonus multiple	Claimants in treatment group 2	Claimants in treatment group 5
< 3.0	28	26
3.0 through 3.4	17	16
3.5 through 3.9	44	30
4.0	2,240	2,243
4.1 through 4.5	10	5
> 4.5	3	14
Total claimants	2,342	2,334
Average multiple	4.0	4.0

Bonus multiple	Claimants in treatment group 3	Claimants in treatment group 6
< 5.0	18	28
5.0 through 5.4	9	11
5.5 through 5.9	19	18
6.0	1,532	1,450
6.1 through 6.5	5	4
> 6.5	7	6
Total claimants	1,590	1,517
Average multiple	6.0	6.0

Figure 3.6 shows the timing of each step in the train-the-trainer session. After introductions, a half hour was spent providing an overview of the experiment, describing its purpose and origins, how the sites were selected, and how the program was being administered by the state. The next 45 minutes were spent describing the necessary WREB local office procedures. Particular attention was paid to printing the Greenbar and to the bonus information that would appear on it for randomly selected clients. An example of how the bonus amount and reemployment deadline would appear on the Greenbar was reviewed. The WREB interview was then described, with attention paid to the need for staff members in all JSCs to present the material to the claimant in the same manner--ad libbing was to be discouraged. The organization of the interview was then discussed, with emphasis on the differences in the procedures to be followed for conducting individual interviews, group interviews and mass applications. Two other items covered in this part of the training session were the office logs and the responsibility of the JSC WREB Coordinator (the title given to the JSC office staff member assigned responsibility for WREB).

The next step, lasting almost two hours, was a detailed description of the enrollment interview, including descriptions and review of each piece of material to be used, namely the Desk Aid[11], the Information Sheet, and the Notice of Hire form. The exact scenarios to be used by the interviewers for different claimant situations were described. In the training sessions, a variety of possible questions that claimants might ask were reviewed. The trainers were told to be sure that the interviewers read the Information Sheet to the claimant so that we could be sure that all claimants heard the same information.

The afternoon session covered the training program that trainers were to conduct for interviewers (claimstakers). This session included discussion of the claimant flow

[11] The Desk Aid provided a check-off list for the interviewer (see Figure 3.5) to be sure that all interviewers covered exactly the same material.

WREB TRAIN-THE-TRAINER AGENDA

8:00 Introduction
- a. Visitors
- b. Demonstration Team
- c. JSC Staffs

8:15 Overview of the Demonstration
- a. Purpose (how it came about)
- b. Design Overview (Flow chart from Design)
- c. Program Administration
- d. Selection of Sites
- e. Time Line
- f. Question and Answer Period

8:45 WREB Local Office Procedures (Local Office Flow Chart)
- a. Use of the Greenbar Monetary Determination
- b. Scenario for Individual Interviews
- c. Scenario for Mass Application Interviews
- d. Roles of JSC WREB Coordinator and Claimstakers
- e. The Office Log

9:30 Coffee Break

9:45 Enrollment Interview
- a. The Desk Aid
- b. The Information Sheet
- c. Q & A for Information Sheet
- d. The Notice of Hire
- e. Q & A for Notice of Hire
- f. Supplementary Q & A

12:00 Break for Lunch

1.:15 Review Local Office and Enrollment Procedures

1:45 Training Claimstakers
- a. The Training Program
- b. Scheduling the Training
- c. Interactive Training--Use of Q & A's, Role-Playing
- d. Questions to Refer to JSC WREB Coordinator

2:45 Coffee Break

3:00 Demonstration and Role Playing of WREB Interview
- a. Individual Interview
- b. Mass Application

4:00 Adjourn

Figure 3.6

chart, Greenbar printing, the interview, the quiz, the practice interviews, and the log. The practice interview was conducted as a simulation of an actual interview in which the trainees were split into groups of three, with one taking the part of a claimant, one a claimstaker, and the third acting as an observer. The "claimant" was assigned one of five situations. At the end of the mock interview, the observers from each group of three reported on the interview, and suggestions were made for improvement in procedures.

After a coffee break, a written multiple choice quiz was administered to ascertain the degree of understanding on the part of the train-the-trainer session participants. This was followed by a general discussion about how each JSC planned to implement procedures for conducting WREB interviews, recognizing that these procedures had to fit into the regular work program of the office. (See Section 3.2.1.2 above for a discussion of the interview options.)

3.4.2 Claimstaker Training

These sessions were conducted in each of the JSCs by the newly trained trainers, with an Upjohn, WSESD, or USDOL staff person present as observer and resource. The sessions were abbreviated versions of the train-the-trainer sessions. They started with the overview of WREB, which had been prepared in a two-page written handout. This was followed by a discussion of local office procedure and how it would be affected by WREB. Emphasis was placed on the printing of the Greenbar, and its importance in the process. Particular attention was paid to the fact that once the initial claim was keyed, the bonus amount and the reemployment deadline were set and unchangeable, the bonus offer was assumed to have been made, and the claimant's qualification period started. Therefore, claimstakers were carefully trained to be sure that the last Greenbar printed was the same as the claim reflected on the Greenbar given to the claimant.[12]

[12] Office managers appreciated this part of the WREB instruction, since it did no more than reinforce correct office procedure.

Most of the training session was spent carefully delineating the enrollment interview, going over each of the information pieces that would be handled in the local office; i.e., the Desk Aid, the Information Sheet, and the Notice of Hire form. Their use was reviewed through a set of questions and answers. Last, the class was divided into groups of three for the simulated interviews.

JSC supervisors were unanimous in expressing the sentiment that among all the training sessions given for special programs implemented in the local offices, the WREB training was the most thorough. On site monitoring later confirmed that we were generally successful in establishing procedures and designing an interview that could be conducted consistently throughout the system. The follow-up survey provides some information on our success in imparting information to the claimants.[13]

3.5 Time Sequence of Events

The field phase of the experiment began with a pilot study conducted in Yakima, Washington, the purpose of which was to test the use of BAS for random selection and Greenbar printing, the training procedures, and the claimant selection and enrollment procedures. Training of Yakima JSC staff responsible for conducting the experiment was carried out at the end of January 1988. The pilot became operational in the first week of February and was the only site in operation until it was joined by the first group of seven sites in the second week of March. The remainder of the 21 operational sites came on line in two batches, with all sites operational starting the last week in March 1988. Enrollment continued through November 23, 1988, the day before Thanksgiving, at which time enrollment ceased simultaneously at all sites. Therefore, not counting Yakima, sites operated from 35 to 37 weeks, depending upon their start dates. Appendix D is The WREB Enrollment Monitoring Simulation Model, a report on the model

[13] For example, in Chapter 8 we report that among the 439 claimants who responded to the follow-up survey and returned to work, which would qualify them for the bonus, only 5 claimants said they failed to submit a Notice of Hire because they did not understand the instructions.

developed to determine the date at which enrollment should stop so that the bonus budget would be just exhausted and the largest affordable sample would result (Spiegelman and O'Leary 1988a).

3.6 Monitoring Experimental Operations

3.6.1 Monitoring Claimant Flow

Information on the proportion of claimants who passed from one stage to another in the process was critical for determining the likely bonus cost of the experiment and calling attention to possible procedural problems in specific local offices. This section discusses how the aggregate flows were monitored to assure that the experiment would be terminated at the right time to maximize the likelihood of exhausting, but not overrunning, the $1.2 million bonus budget, and how the client flows through each office were monitored to detect deviations in the actual parameters from the ex ante expectations which might be indicative of problems.

The Oracle-based PTS system produced reports using data downloaded from BAS on a weekly basis and information key entered at the central office that permitted the close monitoring of the flow of claimants through the program. The monitoring was carried out simultaneously by state, federal and Upjohn Institute staff members using these and other instruments.

The Institute designed a monitoring instrument that would permit the central office staff to evaluate operations at each of the 21 local offices, and would also provide data to permit a periodic (weekly) estimate of expected total bonus cost as a basis for altering the enrollment rate or the experimental end date. The WREB Enrollment Monitoring Simulation Model presented as Appendix D describes the model and data used to monitor the program.

The simulation model was based on four quantities, defined for each JSC, by week:

1. The inflow of new claims that are monetarily valid at time of filing (the basic data for estimating number of eligible claimants enrolled in treatments),
2. The dollar value of the average bonus offer,
3. The proportion of WREB-assigned claimants who become reemployed in the ith week after filing an initial claim, and
4. The proportion of WREB assigned claimants whose reemployment deadline will occur in the ith week.

A prediction was incorporated into the experimental design document that led us to anticipate enrolling claimants in the 21 JSCs for 32 to 33 weeks, based upon an estimated claims load of 89,797 new Washington claims, assignment of 13,827 monetarily eligible claimants to the six treatments, and payment of 2,162 bonuses with an average value of $575 (Spiegelman and O'Leary 1988b). (See Chapter 2, Section 3.2.4.)

Initial claims flow was somewhat below expectations, on average 5.4 percent by the 37th week, with wide divergence across JSCs. For instance, the flow rate in Rainier was 35 percent above the results of the simulation model, due to a poor central city economy, whereas in Renton, a close neighbor, the flow of claims was 20 percent below expectations. Our model also overpredicted eligible claimants rather uniformly. The model anticipated that 85.8 percent of claims would be valid at filing, i.e., have no stops or nonmonetary issues preventing payment of benefits, whereas the results were that only 78.5 percent were eligible. More disconcerting is the fact that on the average 5.3 percent fewer claimants were assigned to a treatment, given they had monetarily valid initial claims. This discrepancy was quite large in some JSCs: Bellingham, Cowlitz County, Lynnwood, Moses Lake, Olympia, and Renton had discrepancies that exceeded 2 standard errors from the expected values. Since assignment was based on the BAS data on the WSESD mainframe, enrollment errors could not be attributed to error in the local JSC; and they remain unexplained.

In a sample weekly enrollment monitoring report, Appendix D shows the relationship between the baseline expected and the updated estimate of Notices of Hire filed. Differences here could either mollify or exacerbate differences in the enrollment rate. Since NOHs flow into the office over a period of time after the date the claimant becomes reemployed, there is no precise date at which all NOHs for a given enrollment period might be expected to have been submitted.[14] Appendix D shows the number of NOHs expected as of the most recent week. It is reasonable to assume that many valid NOHs were yet to be filed. An examination of the data indicated that truncating qualification dates by dropping the most recent seven weeks prior to the current date permitted enough time so that almost all NOHs that would be filed were filed. Using that rule of thumb, the week of 12/31/88 was chosen. By that qualification deadline, 1,997 NOHs had been received, 92 percent of the number expected. The expected number was based upon an estimate of the proportion of eligible UI claimants who would obtain full-time employment (about 40 percent based on historical data) and an estimate of how many of these would submit NOHs and not have returned to their previous job or been placed through a union hiring hall. We predicted that about 50 percent would so qualify, implying that 20 percent of eligible claimants should submit Notices of Hire. As indicated in Appendix D, about 18.4 percent of eligibles ultimately submitted valid NOHs.

Most of the parameters of the system were fairly stable over time, with one important exception. The rate of filing valid NOHs varied considerably from week to week. A final run of the data, including all submitted NOHs, displayed the pattern shown in Figure 3.7. A time series regression indicated that not only was there a large variance, but there was also a systematic decline in the rate of filing NOHs over time. The following regression equation was estimated:

NOH filing rate = .1885 - .0013*(weeks since program start).

[14] Indeed some claimants misunderstood the intended procedure and mailed in their NOH only after four months of continuous reemployment, thinking this was the way to claim a bonus.

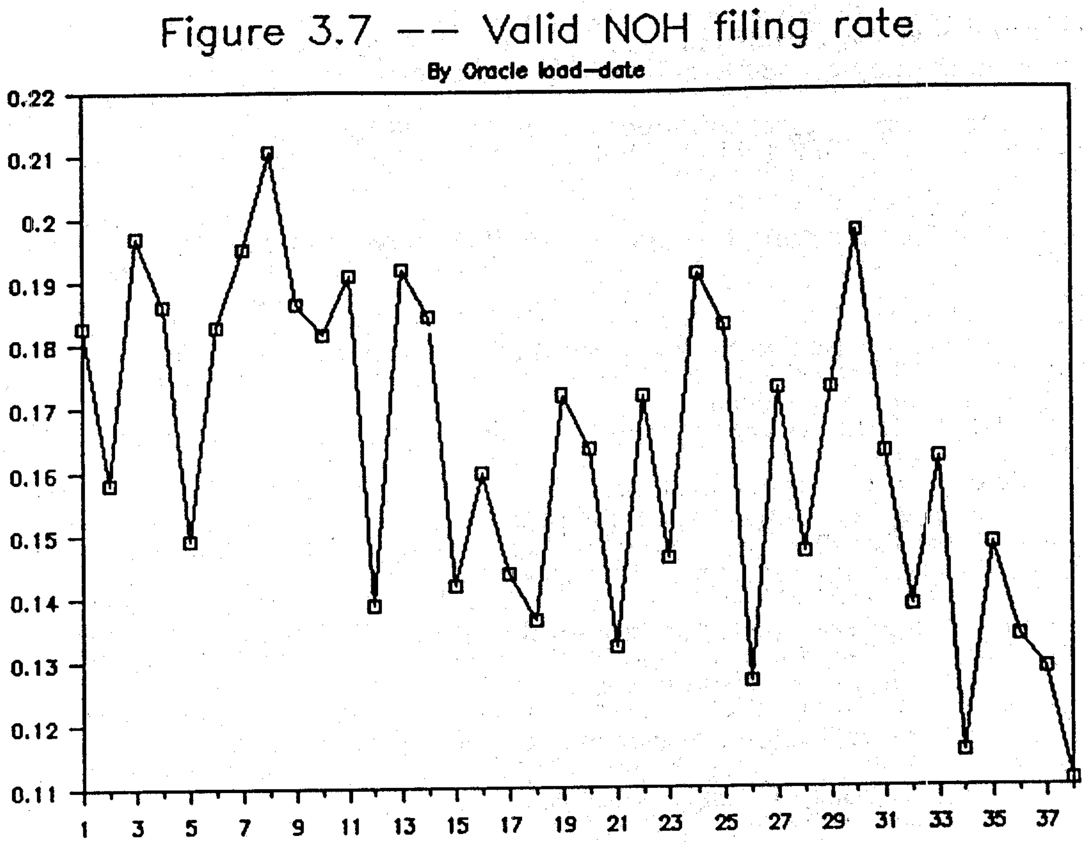
Figure 3.7 -- Valid NOH filing rate
By Oracle load-date
0.22
0.21
0.2
0.19
0.18
0.17
0.16
0.15
0.14
0.13
0.12
0.11
1 3 5 7 9 11 13 15 17 19 21 23 25 27 29 31 33 35 37

The time variable had a t-statistic of 4.11, which is highly significant, clearly indicating that the NOH filing rate declined with time. Thus, the regression indicated that the valid filing rate fell from a regression predicted rate of .188 for claimants enrolled in the first week of the program to .139 for those enrolled in the final week. There was no obvious deterioration in the enrollment process, in that there was no deterioration in the ratio of enrolles to eligibles or other indication of performance lapses. Seasonal employment patterns in agriculture did not account for this effect, which left us with no satisfactory explanation for this decline.

The next step was to estimate the number of bonuses that would be paid. As shown in Table 3-2, 1,816 bonuses were ultimately paid. This amounted to 80.5 percent of the number of claimants who had submitted valid NOHs. The prediction model had used .85 as the proportion of NOHs that would convert into paid bonuses. To predict the final bonus cost, the .805 rate of conversion of NOHs to bonus payments shown in Table 3-5 was used to predict the final bonus cost.

The final input to the estimation process is the average value of the bonus. Table 3-6 shows that the average bonus offer of $567 did not differ significantly from the $573 used in the base line simulation. However, the important variable is not the value of the bonus offer but the value of bonuses collected, and that proved to be quite different. The average value of bonuses offered for those submitting NOHs was $643 (Table 3-7), while the average value of bonuses paid was even higher, $653. Initially, the NOH value was used for projecting cost. Later, we switched to the more conservative approach of using the higher bonus value (the final actual value appears to be somewhere between the two).

The forecast simulation plugged in modified values for all of the relevant variables and predicted the enrollment period just necessary to exhaust the budget (see Appendix D). In the simulation shown in Appendix D, the model predicted enrollment for 39 to 40 weeks. Substituting the bonus value for the NOH value for the estimate of

Table 3-5

Bonus Voucher and Payment Observations

Site	JSC	4-month period elapsed	Vouchers received	Will deny missing vouchers	B
Aberdeen	630	29	23	5	
Auburn	310	190	162	19	
Bellevue	390	234	207	23	
Bellingham	540	58	53	5	
Bremerton	550	48	40	8	
Cowlitz Cnty	650	38	30	7	
Everett	380	130	113	13	
Lewis Cnty	620	25	21	4	
Lynnwood	350	116	107	7	
Moses Lake	840	38	33	5	
Mt. Vernon	560	60	53	7	
North Seattle	360	217	190	15	
Olympia	610	71	57	10	
Rainier	370	315	272	27	
Renton	330	130	112	13	
Spokane	810	195	174	10	
Sunnyside	940	47	32	14	
Tri-Cities	950	48	43	3	
Walla Walla	960	23	20	2	
Wenatchee	870	83	72	10	
Yakima	920	113	92	19	
Totals		2208	1906	226	
Pilot		47	40	5	
Total		2255	1946	231	

Note: The column entitled, "Will deny missing vouchers," means that the claimants would have been denied the bo voucher, because benefits were drawn after their qualification deadline.

Table 3-6

WREB Program Data on Actual and Expected Bonus Amounts
Cumulative Data through 37th Observed Week
Excluding Yakima Pilot Data

JSC	Actual	Expected	Percent Difference	Difference	Sigma	Dif/Sigma	Site
630	592.83	626.57	-5.4	-33.74	332.5	-0.1	Aberdeen
310	585.95	583.91	0.3	2.04	325.0	0.0	Auburn
390	646.02	642.78	0.5	3.24	326.2	0.0	Bellevue
540	563.68	558.46	0.9	5.22	319.5	0.0	Bellingham
550	544.62	545.65	-0.2	-1.03	325.6	-0.0	Bremerton
650	612.96	614.54	-0.3	-1.58	349.1	-0.0	Cowlitz Cnty
380	612.01	582.35	5.1	29.66	336.3	0.1	Everett
620	597.49	577.32	3.5	20.17	337.2	0.1	Lewis Cnty
350	626.05	609.36	2.7	16.69	334.2	0.0	Lynnwood
840	503.29	512.62	-1.8	-9.33	296.9	-0.0	Moses Lake
560	576.95	580.83	-0.7	-3.88	329.6	-0.0	Mt. Vernon
360	612.11	625.46	-2.1	-13.35	321.0	-0.0	North Seattle
610	581.43	576.80	0.8	4.63	321.0	0.0	Olympia
370	561.29	568.98	-1.4	-7.69	311.0	-0.0	Rainier
330	616.38	620.77	-0.7	-4.39	313.2	-0.0	Renton
810	526.20	543.18	-3.1	-16.98	307.4	-0.1	Spokane
940	448.00	451.31	-0.7	-3.31	267.0	-0.0	Sunnyside
950	534.09	546.25	-2.2	-12.16	308.2	-0.0	Tri-Cities
960	458.68	481.64	-4.8	-22.96	274.3	-0.1	Walla Walla
870	489.53	531.25	-7.9	-41.72	281.8	-0.1	Wenatchee
920	485.55	483.28	0.5	2.27	277.4	0.0	Yakima
	567.49	572.88	-0.9	-5.39			Average Values
					318.7	-0.0	Overall Value

Table 3-7

Number of Bonus Payments by Treatment Group, by JSC

Site	JSC	T1	T2	T3	T4	T5	T6	Total bonuses paid	Total dollars paid	Average bonus cost
Aberdeen	630	4	4		3	7	2	20	9,902	495.10
Auburn	310	22	23	19	32	34	26	156	101,626	651.45
Bellevue	390	29	32	26	36	42	32	197	138,992	705.54
Bellingham	540	4	8	10	9	12	10	53	30,708	579.40
Bremerton	550	5	4	6	11	6	6	38	24,292	639.26
Cowlitz Cnty	650	5	3	2	5	6	6	27	18,052	668.59
Everett	380	11	19	18	17	21	19	105	73,234	697.47
Lewis Cnty	620		3	1	3	8	6	21	14,632	696.76
Lynnwood	350	6	17	11	25	24	19	102	68,822	674.73
Moses Lake	840	1	6	2	6	6	8	29	19,244	663.59
Mt. Vernon	560	1	4	8	12	8	12	45	32,022	711.60
North Seattle	360	18	32	20	29	42	39	180	126,178	700.99
Olympia	610	8	7	6	6	17	11	55	35,106	638.29
Rainier	370	22	43	39	50	68	36	258	167,566	649.48
Renton	330	8	21	13	18	24	20	104	73,662	708.29
Spokane	810	17	25	23	33	34	32	164	97,716	595.83
Sunnyside	940	3	6	3	5	6	8	31	19,314	623.03
Tri-Cities	950	6	5	7	4	10	6	38	25,152	661.89
Walla Walla	960	3	3		5	3	4	18	8,476	470.89
Wenatchee	870	8	9	11	8	15	15	66	37,080	561.82
Yakima	920	7	13	10	13	19	11	73	42,046	575.97
Totals		188	287	235	330	412	328	1,780	1,163,822	653.83
Pilot		9	5	2	2	8	10	36	22,222	617.28
Totals		197	292	237	332	420	338	1,816	1,186,044	653.11

the value of bonuses to be paid reduced the predicted enrollment period further to 38 from 39 weeks. In fact, enrollment was stopped after week 37 to assure that the budget would not be exceeded.[15]

3.6.2 Site Monitoring

During the enrollment period, each of the 21 sites was visited at least twice by staff members of the WSESD, the Upjohn Institute, or the U.S. Department of Labor, for a minimum of six on-site visits per JSC. In addition, monitoring of sites was done via weekly reports and periodic telephone communication with each JSC. The main purposes of field monitoring were to detect errors in enrollment and errors in administering the enrollment interview. As noted above, a pattern of underenrollment was detected in some JSCs. Only a few cases of failure to administer enrollment interviews were detected; in all instances these errors were immediately corrected. In a few offices where established procedure had caused interviews to be missed, changes in procedure were developed during on site monitoring to eliminate the problem.

For the purpose of on-site monitoring, a monitoring instrument was devised that permitted consistent information to be collected on all sites. There were specific questions on the procedures for WREB interviewing, general questions as to the attitude of the staff toward the project, and questions allowing the monitor to rate the staff and facilities in terms of WREB procedures. These instruments were used during on-site monitoring, and were reviewed for evidence of problems, they have not been systematically evaluated, but a casual review indicated no persistent deviations from designed procedures. By the end of treatment enrollment, treatment assignment rates in all JSCs converged on the designed proportion.

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[15] An even more conservative model run by USDOL indicated that the enrollment should stop even sooner.

CHAPTER 4

THE WREB EVALUATION DATA BASE

The data base for evaluating the Washington Reemployment Bonus (WREB) experiment was assembled using information from three types of sources: (1) administrative records and research data maintained by the Washington State Employment Security Department (WSESD), (2) records of information specific to the WREB experiment which was provided to and received from treatment-assigned claimants, and (3) responses to the WREB follow-up survey. These sources involved a variety of specific data files or systems. Data from the WSESD administrative records came mainly from a system called Benefits Automated System (BAS), which includes a file called Benefit History and is interfaced with files called TAXIS (Tax Information System), WAGE, and JOBNET (the employment service job-matching data base). Other data came from the Labor Market and Economic Analysis Branch of the WSESD. Much of the information specific to WREB was recorded in BAS at the same time it was provided to claimants; other WREB information on claimants was recorded directly at the central office in the Participant Tracking System (PTS) as forms were sent to or received from treatment-assigned claimants. To conduct the WREB follow-up survey, initial applications for unemployment insurance (UI) benefits from the claimants randomly selected for interview were retrieved from WSESD archives and used to recover information about the previous employer. Responses to the interview were the last source of information for the evaluation.

Other corollary systems provided information for the experiment that was not directly used for the evaluation. To guarantee data integrity, a manual audit system based on office logs of interviews was maintained to ensure that WREB interviews were held with all treatment-assigned claimants (see Section 3.2.1.2). During the design and implementation phases of WREB, the Continuous Wage and Benefit History (CWBH) data were used to select sites and determine enrollment rates.[1] A concise review of all the data sources, the information collected, and the systems used is provided below, following a chronological description of the events that resulted in the data base.

4.1 Chronological Development of the WREB Evaluation Data Base

In August 1987, representatives from WSESD, the U.S. Department of Labor (USDOL), and the W.E. Upjohn Institute for Employment Research (Upjohn) met in Olympia, Washington to discuss the data elements to be included in the weekly extract and update files downloaded from the WSESD mainframe computer to the personal computer-based WREB participant tracking system in the WREB central office in Olympia.

In November 1987, representatives from WSESD, USDOL, and Upjohn attended a seminar in Bethesda, Maryland to learn the Oracle Relational Data Base Management System (RDBMS), which was the software chosen by USDOL to run the PTS.[2] To provide timely monitoring and preliminary evaluation information, identical copies of the PTS were maintained by all three parties.[3] Between November 1987 and February 1988, a personal computer (PC) equipped with a 80386 microprocessor was acquired by each of the three parties, and the PTS was developed as an Oracle application designed

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[1] WSESD has maintained the CWBH data set, which is a 10 percent random sample of UI claimants, for over 10 years.

[2] Representatives from the Pennsylvania Employment Security Department and Mathematica Policy Research, Inc. who were involved in the Pennsylvania Reemployment Bonus (PRB) experiment were also in attendance, since they were to use the same software in the PRB experiment.

[3] All three parties--WSESD, USDOL, and Upjohn--received the exact same information throughout the term of the experiment. WSESD regularly mailed copies of the weekly extract and update files on computer cassette tape to USDOL and Upjohn.

to run on these machines.[4] The PC in the WREB central office in Olympia was equipped with telecommunications equipment, which allowed the PTS to accept weekly downloads from the WSESD mainframe of extract and update records on WREB treatment and control claimants. After the WREB pilot began in February 1988, weekly downloads to the central office continued through January 1990--a date after the benefit year of the last person made a WREB offer had expired. Weekly data base management tasks performed by the WREB central office are documented in Section 3.1 of this report.

Three special data tapes were prepared by the WSESD Information Services department from administrative and research records. One was delivered in April 1989 and the last two were delivered in April 1990. These tapes constituted the main data for the WREB evaluation. They included information on everything from individual benefit payments and payment stops to local labor market data. Information provided on these tapes confirmed the PTS to be a virtually error-free data system.

In June 1989, a pre-test of the WREB follow-up survey written by Upjohn was conducted at the Social and Economic Sciences Research Center (SESRC) at Washington State University in Pullman, Washington. In October 1989, the U.S. Office of Management and Budget gave final approval to the WREB follow-up survey. In November 1989, survey work began, with the last completed interview conducted in May 1990. SESRC released the survey data in August 1990.

4.2 Administrative Records and Research Data

The BAS is the main data base used by claimstakers at WSESD Job Service Centers (JSCs) when processing claims; it is also the principal source of information for

[4] Modifications to the PTS on the PC in the WREB central office in Olympia were occasionally done by the USDOL from Washington, DC during the course of the experiment. This was possible because the PC in the central office had software that allowed remote access.

the WREB evaluation. Through BAS, claimstakers may access the recent quarterly wage earnings history of a claimant on-line while the claimant waits. It is possible for claimstakers to update the quarterly earnings history interactively; that is, claimstakers may add wages for which a claimant provides documentation or delete "stranger" wages which appear erroneously on a claimant's record. As mentioned above, BAS includes the Benefit History file and is interfaced with the TAXIS, WAGE, and JOBNET files.

The Benefit History file includes a record of UI payments and payment stop codes for each claimant. The TAXIS file records the Standard Industrial Classification (SIC) code, employer tax number, and name and address of every firm employing workers covered by UI. The WAGE file accumulates up to 14 quarters of information from employer wage reports on employees' earnings and hours in UI-covered employment. JOBNET is a computerized system of statewide data on unemployed persons seeking jobs and employers looking to hire workers; it includes information on Employment Service (ES) registration, referrals to jobs, job placements, referrals to training, and job vacancies.

Within the WSESD there is a research division called the Labor Market and Economic Analysis (LMEA) Branch. The aim of this group is to provide information to support public and private activities that expand employment and reduce unemployment. LMEA provided monthly data for the WREB project on employment and unemployment by county and industry. LMEA also provided some special monthly data that identified industries believed to be declining in each county. This data was used to examine regional differences in treatment impact. An attempt was also made to use LMEA data in the analysis of dislocated workers.

Every week, extract and update files from BAS were downloaded from the WSESD mainframe computer to the WREB PC-based PTS in the central office. The extract file included information for claimants newly added to the data base in the most recent program week, and new records for previously enrolled claimants for whom a monetary redetermination was done. The update file included new information for each claimant already in the data base. These files contained information on: (1) claimant demographic characteristics such as age, gender, education, and ethnicity; (2) claimant UI eligibility characteristics such as base period earnings, base period hours, the weekly benefit amount (WBA), entitled duration of benefits, the new balance available, the most recent week for which the claimant collected UI benefits, and payment stop information; and (3) claimant WREB characteristics such as treatment or control group assignment, reemployment deadline, and the dollar bonus offer.

Three special data tapes were prepared by the WSESD Information Services department, which included data from the BAS Benefit History file, the TAXIS and WAGE files, the JOBNET system, and LMEA summaries. The first tape relied on the WAGE file and was provided in April 1989. It included information on earnings, hours, and employers in the first quarter of 1985 (8501) through the fourth quarter of 1988 (8804). In April 1990, after all claimants studied in WREB had completed their benefit years, a supplementary tape that provided similar WAGE file information for 8901 through 8904 was delivered.[5] The third tape, which was also delivered in April 1990, provided data from the BAS Benefit History file, the TAXIS file, the JOBNET system, and LMEA data. This tape supplied claimant records organized as a 52-week panel, for net UI payments (used to define spells and measure actual compensation), nonmonetary eligibility status during each week (presence of separation and/or indefinite stops preventing UI payment, used to define the analytic sample), and Employment Service activity (number of job referrals, placements, and referrals to training).[6]

[5] The quarterly data on earnings, hours, and employer provided the basis for evaluating the experiment's impact on earnings, worker attachment to an employer, and worker dislocation.

[6] To check the data used to operate WREB, we compared the weekly data provided in April 1990 with that recorded in the PTS and found the latter to be virtually error-free.

4.3 WREB Specific Records

To perform the automated random assignment process summarized in Section 3.2.1.1, Cobol computer code was added onto the Greenbar Monetary Determination printing algorithm in BAS which resulted in the dollar bonus amount and the reemployment deadline appearing on the Greenbar. This information, along with the treatment number, was passed in the weekly download from BAS to the PTS.

After data had been downloaded, the WREB central office staff used the PTS to administer the experiment and print letters to communicate with treatment assigned claimants. For example, after each weekly data download, the PTS would generate an enrollment letter for each new treatment-assigned claimant included in the extract file--provided there were no indefinite nonmonetary stops on the claim. If information that an issue had been resolved in favor of the claimant was included in the update file, the PTS would automatically generate an enrollment letter for that claimant. A variety of other letters and inquiries (see Appendix A) was generated by the PTS during the course of the experiment. Inquiries returned by claimants provided useful data for the evaluation. An audit trail of every letter and inquiry form sent and received by the WREB central office was maintained by the PTS.

Claimants who found employment by their reemployment deadline were told by JSC staff to mail a Notice of Hire form to the WREB unit in Olympia. This form provided information on the claimant's prior and most recent jobs, which was directly key-entered into the PTS at the WREB central office. Using the PTS, the WREB central office staff would verify that the employment date was before the deadline, that no benefits were drawn following that date, and that the claimant was still eligible for UI benefits. Depending upon this information, the staff would use the PTS to generate an appropriate letter to affirm or rescind the bonus offer, or in some cases to request further information from the claimant.

The Bonus Voucher was another important source of WREB information which was key entered into the PTS. Claimants who found valid reemployment and submitted a Notice of Hire were mailed a Bonus Voucher which they could submit for bonus payment at the end of their four-month reemployment period. At this point in the data process, the PTS contained the history of all forms submitted by the claimant to the WREB unit and all forms which the WREB unit generated for the claimant. Furthermore, because of the weekly downloads from BAS, the PTS contained the most recent UI payment and eligibility information, allowing the WREB staff to verify that the claimant had not drawn UI benefits in the reemployment period.[7] If the employment was valid, the staff would authorize payment of the bonus. Naturally, this action was also recorded in the PTS.

4.4 WREB Follow-up Survey Data

Between November 1989 and May 1990, the Social and Economic Sciences Research Center at Washington State University conducted a follow-up telephone survey on a randomly selected subsample of unemployment insurance claimants studied in the WREB demonstration. The survey was designed to solicit information not available for the evaluation from either the PTS or WSESD records. The survey is the sole source of information on the effects of the bonus offer on union membership, union hiring hall placement, the claimant's contribution to household income, and reasons for nonparticipation. The follow-up survey also provides more precise information about dislocated workers, return to previous employer, intensity of job search, the use of various job search methods, reemployment job stability, and self-employment.

In WREB, 17,578 claimants were tracked as either treatment or control subjects. The criteria for selection into the final sample for analysis were: that the claim must

[7] Before authorizing the mailing of bonus checks, the WREB central office staff also verified the validity of reemployment by contacting employers identified on Bonus Vouchers.

have been monetarily valid, and that there were no indefinite nonmonetary issues on the claim during at least one week in the qualification period. These analytic sample criteria (ASC) were met for 15,534 of the claimants studied; among these, 12,452 were treatments and 3,082 were controls. SESRC attempted to contact 3,851 persons who met the ASC--3,091 treatments and 760 controls. They succeeded in completing interviews with 1,900 claimants (1,518 treatments and 382 controls), and failed to get complete interviews with 1,951 (1,573 treatments and 378 controls).

Appendix E provides a detailed examination of survey response rates. It compares the full WREB sample and the survey sample to survey respondents and nonrespondents on exogenous and endogenous characteristics, and investigates whether the treatment and control groups who responded to the survey differ on observed and unobserved characteristics. This latter review includes a report by call attempt.[8] Since some nonresponse bias is detected in the survey results, we review methods for dealing with the problem and offer an example representing the pattern of nonresponse observed in the WREB Follow-Up Survey. We then give the results of applying nonresponse bias adjustments when using the follow-up survey data to estimate treatment impacts. Finally, we report the results of using the best adjustment methods identified to estimate the impact of the treatment on placement of survey respondents by union hiring halls. The adjustments for survey nonresponse bias do affect parameter estimates so that the direction of the bias can be identified, but they do not improve the reliability of the estimates.

4.5 Structure, Limitations, and Use

The final WREB evaluation data base is structured as a collection of five Statistical Analysis System (SAS) data sets. In each data set, the claimant is the

[8] SESRC policy for the WREB Follow-Up Survey was to make up to seven telephone call attempts to complete an interview.

observation, and the primary key is the claimant identification number (IDN). The IDN is a unique identifier which can be used along with the SAS Merge utility to construct any desired sample for analysis. The five SAS data sets we organized could easily be collected into a single large one, but for manageability and because of some conceptual distinctions we have chosen to keep them separate. The names, contents, and sources of the five SAS data sets in the WREB data base are as follows:

MAIN - All demographic variables and all variables specific to the bonus treatment. The data were gathered during the course of the experiment in the weekly data downloads from BAS to the PTS.

BENEFITS - Variables for the net UI payment and any UI payment stops. There are 52 variables for each concept which record values for each week in the benefit year. Data are from the BAS Benefit History file.

WAGES - Earnings, hours, and SIC of main employer. There are 16 variables for each concept which record values for each quarter of the benefit year, and for the 12 quarters preceding the quarter in which the benefit year starts. Data are from the WAGE and TAXIS files.

LMEA - Total unemployment rate, insured unemployment rate, and total employment in the county in which the Job Service Center at which the claimant filed is located. There are 26 variables for each concept which record values for the months October 1987 through November 1989. This file also includes a binary variable constructed to indicate if a claimant's most recent job was in an industry designated as declining in the county where he/she filed for UI. Data are from LMEA.

SURVEY - Data on variables representing responses to questions in the WREB Follow-Up Survey administered by the SESRC.

While the data are rich in many ways, they are deficient in some. We can thoroughly evaluate treatment impacts on benefit year UI compensation and weeks with some compensation, but we can neither estimate the long-term effects of the bonus offer nor accurately measure the treatment impact on return to work.

If we had information on benefits drawn in previous and subsequent years, we could broaden the analysis to estimate permanent--and not simply transitory--impacts of the bonus offer. We may not reliably measure the duration of unemployment or precisely identify return to work, because we rely on quarterly wage data which provide only reported earnings in covered employment to identify return to work. Questions arise with the present data if a claimant stops drawing benefits and wages are absent for the subsequent period. Does this mean unemployment, or does it mean out of the labor force, employment in a job not covered by UI,[9] or that an employer failed to report covered earnings? On top of this, the follow-up survey, which was intended to fill some important gaps, suffers from a problem of nonresponse bias (see Appendix E).

In spite of these shortcomings, the WREB evaluation data base is comprehensive and fully adequate to perform its intended function, which is to conduct the treatment impact and benefit-cost evaluation of the experiment. The microeconomic data, including their panel aspects along with the aggregate state-level data from LMEA, provide complete information on the behavioral response to the treatment and the economic context of the experiment.

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[9] In 1988, approximately 85 percent of all employment in Washington State was covered by UI. The May, 1989 issue of Employment and Earnings (USDOL, BLS) lists average employment as being 2.154 million for 1988. And the 1989 update of Unemployment Insurance Financial Data, ET Handbook No. 394 (USDOL, ETA) reports total covered employment as being 1.835 million for 1988.

CHAPTER 5

EXPERIMENTAL EFFECTS OF WREB ON UI BENEFITS AND UNEMPLOYMENT

This chapter reviews the effects of the experiment on unemployment insurance (UI) benefits and the duration of insured unemployment for the full sample of claimants eligible to participate. The total assigned sample included 17,554 claimants, of whom 15,534 were determined to be eligible to participate in the bonus offer program. Claimants must have satisfied one of the following criteria for inclusion in the final sample for analysis: (1) the claim must have been monetarily valid at filing and there were no nonmonetary issues on the claim during at least one week in the qualification period, or (2) the claim was monetarily valid at filing and no waiting week was ever claimed. The 2,020 claimants excluded from the analytic sample had indefinite nonmonetary stops on their claim throughout their qualification period. These claimants were omitted because they were not eligible to receive UI compensation and therefore could not reduce compensation in response to a bonus offer. A special category of claimants who did not receive UI compensation was included in the analytic sample; this category was monetarily eligible claimants who filed for benefits but did not claim a waiting week. The bonus eligibility conditions explained in Chapter 2 make it clear that not filing for a waiting week is a legitimate treatment response by a monetarily eligible claimant.

Section 5.1 provides a description of participant characteristics and a discussion of the results of random assignment. The remainder of the chapter discusses experimental impacts estimated on the analytic sample. Section 5.2 presents estimates of the mean effects of the six treatments. Section 5.3 compares mean effects across experimental treatments. The last two sections report on the timing of treatment impacts, and the implications of omitting from the analytic sample claimants who did not file for a waiting week.

5.1 Participant Characteristics and Randomization

Randomization is at the heart of experimentation. In WREB, random assignment was accomplished by using the last two digits of each claimant's Social Security Number (SSN) to make assignments to one of the six treatments or the control group. Chapter 3 documents the extensive efforts made to assure that random assignment occurred. However, even with an error-free assignment process there is no guarantee that the result is homogeneity across the control and six treatment groups.

Table 5-1 shows the mean values across the control and treatment groups of a set of observable exogenous characteristics eventually used as control variables in the impact models. Some of these variables, such as the weekly benefit amount (WBA) and weeks of entitlement are parameters of the UI system, while others describe the socioeconomic characteristics of individual claimants. Statistical tests indicated that, when all characteristics were considered collectively, assignment to the groups was random in terms of these observed characteristics. However, tests on individual characteristics indicated that the mean values of some variables did differ across groups. For instance, the mean value of WBA for treatments 4 and 6 was different from that of the control group at the 90 percent and 95 percent level of statistical confidence, respectively. Base period earnings (BPE) were higher than those in the control group for participants in treatments 5 and 6 at the 90 percent level of statistical confidence. Treatment 5 also showed a higher percentage of claimants in white-collar occupations, while treatment 6 had a smaller proportion of claimants in the other nonwhite racial category.

Although the number of statistically significant differences in measured population characteristics was not more than expected in a random sample, the control and treatment groups were not homogeneous with regard to certain variables that were likely to effect the outcomes, e.g., WBA and BPE. In the next section, we show that sample heterogeneity did affect the results. To reduce experimental error, use is made of both

Table 5-1

Population Characteristics
(t-statistics of treatment group difference from control group means are shown in parentheses)

	Control	Experimental group 1	2	3	4	5	6	F-value across all groups (cv=2.1)
Age	36.33	35.98 (-1.127)	36.14 (-0.637)	36.43 (0.272)	36.50 (0.534)	36.30 (-0.111)	35.96 (-1.075)	0.72
Education	12.30	12.33 (0.341)	12.44* (1.796)	12.34 (0.434)	12.33 (0.338)	12.38 (1.000)	12.44 (1.611)	0.89
Proportion male	0.605	0.611 (0.424)	0.607 (0.165)	0.613 (0.548)	0.615 (0.742)	0.602 (-0.218)	0.620 (0.989)	0.33
Proportion black	0.043	0.052 (1.481)	0.047 (0.652)	0.040 (-0.426)	0.047 (0.668)	0.041 (-0.416)	0.042 (-0.226)	0.85
Proportion other ethnic/races	0.124	0.117 (-0.822)	0.110 (-1.605)	0.116 (-0.843)	0.119 (-0.570)	0.119 (-0.565)	0.097** (-2.689)	1.43
Base period earnings[a]	15,475	15,486 (0.033)	15,860 (1.224)	15,537 (0.173)	15,872 (1.269)	16,073* (1.902)	16,148* (1.877)	1.28
Standard wage deviation	1,388	1,468 (1.280)	1,470 (1.335)	1,382 (-0.090)	1,435 (0.760)	1,461 (1.185)	1,484 (1.366)	0.71
Weekly benefit amount	151.445	152.093 (0.450)	153.636 (1.541)	153.285 (1.146)	154.643** (2.259)	153.680 (1.573)	155.003** (2.194)	1.43
Weeks of entitlement	26.841	26.700 (-1.213)	26.955 (0.994)	26.755 (-0.666)	26.921 (0.704)	26.844 (0.026)	27.003 (1.238)	1.31

[a]The base period is the first four quarters of the five quarters prior to the quarter in which the claim was filed; however, for some claimants, the base period is the last four quarters of the five quarters prior to the quarter in which the claim was filed.

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent level for a two-tail test.

Table 5-1
(Continued)

	Control	1	2	3	4	5	6	F-value across all groups (cv=2.1)
Proportion white collar	0.342	0.333 (-0.701)	0.356 (1.051)	0.348 (0.391)	0.353 (0.834)	0.367* (1.872)	0.356 (0.942)	1.22
Proportion minimum wage benefit amount	0.035	0.035 (-0.039)	0.033 (-0.529)	0.031 (-0.803)	0.032 (-0.642)	0.032 (-0.718)	0.027 (-1.559)	0.52
Proportion maximum wage benefit amount	0.333	0.335 (0.170)	0.341 (0.659)	0.337 (0.324)	0.353 (1.590)	0.346 (0.996)	0.367** (2.306)	1.24
Proportion union or stand-by	0.225	0.217 (-0.661)	0.218 (-0.638)	0.209 (-1.234)	0.223 (-0.176)	0.207 (-1.581)	0.227 (0.144)	0.71
N =	3,083	2,247	2,349	1,584	2,388	2,354	1,536	

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

blocking designs (i.e., creation of more homogeneous population subgroups), and covariance analyses.[1]

5.2 Mean Effects of Experimental Treatments on Compensation and Weeks Compensated for the Total Enrolled Sample

5.2.1 Mean Values of Program Variables Across Treatments

Table 5-2 displays the mean values of several important program outcome variables for the control group, the six experimental treatments, the treatments combined by bonus level, and all treatments combined. The mean values are based on the sample of claimants determined eligible to participate in the experiment. The variables listed are the outcomes which the bonus offer is intended to most directly influence.

The first row in Table 5-2 shows "compensation in the initial spell."[2] The direct effect of the bonus offer is expected to be an increase in the intensity of job search and the probability of accepting early job offers. Thus, the most immediate effect of the experiment should be to reduce the length of the first spell of unemployment and the amount of compensation received during that spell. The second row in the table shows "compensation in the benefit year." The benefit year is a 52-week period beginning the

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[1] See Neter and Wasserman (1974, chap. 22) for a discussion of the use of covariance analysis to reduce experimental error in the case of sample heterogeneity. The authors argue that even with randomized designs, there may be experimental errors due to differences in the composition of the control and experimental groups. Covariance models (introduction of control variables in addition to treatment variables) can be employed to remove the bias in the estimates of the treatment parameters caused by sample heterogeneity.

[2] The end of a spell is a somewhat arbitrary concept. A one-week break in the payments could occur for many reasons, such as receipt of temporary work, illness that made the claimant unavailable for work and ineligible for benefits, or a vacation from job search that led the claimant not to file for benefits in the week. None of these interruptions in the claim should be considered as having ended a spell of unemployment in the context of the bonus experiment. Ending a spell of unemployment in the experiment implies obtaining full-time work. Absent precise information as to why there is a gap in the payment series, we have arbitrarily defined the end of a spell as occurring when the claim break is two weeks or longer. Adding a third week did not materially change the results.

Table 5-2

Means of Program Variables by Experimental Group
(standard errors in parentheses)

	Control Group	Treatment Group 1	2	3	4	5	7	T1,4	T2,5	T3,6	All Ts
Compensation received:											
Initial spell	1525.25 (32.68)	1560.05 (38.53)	1545.58 (36.99)	1475.26 (44.33)	1496.47 (36.23)	1566.43 (37.12)	1419.61 (44.29)	1527.29 (26.41)	1556.02 (26.20)	1447.86 (31.34)	1519.64 (14.37)
Benefit year	2065.71 (33.86)	2095.54 (40.35)	2070.79 (38.51)	1996.84 (46.67)	2007.49 (37.96)	2078.17 (38.44)	1979.41 (46.87)	2050.18 (27.66)	2074.48 (27.20)	1988.26 (33.07)	2048.18 (15.00)
Weeks of insured unemployment:											
Initial spell	11.37 (0.195)	11.47 (0.227)	11.37 (0.218)	11.10 (0.268)	11.10 (0.218)	11.60 (0.224)	10.55 (0.267)	11.28 (0.157)	11.49 (0.156)	10.83 (0.189)	11.27 (0.086)
Benefit year	15.22 (0.196)	15.16 (0.231)	15.04 (0.221)	14.60 (0.273)	14.71 (0.225)	15.08 (0.225)	14.49 (0.278)	14.93 (0.161)	15.06 (0.158)	14.55 (0.195)	14.95 (0.087)
Proportion of claimants who:											
Exhausted benefits	0.239 (0.0077)	0.251 (0.0091)	0.236 (0.0088)	0.227 (0.0105)	0.210 (0.0083)	0.226 (0.0086)	0.216 (0.0105)	0.230 (0.0062)	0.231 (0.0061)	0.222 (0.0074)	0.230 (0.0034)
Terminated benefits before deadline*		0.421 (0.0104)	0.427 (0.0102)	0.443 (0.0125)	0.552 (0.0102)	0.542 (0.0103)	0.580 (0.0126)	0.488 (0.0073)	0.484 (0.0073)	0.511 (0.0090)	0.503 (0.0040)
N =	3,082	2,246	2,348	1,583	2,387	2,353	1,535	4,633	4,701	3,118	12,452

*Includes all claimants who terminated benefits prior to the deadline and show wages subsequent to termination of benefits. Also includes claimants who served no waiting week and have wages in the quarter after filing.

week the claimant files for benefits (except in cases of claim backdating). It is the period for which a claimant has established entitlement to UI benefits. Benefit year outcomes are examined to assess longer-term effects of the bonus offer.

The third and fourth rows in the table display the mean weeks of insured unemployment in the first spell and the benefit year respectively.[3] The fifth row provides estimates of the impact of the treatments on the probability of exhausting benefits. Treatment impacts on benefit exhaustion are presented in this chapter, but exhaustion is not one of the principal outcomes examined in this report.

5.2.2 Differences in Mean Values of Program Parameters Across Treatments

Treatment effects in a classically designed random assignment experiment can be estimated by simply computing the difference between treatment and control groups in the mean value of an outcome variable. These impacts can also be estimated using ordinary least squares (OLS) regression of an outcome variable on dummy variables representing treatments. For the (WREB) experiment, the impact regression model is:

$$Y = a + B'T + e, \qquad (1)$$

where T is 6 x n matrix of dummy variables and B is a conformable vector of coefficients. The intercept, a, is the mean value of the outcome variable, Y, for the control group.

[3] It is important to remember that rows three and four report weeks of insured unemployment, which is a variable truncated by the exhaustion of entitlement. It is therefore only a part of total unemployment. Also note that weeks of insured unemployment is a count of the number of weeks in which some compensation is paid, or a waiting week is earned. For clarity, throughout the report this concept is referred to as "weeks with some compensation." Because of partial benefit payments, the count of weeks with some compensation may exceed the entitled duration of benefits.

Table 5-3 presents the parameter estimates and standard errors for the mean difference in total UI compensation and weeks of insured unemployment in the benefit year for each of the six treatments. These mean differences are estimates of the treatment impacts.

Statistical tests on the treatment impact estimates on UI compensation and weeks with some compensation indicated that in each case the six treatment impact estimates were simultaneously not different from zero at the 90% level of statistical confidence.[4] Furthermore, tests revealed none of the treatment impacts estimated in the equation explaining UI compensation were individually significant at the 90 percent level of confidence.[5] These results suggest that the experiment had no effect on the receipt of UI compensation over a benefit year. However, three of the treatments--T3, T4, and T6--had statistically effective impacts on weeks of insured unemployment.

Since several of the treatment coefficients in the compensation equation had the expected negative sign and were not small, it was important to determine if the lack of statistical significance was a small sample problem. Although we cannot ex post increase the sample size, we can determine if the small samples led to differences in the composition of the control and experimental groups. If differences in characteristics of members of the two groups affect the response variables, then the estimates of the treatment effect may be biased. As noted previously, such heterogeneity can be corrected by use of a covariance model with control variables.

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[4] To test hypotheses about two or more impact estimates simultaneously, an F-test is used; to test hypotheses about individual impact estimates, we use t-tests.

[5] Throughout this report, we test the statistical significance of impact estimates using 90 percent and 95 percent confidence levels in a two-tail test. Although using a one-tail test may seem reasonable, because the treatment is intended to have the effect of decreasing UI compensation and length of insured unemployment it is possible that the treatment could increase compensation. This could occur through an income effect. The increased income represented by the bonus could cause some claimants to wait longer--within the qualification period--to obtain a job. This is likely to occur only if the claimants are assured that they can obtain a job when they wish. This is not a likely scenario, but its feasibility makes it necessary to use a two-tail instead of a one-tail test of significance.

Table 5-3

Mean Experimental and Control Differences in Compensation and Weeks Compensated in Benefit Year

	Compensation in Benefit Year		Weeks of Insured Unemployment	
Variable	Parameter Estimate	Standard Error	Parameter Estimate	Standard Error
Intercept	2065.71	33.68	15.22	0.196
T1	29.83	51.87	-0.06	0.302
T2	5.08	51.21	-0.17	0.299
T3	-68.87	57.81	-0.61*	0.337
T4	-58.21	50.98	-0.50*	0.297
T5	12.46	51.18	-0.13	0.298
T6	-86.30	58.41	-0.73**	0.340
N= 15,534	F = 1.177		F = 1.449	

* Significant at the 90 percent confidence level for a two-tail test.
** Significant at the 95 percent confidence level for a two-tail test.

Table 5-4 displays treatment effect estimates from a model having the following general formulation:

$$(2) \qquad Y = a + B'T + C'Z^* + u,$$

which is the covariance model version of equation (1). The introduction of control variables, designated Z^*, into the model reduces experimental error that results from differences in the observable characteristics of the control and treatment groups (Netter and Wasserman 1974). The control variables attempt to correct nonrandomness by adjusting for differences across the groups in observable characteristics, thereby also improving the precision of impact estimates. More specifically, the model is:

$$(3) \qquad Y = a + B'T + C'(Z - \underline{Z}) + u,$$

where C is a parameter vector, and $\underline{Z}$ is a matrix of mean values of control variables. The set of control variables used throughout this report is described in Appendix F.[6]

The model shown in equation (3) differs from equation (2) in that all control variables are defined as individual differences from the mean. The advantage of this formulation is that the intercept can be interpreted as the mean value of the dependent variable for a sample member who is not exposed to the treatment and has the characteristics of the mean individual in the overall sample.[7]

The effect of using control variables in the regression to estimate treatment impacts on UI compensation is striking. The impact estimates are larger, and the standard errors smaller. Three of the treatments--T3, T4, and T6--show statistically significant impacts on compensation at the 90 percent level of confidence or better. Moreover, the average treatment effect in the benefit year is significant when estimated using the covariance model. The six treatment impact estimates are jointly significant at the 95 percent confidence level. The increased accuracy achieved in measuring the bonus impacts on UI compensation is the result of the control variables eliminating, or

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[6] Although quarter of filing was not used as a control variable, there were differences in effect across quarters, as discussed in Appendix G.

[7] For instance, the intercept in the regression for the treatment impact on UI compensation in the benefit year reported in Table 5-4 is $2100.44. This is the UI compensation received during the benefit year for a hypothetical person in the sample who was not exposed to the experimental treatment, and whose age, level of education, WBA, probability of being exempt from work search, etc., was at the mean value for each of the control variables across the total sample (control and experimental groups combined). The treatment effect is the impact of the treatment on compensation for that hypothetical individual.

Table 5-4

Differences Between Experimental and Control Group Means Controlling for Population and Program Characteristics (standard errors in parentheses)

	All Claimants						
	Treatment group difference from control						
	1	2	3	4	5	6	T1,4
Compensation received:							
Initial spell	24.84	-23.61	-87.86*	-78.23*	-16.22	-151.27**	-28.26
	(44.35)	(43.79)	(49.42)	(43.58)	(43.76)	(49.95)	(37.16)
Benefit year	18.66	-40.70	-106.92**	-117.15**	-39.79	-140.53**	-51.32
	(45.74)	(45.16)	(50.98)	(44.95)	(45.14)	(51.52)	(38.33)
Weeks of insured unemployment:							
Initial spell	0.09	-0.14	-0.40	-0.41	0.02	-0.89**	-0.16
	(0.281)	(0.278)	(0.314)	(0.277)	(0.278)	(0.317)	(0.236)
Benefit year	-0.04	-0.27	-0.70**	-0.62**	-0.26	-0.75**	-0.34
	(0.293)	(0.289)	(0.326)	(0.287)	(0.289)	(0.329)	(0.245)
Proportion of claimants who:							
Exhausted benefits	0.008	-0.005	-0.017	-0.032**	-0.019*	-0.021*	-0.012
	(0.011)	(0.011)	(0.013)	(0.011)	(0.011)	(0.013)	(0.010)
Sample size	2,246	2,348	1,583	2,387	2,353	1,535	4,633

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

significantly reducing, the differences in UI compensation between control and treatment groups that result solely from differences in the composition of the control and treatment groups.[8]

The first set of regressions in Table 5-4 report the impact on compensation paid in the first spell of unemployment. The first spell is defined as a period starting with the Sunday before the date of filing for benefits for those unemployed at filing, or starting with the Sunday of the waiting week, for those who only become unemployed subsequent to filing. The spell ends when benefits are exhausted or benefit payments stop for two weeks or more. Treatments 3, 4 and 6 displayed statistically significant impacts on compensation in the first spell.

The effects of the experiment are stronger over the full benefit year than during the first spell of insured unemployment. Except for T6, the parameter estimates of impact are larger, and the relative standard errors smaller in the benefit year equation. These results are clearer in the three treatment groups which differ only in bonus level, and in the equation combining all treatments into a single overall average treatment.

In the Illinois experiment, the effects in the benefit year were smaller than in the first spell. This would occur if participants who become employed sooner are obtaining less satisfactory jobs, and are shifting some of the unemployment to a later period in the benefit year. The results for WREB suggest the opposite, i.e., that the jobs being obtained as a result of the more rapid reemployment caused by the WREB bonus offer represent fully satisfactory job matches, and are not leading to increased job turnover later in the benefit year.

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[8] The most important variable in the control set is the weekly benefit amount (WBA). Statistical tests revealed that if this variable was ignored, then measurements of treatment impacts would be biased. For consistency, all impact estimates presented throughout this report were computed using WBA and 12 other variables as controls. These are discussed in Appendix F.

The short and long qualification periods are combined to form three treatment groups that differ in the level of the bonus offer but have the same average qualification period. Ignoring differences in the effects of the length of the qualification period makes clear the absence of any statistically significant effect of the bonus amount at the middle and lower bonus levels. The relationship among the bonus levels is obscured when looking at each of the six treatments separately because of the large, anomalous, and almost significant coefficient on T4 (low bonus, high qualification period).

Table 5-5 shows a set of results similar to those in Table 5-4, except that the sample has been changed to eliminate claimants who were exempt from job search due to union affiliation or job attachment. Excluded are members of full-referral unions (Alpha Work Search (AWS) code U) and claimants on standby, awaiting recall by their prior employer (AWS code S). Although these claimants could qualify for the bonus, each needed to do so by taking a job that did not entail placement through a union hiring hall or recall to a previous job.

The results changed only slightly when these restrictions on the sample were imposed. The impact for T3 was weaker, and that for T6 was stronger. Generally stronger effects were expected for the reduced sample because the reemployment opportunities of the omitted groups, those on standby and members of full-referral unions, would be less subject to change by a bonus offer. A UI claimant expecting to be recalled could receive a bonus only by obtaining an interim job that was different, or by moving on to another job. The claimant who belonged to a full-referral union could receive a bonus only if the first job was not obtained through the union hiring hall.

Table 5-5

Differences Between Experimental and Control Group Means
Controlling for Population and Program Characteristics
(standard errors in parentheses)

All claimants except those exempt from work-search

Treatment group difference from control

	1	2	3	4	5	6	T1,4	T
Compensation received:								
Initial spell	37.25	-4.05	-59.55	-71.86	-6.93	-155.83**	-18.77	-5.
	(51.85)	(51.20)	(57.57)	(51.06)	(50.97)	(58.61)	(43.51)	(43.
Benefit year	30.20	-23.88	-98.10*	-116.02**	-43.00	-162.51**	-44.88	-33.
	(51.77)	(51.12)	(57.47)	(50.97)	(50.89)	(58.51)	(43.45)	(43.
Weeks of insured unemployment:								
Initial spell	0.22	0.02	-0.26	-0.40	0.08	-0.90**	-0.10	0.
	(0.335)	(0.331)	(0.372)	(0.330)	(0.329)	(0.379)	(0.281)	(0.
Benefit year	0.08	-0.12	-0.65*	-0.55*	0.21	-0.79**	-0.25	-0.
	(0.340)	(0.336)	(0.378)	(0.335)	(0.335)	(0.385)	(0.286)	(0.
Proportion of claimants who:								
Exhausted benefits	0.013	0.001	-0.014	-0.031**	-0.020	-0.022	-0.010	-0.
	(0.0136)	(0.0135)	(0.0151)	(0.0134)	(0.0134)	(0.0154)	(0.0114)	(0.
Sample size	1,758	1,837	1,252	1,855	1,866	1,187	3,613	3,7

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

Returning to Table 5-4, the results for "weeks of insured compensation" parallel those for "compensation received."[9] For the full sample, the strongest effects are for the large bonuses. For the first spell, only T6 shows statistically significant effects, whereas for the benefit year, T3, T4, and T6 are statistically significant. The effects are larger for the benefit year than for the first spell. Excluding those exempt from work search only slightly increased the estimated experimental effects on the number of weeks compensated.

All of these results suggest that (except for the large and anomalous impacts shown for T4) only a bonus offer as high as six times the WBA had an effect on job acquisition behavior. The results do not show a neat linear progression of impacts. The middle level bonus treatments had surprisingly small and statistically insignificant effects.

5.3 Comparison of Experimental Effects Among Treatments

A central purpose of the multi-treatment WREB experiment is to enable policy makers to select from among alternative treatments the one treatment which works best. The basic treatment-control comparisons provided in the previous section reveal the high WBA multiple treatments (T3 and T6) to have the biggest impacts. The best treatment among those considered is the one that yields the largest net benefits or has the largest benefit-cost ratio. The benefit-cost analysis is presented in Chapter 9. In this section, we investigate the effects of the bonus offer by isolating the impacts of differences in the bonus amount and differences in the length of the qualification period.

In the WREB experiment, we studied the effects of three different bonus amounts (low, medium, and high) and two different qualification periods (short and long). In

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[9] Although the weeks compensated results are similar to the results for compensation, they are not precise arithmetic equivalents. One might expect that, arithmetically, the compensation results equal the change in weeks compensated times the WBA. For two reasons this is not the case. First, the impacts may be positively or negatively correlated with the WBA. Second, the weeks compensated include weeks of partial payment.

other words, we examined two changes in the bonus amount parameter and one change in the qualification period parameter. Following Corson and his colleagues (1991), who report on the Pennsylvania Reemployment Bonus demonstration, the impact on an outcome measure of varying the bonus amount is termed a price effect, and the impact of changing the qualification period is called a duration effect.

Estimates of the price and duration effects are presented in Table 5-6 While the price effect of increasing the bonus from the low to the medium WBA multiple is positive but not significant, the price effect for increasing the WBA multiple from the medium to the high level is negative and strongly significant. That is, when the WBA multiple is increased by a factor of 1.5 (from four times the WBA to six times the WBA) from the medium bonus level, UI compensation decreased an average of $83 and weeks with compensation decreased by one-half week. Results also suggest that the duration effect on benefit year compensation is negative and significant, with the change from short to long qualification period resulting in an average reduction in compensation of about $58.

The estimates given in Table 5-6 were computed under the restriction that the price effects are the same for the short and long qualification periods, and that the duration effect is the same for the three bonus levels. Note that the price effect of moving from the low to the medium bonus amount is positive, whereas we would expect it to be negative. This inconsistency is due to the anomalous effect of treatment 4. Indeed, in the absence of T4, the price effects would be negative and the duration effect would be indistinguishable from zero.[10] Without the outlier impact estimate for T4, it would also be impossible to reject the hypothesis that the low to medium and medium to high price effects are the same.

[10] Mortenson's (1988) search theory model of the reemployment bonus predicted a positive duration effect, i.e. a shorter qualification period should result in a shorter duration of insured unemployment.

Table 5-6

Effects of Increases in WREB Parameters
(standard errors in parentheses)

Parameter Adjustment	Benefit Year UI Compensation	Benefit Year Weeks of Insured Unemployment
Price Effects		
Low to Medium Bonus Amount	11.08	0.08
	(34.14)	(0.22)
Medium to High Bonus Amount	-83.22**	-0.47*
	(38.08)	(0.24)
Duration Effect		
Short to Long Qualification Period	-57.75*	-0.22
	(29.56)	(0.19)

* Difference significant at the 90 percent level for a two-tail test.
** Difference significant at the 95 percent level for a two-tail test.

Another useful way to compare the inter-treatment effects of the reemployment bonus parameters is to estimate the marginal effect of the dollar bonus amount and weeks in the qualification period using linear regression on the bonus parameters in continuous form. In the experiment, the bonus levels are defined as multiples of the WBA. In what is called the continuous variable model, treatments are defined as being a bonus offer of a given dollar amount with a qualification period having a certain number of weeks. The basic model can be stated as:

$$Y = a + b_1 B + b_2 Q + C'Z^* + e, \tag{4}$$

where the treatment is represented by two continuous variables: B the bonus amount in dollars and Q the qualification period length in weeks. Z^* represents the set of control variables discussed in Appendix F, each defined as the difference from its mean value, so that $Z^* = Z - \underline{Z}$.[11]

Table 5-7 shows results of estimating the linear continuous variable treatment model on the analytic sample of 15,534 treatment and control claimants. Several nonlinear specifications of the continuous variable model were estimated, but we were unable to improve on fit of the linear model.[12] In the equation for UI compensation, while the individual coefficients are not statistically significant, they are jointly significant

Table 5-7

Estimated Impact of Variation in the Bonus Amount and Qualification Period in the Continuous Model (standard errors in parentheses)

Treatment Variable	Benefit Year UI Compensation	Benefit Year Weeks of Insured Unemployment
Bonus Amount	-65.13	-0.46
[Thousands of Dollars]	(48.23)	(0.31)
Qualification Period	-5.48	-0.02
[Weeks]	(3.80)	(0.02)

Note: The set of control variables discussed in Appendix F was included in the estimation. In both the UI compensation and the weeks-with-compensation equation, the parameters estimated on bonus amount and qualification period are jointly significant at the 95 percent confidence level. N = 15,534.

[11] Just as in the dummy variable treatment impact model, control variables are included to correct any sample heterogeneity in observed variables that resulted during the assignment process. By including the WBA in the vector of control variables, Z, we improve within-treatment homogeneity in the bonus amount, B. The impact of variations in the bonus amount, b_1, is therefore estimated using mainly the between-treatment variation in the bonus offer. Similarly, inclusion of the entitled duration of benefits as a control variable improves the exogeniety of the treatment parameter Q.

[12] The following specifications were attempted: log-linear, log-log, quadratic in the bonus amount, and quadratic in the qualification period.

at the 95 percent confidence level. In the weeks-with-compensation equation, the pattern is similar.[13]

The results indicate that a $1,000 increase in the bonus offer reduces UI compensation over the benefit year by $65 and weeks with compensation by just under one-half week; each one-week increase in the qualification period reduces compensation by about $5 and has virtually no effect on weeks of insured unemployment. Under the assumption of linear response, these results may be viewed as alternative estimates of price and duration effects. Again it is the case that the effect of increasing the bonus amount is a reduction in compensation and weeks, while the effect of lengthening the qualification period is very small.

The linear continuous variable model permits us to construct a map displaying the effects of the bonus offer and qualification period within the observed range. Figure 5.1 shows such a map for effects on UI compensation over the benefit year. Each diagonal line represents a different length qualification period. To estimate the "Change in Benefit Year UI Compensation" using the map, read down from the horizontal line that represents the "Dollar Bonus Amount" and across to the vertical axis. For example, results from the continuous model imply that a bonus amount of $600 and a qualification period of 11 weeks would generate a reduction in compensation averaging about $100 per claimant over the benefit year. As evident in Figure 5.1, the continuous model yields considerably greater reductions in compensation for smaller bonuses than were actually observed in the experimental treatments.

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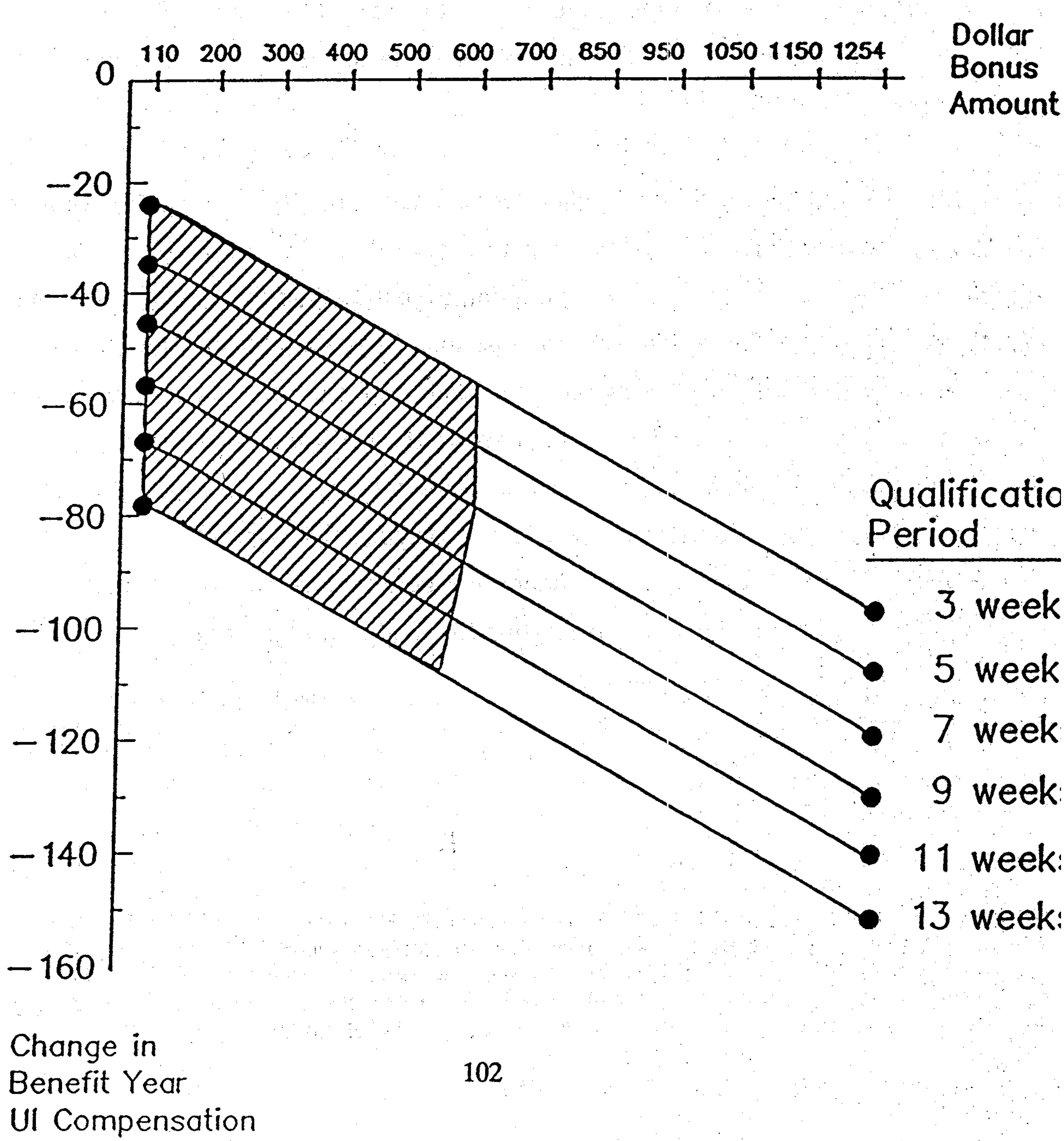
[13] To further investigate the structure of treatment impacts, the continuous variable model was estimated on the sample of treatment-assigned claimants. The t-statistics on the coefficients for bonus amount and qualification period were much higher than those for parameter estimates reported in Table 5-7. This suggests that a significant discontinuity from no bonus offer (control) to positive bonus offer (treatment) prevents the linear regression response surface from having a good fit for the continuous variable model.

Figure 5.1

Linearized Effect of the Bonus Offer on Benefit Year UI Compensation

$$\begin{bmatrix}\text{Benefit Year} \\ \text{UI} \\ \text{Compensation}\end{bmatrix} = 2015.09 - 0.065 * \begin{bmatrix}\text{Dollar} \\ \text{Bonus} \\ \text{Amount}\end{bmatrix} - 5.48 * \begin{bmatrix}\text{Weeks in t} \\ \text{Qualificati} \\ \text{Period}\end{bmatrix}$$



5.4 The Timing of Treatment Impacts on Insured Unemployment

Up to this point we have presented the effects of reemployment bonus offers only as average impacts on insured unemployment for the various treatment groups. In this section, we study the time pattern of impacts using the methods of economic duration analysis.[14] The fundamental duration concept reported on here is called the <u>conditional UI exit rate</u>. For the group of claimants drawing compensation at the end of one period, the conditional UI exit rate is the proportion of claimants not drawing at the end of the next period.[15]

Conditional UI exit rates--called UI exit rates here for short--are examined only for the first spell of covered unemployment.[16] If the bonus offer is effective, treatment assigned claimants should have higher exit rates than controls. The maximum entitled duration of benefits in Washington is 30 weeks. The short qualification period (T1, T2, and T3) is twenty percent of entitled duration plus one week, while the long qualification period (T4, T5, and T6) is forty percent of entitled duration plus one week. Therefore for treatment assigned claimants attempting to qualify for a bonus, seven weeks is the longest period of unemployment allowed for a claimant given a short qualification period, and thirteen weeks is the longest period of unemployment allowed for a claimant

[14] The methods of economic duration analysis are summarized by Kiefer (1988), they have been used to analyze UI bonus experiments by Decker (1990), Meyer (1988), and Woodbury and Davidson (1990).

[15] While we would prefer to analyze the timing of return to work, no reliable data is available on that behavior. The phrase "drawing UI compensation," means the claimant has opened a new claim and has a status where he/she could file for benefits or a waiting week with a continuing claim form, i.e., the claimant would not have to re-open a claim to draw a benefit. The conditional UI exit rate is an application of a concept called a hazard rate in the literature on duration analysis.

[16] The number of claimants eligible for WREB at filing is called the initial risk set. The initial risk set includes all claimants who served a waiting week within 30 weeks of opening a claim for benefits. It also includes persons who opened a claim but never filed for a waiting week or for compensation; these persons were presumed to exit covered unemployment in the first week after opening a claim. The initial risk set contained 15,478 claimants, of whom 12,413 were treatment assigned and 3,065 were in the control group. To say that only first spells are analyzed means that once a claimant leaves UI--defined as a two week gap in the payment series--he/she is not added back to the risk set if they return to UI.

given a long qualification period. It is expected that exit rates for claimants given a short qualification period should exceed that for controls by the greatest margin in weeks 1 to 7 after filing, while exit rates for claimants given a long qualification period should exceed that for controls by the greatest margin in weeks 1 to 13. For claimants with a long qualification period, a priori we cannot say whether the bulk of the response should be in the early or later part of the qualification period.

UI exit rates for the control group and differentials for treatment groups are reported in Table 5-8.[17] For the short qualification period offers--T1, T2, and T3--the UI exit rate is higher than controls in weeks 1-7, while the pattern is reversed in weeks 8-13. This timing in treatment response is sharpest for T3 which shows a large and statistically significant impact of 3 percent over weeks 1-7 and a sharp reversal to -1.7 percent over weeks 8-13. For the long qualification period offers--T4, T5, and T6--the response is generally stronger over weeks 8-13 than weeks 1-7. The impact for T6 is significant over both periods, but it is larger in the second. The increase in response between the two periods is most pronounced for T5. The response in weeks 14-31 is bigger for the short qualification period offers than for the long offers, so that there appears to be a balancing out of effects over time.[18]

Within the first 31 weeks of the benefit year we have seen that UI exit rates are generally highest for the short qualification period treatments in weeks 1-7 and for the long qualification period treatments in weeks 8-13. To examine the permanence of the

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[17] The treatment impacts on UI exit rates were estimated by ordinary least squares in linear probability models, which included the set of control variables discussed in Appendix F. The figures in Table 5-8 are therefore referred to as adjusted UI exit rates for the control group, and adjusted differences from the control group in UI exit rates for the treatment groups.

[18] We examine UI exit rates through week 31 since, for persons who do not draw partial benefits, that is the maximum period of insured unemployment. It includes 30 compensable weeks plus the waiting week. The cumulative UI exit rate for controls at 31 weeks is less than 100 percent because claimants who draw partial benefits may actually receive a payment for more than 31 weeks.

Table 5-8

Estimated Treatment Impacts on Conditional UI Exit Rates
(standard errors in parentheses)

Week	Control Group Exit Rate	Treatment Impacts on Exit Rate						
		T1	T2	T3	T4	T5	T6	All Ts
1-7	48.8	-0.3 (1.3)	1.2 (1.3)	3.0** (1.5)	1.2 (1.3)	-1.2 (1.3)	3.6** (1.5)	1.0 (1.0)
8-13	28.9	-0.3 (1.8)	-2.2 (1.7)	-1.7 (2.0)	2.8* (1.7)	3.1* (1.7)	4.6** (2.0)	1.0 (1.3)
14-31	88.0	0.3 (1.4)	3.1** (1.4)	2.2 (1.6)	2.2 (1.4)	1.1 (1.4)	0.7 (1.7)	1.6 (1.0)
Initial Sample	3,065	2,239	2,343	1,577	2,380	2,344	1,530	12,413

* Impact significant at the 90 percent level for a two-tail test.
** Impact significant at the 95 percent level for a two-tail test.

treatment effects on UI exit rates, Table 5-9 presents estimates of cumulative UI exit rates for control group members and differentials for treatment assigned claimants for three points in time after the week of filing for benefits: week 7, week 13, and week 31. The top row of Table 5-9 is identical to the top row in Table 5-8; it is included to facilitate the examination of cumulative impacts. The second row in Table 5-9 lists cumulative treatment impacts on UI exit rates through week 13. The cumulative treatment impact on the UI exit rate up to week 13 is generally diminished relative to week 7 for the short qualification period treatments, while it is increased for the long qualification period treatments. The bottom row of Table 5-9 indicates the permanence of the treatment impact. For all treatments, the cumulative UI exit rate over the first 31

Table 5-9

Estimated Treatment Impacts on Cumulative UI Exit Rates
(standard errors in parentheses)

Week	Control Group Exit Rate	Treatment Impacts on Exit Rate						
		T1	T2	T3	T4	T5	T6	All Ts
7	48.8	-0.3	1.2	3.0**	1.2	-1.2	3.6**	1.0
		(1.3)	(1.3)	(1.5)	(1.3)	(1.3)	(1.5)	(1.0)
13	63.6	-0.3	-0.3	1.4	2.3*	0.8	4.7**	1.2
		(1.3)	(1.3)	(1.4)	(1.3)	(1.3)	(1.4)	(0.9)
31	95.6	0.1	1.1**	1.1*	1.0**	0.4	0.9	0.7**
		(0.5)	(0.5)	(0.6)	(0.5)	(0.5)	(0.8)	(0.4)

* Impact significant at the 90 percent level for a two-tail test.
** Impact significant at the 95 percent level for a two-tail test.

weeks of the benefit year is greater than that for the control group, with the increase averaging a statistically significant 0.7 weeks.

Since the entitled duration of benefits in Washington during the experiment ranged from 10 to 30 weeks during the WREB experiment, the timing of response by treatment may have been muddied somewhat. That is, a claimant assigned to T4, T5, or T6 who was initially entitled to 15 weeks of benefits would have a qualification period of 7 weeks, and would therefore be expected to leave UI by week 7 rather than in weeks 8-13. To investigate if the timing of treatment impacts on the UI exit rate would be sharper in the absence of the variable entitled duration, UI exit rates are examined for the subsample of claimants at the maximum entitled duration of benefits--30 weeks. Just over 50 percent of the WREB sample had a UI entitlement of 30 weeks. Because of the formula for determining UI entitlement, it can be inferred that this group includes claimants who tend to be permanent members of the labor force and usually work full time. The conditional and cumulative UI exit rates for this group are summarized in Tables 5-10 and 5-11 respectively.

Table 5-10

Estimated Treatment Impacts on Conditional UI Exit Rates
for Claimants with 30 Weeks of Benefit Entitlement
(standard errors in parentheses)

Week	Control Group Exit Rate	Treatment Impacts on Exit Rate						
		T1	T2	T3	T4	T5	T6	All Ts
1-7	49.3	-2.1	1.0	4.5**	1.0	-2.3	3.8*	0.4
		(1.8)	(1.8)	(2.1)	(1.8)	(1.8)	(2.0)	(1.3)
8-13	25.0	-0.3	0.6	0.2	3.7	3.7	6.1**	2.1
		(2.4)	(2.4)	(2.8)	(2.4)	(2.3)	(2.7)	(1.7)
14-31	81.3	2.2	4.6**	3.1	4.7**	2.1	3.3	3.4**
		(2.3)	(2.3)	(2.7)	(2.3)	(2.3)	(2.7)	(1.7)
Initial Sample	1,572	1,117	1,198	785	1,217	1,197	805	6,319

* Impact significant at the 90 percent level for a two-tail test.
** Impact significant at the 95 percent level for a two-tail test.

Table 5-11

Estimated Treatment Impacts on Cumulative UI Exit Rates
for Claimants with 30 Weeks of Benefit Entitlement
(standard errors in parentheses)

Week	Control Group Exit Rate	Treatment Impacts on Exit Rate T1	T2	T3	T4	T5	T6	All Ts
7	49.3	-2.1 (1.8)	1.0 (1.8)	4.5** (2.1)	1.0 (1.8)	-2.3 (1.8)	3.8* (2.0)	0.4 (1.3)
13	61.9	-1.5 (1.8)	0.3 (1.8)	3.5* (2.0)	2.5 (1.7)	0.1 (1.8)	5.8** (2.0)	1.4 (1.3)
31	92.9	0.6 (0.9)	1.8** (0.9)	1.6 (1.0)	2.1** (0.9)	0.8 (0.9)	2.1** (1.0)	1.5** (0.7)

* Impact significant at the 90 percent level for a two-tail test.
** Impact significant at the 95 percent level for a two-tail test.

Limiting the sample to claimants with 30 weeks entitlement sharpens response most for the high bonus offers. Increased impacts on the UI exit rate are estimated in weeks 1-7 for T3, and weeks 8-13 for T6. For the other long qualification period offers, T4 and T5, restricting the sample to claimants with 30 weeks entitlement also affects UI exit rates in the expected way; impacts get bigger over the 8-13 week period, and smaller over the 1-7 week period. However, even for the sample of claimants with 30 weeks of entitlement, estimates for T1 and T2 remain small and insignificant over weeks 1-13.

Overall, treatment impact estimates on the UI exit rate for this subsample are larger than for the full WREB sample, with the average cumulative impact nearly double at 1.5 percent. Part of the average cumulative impact is due to the response in weeks 14-31, which may be due to a delayed effect of treatment induced work search during the qualification period. In any event, the treatment impact on the UI exit rate over weeks 1-31 is consistent with the finding that the bonus offer reduced the benefit exhaustion rate.

The conditional UI exit rate estimates support and strengthen the overall findings. The high bonus offers elicited strong responses during the periods in which they were operative; i.e., weeks 1-7 for T3, and weeks 1-13 for T6. By the time the maximum entitled duration of benefits in Washington elapsed, 0.7 percent more treatment assigned claimants than control claimants had left UI.

5.5 The Effect of Excluding Claimants Who Did Not Claim a Waiting Week

As mentioned earlier in this chapter, all treatment-assigned claimants who did not claim a waiting week were eligible for a bonus, even if there was an issue on their claim. This was done to eliminate the incentive to lengthen insured unemployment, which would have been present if WREB required that a waiting week be served to establish bonus eligibility.

Before proceeding, it should first be mentioned that because duration of insured unemployment is a principal outcome variable of interest, it is generally not appropriate to partition the sample on this variable before estimating treatment impacts. However, since the other UI reemployment bonus experiments conducted in Illinois, New Jersey, and Pennsylvania limited bonus eligibility to claimants who served a waiting week, estimates of WREB impacts among claimants who served a waiting week are given in this section to provide for comparison. The effects of the sample selection are discussed below.

Table 5-12 presents a comparison of treatment impacts on UI compensation and weeks of insured unemployment by treatment group for two samples--one with and the other without 1,233 claimants studied in WREB who did not receive waiting week

Table 5-12

Comparison of Experimental Effects on Samples
With and Without Those Not Serving a Waiting Week
(standard errors in parentheses)

	Treatment group difference from control									
	1	2	3	4	5	6	T1,4	T2,5	T3,6	All T's
Compensation received:										
Total sample	18.66 (45.74)	-40.70 (45.16)	-106.92** (50.98)	-117.15** (44.95)	-39.79 (45.14)	-140.53** (51.52)	-51.32 (38.33)	-40.23 (38.22)	-123.45** (41.89)	-65.18** (33.18)
N = 15,534										
Excluding No Waiting Week	7.51 (46.80)	-82.07* (45.99)	-133.94** (51.98)	-126.39** (46.05)	-67.16 (46.03)	-156.85** (52.69)	-61.34 (39.22)	-74.62* (38.99)	-145.16** (42.79)	-87.37** (33.91)
N = 14,301										
Weeks compensated:										
Total sample	-0.04 (0.293)	-0.27 (0.289)	-0.70** (0.326)	-0.62** (0.287)	-0.26 (0.289)	-0.75** (0.329)	-0.34 (0.245)	-0.26 (0.244)	-0.73** (0.268)	-0.41* (0.212)
Excluding No Waiting Week	-0.03 (0.291)	-0.52* (0.286)	-0.86** (0.323)	-0.57** (0.287)	-0.39 (0.286)	-0.80** (0.328)	-0.31 (0.244)	-0.46* (0.243)	-0.83** (0.266)	-0.50** (0.211)

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

credit.[19] Treatment impact estimates based on the group of claimants who did serve a waiting week were considerably stronger than those for the total sample. For all six treatments, the mean effects were larger, the standard errors about the same, and the significance levels higher. Four, instead of three, treatments were statistically significant at the 90 percent confidence level.

Eliminating the no-waiting-week group from the sample also caused the effects of the bonus offer on weeks of insured unemployment to increase, although the increase was proportionately smaller than for compensation. The average treatment effect on weeks of insured unemployment changed from -0.41 to -0.50, a difference of about 22 percent, while the average treatment effect on UI compensation changed from -$65 for the total sample to -$87 for the waiting week sample, a change of about 34 percent.

In estimating effects on both UI compensation and weeks of insured unemployment, the biggest change in treatment impact occurred for the middle level bonus offers. As a result, the impact estimates aggregated across qualification periods displayed the expected pattern with treatment effects increasing with the bonus level. This pattern failed to emerge in any impact analyses on the total sample.

The implication that might be drawn from Table 5-12 is that a reemployment bonus program that requires a waiting week to be served to establish bonus eligibility will have larger effects than a program that offers bonuses to those not claiming a waiting week. However, the appropriateness of estimating treatment impacts on a subsample selected on the level of an outcome variable should be first examined.

[19] Since someone denied a waiting week would be ineligible for the WREB bonus, the technically proper distinction in our sample is between claimants who served a waiting week and those who never claimed one.

When a sample is restricted by some threshold value of the outcome variable it is said to be truncated. The condition for unbiased estimation of regression coefficients with a truncated sample was stated by Heckman.[20] In the present context, this condition is satisfied if claimants were not any more or less likely to serve a waiting week as a result of the reemployment bonus offer. Sample selection bias would be a problem if claimants with an issue on their claim avoid filing for a waiting week to preserve bonus eligibility.

Of the 15,534 claimants studied in WREB, 1,233 never filed for a waiting week; this included 979 treatments and 254 controls. The treatment-control composition of the group of claimants who never filed for a waiting week was not statistically different from the designed proportions. Indeed 20.6 percent of those not filing for a waiting week were controls, indicating that treatments actually filed for a waiting week at a slightly greater rate than controls. Furthermore, among the 1,233 not filing for a waiting week, 91 had an issue stop placed on their claim in the week of their new initial claim for benefits, and only 43 of these were treatments while 48 were controls. Even treatments

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[20] To illustrate the Heckman (1976) condition for unbiased estimation of parameters on a selected sample, our regression model for impact estimation may be written as:

$$Y = B'X + u,$$

where B is a set of parameter coefficients, X is a matrix of exogenous variables including treatment variables, and Y is the outcome variable. If u is a mean zero error term obeying the usual least squares assumptions, then

$$E(Y|X) = B'X.$$

However, if there has been sample truncation on some selection rule, then

$$E(Y|X, \textit{Selection Rule}) = B'X + E(u|\textit{Selection Rule}).$$

If $E(u|\textit{Selection Rule}) = 0$, then OLS estimation on the selected sample yields an unbiased estimate of B. We believe that the expected value of the error term has not been biased by the exclusion of the no waiting week group, since there is evidence that this group was not any more or less likely to serve a waiting week as a result of the experimental treatment.

with an issue stop, which means a greater risk of losing bonus eligibility, filed for a waiting week at a higher rate than controls.

As a formal test to see if there was a tendency for claimants in treatment groups to be less likely to serve a waiting week than claimants in the control group, the following probability model was estimated by OLS and probit:

$$(5) \qquad y = a + B'T + u,$$

where y equals one if a claimant served a waiting week and zero otherwise, T is a matrix of dummy variables for each of the six treatments, and B is a vector of parameter estimates measuring the change in the probability that a claimant in the treatment group would serve a waiting week. Taken together, the set of treatment impacts were not significantly different from zero. Furthermore, the treatment variables collectively explained very little of the variation in the probability of filing a waiting week.[21] The parameter estimates on the treatment variables were generally small and statistically insignificant. The single exception was for treatment 2, which induced a statistically significant increase in the probability of claiming a waiting week, a result which is opposite the anticipated effect.

We conclude that WREB treatment impacts estimated on the sample of claimants who served a waiting week do not suffer from sample selection bias, and believe they may fairly be compared to impacts estimated in field experiments that impose this exclusion at the outset.

[21] The R^2 in the linear probability, or ordinary least squares, version of the model was close to zero.

CHAPTER 6

TREATMENT EFFECTS BY POPULATION SUBGROUP

6.1 The Rationale for Subgroup Analysis

In this chapter, we present estimates of the experimental impact on benefit year UI compensation and weeks of insured unemployment for several selected subgroups of the total population. There are two reasons for considering subgroups. One reason is to provide information to policymakers who may consider targeting a reemployment bonus program to certain groups, such as dislocated workers or older workers. The second reason to consider the impacts on subgroups is to be aware of possible biases in the effects. A program that only benefits one gender or certain racial/ethnic groups may not be considered good policy, even if the overall effects are beneficial.

To estimate the impact on a selected subgroup or selected subgroups, a single regression model is constructed utilizing all the data. In that model, the treatment impact is estimated on one of the subgroups, and the differential treatment impact is estimated for other subgroups. For example, estimates are obtained for the treatment impact on males and the differential treatment impact for females in the same model. By adding the differential for females to the impact for males, the same estimate is obtained for females as would be if a separate regression had been run on females only, provided there is full interaction with all control variables. The equations estimated are a generalization of those stated in Chapter 5:

$$Y = a + B'T + C'Z + D'G + E'TG' + F'ZG' + u$$

where Y is the outcome measure, either UI compensation or weeks of insured unemployment, a is the intercept, B, C, D, E, and F are parameter vectors, T is the matrix of six treatment dummies, Z is the matrix of control variables, G is the matrix of dummy variables that code for membership in a subgroup, and u is a mean zero normally distributed random error term. E is the vector of treatment impact estimates for subgroups G, and B is the vector of treatment impacts for the omitted subgroup.

6.2 Effects by Dislocated Worker Status

The definition of worker economic dislocation in general use has both a supply and a demand dimension. In the Washington employment security statutes (Revised Code of Washington 50.04.075), "Dislocated Worker means an individual who: (1) Has been terminated or received a notice of termination from employment; (2) Is eligible for or has exhausted entitlement to unemployment compensation benefits; and (3) Is unlikely to return to employment in the individual's principal occupation or previous industry because of a diminishing demand for their skills in that occupation or industry."

Current public policy directed toward dislocated workers is largely administered through Title III of the Job Training Partnership Act (JTPA). Eligibility conditions for these programs are stated in the Economic Dislocation and Worker Adjustment Assistance Act (EDWAA) of 1988. These rules encompass persons covered by the Washington statute and are broadened to include the long-term unemployed, older workers, and the previously self-employed.

Since we do not have the appropriate demand information, our data do not permit an analysis of dislocated workers defined in precise conformity with legal or administrative regulations. However, we do apply three definitions that are likely to encompass the relevant population. These definitions are based on the work history of UI claimants. The definitions are consistent with those used in previous studies as summarized by Leigh (1990) who writes that "dislocated workers are usually defined as persons on layoff who possess a stable employment history" (p. 1). In these definitions, UI claimants were classified as to dislocated worker status using information about their previous 12 quarters of employment from the Wage History file maintained by the Washington State Employment Security Department (WSESD). The added labor demand condition that they not be awaiting recall is also applied. In order of decreasing restrictiveness the definitions used are:

Definition 1: An individual's principal job was with the same employer for 12 quarters prior to filing for UI.

Definition 2: An individual's principal job was in the same industry for 12 quarters prior to filing for UI. (Defined by the two-digit Standard Industrial Classification (SIC) code.)

Definition 3: An individual was continuously employed for 12 quarters prior to filing for UI.

In definitions 1 and 2, the principal job in each quarter is the job contributing the largest proportion of earnings. The number of claimants categorized as dislocated workers in each of the three definitions was 2,241, 3,108, and 5,677 respectively. These samples represented 14.7 percent, 20.0 percent, and 36.5 percent of the total sample of 15,534 claimants.

The differential treatment response of claimants classified as dislocated using the full sample information are reported in Tables 6-1 and 6-2 for UI compensation and weeks of insured unemployment, respectively. Impact estimates are presented for the six treatments, the three bonus multiples, and a pooled treatment versus control comparison. Note that each group of claimants categorized as dislocated is a subset of those dislocated according to higher number definitions.

For dislocated workers using either definition 1 or definition 2, only treatment 3 (T3) shows significant effects on compensation (Table 6-1) and the impact coefficients for treatments overall are smaller than for nondislocated workers and are statistically

Table 6-1

Treatment Impacts on UI Compensation in the Benefit Year for Dislocated and Nondislocated Workers (standard errors in parentheses)

Definition 1
The Same Employer for 12 Quarters Prior to Filing

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Dislocated Workers	180.68	9.34	-267.13**	-104.34	72.80	-27.65	25.33	44.86	-150.41	-13.37
	(122.62)	(123.91)	(131.63)	(116.35)	(115.50)	(133.69)	(100.67)	(100.74)	(108.77)	(86.99)
Nondislocated Workers	-6.57	-41.38	-74.16	-114.54**	-55.93	-165.17**	-61.69	-48.50	-119.01**	-70.94**
	(49.08)	(48.31)	(55.04)	(48.50)	(48.82)	(55.56)	(41.26)	(41.13)	(45.18)	(35.73)

Definition 2
The Same Industry for 12 Quarters Prior to Filing

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Dislocated Workers	101.63	50.93	-234.80**	-61.53	80.98	-141.46	16.42	66.99	-188.22**	-17.09
	(103.16)	(102.96)	(113.48)	(100.53)	(99.14)	(113.32)	(86.02)	(85.57)	(93.04)	(74.36)
Nondislocated Workers	-3.35	-59.52	-73.28	-125.26**	-67.34	-142.69**	-65.96	-63.33	-106.06**	-74.94**
	(50.81)	(50.05)	(56.81)	(50.02)	(50.49)	(57.58)	(42.62)	(42.54)	(46.70)	(36.92)

Definition 3
Employed During Each of the 12 Quarters Prior to Filing

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Dislocated Workers	100.05	-53.54	-196.11**	-135.71*	-26.35	-238.02**	-22.62	-39.30	-217.13**	-78.54
	(76.34)	(75.50)	(84.26)	(74.57)	(73.64)	(84.07)	(63.72)	(63.18)	(68.98)	(55.00)
Nondislocated Workers	-23.84	-35.40	55.34	102.34**	-50.50	-88.92	-64.07	-42.74	-71.67#	-57.93
	(56.97)	(56.20)	(63.85)	(56.18)	(56.97)	(65.01)	(47.85)	(47.85)	(52.57)	(41.49)

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.
#The subgroup treatment impact estimate is significantly different from the estimate for dislocated workers for a two-tail test at the 90 percent confidence level.

insignificant. The same pattern holds for weeks of insured unemployment (Table 6-2). Under the broadest definition of worker dislocation (definition 3), the overall experimental impact is larger for dislocated than for nondislocated workers, but the difference in impacts for the two groups is not statistically significant. For the combined high bonus treatments (T3 and T6), the effect on dislocated workers (using definition 3) of $217 is statistically significant at the 95 percent confidence level, and the difference between the high bonus impact on dislocated and nondislocated workers is statistically significant at the 90 percent level.

Overall, the results in Tables 6-1 and 6-2 show that the standard errors of the impact decline and the impact coefficients increase as the dislocated worker definition is broadened. This pattern is primarily due to treatment 6, which increases from an insignificant $28 reduction in UI compensation in the first definition to a statistically significant $238 reduction under the third definition. Only for the broadest definition of dislocation, i.e., continuous employment for 12 quarters, and for the highest bonus offer, do the responses for dislocated workers really differ from those of nondislocated workers.

6.3 Effects by Gender, Race/Ethnicity and Age

In this section, we discuss treatment impacts for population subgroups defined by three demographic characteristics: gender, race/ethnicity, and age. We first compared impacts for seven different age groups, each having a 10-year range; but since the only significant differences where between those below and those at or above 45 years of age, we consolidated the age groups into two.

Mean values of control variables and sample sizes for these subgroups are given in Table 6-3. The most notable difference in characteristics across groups is the low level of educational attainment of Hispanics, who average less than eight years compared

Table 6-2

Treatment Impacts on Weeks of Insured Unemployment in the Benefit Year for Dislocated and Nondislocated Workers
(standard errors in parentheses)

Definition 1
The Same Employer for 12 Quarters Prior to Filing

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Dislocated Workers	0.66 (0.78)	-0.35 (0.79)	-1.30 (0.84)	-0.11 (0.74)	-0.09 (0.74)	-0.48 (0.86)	0.24 (0.64)	-0.21 (0.64)	-0.90 (0.70)	-0.22 (0.56)
Nondislocated Workers	-0.15 (0.31)	-0.21 (0.31)	-0.58* (0.35)	-0.70** (0.31)	-0.28 (0.31)	-0.83** (0.36)	-0.43* (0.26)	-0.25 (0.26)	-0.70** (0.29)	-0.43* (0.23)

Definition 2
The Same Industry for 12 Quarters Prior to Filing

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Dislocated Workers	0.37 (0.66)	0.07 (0.66)	-0.89 (0.73)	0.35 (0.64)	0.13 (0.63)	-0.76 (0.73)	0.36 (0.55)	0.10 (0.55)	-0.83 (0.60)	-0.04 (0.48)
Nondislocated Workers	-0.17 (0.33)	-0.34 (0.32)	-0.66* (0.36)	-0.84**# (0.32)	-0.34 (0.32)	-0.7** (0.37)	-0.51* (0.27)	-0.34 (0.27)	-0.71** (0.30)	-0.49** (0.08)

Definition 3
Employed During Each of the 12 Quarters Prior to Filing

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Dislocated Workers	0.29 (0.49)	-0.38 (0.48)	-0.90* (0.54)	-0.44 (0.48)	-0.51 (0.47)	-1.20** (0.54)	-0.09 (0.41)	-0.45 (0.40)	-1.05** (0.44)	-0.47 (0.35)
Nondislocated Workers	-0.22 (0.36)	-0.22 (0.36)	-0.60 (0.41)	-0.71** (0.36)	-0.12 (0.36)	-0.54 (0.42)	-0.47 (0.31)	-0.17 (0.31)	-0.57* (0.34)	-0.38 (0.27)

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.
#The subgroup treatment impact estimate is significantly different from the estimate for dislocated workers for a two-tail test at the 90 percent confidence level.

Table 6-3

Claimant Characteristics by Gender, Race, and Age

Variable	Total	Male	White	Black	Hispanic	Other	Over 45
AGE	36.25	35.81	36.40	34.54	34.37	35.41	53.49
EDUCATION	12.36	12.29	12.72	12.84	7.84	10.46	11.79
GENDER	0.61	1.00	0.61	0.65	0.64	0.62	0.56
BLACK	0.04	0.05	0.00	1.00	0.00	0.27	0.03
OTHER	0.12	0.12	0.00	0.00	1.00	0.72	0.12
BWAGES	15759.23	17889.53	16482.06	14015.73	9639.38	11978.03	18240.06
STDWAGE	1439.07	1691.45	1470.10	1472.10	1214.45	1276.70	1584.68
WBA	153.24	165.10	157.02	145.26	118.68	133.47	161.85
ENTITLE	26.86	26.92	27.11	26.16	24.18	25.53	27.41
WHITECOL	0.35	0.23	0.38	0.34	0.10	0.22	0.33
WBAMIN	0.03	0.02	0.03	0.04	0.06	0.05	0.02
WBAMAX	0.34	0.45	0.37	0.28	0.10	0.19	0.43
SEARCHEX	0.22	0.28	0.23	0.20	0.12	0.16	0.29
SAMPLE SIZE	15534	9471	13041	695	1036	762	3332

AGE - Age in years.
EDUCATION - Years of formal education completed.
GENDER - 1 if male, 0 if female.
BLACK - 1 if black, 0 if nonblack.
OTHER - 0 if black or non-Hispanic White, 1 otherwise.
BWAGES - UI base period (first four of last five quarters) wages.
STDWAGE - Standard deviation in wages across base period quarters.
WBA - UI Weekly Benefit Amount.
ENTITLE - Entitled weeks of full UI benefits.
WHITECOL - 1 if previous job was white-collar (DOT 0-3), 0 otherwise.
WBAMIN - 1 if WBA is at the Washington State minimum, 0 otherwise.
WBAMAX - 1 if WBA is at the Washington State maximum, 0 otherwise.
SEARCHEX - 1 if excluded from UI work search requirement, 0 otherwise.

to an average of over 12 years for the full sample. Hispanics also have base period wages which average only two-thirds of the full sample average, and an average Weekly Benefit Amount (WBA) which is over 20 percent less. Among the 1,036 Hispanics, 83.7 percent registered for UI in eastern Washington, and over half of these had their previous job in agriculture. Differences for blacks and others in these characteristics are in the same direction but are less pronounced. These differences could affect measured changes in UI compensation. In computing impact estimates, these and other characteristics are controlled to the extent possible.

6.3.1 Effects by Gender

Dividing the analytic sample into subgroups by gender reveals some dramatic differences in characteristics that suggest potentially different responses to the reemployment bonus offer. Table 6-3 shows that, on average, males have much higher base period earnings ($17,890) than females ($12,440), which translates into a higher average WBA and benefit entitlement period for men. There appears to be no difference in average age or years of education across gender.

Table 6-4 shows the principal experimental effects for males and females. For males, T3, T4, and T6 have statistically significant effects on both UI compensation and weeks of insured unemployment. For females, none of the six experimental treatments show a statistically significant effect, and the coefficients suggest much lower effects on females than on males. However, use of a pooled regression model failed to disclose a statistically significant differential impact for females from males.

Combining the six treatments into three bonus-level groups, the gender differences in impact on UI compensation are sharper, albeit not statistically significant. For males, the highest bonus multiplier has a strong effect, whereas the effects of the two lower treatment levels are either negligible or too small to be statistically significant given the sample size. For females, the impact appears to be smaller, in fact too small

Table 6-4

Treatment Impacts by Gender
(standard errors in parentheses)

Impacts for Males, Treatment group differences from Controls

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Benefit Year UI Compensation	18.32 (58.44)	-333.27 (57.82)	-104.68* (65.01)	-154.60** (57.32)	-63.09 (57.91)	-167.46** (65.47)	-71.08 (48.98)	-48.15 (48.98)	-135.73** (53.42)	-78.86* (42.47)
Weeks of Insured Unemployment	-0.07 (0.37)	-0.32 (0.37)	-0.85** (0.42)	-0.95** (0.37)	-0.48 (0.37)	-0.92** (0.42)	-0.52* (0.31)	-0.40 (0.31)	-0.88** (0.34)	-0.57** (0.27)
Male Sample Size (Control = 1865)	1372	1426	971	1468	1417	952	2840	2843	1923	7606

Impacts for Females, Treatment group differences from Controls

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Benefit Year UI Compensation	20.79 (66.24)	-41.55 (65.24)	-98.91 (74.02)	-46.58 (65.30)	2.84 (64.98)	-82.40 (75.23)	-13.72 (55.49)	-19.15 (55.12)	-90.84 (60.83)	-34.86 (47.92)
Weeks of Insured Unemployment	-0.02 (0.48)	-0.18 (0.47)	-0.45 (0.54)	-0.05 (0.47)	0.11 (0.47)	-0.43 (0.54)	-0.03 (0.40)	-0.03 (0.40)	-0.44 (0.44)	-0.13 (0.35)
Female Sample Size (Control = 1217)	874	922	612	919	936	583	1793	1858	1195	4846

Note: The regressions were estimated on the full sample, n = 15,534.

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

Differences in impacts between males and females are not statistically significant.

to be statistically significant. When all treatments are pooled into one, the male impact is statistically significant and more than double the female impact, which again is not significant.

The results in terms of weeks of insured unemployment are generally stronger for males, with males exhibiting statistically significant impacts of about one week for treatments 3 and 6. Combining the six treatment groups into the three bonus multiplier levels shows even stronger results for males. Because of the persistently high impact of treatment 4 relative to others, the expected progression in impact from lowest to highest bonus multipliers does not occur. The impact on females is decidedly lower and statistically insignificant.

6.3.2 Effects by Race/Ethnicity

Among industrial states, Washington is somewhat unique in that it has a relatively small nonwhite population. In the general population, 90.5 percent of Washington residents are white, and only 2.9 percent are black. In our analytic sample of UI claimants, 4.5 percent are black. This compares favorably to the percentage of UI claimants in the state who are black (4.4 percent), but is inadequate to generate precise treatment impact estimates. The Hispanic and other subgroups in the analytic sample are also too small to generate very precise impact estimates.

The experimental responses in Table 6-5 do not present a clear picture of lower nonwhite response to the experiment. For compensation in the benefit year, the impact estimates for whites correspond closely to those estimated on the full sample and listed in Table 5-4, and there are no statistically different impact estimates for any treatment among the race/ethnic subgroups. Ignoring the lack of statistical significance, the results for benefit year UI compensation suggest that blacks have generally weaker treatment impacts compared to whites, while Hispanics respond similarly to whites and persons in

Table 6-5

Treatment Impacts by Race/Ethnicity
(standard errors in parentheses)

Impacts on UI Compensation in the Benefit Year

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	Treatment
Whites	25.30	-47.17	-123.84**	-117.03**	-36.82	-130.32**	-48.13	-41.99	-127.05**	-65.84*
	(50.09)	(49.26)	(55.56)	(49.19)	(49.29)	(55.76)	(41.95)	(41.74)	(45.58)	(36.27)
Blacks	72.98	263.02	-241.58	31.08	128.72	102.84	51.83	200.21	48.00	104.19
	(210.20)	(213.32)	(252.26)	(212.35)	(222.40)	(253.00)	(180.31)	(184.20)	(205.53)	(159.70)
Hispanics	61.38	-4.10	-73.44	-99.76	-61.66	-245.96	-22.46	-33.04	-146.86	-54.95
	(174.48)	(175.48)	(194.69)	(169.91)	(173.14)	(214.84)	(145.24)	(146.87)	(164.62)	(126.51)
Others	-205.27	-245.86	59.62	-252.31	104.83	-286.07	-230.39	-172.57	-104.29	-178.31
	(203.41)	(205.43)	(231.26)	(200.94)	(199.74)	(240.13)	(169.61)	(170.35)	(189.94)	(145.97)

Impacts on Weeks of Insured Unemployment

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	Treatment
Whites	0.02	-0.28	-0.72**	-0.55*	-0.15	-0.56	-0.27	-0.22	-0.64**	-0.34
	(0.32)	(0.31)	(0.36)	(0.31)	(0.32)	(0.36)	(0.27)	(0.27)	(0.29)	(0.23)
Blacks	-0.42	1.25	-0.95	-0.69	0.11	-0.98	-0.55	0.72	-0.96	-0.17
	(1.34)	(1.36)	(1.61)	(1.36)	(1.42)	(1.62)	(1.15)	(1.18)	(1.31)	(1.02)
Hispanics	0.33	0.17	-0.84	1.07	-0.73	-1.81	-0.54	-0.29	-1.25	-0.55
	(1.12)	(1.12)	(1.24)	(1.09)	(1.11)	(1.37)	(0.93)	(0.94)	(1.05)	(0.81)
Others	-1.46	-2.27*	-0.27	-1.55	-1.39	-2.41	1.51	-1.82*	-1.28	-1.57*
	(1.30)	(1.31)	(1.48)	(1.28)	(1.28)	(1.54)	(1.08)	(1.09)	(1.21)	(0.92)

Sample Sizes for Race/Ethnic Sub-Groups

	Control	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	Treatment
White	2567	1868	1980	1336	991	1977	1322	3859	3957	2658	10474
Black	133	116	110	64	112	96	64	228	206	128	562
Hispanic	217	152	150	108	168	159	82	320	309	190	819
Other	165	110	108	75	116	121	67	226	229	142	597

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.
Differences in impacts among racial subgroups are not statistically significant.

the other racial/ethnic group may have responded even more strongly than whites. These general findings hold up when treatment impacts on weeks with UI compensation are considered.

6.3.3 Effects by Age

A reemployment bonus in UI could be used as a program targeted to older workers. This might be particularly advisable if older workers responded relatively strongly to bonus offers. To investigate if this occurred in WREB, the study population was divided into two groups, persons less than 45 years of age and persons 45 and over. Impacts estimated for the six treatments, the three bonus levels, and an overall treatment versus control are presented in Table 6-6.

Treatments 2, 3, 4, and 5 show dramatically larger impacts on compensation for the older workers than for the younger group. However, only for treatment 4 is there a statistically significant difference in treatment impact across the two age groups.

6.3.4 Effects by Gender, Race/Ethnicity, and Age Combined

Table 6-7 displays the results for subgroups partitioned by age, gender, and three race/ethnicity categories: black, Hispanic, and other (including white non-Hispanic). Thus, there are a total of 12 subgroups (two genders, two ages, and three ethnic distinctions). A striking result apparent in Table 6-7 is that younger black males respond to the average of all six treatments differently from all other subgroups. Small sample size prevents placing too much stock in the sizes of the coefficients. However, younger black males, despite small samples, show statistically significant results opposite to expectations. The bonus offer apparently caused members of this group to <u>increase</u> the level of compensation they received. On the other hand, older black males responded similarly to, and perhaps even more strongly than, older white males. The response for

Table 6-6

Treatment Impacts by Age
(standard errors in parentheses)

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Impacts on Compensation in the Benefit Year										
45 and Over	22.89	-102.33	-173.69	-300.35**	-81.88	-171.97	-152.19*	-91.46	-172.57*	-134.63*
	(99.53)	(98.94)	(109.11)	(94.91)	(96.47)	(112.76)	(82.09)	(82.44)	(90.43)	(71.25)
Under 45	23.15	-22.39	-83.55	-55.27##	-24.06	-130.56**	-16.65	-23.20	-106.85**	-41.75
	(51.40)	(50.67)	(57.54)	(50.95)	(50.97)	(57.83)	(43.26)	(43.05)	(47.19)	(37.42)
Impacts on Weeks of Insured Unemployment										
	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
45 and Over	0.21	-0.57	-0.84	-1.31**	-0.37	-0.36	-0.62	-0.46	-0.61	-0.56
	(0.64)	(0.63)	(0.70)	(0.61)	(0.62)	(0.72)	(0.52)	(0.53)	(0.58)	(0.45)
Under 45	-0.08	-0.21	-0.66*	-0.36	-0.20	-0.89**	-0.23	-0.20	-0.77**	-0.36
	(0.33)	(0.33)	(0.37)	(0.33)	(0.33)	(0.37)	(0.28)	(0.28)	(0.30)	(0.24)

Note: Impacts and differentials were estimated in regression models where a dummy variable U45 took the value of 1 for persons aged less than 45 years, and 0 for persons 45 and over. The regressions were estimated on the full sample, n = 15,534. The number of claimants coded into each of the age groups was: under 45, 12,202;and 45 and over, 3,332.

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.
#The subgroup treatment impact estimate is significantly different from the estimate for claimants 45 years of age and over for a two-tail tests at the 90 percent confidence level.
##Indicates a significant difference at the 95 percent level.

Table 6-7

Average Treatment Impact
by Age, Gender, and Race/Ethnicity Groups

Gender, Race	Older			Younger		
	Parameter Estimate	Standard Error	Sample Size	Parameter Estimate	Standard Error	Sample Size
UI Compensation in the Benefit Year						
Male, White	-208.41**	100.20	1,689	-65.88	50.68	6,670
Male, Black	-926.76**	470.44	77	557.13**##	222.85	372
Male, Hispanic	-34.88	387.56	108	-163.59	174.03	555
Female, White	19.46	114.47	1,348	-31.83	63.66	4,096
Female, Black	-680.79	830.23	28	-285.26	288.14	218
Female, Hispanic	-76.74	479.62	82	116.07	241.81	291
Weeks of Insured Unemployment						
Male, White	-0.80	-0.64	1,689	-0.57*	0.33	6,670
Male, Black	-5.45*	3.02	77	1.97#	1.43	372
Male, Hispanic	-0.67	2.61	108	-1.42	1.12	555
Female, White	0.25	0.73	1,348	-0.11	0.41	4,096
Female, Black	-0.75	5.31	28	-2.68	1.84	218
Female, Hispanic	-3.81	2.99	82	1.45	1.55	291

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.
#The subgroup treatment impact estimate is significantly different from the estimate for older white males for a two-tail test at the 90 percent confidence level.
##Indicates a significant difference at the 95 percent level.

Older: Age ≥ 45 years
Younger: Age < 45 years

Hispanic males is not statistically significant, but neither is it significantly different from the response for other males.

The pattern for females is ambiguous. None of the responses by females in any of the age and race/ethnicity groups were statistically significant. In addition, there were not statistically significant differences between impacts for any of the female-age-race/ethnicity groups and older white males. However, sample sizes for some of the female subgroups were too small to allow any reliable statements to be made as to effects.

A full interaction model of age, gender, and race/ethnicity was run to test the effects of each of the six treatments. The exercise revealed no distinct patterns by size of bonus offer or length of qualification period.

6.4 Effects by Industry and Occupation

Reemployment bonuses could be targeted to workers released from particular industries or employed in particular occupations. In the present section we use a broad aggregation of industries and occupations to give a feel for the potential of industry or occupation targeting.

6.4.1 Effects by Industry

Treatment impacts and differentials are presented for four groupings of claimants by industry of prior employment using Standard Industrial Classification (SIC) code numbers. The groupings are (1) agriculture, forestry, and fishing [0-9]; (2) mining and manufacturing [10-14, 20-39]; (3) construction [15-17]; and (4) trade and services [40-99]. Differential impacts are presented for the six treatments, the three bonus levels, and an overall treatment versus control contrast in Table 6-8.

Table 6-8

Treatment Impacts by Industry
(standard errors in parentheses)

Impacts on UI Compensation in the Benefit Year

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Mine-Manufac	93.35	-137.72	-308.43**	-17.50	-120.57	-272.85**	35.28	-129.28*	-290.71**	-107.91
	(94.72)	(92.67)	(105.89)	(92.13)	(93.10)	(106.13)	(78.67)	(78.40)	(86.34)	(67.90)
Agriculture	42.89	164.11	93.00#	25.44	-12.95	63.23	34.21	71.39	76.76##	58.06
	(169.08)	(176.36)	(197.24)	(170.18)	(171.69)	(190.81)	(144.68)	(147.41)	(159.21)	(127.34)
Construction	212.88	-59.97	-13.21#	-154.63	132.88	-76.59	15.17	31.93	-45.94#	5.99
	(134.55)	(129.30)	(146.93)	(128.98)	(132.13)	(146.29)	(111.88)	(111.39)	(120.88)	(97.47)
Trade-Service	-60.08	-29.95	-90.27#	-172.57**	-51.82	-129.71*	-117.42**#	-40.91	-109.31**#	-86.37**
	(59.25)	(48.65)	(65.87)	(58.59)	(58.40)	(67.37)	(49.75)	(49.52)	(54.38)	(43.01)

Impacts on Weeks of Insured Unemployment

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Mine-Manufac	0.26	-0.98*	-1.76**	-0.12	-0.69	-1.59**	0.06	-0.84*	-1.67**	-0.71*
	(0.61)	(0.59)	(0.68)	(0.59)	(0.60)	(0.68)	(0.50)	(0.50)	(0.55)	(0.44)
Agriculture	0.28	1.33#	0.90#	0.12	-0.16	-0.06	0.20	0.55	0.40#	0.37
	(1.08)	(1.13)	(1.26)	(1.09)	(1.10)	(1.22)	(0.93)	(0.94)	(1.02)	(0.82)
Construction	0.90	-0.47	-0.15	-0.61	0.42	-0.31	0.09	-0.05	-0.23	-0.04
	(0.86)	(0.83)	(0.94)	(0.83)	(0.85)	(0.94)	(0.72)	(0.71)	(0.77)	(0.62)
Trade-Service	-0.42	-0.15	-0.69	-0.93**	-0.25	-0.65	-0.68**	-0.20	-0.66*	-0.49*
	(0.38)	(0.38)	(0.42)	(0.38)	(0.37)	(0.43)	(0.32)	(0.32)	(0.35)	(0.28)

Note: Claimants were categorized into groups based on the SIC industry group of their principal employer in the quarter prior to filing for UI. The ranges of two-digit SIC numbers defining industry groupings are as follows: Mining and Manufacturing, 10-14, 20-39; Agriculture, 01-09; Construction, 15-17; Trade and Services, 40-99. The regressions were estimated on the full sample, n = 15,534. The number of claimants coded into each industry group was as follows: Mining and Manufacturing, 3,538; Agriculture, 1,082; Construction, 1,871; Trade and Services, 9,015.

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.
#The subgroup treatment impact estimate is significantly different from the estimate for claimants whose previous job was in mining or manufacturing for a two-tail test at the 90 percent confidence level.
##Indicates a significant difference at the 95 percent level

For workers employed in mining or manufacturing, there is a pattern of increasing impact with increasing size of the bonus offer. While both middle- and high-level bonus offers show large and statistically significant treatment effects on compensation and weeks of insured unemployment, there is no discernable difference in impact by length of qualification period. For the high-level bonuses, treatment effects are statistically different from those on workers in all other industries. In fact, the impact estimates for workers in agriculture and construction approach zero.

6.4.2 Effects by Occupation

In this subsection, we discuss the treatment effect and differential estimates for six different categories of occupations. Using the two-digit Dictionary of Occupational Titles (DOT) codes, claimants were grouped into the following occupation categories: goods producing [50-79]; professional and technical [00-19]; clerical and service [20-38]; agriculture [40-46]; construction trades [80-89]; and miscellaneous occupations [90-97]. Impact and differential estimates for the six treatments, the three bonus levels, and an overall treatment versus control effect are reported in Table 6-9.

Comparing the impact results by occupation and by industry provides a striking contrast in that the dominance of manufacturing in the industry subgroup analysis is not repeated for goods producing occupations. In fact, the pattern of results emerging from Table 6-9 is weak and ambiguous. The groups whose UI compensation demonstrated the strongest responses were claimants who had jobs in clerical or service occupations and the variety of miscellaneous occupations. Clerical and service workers responded strongly to the high bonus offers, especially to the one with the long qualification period, treatment 6. The results for weeks of insured unemployment were even weaker than for dollars of UI compensation (see Table 6-9).

Table 6-9

Treatment Impacts by Occupation
(standard errors in parentheses)

Impacts on UI Compensation in the Benefit Year

	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Goods Producing	91.4	91.6	-77.5	-274.8	-16.4	-66.8	-107.2	38.1	-73.0	-43.5
	(118.5)	(113.9)	(126.8)	(112.9)	(119.0)	(131.7)	(97.1)	(86.5)	(105.4)	(83.5)
Professional/ Technical	-16.6	-144.9	-58.3	-148.9	-7.4	-122.9	-85.7	-72.2	-88.6	-81.2
	(115.5)	(113.2)	(123.2)	(112.7)	(109.7)	(128.1)	(96.5)	(94.9)	(103.3)	(83.0)
Clerical	-52.2	-42.9	-116.8	-13.1	-18.1	-173.7*	-32.0	-30.8	-145.1*	-58.8
	(82.4)	(80.4)	(93.4)	(80.9)	(81.4)	(93.8)	(68.9)	(68.5)	(76.2)	(59.6)
Agriculture	100.9	89.2	4.1	-79.6	46.2	-148.7	12.2	66.1	74.4	10.5
	(169.2)	(176.5)	(199.2)	(171.0)	(172.5)	(196.5)	(144.5)	(147.1)	(161.1)	(126.5)
Construction	67.4	-48.9	10.1	-7.5	49.8	-58.7	29.1	-2.1	-23.6	4.3
	(112.8)	(110.9)	(125.7)	(111.3)	(113.9)	(127.2)	(95.4)	(95.6)	(104.0)	(83.4)
Miscellaneous	30.8	-54.9	-254.2**	-163.8	-223.6**	-149.3	-66.7	-142.4	-199.2**	-128.2
	(108.2)	(110.9)	(123.5)	(107.9)	(107.9)	(120.6)	(91.1)	(91.8)	(99.6)	(8.9

Table 6-9
(Continued)

	Impacts on Weeks of Insured Unemployment									
	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	All T's
Goods Producing	0.32 (0.76)	0.00 (0.73)	-0.86 (0.82)	-2.01** (0.73)	-0.15 (0.74)	-0.06 (0.85)	-0.94 (0.62)	-0.08 (0.62)	-0.49 (0.68)	-0.50 (0.54)
Professional/ Technical	-0.35 (0.74)	-0.75 (0.73)	-0.26 (0.79)	-0.61 (0.72)	0.30 (0.71)	-1.10 (0.82)	-0.48 (0.62)	-0.19 (0.61)	-0.66 (0.66)	-0.42 (0.53)
Clerical	-0.32 (0.53)	-0.21 (0.52)	-0.89 (0.60)	0.00 (0.52)	-0.04 (0.52)	-0.87 (0.60)	-0.15 (0.44)	-0.13 (0.44)	-0.89* (0.49)	-0.32 (0.38)
Agriculture	0.41 (1.09)	0.56 (1.14)	-0.42 (1.28)	-0.72 (1.10)	-0.31 (1.10)	-1.40 (1.26)	-0.16 (0.93)	0.10 (0.95)	-0.93 (1.04)	-0.25 (0.81)
Construction	0.16 (0.73)	-0.42 (0.71)	0.19 (0.81)	0.19 (0.72)	0.18 (0.73)	-0.02 (0.82)	0.18 (0.61)	-0.14 (0.61)	0.08 (0.67)	0.04 (0.54)
Miscellaneous	0.16 (0.70)	0.16 (0.71)	-1.20 (0.79)	-0.75 (0.69)	-1.40** (0.69)	-0.76 (0.78)	-0.29 (0.59)	0.50 (0.59)	-0.97 (0.64)	-0.60 (0.51)

<u>Note</u>: Using the two-digit Dictionary of Occupational Titles (DOT) codes, claimants were grouped into the following occupation categories: goods producing [50-79]; professional and technical [00-19]; clerical and service [20-38]; agriculture [40-46]; construction trades [80-89]; and miscellaneous occupations [90-97]. Treatment impacts were estimated in regressions on the full sample, n = 15,534. The number of claimants coded into each occupation group was as follows: goods producing n = 2,321; professional and technical n = 2,469; clerical and service n = 4,604; agriculture n = 1,038; construction trades n = 2,496; and miscellaneous occupations n = 2,606.

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

6.4.3 Effects of Gender, Race/Ethnicity, Age, and Industry Combined

Table 6-10 displays the experimental impacts on subgroups defined by gender, race/ethnicity, age, and industry.[1] The results presented are for the average treatment impact. The groups have all been defined dichotomously on each characteristic; that is, two genders, two races (black/hispanic and other), two ages (under 45 and 45 plus), and two industries (mining/manufacturing and other).

From Table 6-10 it can be seen that young blacks, both male and female, in manufacturing or mining either did not respond to the experimental treatment, or may even have responded by lengthening their spell of unemployment. This anomalous response to the experimental treatment is even clearer when the interaction analysis includes base period earnings as a factor. White females, both young and old, in industries other than mining or manufacturing also failed to respond to the treatment. There is no clear explanation for the particular industry orientation of the response.

6.5 Effects by Base Period Earnings Combined with Effects by Age, Gender and Race/Ethnicity

After some experimentation with the distribution of base period earnings (BPE), it was found that the strongest response differences occurred for those above and below the 66 2/3 percentile for the full sample. In other words, those with earnings in the top third of the earnings distribution (BPE above $17,366) responded differently from those in the lower two-thirds. Table 6-11 shows average treatment impacts for the three different two-way comparisons of earnings with age, gender, and race/ethnicity.

[1] We also analyzed the combined interactions for gender, race/ethnicity, age, occupation, and industry. In this five-way interaction analysis, no differential effects by occupation were detected. Since this analysis did not provide any additional information over that in the four-way interaction analysis, the results are not included in this report.

Table 6-10

Average Treatment Impact
by Age, Gender, Race/Ethnicity, and Industry Groups

	Older		Younger	
Gender,Race, Industry	Parameter Estimate	Standard Error	Parameter Estimate	Standard Error
	UI Compensation in the Benefit Year			
Male, White, non-Manu	-255.0**	117.9	-73.7	60.0
Male, White, Manu	-134.6	188.9	-63.2	94.6
Male, Black-Hisp, non-Manu	-14.4	321.0	-29.0	158.2
Male, Black-Hisp, Manu	-393.3	771.2	323.5#	286.2
Female, White, non-Manu	146.3##	126.4	15.8#	68.5
Female, White, Manu	-718.2**##	256.2	-321.3*	179.5
Female, Black-Hisp, non-Manu	570.4	548.6	-260.2	204.5
Female, Black-Hisp, Manu	-199.1	619.2	719.2*##	409.2
	Weeks of Insured Unemployment			
Male, White, non-Manu	-0.69	0.76	-0.75*	0.39
Male, White, Manu	-1.24	1.21	-0.13	-0.61
Male, Black-Hisp, non-Manu	-0.52	2.06	-0.65	1.02
Male, Black-Hisp, Manu	-1.84	4.95	0.45	1.84
Female, White, non-Manu	1.11	0.81	0.21	0.44
Female, White, Manu	-4.39**##	1.65	-2.32**	1.15
Female, Black-Hisp, non-Manu	-2.63	3.52	-1.95	1.31
Female, Black-Hisp, Manu	-2.71	3.98	6.21**##	2.63

* Coefficient significant at the 90 percent confidence level for a two-tail test.
** Coefficient significant at the 95 percent confidence level for a two-tail test.
# The subgroup treatment impact estimate is significantly different from the estimate for older white males in nonmanufacturing industries for a two-tail test at the 90 percent confidence level.
Indicates a significant difference at the 10 percent level.

Older: Age ≥ 45 years
Younger: Age < 45 years

Table 6-11

Average Treatment Impacts
by Base Period Earnings (BPE), Age, Gender, and Race/Ethnicity
(standard errors in parentheses)

Interaction Variable	High BPE	Low BPE
	UI Compensation in the Benefit Year	
Older (Age ≥ 45	-220.54** (111.39)	-73.87 (92.31)
Younger (Age < 45)	28.09# (67.11)	-474.54* (254.29)
Male	-48.90 (65.42)	-111.77** (55.34)
Female	22.54 (119.11)	-52.80 (194.38)
Not Black or Hispanic	-40.11 (59.05)	-85.95** (43.39)
Black or Hispanic	-1.36 (245.00)	-24.49 (295.54)
	Weeks of Insured Unemployment	
Older (Age ≥ 45)	-0.926 (0.718)	-0.302 (0.595)
Younger (Age < 45)	0.197 (0.433)	-2.401 (1.639)
Male	-0.232 (0.420)	-0.859** (0.356)
Female	0.517 (0.765)	-0.417 (1.249)
Not Black or Hispanic	-0.010 (0.379)	-0.619 (1.898)
Black or Hispanic	-1.615 (1.573)	1.864 (1.898)

* Coefficient significant at the 90 percent confidence level for a two-tail test.
** Coefficient significant at the 95 percent confidence level for a two-tail test.
\# The subgroup treatment impact estimate is significantly different from the estimate for older males with a high BPE for a two-tail test at the 90 percent confidence level.

High BPE: Top 1/3 of BPE distribution.

Although low earning males was the strongest responding earnings/gender group, the differences among the four earnings/gender groups were not statistically significant. Similarly, although the nonblack/non-Hispanic lower earnings group was the strongest responder among the earnings/race groups, there were no statistically significant differences among the four earnings/race groups.

There are statistically significant differences in bonus offer impacts on compensation by earnings and age. (See Table 6-12.) For the average of the six treatments, impacts on UI compensation drawn by high earnings/older workers and low earnings/younger workers were very strong. Even though only two of the treatments (T4 and T6) are statistically significant, the size of the coefficients indicate that low earnings/young claimants might have been the strongest responding group. On average, high earnings/young claimants did not respond to the experimental treatment, and their response was statistically different from that of the other earnings/age groups. However, this group might have responded to the high bonus treatments.

What could account for these differences in response by earnings and age? It is probably the case that the bonus offer elicits the strongest response where there is room for response, i.e., where job search prior to the bonus offer is suboptimal. The high earnings/older workers might be dislocated workers whose strong response is possible because of a discouraged worker syndrome that provides an opportunity for incentives to change job search behavior. The low earnings/young workers may be those not yet strongly attached to the workforce who can be encouraged to increase job search. Furthermore, the high earnings/young workers may be those most strongly attached to the workforce, who are already maximizing their job search effort, and whose job search behavior cannot be successfully increased by a reemployment bonus incentive.

Table 6-12

Treatment Impact by Age and Base Period Earnings

Base Period Earnings Level	Treatment	Older		Younger	
		Parameter Estimate	Standard Error	Parameter Estimate	Standard Error
High BPE	T1	5.6	153.18	188.5**	94.16
	T2	-109.2	157.52	56.3	89.84
	T3	-230.0	179.31	-107.9	102.28
	T4	-539.1**	145.48	-37.8##	90.83
	T5	-105.3	147.52	113.1	91.29
	T6	-347.2**	176.42	-108.8	100.72
Lower BPE	T1	9.5	129.98	-241.1	351.17
	T2	-106.4	126.50	-227.4	356.42
	T3	-139.4	137.22	-290.7	403.33
	T4	-108.2##	124.58	-1001.8**##	334.90
	T5	-68.9	126.57	-348.4	339.30
	T6	-15.6	145.63	-714.1*	400.95
		Weeks of Insured Unemployment			
High BPE	T1	0.28	0.99	0.92	0.61
	T2	-0.84	1.02	0.32	0.58
	T3	-1.14	1.16	0.51	0.66
	T4	-2.44**	0.94	-0.06##	0.59
	T5	-0.38	0.95	0.70	0.59
	T6	-0.93	1.14	-0.54	0.65
Lower BPE	T1	0.04	0.84	-0.97	2.26
	T2	0.44	0.82	-2.04	2.30
	T3	-0.69	0.89	-1.83	2.60
	T4	-0.39##	0.80	-4.99**#	2.16
	T5	-0.40	0.82	-1.73	2.19
	T6	0.19	0.94	-2.59	2.59

* Coefficient significant at the 90 percent confidence level for a two-tail test.
** Coefficient significant at the 95 percent confidence level for a two-tail test.
\# The subgroup treatment impact estimate is significantly different from the estimate for older males with high BPE for a two-tail test at the 90 percent confidence level.
\#\# Indicates a significant difference at the 95 percent level. The *t*-statistic listed in this table is for the total sub-group treatment impact.

High BPE: Base Period Earnings at or above $17,366.
Lower BPE: Base Period Earnings below $17,366.
BPE: $17,366 is the BPE for the claimant at the 67th percentile in the distribution of BPE.

6.6 Experimental Effects by Location and Economic Conditions

In this section, we examine the results to determine if location has an influence on experimental outcome. Three separate analyses are undertaken--by geographic area in the state, by the rate of unemployment in the county of filing at the time each claimant files for benefits, and lastly, by the rate of growth of employment in the county at the time of filing. For each analysis, the 21 JSCs are divided into three groups. The locational effects are described below, and the placement of each JSC in one of the three groups for each of the three analyses is shown in Table 6-13.

6.6.1 Effects by Location in Washington

In considering the initiation of a state-wide bonus offer program, it is important to know if significant regional differences in response to the program can be expected. The map of the State of Washington provides the basis for an obvious division of the state into three areas. The Cascade Mountains unquestionably divides the state into a western and eastern region--the geography, climate and economic structures of the two parts of the state differ considerably, with the west being coastal, mountainous and industrial, while the east is dry, flat and agrarian. Further, within the western region, the Seattle Metropolitan Area is the modern, cosmopolitan industrial sector, heavily influenced by the Boeing Aircraft Company, while the remainder of western Washington is more sparsely populated with smaller towns and cities and dominated by the timber industry.

Table 6-14 displays the experimental results for each of three areas of the state: the Seattle Metropolitan Area, the rest of Washington west of the Cascades, and Washington east of the Cascades. The effects of the experiment on compensation are quite similar in Seattle and eastern Washington. Responses in western Washington excluding Seattle are weaker than in the other two areas, but none of the differences are statistically significant. The effects of the experiment on weeks of insured unemployment

Table 6-13

Geographic, Unemployment, and Employment Growth Data on Washington Job Service Centers

Job Service Center	Region of Washington	TUR	TUR Category	Employment Growth Rate	Employment Growth Category
Aberdeen	WESTXSEA	9.5	HIGH	-0.4	NEGATIVE
Auburn	SEATTLE	4.7	LOW	1.3	HIGH
Bellevue	SEATTLE	3.5	LOW	1.2	HIGH
Bellingham	WESTXSEA	6.6	MODERATE	0.4	LOW
Bremerton	WESTXSEA	5.0	LOW	0.6	LOW
Cowlitz County	WESTXSEA	7.5	HIGH	0.7	LOW
Everett	SEATTLE	5.4	MODERATE	1.5	HIGH
Lewis County	WESTXSEA	8.0	HIGH	0.8	LOW
Lynnwood	SEATTLE	4.3	LOW	1.4	HIGH
Moses Lake	EAST	9.7	HIGH	0.9	LOW
Mount Vernon	WESTXSEA	6.6	MODERATE	0.5	LOW
North Seattle	SEATTLE	4.0	LOW	1.1	HIGH
Olympia	WESTXSEA	5.9	MODERATE	0.8	LOW
Rainier	SEATTLE	6.1	MODERATE	1.1	HIGH
Renton	SEATTLE	4.4	LOW	1.1	HIGH
Spokane	EAST	5.3	MODERATE	0.1	LOW
Sunnyside	EAST	10.6	HIGH	-0.7	NEGATIVE
Tri-Cities	EAST	8.0	HIGH	-0.9	NEGATIVE
Walla Walla	EAST	6.8	MODERATE	-1.9	NEGATIVE
Wenatchee	EAST	9.3	HIGH	5.5	HIGH
Yakima	EAST	11.6	HIGH	3.1	HIGH

TUR: Total Unemployment Rate
Employment Growth Rate: The mean percentage change in area employment in the two months after experimental claimants filed for benefits.

Table 6-14

Treatment Impact by Location in Washington

Treatment	In Seattle: Parameter Estimate	In Seattle: Standard Error	West Washington Except Seattle: Parameter Estimate	West Washington Except Seattle: Standard Error	East Washington: Parameter Estimate	East Washington: Standard Error
			UI Compensation in the Benefit Year			
T1	29.26	64.03	78.01	103.56	-36.12	83.36
T2	-48.61	63.19	75.25	102.56	-98.92	82.26
T3	-79.37	71.76	-4.32	111.55	-197.40**	94.18
T4	-143.12**	62.51	50.02	102.03	-161.81**	82.76
T5	-24.23	62.91	-142.45	105.55	-3.81	81.34
T6	-187.71**	72.79	-22.55	116.84	-146.81	92.32
T1,4	-60.58	53.63	63.30	86.58	-99.68	70.01
T2,5	-36.30	53.52	-28.34	87.27	-50.42	69.25
T3,6	-132.37**	59.07	-12.41	93.41	-171.29**	76.12
All T's	-68.82	46.65	10.14	74.68	-98.76	60.23
			Weeks of Insured Unemployment			
T1	0.21	0.41	0.21	0.66	-0.66	0.53
T2	-0.13	0.40	0.45	0.66	-0.92*	0.53
T3	-0.40	0.46	0.12 †	0.71	-1.68**#	0.60
T4	-0.57	0.40	0.47 ††	0.65	-1.28**	0.53
T5	-0.08	0.40	-0.59	0.67	-0.35	0.52
T6	-0.94**	0.47	-0.30	0.75	-0.84	0.59
T1,4	-0.20	0.34	0.34 †	0.55	-0.97**	0.45
T2,5	-0.11	0.34	-0.05	0.56	-0.63	0.44
T3,6	-0.66*	0.38	-0.08	0.60	-1.25**	0.49
All T's	-0.28	0.30	0.09	0.48	-0.91**	0.38

SEATTLE: Auburn, Renton, Lynnwood, North Seattle, Rainier, Everett, Bellevue.
WESTXSEA: Bellingham, Bremerton, Mount Vernon, Olympia, Lewis County, Aberdeen, Cowlitz County.
EAST: Spokane, Moses Lake, Wenatchee, Yakima, Tri-Cities, Walla Walla.

* Coefficient significant at the 90 percent confidence level for a two-tail test.
** Coefficient significant at the 95 percent confidence level for a two-tail test.
\# Impact estimate for claimants filing in a Seattle office is significantly different from the estimate for claimants filing on other regional offices for a two-tail test at the 90 percent confidence level.
† Impact for claimants filing in a West-X Seattle office is significantly different from the estimate for claimants filing in an East office for a two-tail test at the 90 percent confidence level.
†† Impact for claimants filing in a West-X Seattle office is significantly different from the estimate for claimants filing in an East office for a two-tail test at the 95 percent confidence level.

differ more between the areas, with the weak response in western Washington more apparent. Treatment 3 response is significantly larger in eastern Washington than in either of the other two areas, and Treatment 4 response is greater in eastern than in western (excluding Seattle) Washington.

6.6.2 Effects by Total Unemployment Rate (TUR) in the area

It was important to determine if differences in economic conditions result in different responses to the experiment. Such differences might suggest that a bonus offer program should be triggered on and off as the economy changes. We measured economic conditions in two ways, first by the level of the unemployment rate (namely, the Total Unemployment Rate (TUR), and second by the rate of growth of employment in the area. Both relate to the level of opportunities in the area at the time of filing for benefits.

Theory does not provide sound guidance as to expectations in this regard. Poor economic conditions may mean that many job seekers are discouraged and do not put forth maximum effort to find a job, giving room for a bonus offer to change behavior. On the other hand, good economic conditions provides better opportunities for individuals to find jobs if they choose to search.

The first measure of economic opportunity is the TUR, defined for purposes of this analysis to be the TUR in the county in the month of filing for each treatment-enrolled filer. A TUR is calculated for each JSC, representing the average TUR over all the enrolled filers in that JSC. Table 6-15 shows the implications of dividing the 21 JSCs into three groups, depending upon the average TUR at the time of filing in the county in which the JSC is located. (See Table 6-13 for list of JSCs). The median TUR was 6.6 percent, and it ranged from a low of 3.5 percent in Bellevue to a high of 11.6 percent in Yakima. The three groups were formed rather arbitrarily, looking for natural breaks in the sequence of TURs across JSCs. We designate low TUR as being less than or equal to 5 percent, moderate TURs as being greater than 5 percent but not greater than 7 percent, and high TURs being over 7 percent.

According to Table 6-15, TUR seems to make a significant difference in eliciting response to the experiment. UI compensation declined most strongly in areas experiencing low TURs at the time of filing. Five of the six treatments had statistically significant responses in these areas. When treatments are combined across qualification periods, almost all of the treatment groups displayed statistically significant differences across TUR levels.

The results in weeks of insured unemployment were not nearly as strong as those displayed in UI compensation. The low TUR areas had statistically significant responses, which were larger than those in areas with higher TURs. However, none of the differences in response to treatments across areas were statistically significant.

Locationally, most of the low TUR JSCs were in Seattle, but the medium and high TUR areas were regionally mixed, as seen from the following tabulation showing the distribution of JSCs by location and TUR:

	TUR			
	Low	Moderate	High	Total
Seattle	5	2	0	7
Other West	1	3	3	7
East	0	2	5	7
Total	6	7	8	21

Although somewhat correlated with location, the differences exhibited in response by TUR cannot be primarily attributed to location.

Table 6-15

Treatment Impact by Total Unemployment Rate (TUR)
(standard errors in parentheses)

Treatment	Low TUR[a]	Moderate TUR[a]	High TUR[a]
T1	-114.92 (78.61)	120.43*## (72.69)	70.12 (87.61)
T2	-180.65** (76.96)	65.92## (72.17)	1.99 (86.74)
T3	-198.24** (86.90)	-16.79 (80.87)	-93.26 (99.02)
T4	-306.90** (76.51)	15.28## (71.22)	-44.39## (87.66)
T5	-134.04* (76.97)	87.09## (71.86)	-76.75 (87.04)
T6	-297.36** (87.62)	-59.94## (82.41)	-44.01## (98.99)
T1,4	-215.52** (65.64)	65.96## (60.85)	12.92## (73.87)
T2,5	-157.48** (65.30)	76.58## (60.90)	-37.11 (73.39)
T3,6	-247.24** (71.50)	-37.74## (66.68)	-68.53# (80.72)
All T's	-201.45** (56.89)	44.07## (52.77)	-26.52## (63.63)

[a] Low TUR = ≤ 5 percent; Moderate TUR = 5 percent < TUR ≤ 7 percent; High TUR = > 7 percent.

* Coefficient significant at the 90 percent confidence level for a two-tail test.
** Coefficient significant at the 95 percent confidence level for a two-tail test.
\# Impact estimate for claimants filing in a Low TUR office is significantly different from the estimate for claimants filing in Moderate or High TUR offices for a two-tail test at the 90 percent confidence level.
\## Impact estimate for claimants filing in a Low TUR office is significantly different from the estimate for claimants filing in Moderate or High TUR offices for a two-tail test at the 95 percent confidence level.

6.6.3 Effects by Rate of Employment Growth in the Area

Employment growth is uniquely defined for this analysis to be the mean percentage change in employment in the county of filing for each treatment-enrolled filer over the two month period starting with the month of filing. Thus, the employment growth rate defines the new employment opportunities facing each filer at the time of filing. for each JSC, an employment growth rate is calculated as the average over all the treatment-enrolled filers at that JSC. The median employment growth rate over the 21 JSCs was 0.85 percent, and it ranged from -1.9 percent in Walla Walla to a high of 5.5 percent in Wenatchee. The 21 JSCs were divided into three groups as follows: those JSCs in which filers on average experienced negative change in employment over the two months following filing, those JSCs in which filers on average experienced positive growth less than 1 percent over the two month period, and those JSCs in which filers on average experienced growth of 1 percent or more over the two-month period. (See Table 6-13).

The emerging picture is not as strong for employment growth as for the unemployment rate, as very large coefficients in the low growth areas are negated by high standard errors, resulting in only one of the six treatments in low growth areas eliciting statistically significant responses. See Table 6-16. Four of the six treatments in high growth areas showed statistically significant responses. Although the coefficients indicate greater response in low growth areas, the differences between responses in high and low growth areas were not statistically significant. Areas in which growth actually declined, however, did seem to be associated with lower response. High bonus offer treatments (Treatments 3 and 6) elicited large, statistically significant, responses in both UI compensation received and weeks of insured unemployment in both low and high growth areas. Declining areas had no statistically significant responses, but the differences were statistically significant in only a few treatments.

Table 6-16

Treatment Impact by Employment Growth in Area

Treatment	High Growth[a]		Low Growth[a]		Negative Growth[a]	
	Parameter Estimate	Standard Error	Parameter Estimate	Standard Error	Parameter Estimate	Standard Error
T1	-38.36	60.94	-16.57	157.42	121.24	77.23
T2	-115.03*	60.01	-144.26	160.83	99.35##	76.54
T3	-121.68*	68.08	-195.05	179.84	-59.79	85.31
T4	-145.42**	60.26	-169.82	157.27	-65.12	74.82
T5	-37.84	60.86	-147.74	156.34	-12.85	74.75
T6	-121.53*	68.19	-417.88**	175.07	-104.08	88.17
T1,4	-92.91*	51.17	-93.00	133.44	22.78	64.28
T2,5	-77.38	51.07	-146.07	134.33	41.53	63.94
T3,6	-121.61**	55.71	-311.07**	145.35	-80.79	70.76
All T's	-94.38**	44.24	-168.28	116.14	4.79	55.72

[a] Growth is defined as percentage change in Employment in the county in the two months after filing. Negative growth = < 0.0 percent; Low growth = 0.0 percent < growth < 1 percent; High growth = ≥ 1 percent.

* Coefficient significant at the 90 percent confidence level for a two-tail test.
** Coefficient significant at the 95 percent confidence level for a two-tail test.
\# Impact estimate for claimants filing in a High growth office is significantly different from the estimate for claimants filing in a Low or Negative growth office for a two-tail test at the 90 percent confidence level.
\## Impact estimate for claimants filing in a High growth office is significantly different from the estimate for claimants filing in a Low or Negative growth office for a two-tail test at the 95 percent confidence level.

6.7 Summary of Results for Subgroup Analyses

In this chapter we have estimated and compared WREB treatment impacts for a variety of population subgroups. This exercise has provided a deeper understanding of the effects of the bonus offer and laid a foundation for using WREB as a basis for an incentive policy. We have measured bonus impacts for potential policy target groups, and highlighted possible biases toward certain population subgroups. This chapter includes subgroup analyses by worker dislocation status, gender, race/ethnicity, age, industry, occupation, base period earnings, and WREB interview procedure.

Defining worker dislocation based on recent attachment to the labor force, under a narrow definition of attachment to an employer or an industry, the treatment impact estimates are generally neither significant for dislocated workers nor different from nondislocated workers. Using a broader definition of worker dislocation--being continuously employed in recent years--the high bonus treatments had large and significant effects for dislocated workers which were also significantly different from impacts for nondislocated workers. Thus, there is statistical evidence that claimants with continuous work histories respond more strongly to a high bonus offer than do workers without such work histories, but these results do not hold for dislocated workers defined more narrowly.

Males showed a significant response to WREB bonus offers while females did not, and even though the differences in treatment impacts across gender were large in magnitude they were not statistically significant.

For compensation in the benefit year, the impact estimates for whites correspond closely to those estimated on the full sample, and there are no statistically different impact estimates for any treatment among the race/ethnic subgroups. While not statistically significant, the results suggest that when offered a reemployment bonus, blacks respond less than whites, Hispanics respond similar to whites, and persons in the other racial/ethnic group (American Indians, Asians, Eskimos, and Pacific Islanders) respond even more strongly than whites.

Older claimants (aged 45 and over) drew an average $135 less in UI over the benefit year in response to the WREB offers. This impact estimate is statistically significant, but because of high variation in the impact it is not significantly different from the treatment response for younger workers (under 45). Treatments 2, 3, 4, and 5 all had much larger impacts on compensation for the older workers, but only for treatment 4 was the difference from younger workers statistically significant.

In the analysis of impacts by industry of prior employment, high bonus offers led to large and statistically significant treatment impacts by persons previously employed in mining or manufacturing. In magnitude, the impacts were more than twice as large as those estimated on the full sample, and were statistically different from the impacts on claimants from other industries. Furthermore, claimants previously employed in mining or manufacturing is the only subgroup examined to exhibit a consistent pattern of increasing treatment impact with increasing size of the cash bonus offer.

In a full interaction analysis of impacts by age, gender and race/ethnicity, the average treatment response of younger black males is very different from the response of all other subgroups. Despite the small sample size, younger black males show a statistically significant response which is opposite to expectations. The bonus offer apparently caused members of this group to <u>increase</u> the level of compensation they received. Adding industry of previous employment to the full interaction analysis yielded results similar to the above: young black or Hispanic males and females in manufacturing either did not respond to the experimental treatment, or may have responded by actually lengthening their spell of unemployment. Other subgroup responses were not significantly different from one another.

Using base period earnings as a factor for comparing treatment impacts, we found statistically significant differences in impacts by earnings and age. The average impact on UI compensation drawn by high earning/older workers and low earning/younger workers were very strong. However, high earning/young claimants did not respond to the experimental treatment, and their response was statistically different from that of the other earnings/age groups. When distinguished by earnings and either race/ethnicity or gender, the results are ambiguous. Low earning males and low earning nonminorities showed a statistically significant average treatment response. However, none of the differences between the earnings/race, or earnings/gender groups were statistically significant.

The differences in response by earnings and age probably occur because the bonus offer elicits the strongest response where there is room for response, i.e., where job search prior to the bonus offer is suboptimal. The high earning/older workers might be dislocated workers whose strong response is possible because of a discouraged worker syndrome that provides an opportunity for incentives to change job search behavior. The low earning/young workers may be those not yet strongly attached to the workforce who can be encouraged to increase job search. Furthermore, the high earning/young workers may be those most strongly attached to the workforce, who are already maximizing their job search effort, and whose job search behavior cannot be successfully increased by a reemployment bonus incentive.

In considering the initiation of a state-wide bonus offer program, it is important to know if significant differences in response to the program can be expected by region or by an area's economic condition. The latter is particularly useful for designing a program that would be triggered by specific economic events. Geographically, Washington can be easily depicted as comprising three major areas: Seattle, the rest of Washington west of the Cascades, and eastern Washington. The regional differences, though not overwhelmingly strong, did indicate a tendency for there to be lower responses in western Washington (excluding Seattle) than in Seattle or in eastern Washington. Differences due to economic conditions in the area were more pronounced, with strong responses in both compensation and weeks of insured unemployed occurring in areas with low unemployment (measured by the Total Unemployment Rate at the time each claimant filed for UI benefits). In compensation, the differences in response between areas with low unemployment rates and other areas was statistically significant.

CHAPTER 7

EXPERIMENTAL EFFECTS ON OTHER ECONOMIC OUTCOMES

The intended effects of the bonus offer program, to reduce unemployment and to reduce the amount of UI compensation paid to claimants were studied in Chapters 5 and 6. Other possible effects of the experiment on economic outcomes, some intended and some unintended, are studied in this chapter. Two unintended negative effects would be a reduction in earnings that could result from taking poorer jobs than would have been taken without the bonus offer, and a loosening of attachment to the separating employer in cases where the employer intended to recall the separated claimant.

Other possible secondary effects of the bonus offer investigated in this chapter are: (1) union affiliation and placement through union hiring halls, (2) use of employment services provided through the Job Service Centers, (3) intensity of job search efforts, (4) job turnover, (5) becoming self-employed, and (6) contribution to household income by the participating claimant.

7.1 Effects of the Bonus Offer On Post-Filing Earnings

The following question needs to be addressed regarding the effect of the bonus offer: To the extent that the bonus offer encourages claimants to obtain jobs sooner than they otherwise would, are the jobs better or worse than they would have obtained or kept without the bonus offer? Job search theory suggests that if search had been optimal before the bonus offer, then speeding reemployment implies taking jobs that are somehow less than optimal. Jobs may be compared across many characteristics, as job satisfaction depends upon more than simply the wage, but data on other aspects of job satisfaction are not available to us. In fact, the only data available from administrative records for wage comparison are total quarterly earnings. For the post-termination earnings analysis, the full quarter <u>after</u> the quarter in which the claimant terminated receipt of UI benefits is used. To analyze the change in earnings the full quarter before the quarter in which the claimant filed for benefits is also used.[1]

7.1.1 Overall Effects on Earnings

Table 7-1 shows the effect of each of the six treatments on quarterly earnings in the first full quarter after terminating benefits for those claimants who obtain post-filing jobs. The model was run on the 10,099 claimants who were either in a treatment or control group, terminated benefits before exhaustion, and had wages in the UI Wage File in both the full quarter before filing and the one after termination of benefits. The model uses the control set in Appendix F and an additional control variable for earnings in the full quarter prior to filing for benefits. This variable is added to compensate for any differences among treatments in post-termination wages that are related to differences in pre-filing wages.

As reported in the top row of Table 7-1, for all claimants the bonus offer does not significantly affect earnings. In spite of the small but consistently negative impacts on earnings, the lack of statistical significance persists whether impacts are examined by individual treatment, by bonus level, or as an average across all treatments. In other words, we find no evidence that the bonus offer leads claimants to accept jobs that provide lower earnings (wage rate and/or hours worked) than would have been obtained without the bonus offer.[2]

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[1] The terminating and filing quarters are not used in the analysis, because earnings occur in only part of these quarters, and their use would introduce censoring problems. If the experiment reduces the length of unemployment, then this reduction would affect observed quarterly earnings, thereby inflating the estimated treatment impact on earnings. As shown in Appendix B, there is no bias introduced by studying only claimants with post-termination wages.

[2] An alternative approach to testing the experimental effect on earnings uses the difference in pre- and post-filing wages as the dependent variable. The mean change in wages is a negative $248. This decline in wages may reflect a general tendency for UI claimants to take less satisfactory jobs, or it may simply be a wage reporting problem (some wage credits for UCX, UCFE, and former state employees may not be included in the file for post-filing wages). However, since the control and experimental subjects face the same reporting problem, there is no reason to believe that the model in change form produces biased

Table 7-1

Treatment Effects on Quarterly Earnings

	1	2	3	4	5	6	T1,4	T2,5	
All Claimants	-55.75 (114.53)	-88.99 (113.41)	-92.66 (127.00)	-76.91 (111.61)	-78.18 (112.42)	-22.49 (126.83)	-66.84 (95.63)	-83.53 (95.56)	- (1
Males	14.52 (162.64)	-33.50 (160.73)	-6.90 (181.40)	-32.07 (157.67)	-33.74 (160.08)	41.15 (179.49)	-10.10 (135.88)	-33.70 (136.10)	(1
Females	-168.25 (142.54)	-189.67 (141.68)	-264.29* (156.46)	-148.95 (139.99)	-125.39 (139.35)	-130.64 (158.50)	-158.28 (118.89)	-156.59 (118.49)	-1 (1
Whites, except Hispanics	12.73 (125.34)	-108.72 (123.40)	-52.52 (137.98)	-48.48 (122.01)	-32.55 (122.55)	8.92 (137.57)	-19.37 (104.57)	-70.20 (104.18)	- (1
All Other Race/Ethnic Groups	-457.76 (282.53)	43.44 (288.69)	-351.26 (326.09)	-232.60 (276.86)	-351.26 (283.45)	-254.89 (329.47)	-340.89 (237.02)	-160.29 (240.77)	-3 (2
Hispanic Only	-801.42* (483.75)	-513.52 (491.01)	-513.36 (545.73)	-640.65 (458.29)	-835.76* (474.61)	-544.53 (568.34)	-713.00* (396.39)	-684.80* (406.07)	-5 (4
Black Only	-313.93 (418.90)	-125.08 (443.20)	-337.58 (494.02)	167.95 (427.17)	160.51 (451.68)	-270.82 (485.25)	-86.62 (364.96)	14.28 (379.27)	-3 (4
Age > 45 years	271.90 (241.80)	-146.08 (240.72)	-79.31 (260.15)	157.52 (223.27)	83.48 (231.78)	160.53 (268.59)	206.41 (195.45)	-23.99 (198.06)	(2
Dislocated Workers									
12 Qtr/Same Employer	-98.20 (270.64)	-557.84** (278.31)	-430.68 (283.33)	-483.53* (256.05)	-187.41 (254.53)	-438.39 (291.51)	-309.29 (222.18)	-344.60 (223.42)	-4 (2
12 Qtr/Same Industry	-139.92 (218.59)	-487.27** (222.02)	-450.72* (236.11)	-342.59 (213.29)	-227.94 (211.88)	-405.69* (237.11)	-245.60 (182.29)	-347.23* (182.77)	-4 (1
12 Qtr/Continuous Employment	-43.88 (175.89)	-221.04 (175.51)	-98.98 (191.61)	-217.31 (170.93)	-112.15 (169.49)	-153.18 (190.40)	-134.90 (146.63)	-163.26 (146.02)	-1 (1

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

7.1.2 Effects on Earnings of Population Subgroups

Tests were run on population subgroups distinguished by the following characteristics: gender, race/ethnicity, age, and dislocation status. These results are shown in Table 7-1. By gender, there were no significant effects. For males, the coefficients were small, varying in sign across treatments and with large standard errors. For females, the coefficients were much larger, but only treatment 3 produced statistically significant reductions in earnings.

The race/ethnic group evaluations are inconclusive, but indicate that Hispanics and blacks may have lost earnings in responding to the experiment. There were no effects on earnings of non-Hispanic whites. The impact coefficients were slightly smaller than for the population as a whole, with large standard errors. For other ethnic groups the results were mixed, as the coefficients were negative, often large, but rarely statistically significant. For Hispanics, the treatment effect coefficients were ridiculously large. Although the overall effect of the experiment on Hispanic earnings was negative, large, and statistically significant, the analysis is inconclusive, because of the lack of consistent results across treatments.

Effects on earnings for claimants defined as being dislocated workers are shown in Table 7-1 using the three definitions: (1) having been employed by the same employer for the 12 quarters prior to filing, (2) having been employed in the same industry for the 12 quarters prior to filing, and (3) having been continuously employed for 12 quarters.[3] These results are quite surprising, showing large negative and statistically significant

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estimates. The treatment impact estimates from the model using the change in earnings as the dependent variable did not differ from those in the model using post-termination earnings as the dependent variable, except that the coefficients were less stable across treatments and the standard errors were somewhat larger.

[3] To be classified as dislocated, the additional condition that workers not be awaiting recall is also imposed.

impacts on earnings of dislocated workers defined by two of the definitions. The average treatment using the first or second definition of dislocated workers appeared to cause a reduction in post-termination quarterly earnings of over $300. There is also a progression of coefficients from the low to the high bonus multiplier, with statistically significant coefficients on the middle and high bonus multiplier treatments. Although not shown in the table, treatment impact estimates using the pre- to post-filing earnings change as the dependent variable are large and negative, which is consistent with these findings. However, in the change model, the coefficients are somewhat smaller and not statistically significant. All these effects disappear when dislocated workers are defined as having been continuously employed for 12 quarters. The impact coefficients drop to half the magnitude found using the other definitions and are not statistically significant.

These results are surprising because only for the third definition (i.e., 12 quarters of continuous employment) is there evidence that dislocated workers responded to the bonus offer more strongly than nondislocated workers (See Tables 6-1 through 6-3). Thus, the finding that the treatment negatively affected earnings of dislocated workers whose compensated unemployment did not differ from that of nondislocated workers (definitions one and two), while the treatment had no discernable effect on earnings of dislocated workers whose compensated unemployment was shorter (at least with regard to the high bonus multiplier), is hard to reconcile.

7.2 Effects of the Bonus Offer on Employer Attachment

A design element in the experiment that could be of concern to employers is the explicit prohibition against paying bonuses to claimants who return to their previous job.[4] An original intent of the unemployment insurance system was to help maintain employer-employee relationships in times of slack demand. We might expect employers to oppose a bonus offer program that rewarded workers for seeking other employment.

[4] Bonuses were paid to participants hired by their previous employer for a different job.

We addressed this concern by investigating whether those in the experimental treatments who returned to work before exhausting benefits were more or less likely than controls to return to their previous employer. The question was addressed for the total sample and for those on standby. It is the latter group that is of particular interest, since for this group the employer has explicitly stated a desire to retain the employee.

7.2.1 Return to Separating Employer: Evidence from the Full Sample

As shown in Table 7-2, we identify 10,060 claimants in the experiment who went back to work prior to exhausting benefits. These claimants either served a waiting week within three weeks of filing and terminated benefit payments before exhaustion and had wages in the post-termination quarter, or they did not serve a waiting week and had wages in the post-filing quarter. Of these claimants who went back to work, 44 percent returned to their separating employer and 56 percent went to work for another employer.

Table 7-2

Analysis of Claimant Sample
To Determine Who Went Back To Work

	All Claimants	Claimant Served Waiting Week ≤ 3 weeks after filing	Served No Waiting Week
Total in Sample	15,534	13,754	1,233
Minus: Waiting week served but not within weeks of filing	549	--	--
Minus: Did not terminate benefits	2,485	2,485	--
Minus: Did not have earnings after terminating benefits	2,440	2,168	272
Yields: Claimants Back to Work	10,060	9,101	961
Minus: Returned to prior employment	4,419		
Yields: Did not return to prior employment	5,641		

It is possible that the bonus offer slightly reduced the probability of a claimant returning to his/her separating employer. Table 7-3 shows that for the combined middle-level bonus offers, there was a statistically significant reduction in return to the separating employer of 2.4 percent. However, the absence of an effect on return to previous employer for the high Weekly Benefit Amount (WBA) multiple treatments make the results suspect, since it was only the high-level treatments that induced a

Table 7-3

Treatment Impact on Probability of Return to Previous Primary Employer Using Administrative Data

Variable	Parameter Estimate	Standard Error
All Claimants Who Became Reemployed (N = 10,060)		
INTERCEPT	0.450**	0.010
T1	-0.025	0.016
T2	0.023	0.016
T3	0.010	0.018
T4	0.007	0.016
T5	-0.025	0.016
T6	-0.016	0.018
T1,4	-0.008	0.013
T2,5	-0.024*	0.013
T3,6	-0.003	0.015
TREAT	-0.013	-0.012
Claimants Who Become Reemployed and Were On Standby (N = 1,824)		
INTERCEPT	0.775**	0.021
T1	-0.013	0.032
T2	0.040	0.032
T3	0.053	0.036
T4	0.030	0.031
T5	-0.019	0.033
T6	0.012	0.037
T1,4	0.009	0.027
T2,5	0.011	0.027
T2,6	0.034	0.030
TREAT	0.016	0.023

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

statistically significant response in compensation or weeks of unemployment. Therefore, we are inclined to conclude that the data do not support the finding of a significant treatment impact on the probability of returning to the previous employer.

Employers are most concerned about the attachment of workers placed on standby. We identified 2,134 claimants in the experiment as being on standby; of these, 79 percent were recalled by their separating employer and the remainder went on to other jobs.[5] The analysis of those on standby who went back to work (Table 7-3) wouldstrongly suggest that employers have nothing to fear from the bonus program. These results show no statistically significant effects of the bonus treatment on the probability of standby workers returning to their previous employer. The coefficients, in fact, are generally positive, although not statistically significant for individual treatments, or combinations of treatments.

Table 7-4

Comparison of Using Survey Sample and Full Sample to Estimate Probability of Recall Using Administrative Data

	Full Sample		Survey Sample	
	Parameter Estimate	Standard Error	Parameter Estimate	Standard Error
INTERCEPT	0.450**	0.010	0.475**	0.028
TREATMENT	-0.013	0.012	-0.007	0.032
Sample Size	10,060		1,327	

** Statistically significant at the 95 percent confidence level.

Note: Both equations included the control variable set described in Appendix F when estimated.

[5] Ninety-six percent of the workers on standby terminated benefits prior to exhausting entitlement. All the workers on standby went back to work, since all had earnings in the quarter following termination of benefits.

7.2.2 Return to Separating Employer: Evidence from Survey Respondents

In this section, we report on treatment effects on return to previous employer by respondents to the follow-up survey. First, to test the usefulness of the survey data to study recall, we re-estimated the recall equation that had been reported in Table 7-3 for the survey respondents. The same data and variables used above were used on the restricted sample of 1,327 survey respondents who returned to work. Table 7-4 shows that both the full sample and the survey sample have the same probability of recall, using administrative data and the same criteria. In both sets, the control groups have recall probabilities just under 50 percent, and the parameter estimates for the average over all treatments indicate very small, not statistically significant, effects of the treatments on recall.

We now turn to the survey data to analyze the responses of 1,459 respondents who stated in the survey that they had found employment before the end of their benefit year.[6] Two different definitions were used to analyze recall using survey data: (1) the first employer after filing is the same as the employer listed on the UI application as the separating employer; and (2) the first employer after filing is the primary employer in the five years prior to filing.[7] As seen in Table 7-5, the control group experienced a 36 percent probability of being recalled under recall definition 1, with the experimental treatment causing a large and statistically significant reduction in recall probability among those who returned to work. The average reduction in recall over all treatments was a statistically significant 6.0 percent. The results were somewhat stronger with recall defined as returning to the major employer in past five years (Recall 2). The results,

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[6] This number is larger than the sample used in Table 7-4, because reemployment is self-defined in the survey and includes some claimants who did not have wage records indicating reemployment.

[7] The survey information differs from that provided in the administrative file by explicitly identifying the separating and new employer, instead of relying on the more ambiguous information provided in the quarterly wage file. On the other hand, self-reported data may be less accurate, and the sample of survey respondents is smaller and perhaps a biased subset of the total claimant population.

however, differed greatly from the findings using the total sample (reported in Table 7-3), in which the average treatment effect was a statistically insignificant 1.3 percent.

Table 7-5

Treatment Impact on Probability of Return to Separating Employer Using Survey Responses

Variable	Recall 1 Parameter Estimate	Recall 1 Standard Error	Recall 2 Parameter Estimate	Recall 2 Standard Error
INTERCEPT	0.356**	0.026	0.353**	0.026
T1	-0.087**	0.039	-0.088**	0.039
T2	-0.048	0.039	-0.076*	0.039
T3	-0.051	0.044	-0.019	0.044
T4	-0.033	0.038	-0.040	0.038
T5	-0.078**	0.038	-0.093**	0.038
T6	-0.062	0.044	-0.085**	0.043
T1,4	-0.059*	0.033	-0.063*	0.033
T2,5	-0.063*	0.033	-0.084**	0.033
T3,6	-0.057	0.036	-0.053	0.036
TREAT	-0.060**	0.029	-0.069**	0.029
N = 1,459				

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

Recall 1: The first employer after filing is the same as the employer listed on the UI application as the separating employer.

Recall 2: The first employer after filing is the major employer in the five years prior to filing.

7.3 Effect of the Experimental Treatment on Union Membership

Included in this study were survey respondents who found employment by the end of the benefit year and were union members prior to filing for benefits. Out of the sample of 1,900 survey respondents, 391 met the conditions for inclusion in this analysis. In the control group, 32 percent of the union members switched to nonunion jobs upon becoming reemployed. Although the average treatment impact coefficient indicated an experimentally induced 2.3 percent lower probability of switching, the coefficient was not statistically significant at the 90 percent confidence level, and no individual treatment coefficient was statistically significant, although most coefficients had negative signs. (see Table 7-6). The survey results indicated that the experiment had either a small positive impact or no impact on the probability of a union member returning to a union job.

Table 7-6

Treatment Impact on Probabilities of Switching From a Union to a Nonunion Job and Being Placed on a Job through a Union

	Probability of Switching to Nonunion Job		Probability of Being Placed by Union	
Variable	Parameter Estimate	Standard Error	Parameter Estimate	Standard Error
INTERCEPT	0.319**	0.050	0.089**	0.015
T1	-0.029	0.070	-0.013	0.022
T2	-0.061	0.073	-0.015	0.022
T3	-0.057	0.087	0.004	0.025
T4	-0.020	0.073	-0.017	0.022
T5	-0.017	0.074	-0.029	0.022
T6	0.060	0.083	-0.008	0.025
T1,4	-0.024	0.061	-0.015	0.019
T2,5	-0.039	0.062	-0.022	0.019
T3,6	0.005	0.070	-0.002	0.020
TREAT	-0.023	0.055	-0.015	0.016
N =	391		1,459	

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

7.4 Effects of the Experimental Treatment on the Probability of Being Placed on a Job Through a Union Hiring Hall

All survey respondents who returned to work by the end of the benefit year were included in this study. Of the sample of 1,900 respondents, 1,459 met this condition.[8] In the control group, 8.9 percent of the sample was placed on the job through a union hiring hall. The parameter estimate of the impact of the average treatment impact, was -1.5 percent. Although a large coefficient relative to the control group average, it was not statistically significant at the 90 percent confidence level (see Table 7-6). Estimates for each of the individual treatments indicated a reduction in placement through hiring halls. Using ordinary least squares (OLS), none of the treatment impact estimates were statistically significant; however, using binary probit, the impact estimate for treatment 5 (middle bonus, long qualification period) was statistically significant at the 90 percent confidence level, and the overall average treatment effect estimate was quite large. Thus, there is weak evidence that the experiment had a negative effect on the probability of a claimant being placed on a job through a union hiring hall.

7.5 Effect of the Experimental Treatment on Use of the Job Service Center for Work Search Assistance

We hypothesize that if the experiment encouraged more intensive work search, this should have involved a greater demand for search assistance and greater use of the Employment Service (ES). Survey respondents were specifically asked if they used the ES for help in work search, and if so, which of a list of services they used. The analysis was conducted on a set of 1,034 survey respondents who either were registered for work search or responded "yes" when asked if they used the ES for work search. In the control group, 61 percent actually used one or more of the services available from the Employment Service, and there was no statistically significant difference between the

[8] Again, return to work was established by earnings in the quarter following termination of benefits.

control group and any of the treatment groups (see Table 7-7). Overall, the parameter estimate measuring the effect of the average treatment effect on use of the Employment Service was less than 1 percent, and it was not statistically significant.

Of the 620 claimants who did use one or more services, we tested to see if the treatment group tended to use more of these services. On the average, a control group member used 2.2 services, and there was no statistically significant or consistent difference between the treatment and control group. The parameter estimate for the average treatment was a trivial -0.02, which was not statistically significant.

Of the 620 who used the ES for work search assistance, 97, or 16 percent, said it was instrumental in getting them a job. There was no apparent difference in this statistic between treatment and control group members.

All in all, we found no evidence that the bonus offer encouraged an increase in the use of the Employment Service for job search assistance.

7.6 Effect of the Experimental Treatment on Job Search Intensity

The survey provided information on the timing and number of employer contacts that were used to test the hypothesis that the bonus offer increased the intensity of job search. Respondents were asked the date of the first time they talked to an employer about a job; 1,161 respondents provided sufficient information to permit us to analyze the experimental impact on the elapsed time from the date of filing for benefits to this first contact. For the control group, the first contact occurred an average of 12.1 days after filing, and there was no statistically significant difference for the experimental group; moreover, the estimated treatment impact coefficients were unstable, with three treatment impact estimates being negative and the other three being positive with a range from one to two and one-half days.

Table 7-7

Effect of the Experiment on Use of Employment Service (ES)
(standard errors in parentheses)

Variable	Proportion of Claimants Using ES Services	Number of Services Used	Proportion of Claimants for Whom ES Services Resulted in a Job
INTERCEPT	0.606**	2.221**	0.169**
	(0.033)	(0.115)	(0.032)
T1	0.004	0.128	0.012
	(0.052)	(0.182)	(0.050)
T2	-0.046	-0.188	-0.078
	(0.053)	(0.189)	(0.052)
T3	-0.028	-0.203	0.010
	(0.059)	(0.208)	(0.057)
T4	0.076	-0.071	-0.030
	(0.051)	(0.171)	(0.047)
T5	-0.048	0.113	-0.022
	(0.050)	(0.179)	(0.049)
T6	-0.022	0.083	0.036
	(0.058)	(0.206)	(0.057)
T1,4	0.043	0.020	-0.011
	(0.043)	(0.149)	(0.041)
T2,5	-0.047	-0.026	-0.048
	(0.043)	(0.153)	(0.042)
T3,6	-0.025	-0.058	0.023
	(0.047)	(0.168)	(0.046)
TREAT	-0.009	-0.015	-0.015
	(0.037)	(0.130)	(0.036)
N =	1,034	620	620

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

There were 1,317 responses to the less specific question, "In the week that you made your first contact, how many did you make?" The average member of the control group made 4.4 employer contacts in that first week of contact. On average, the treatment group made 0.3 fewer contacts, but the difference was not statistically significant.

Claimants were asked to state the total number of different employers to whom they spoke about getting a job during the entire period of compensated unemployment. This number was divided by the duration of their spell of unemployment to obtain an average number of weekly employer contacts. For the control group, an average of 1.27 contacts were made per week of unemployment. For the treatment group, the number of weekly contacts was significantly greater (see Table 7-8). On average, treatment group members made 0.79 more contacts per week, which was statistically different from the control group number at the 99 percent confidence level. Although the pattern of effects across treatments did not consistently increase with the size of the bonus offer, the largest effect did occur for the treatments with the high WBA multiple (T3 and T6).

7.7 Effects of the Bonus Offer on Length of Time on the First Post-Filing Job

It is hypothesized that if job search prior to the bonus offer was optimal and the bonus encouraged more rapid reemployment, the consequence may be acceptance of less satisfactory jobs. In Section 7.1, we determined that the evidence did not substantiate a finding of an experimental effect on post-filing earnings. In this section, we discuss the effects on the length of time the first post-filing job is held. An increase in job turnover might also be indicative of a less optimal match. The survey data indicated that among the control group, the average length of stay (including jobs still held at the time of the survey, truncated to the length of time from job start to the date of the survey) was 37.2 weeks, which is clearly downward biased because of the truncation. The results, however, are encouraging for the experiment in that the treatment parameters are mostly positive (five out of the six treatments), although not statistically significant (see

Table 7-8

Effects of the Experiment
on Number of Employer Contacts per Week

Variable	Parameter Estimate	Standard Error
INTERCEPT	1.268**	0.272
T1	0.740*	0.418
T2	0.586	0.422
T3	0.815*	0.469
T4	1.011**	0.409
T5	0.528	0.412
T6	1.165**	0.476
T1,4	0.880**	0.350
T2,5	0.556	0.352
T3,6	0.986**	0.385
TREAT	0.786**	0.304
N = 1,223		

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

Table 7-9

Effects of the Experiment
on Length of Time on First Job

Variable	Parameter Estimate	Standard Error
INTERCEPT	37.184**	1.175
T1	-1.327	1.773
T2	1.839	1.799
T3	1.778	2.037
T4	1.010	1.767
T5	2.144	1.804
T6	2.325	1.977
T1,4	-0.151	1.501
T2,5	1.991	1.520
T3,6	2.069	1.642
TREAT	1.188	1.311
N = 1,276		

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

Table 7-9). Thus the results suggest that the treatments either had no effect on job turnover, or may have caused a slight decrease in turnover.

7.8 Effects of the Bonus Offer on the Probability of Being Self-Employed

A small proportion of UI claimants did not return to a wage earning job, but rather became self-employed. Using the 1,459 survey respondents who found employment before the end of their benefit year for the analysis, it was estimated that 6.4 percent of the control group became self-employed. The parameter estimates on the treatments indicated a possible tendency for the experiment to cause a reduction in the proportion becoming self-employed (see Table 7-10). The coefficient for the average treatment was -1.7 percent, indicating a relatively large effect. Almost all of the individual treatment coefficients (except T6) were negative. However, none of the coefficients were statistically significant at the 90 percent confidence level, using either an OLS or a binary probit model. Since self-employment constituted bona fide reemployment in the bonus offer program, there should not have been an incentive to shift away from this labor market outcome. Although the estimated effects indicate the possibility of a negative effect on self-employment, the absence of statistically significant results precludes us from drawing any conclusion.

7.9 Effect of the Bonus Offer on the Proportion of Household Income Contributed by Claimant's Earnings

Survey respondents were asked what proportion of total household income was from their job earnings before filing and at the time of the survey; 1,815 respondents supplied the two figures. By comparing the proportions, it was determined that control group members experienced an 8.5 percent decline in their contribution to household income, whereas claimants assigned to the experiment experienced an average decline of only 5.2 percent, and the difference between treatments and controls was statistically significant at the 95 percent confidence level. Table 7-11 shows that all six treatment

Table 7-10

Effects of the Experiment on the Self-Employed

Variable	Parameter Estimate	Standard Error
INTERCEPT	0.064**	0.013
T1	-0.024	0.020
T2	-0.017	0.020
T3	-0.027	0.022
T4	-0.014	0.019
T5	-0.026	0.019
T6	0.012	0.022
T1,4	-0.019	0.017
T2,5	-0.022	0.017
T3,6	-0.007	0.018
TREAT	-0.017	0.014
N = 1,459		

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

Table 7-11

Effects of the Experiment on Proportion of Household Income Accounted for by Claimant

Variable	Parameter Estimate	Standard Error
INTERCEPT	-8.486**	1.412
T1	2.501	2.183
T2	6.120**	2.210
T3	1.356	2.478
T4	3.580*	2.144
T5	1.369	2.174
T6	4.441*	2.459
T1,4	3.050*	1.827
T2,5	3.682**	1.844
T3,6	2.910	2.010
TREAT	3.246**	1.584
N = 1,815		

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

coefficients were positive, and three of them (T2, T4, and T6) were statistically significant. However, the pattern of results did not conform to differences in the size of the bonus multiplier. The survey results do tend to support the conclusion that participants in the experiment did better than controls in maintaining their positions as providers within the household.

7.10 Summary of Impacts on Other Economic Outcomes

In this chapter we investigated a number of possible secondary effects of the bonus offer program. The effect of the experiment on job quality is a major concern because if the bonus offer encourages claimants to accept jobs more quickly, the new jobs might be of lower quality than those obtained after a more time-consuming search. To study whether the experiment had a negative effect on job quality, we examined quarterly earnings following benefit termination. On average, treatment-assigned claimants experienced a $70 decline in post-termination quarterly earnings, but this estimate was not statistically significant. We conclude that the evidence does not support a finding that the bonus offer led to acceptance of lower paying jobs. In testing effects on earnings by gender, race/ethnicity, and age we found some large negative coefficients, but only Hispanics had a statistically significant decline in earnings after a bonus offer.

The most important finding was that dislocated workers, defined as having been employed by the same employer or in the same industry in the 12 quarters prior to filing for benefits, suffered a statistically significant $300 average loss in quarterly earnings when offered a reemployment bonus. A $400 loss was estimated for the high WBA multiplier treatments. If correct, this effect would eliminate any possible net benefit to these claimants from the bonus offer. However, dislocated workers defined more broadly as simply having worked 12 consecutive quarters before filing may not have suffered an earnings loss. This group had an average estimated loss of about $140 when offered a bonus, which was not statistically significant.

One other indicator of job quality that we were able to test was the length of time on the first post-filing job. Increased job turnover would indicate less satisfactory job matches. Using the survey data, we found no evidence of increased turnover.

The follow-up survey was used to test for several other possible side effects. The results are summarized for the average of the six treatments in Table 7-12. These results show a large and statistically significant effect on the probability that a claimant who became reemployed returned to his/her separating employer. However, the 6.0 percent negative effect based on survey results was not consistent with the very small and statistically insignificant effect on recall found using administrative data on the full sample. Thus, the results are either inconclusive, or suggest on impact on a limited group of strong responders to the bonus offer.[9]

There was no evidence that union members switch to nonunion jobs more readily due to the bonus offer. The effect on placement through the union hiring hall is inconclusive as there are large negative coefficients that are generally not statistically significant.

There is no evidence that claimants offered a bonus made more use of the Employment Service to obtain jobs. The proportion of claimants using ES services did not increase, the proportion of claimants for whom ES services resulted in a job did not increase, and the number of services used did not increase. However, claimants offered the bonus did appear to increase job search activity, as the average number of employers contacted per week over the period of unemployment increased substantially, and the increase of 0.8 employer contacts per weeks (from the control group average of 1.3 contacts per week) was statistically significant.

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[9] Evidence on the effect of survey nonresponse presented in Appendix G suggests that the effect on return to work was negative for the population of treatment-assigned claimants, but that the magnitude of the effect was less than 6 percent.

Table 7-12

Summary of Secondary Economic Impacts
of Average Bonus Offer Estimated on Survey Data

Outcome Variable	Parameter Estimate	Standard Error
Probability of Return to Separating Employer	-0.060**	0.029
Probability of Switch to a Nonunion Job	-0.023	0.055
Probability of Placement by Union Hiring Hall	-0.015	0.016
Probability of using ES Services	-0.009	0.036
Probability ES Services Resulted in a Job, given use of ES Services	0.015	0.036
Number of ES Services Used	-0.015	0.130
Number of Employer Contacts Per Week	0.786**	0.304
Weeks Worked on First Reemployment Job	1.188	1.311
Probability of Becoming Self-Employed	-0.017	0.014
Percentage of Household Income Accounted for by Claimant	3.246**	1.584

Last, claimants offered the bonus appeared to experience less decline in contribution to family income than did members of the control group, which implies greater success in obtaining employment.

All in all, there were few dramatic or unexpected side effects of the bonus offer. The most important was the decline in post-filing earnings experienced by dislocated workers offered the bonus. The only other subpopulation that experienced an earnings decline was Hispanics. No overall effects, or effects by gender or age were found. There may have been an increase in the propensity for some claimants to obtain jobs with employers other than their separating employer, although the results in this regard are ambiguous. There was no increase in the use of ES services, although there was some increase in the intensity of job search, and some improvement in contribution to family income. There was no effect on union membership, although there might have been some tendency to reduce job acquisition through union hiring halls.

CHAPTER 8
PARTICIPATION IN THE BONUS EXPERIMENT

Participation in this experiment is a surprisingly complex issue. A restrictive definition of participation might be the act of submitting a Notice of Hire (NOH) or collecting a bonus. The ratio of claimants collecting bonuses to claimants who meet all of the criteria for collecting a bonus is an important indicator of the external validity of the experiment. If this ratio is low, many eligible claimants are not collecting bonuses, and the potential net benefit of the program may be overstated.

A more theoretically sound definition would define participation as an alteration in job search behavior to take into account the bonus offer. If participation in this sense is low, then the program would be ineffective. To investigate participation in terms of altered job search behavior requires information about the search process of claimants, which is available from the follow-up survey. In Chapter 7, survey information was used to explore the impact of the experiment on use of the Employment Service (ES) and on intensity of job search. Although the data did not indicate any increased use of the ES, it did indicate some increase in intensity of job search. In this chapter, we use the survey data to determine more directly the nature of participation in the experimental program.

8.1 Measures of Participation in the Experiment

Table 8-1 shows the proportion of eligible claimants who met certain partial qualifying conditions (namely, terminated receipt of benefits within the qualification period and did not receive benefits for at least 17 weeks), who submitted NOHs, and who collected bonuses. These manifestations of participation are shown for all claimants and several subpopulations. The proportion submitting NOHs and collecting bonuses increased with the size of the bonus multiplier and increased with the length of the

Table 8-1

Qualification and Participation in the Experiment

	C		1		2		3		4		
	C[T1,2,3]	C[T4,5,6]	N	Prop	N	Prop	N	Prop	N	Prop	N
					All						
Eligible Sample	3082	3082	2246	1.000	2348	1.000	1583	1.000	2387	1.000	2353
Partially Qualified*	954 .310	1232 .400	686	.305	762	.325	559	.353	1013	.424	1007
Submitted NOH	--	--	281	.125	422	.180	322	.203	463	.194	537
Collected Bonus	--	--	196	.087	292	.124	237	.150	332	.139	419
					All Except Union and Standby						
Eligible Sample	2389	2389	1758	1.000	1837	1.000	1252	1.000	1855	1.000	1866
Partially Qualified*	718 .301	951 .398	521	.296	570	.310	442	.353	777	.419	795
Submitted NOH	--	--	242	.138	357	.194	288	.230	412	.222	470
Collected Bonus	--	--	176	.100	245	.133	214	.171	300	.162	370
					White Males (except Hispanic)						
Eligible Sample	1556	1556	1126	1.000	1191	1.000	820	1.000	1210	1.000	1192
Partially Qualified*	506 .325	655 .421	356	.316	405	.340	308	.376	538	.445	538
Submitted NOH	--	--	147	.131	219	.184	170	.207	240	.198	268
Collected Bonus	--	--	112	.099	158	.133	132	.161	181	.150	220
					White Females (except Hispanic)						
Eligible Sample	1011	1011	742	1.000	789	1.000	516	1.000	781	1.000	785
Partially Qualified*	295 .292	391 .387	227	.306	252	.319	178	.345	319	.408	321
Submitted NOH	--	--	102	.137	148	.188	106	.205	163	.209	193
Collected Bonus	--	--	71	.096	108	.137	82	.159	123	.157	151

*Terminated benefits within qualification period and did not receive benefit payments for 17 weeks.

Table 8-1
(Continued)

	C				1		2		3		4	
	C[T1,2,3]		C[T4,5,6]		N	Prop	N	Prop	N	Prop	N	Prop
Black Males												
Eligible Sample	84		84		76		77		38		75	
Partially Qualified*	23	.274	27	.321	19	.250	14	.182	8	.211	33	.440
Submitted NOH	--		--		6	.079	12	.156	5	.132	7	.093
Collected Bonus	--		--		4	.053	3	.039	2	.053	2	.027
Black Females												
Eligible Sample	49		49		40		33		26		37	
Partially Qualified*	13	.265	16	.327	13	.325	11	.333	9	.346	12	.324
Submitted NOH	--		--		3	.075	3	.091	6	.231	5	.135
Collected Bonus	--		--		1	.025	2	.061	5	.192	5	.135
Other Males (Hispanic and other Nonwhite)												
Eligible Sample	225		225		170		158		113		183	
Partially Qualified*	68	.302	82	.364	50	.294	51	.323	32	.283	72	.393
Submitted NOH	--		--		15	.088	22	.139	19	.168	31	.169
Collected Bonus	--		--		5	.029	12	.076	8	.071	13	.071
Other Females (Hispanic and other Nonwhite)												
Eligible Sample	157		157		92		100		70		101	
Partially Qualified*	49	.312	61	.389	21	.228	29	.290	24	.343	39	.386
Submitted NOH	--		--		8	.087	18	.180	16	.229	17	.168
Collected Bonus	--		--		3	.033	9	.090	8	.114	8	.079

*Terminated benefits within qualification period and did not receive benefit payments for 17 weeks.

qualification period.[1] This pattern is consistent with prior expectations. The higher participation rate for those with the longer qualification period may reflect opportunity, as claimants have longer to qualify for the bonus. The increase in proportion receiving bonuses as the bonus multiplier increases may reflect some behavioral change, as the proportion of claimants who (partially) qualify for the bonus also increases with the size of the bonus multiplier. However, these increases could simply represent the increase in the take-up rate, i.e., the proportion of claimants who collected the bonus among those who qualified, by those who qualified without changing behavior.

The large gap between the proportion partially qualifying and the proportion submitting NOHs requires explanation. The ratio between these two proportions was about one-half. A large proportion of the gap may be explained by the intervention of qualifying requirements such as: benefits must have been terminated for reasons of reemployment, the claimant must not have been placed on the job through a union hiring hall, and the claimant must not have been recalled to his/her previous job. In this chapter, we use the survey data to estimate the take-up rate. One minus the take-up rate is the proportion of claimants who did not collect the bonus among those who qualified. The size of this group indicates a potential for bonus costs that were not realized in the experiment.

The ultimate test of participation is collecting a bonus. In Table 8-1 it can be seen that Hispanic and nonwhite racial groups of both genders collect bonuses at strikingly lower rates than non-Hispanic whites. Both white males and females (except Hispanic) collected bonuses at rates that varied from 10 to 24 percent across the six treatments, whereas male and female Hispanics, blacks and other nonwhites collected bonuses at rates one-third to one-half as large. Since these large differences in the proportions collecting bonuses were not reflected in the proportions partially qualifying

[1] This pattern was not observed for blacks and other races, and may be indicative of a sample size problem.

for bonuses, it may be concluded that the differences reflected either large differences in the intervening variables that effect qualification or differences in the voluntary take-up rate. We investigate this further in Section 8.3.

Table 8-2 shows the parameter estimates, which measure the impact of the experimental treatments on the probability of (partially) qualifying for the bonus. Although incomplete as a definition of qualification, it is an important indicator of behavioral change, because it bypasses some of the voluntary take-up issues involved in analyzing NOH filing and bonus collection. To measure treatment impact, an estimate must be made of the rate of qualification for the control group separately for the short and the long qualification period. For comparison with treatments 1 through 3, control group members are assigned the short qualification period, whereas for comparison with treatments 4 through 6, they are assigned the long qualification period. Statistically significant difference in this proportion may be taken as evidence of a behavioral change for the treatment group in response to the bonus offer, since there is no other reason for these proportions to differ.

Table 8-2 reports only the coefficients of the treatment variables, although all equations made use of the control set (reported in Appendix F). The results for all claimants, shown in Table 8-2, indicate strongly that the experiments caused a behavioral change. The coefficients on T3, T4, T5, and T6, all were large and statistically significant. Unlike the impact measures in Chapter 5, these measures show a consistently rising pattern of impact with increases in the size of the bonus multiplier. However, the differences between the large and medium Weekly Benefit Amount (WBA) multipliers are much larger than the differences between the medium and the small multipliers. The unexplainable but persistently large impact of T4 observed in the outcomes reported in Chapter 5 does not appear here. The inconsistency between the findings for T4 in the outcome measures and in the measure of (partial) qualification reported in Table 8-2 strengthens the view that the large impacts on compensation and weeks unemployed for T4, described in Chapter 5, were an anomaly.

Table 8-2

Effect of Treatment on (Partial) Qualification for the Bonus (Change in Proportion Qualifying)

	N	T1	T2	T3	T4	T5	T6
All Claimants	15,534	-0.003 (0.013)	0.016 (0.013)	0.047** (0.014)	0.027** (0.013)	0.030** (0.013)	0.056** (0.015)
All except Union and Standby	13,144	-0.004 (0.014)	0.010 (0.014)	0.056** (0.016)	0.023* (0.015)	0.030** (0.015)	0.064** (0.017)
White Males	7,920	-0.006 (0.018)	0.015 (0.018)	0.049** (0.020)	0.024 (0.019)	0.027 (0.019)	0.056** (0.021)
White Females	5,121	0.015 (0.022)	0.030 (0.022)	0.061** (0.025)	0.026 (0.023)	0.030 (0.023)	0.046* (0.027)
Black Males	449	-0.053 (0.066)	-0.093 (0.066)	-0.066 (0.082)	0.125* (0.075)	0.017 (0.082)	0.015 (0.089)
Black Females	246	0.026 (0.101)	0.004 (0.106)	0.115 (0.115)	0.032 (0.102)	0.029 (0.099)	0.269 (0.124)
Other Males	1,102	0.002 (0.047)	0.017 (0.048)	-0.013 (0.053)	0.031 (0.049)	0.089* (0.050)	0.028 (0.063)
Other Females	696	-0.089 (0.060)	-0.033 (0.058)	0.028 (0.066)	-0.003 (0.063)	-0.042 (0.061)	0.140** (0.073)

*Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

Table 8-2 also reports findings for several subpopulations. For instance, there is no statistically significant difference in the probability of qualifying for the total sample and the population subgroup that excludes those groups exempt from work search. Also, unlike the experimental impact results reported in Chapter 6, we see no difference in this measure of behavior between white males and white females. The results for all the nonwhite or Hispanic groups are difficult to interpret, because the sample sizes are too small and the coefficients unstable. However, there are many large, positive coefficients, and the results would not support the hypotheses that nonwhite response in the sense measured here differs from that of whites.

8.2 Analysis of the Take-Up Rate

The survey data were utilized to help determine what proportion of claimants eligible to submit NOHs actually did so. This proportion is the operational definition of the (voluntary) take-up rate. To be eligible to submit a NOH, the claimant needed to terminate benefit receipts and obtain a full-time job before the reemployment deadline. The job would not qualify the claimant if it represented a recall to the job held prior to filing for benefits, or if the claimant was placed on the job through a union hiring hall.

Table 8-3 presents the data from the survey on the characteristics of assigned claimants with regard to their participation in WREB. From Table 8-3 we see that 288 treatment-assigned claimants had acquired full-time work by the reemployment deadline and submitted a NOH, while 478 claimants had met the same work requirements but had not submitted a NOH. Of this number, 327 were probably ineligible to receive a bonus (and to submit a NOH). These ineligible claimants either returned to the previous employer (proxy for return to previous job), failed to terminate benefit receipts prior to the qualification deadline, were placed through a union hiring hall (13), or did not have post-termination wages to assure that reemployment had occurred (8). We estimate that the remaining 151 claimants who did not submit NOHs were eligible to do so.

In their responses to the survey, the following reasons for failing to participate were given by the 151 eligibles who did not submit NOHs:

Did not remember or forgot	87
Did not understand instructions	5
Refused to participate	13
Incorrectly believed not qualified	35
Other reasons	11

Table 8-3

Rate of Participation by Eligible Claimants
and Reasons for Nonparticipation

Reasons	Claimants Submitting and Not Submitting NOHs Number	Not Submitting and Not Eligible Number	Not Submitting and Eligible Number
Total Claimants	766		
Claimants Submitting	288		
Not Submitting, because:			
Did not remember Bonus Offer	44	33	11
Did not remember NOH	167	105	62
Forgot to submit, didn't understand, refused to participate	119	76	43
Didn't Think Qualif.	148	113	35
Total Not Submitting	478	327	151
Take-up Rate = 288/(288 + 151) = .66			

Note: The participation rate is underestimated because placement by union hiring hall is only partially eliminated from the denominator; it is overestimated because all those returning to previous employer are regarded as not eligible, although noneligibility occurs only if claimant returns to previous job.

Source: Survey of a Sample of Claimants Assigned to WREB.

Undoubtedly, knowledge of the rules would improve with repeated use of the system, and recollection would also improve with repeated exposure to an offer. Refusal might also be reduced, since distrust of the system might be mitigated over time.

Based on the data in Table 8-3, the take-up rate is estimated to be .66 (eligibles submitting a NOH/all eligibles). This implies that for every two bonuses collected, another bonus could have been collected without any additional change in job search behavior on the part of treatment-assigned claimants. Since take-up appeared to be higher among the high bonus offers, the program costs would increase by a smaller proportion. (The sample is too small, however, to do an analysis of the take-up rate by treatment level using the survey.) However, as noted in Appendix E, the survey response is biased in favor of those who responded more to the treatment; and, for the same reason, the take-up rate is also likely to be positively biased. In fact, the data in the section below from the administrative file show a take-up rate of only 53 percent, implying that the bonus costs could be as high as twice the rate evidenced in the experiment. This, however, is a worse case scenario, since take-up rates in such programs are never 100 percent. Increase in participation could also result in a larger effect on compensation, making extrapolation of net benefits more problematic.

8.3 Who Participates?

In this section, the entire sample is used in an effort to determine the characteristics of claimants who chose to actively participate in the experiment. For this analysis, active participation is defined as submitting a valid NOH. Such submission indicates the desire of the claimant to collect a bonus and his/her belief that he/she has completed the first requirement by obtaining a job that, if held for four months, would lead to the payment of a bonus. To measure participation, administrative data was used to select a group of treatment-assigned claimants who apparently met the qualifications for submitting a valid NOH. A dependent dummy variable was structured in which the dummy was equal to 1 if the claimant submitted a NOH. An ordinary least squares (OLS) regression was run that used a set of treatment variables, earning history variables, and other characteristics of the individual as independent variables to predict the claimant's probability of participating (i.e., submitting a NOH).

To select the sample for analysis, it was first necessary to determine qualification using administrative data. To "qualify," the assigned claimant had to meet the following set of conditions, which differed somewhat for those who did and those who did not serve a waiting week. For those serving a waiting week, the conditions were: (1) the claimant had to serve the waiting week within three weeks of filing for benefits (to assure that the unemployment period related to that filing); (2) the claimant had to terminate benefits prior to the qualification deadline; (3) the claimant had to have wages recorded in the file in the immediate post-termination quarter (to assure that the termination was employment- related); and (4) the first employer after terminating benefits (or major employer if more than one) could not be the separating employer. For claimants not serving a waiting week, the claimant had to have wages in the quarter after filing for benefits, and the first post-filing employer could not be the separating employer.

Of the 12,452 treatment-assigned claimants, 24 percent, or 2,987 claimants, met the qualifying conditions set forth above. These conditions serve only as a proxy for the true eligibility conditions, which tend to be less stringent. For instance, the true condition precludes recall to previous job, not to previous employer. On the other hand, the union hiring hall proscription is not captured.[2] However, this analysis does cover a large portion of the participants and is indicative of their characteristics.

Table 8-4 shows the least squares estimates for the equation predicting participation of qualifiers.[3] Of the group of 2,987 identified qualifiers, 1,577 (53 percent) submitted NOHs. Several participant characteristics in the equation were statistically significant predictors of participation. Being white, being employed as a white-collar worker, and not being in the goods-producing industries all strongly increased participation. Higher levels of education also contributed to participation, although the coefficient is not large. Four years more of schooling only increased participation by 3 1/2 percentage points. Having a strong earnings history greatly increased participation. In the base year, each additional quarter in which the claimant had earnings of $2000 or more greatly increased the probability of submitting a NOH. A

[2] Indication that the conditions for this analysis are too stringent is shown by the fact that about 30 percent of claimants submitting valid NOHs are excluded from the analysis.

[3] We also ran a probit model. Since, however, the mean of the dependent variable was near the middle of the distribution, the parameter estimates for the Probit and OLS regressions were very close; thus, for ease of exposition we are using the OLS estimates.

Table 8-4

Probability of Submitting Notice of Hire by those Qualified

	Parameter Estimate	Standard Error
WBA Multiples (4xWBA=1, 6xWBA=2)	.074***	.011
Qualification Period (long = 1)	.014	.017
White	.104***	.027
Male	-.026	-.020
Age	.0004	.0008
Education	.0086**	.0039
Goods producing industry (SIC 1,2,3 = 1)	-.088***	-.020
White collar occupation	.160***	.021
Earn 1 ($2000 in 1/4 BaseQ)	-.013	.045
Earn 2 ($2000 in 2/4 BaseQ)	.096**	.041
Earn 3 ($2000 in 3/4 BaseQ)	.154***	.039
Earn 4 ($2000 in 4/4 BaseQ)	.249***	.037
R^2 (adj) = .115 N = 2987		

***Coefficient is significant at the 99 percent confidence level for a two-tail test.
**Coefficient is significant at the 95 percent confidence level for a two-tail test.
*Coefficient is significant at the 90 percent confidence level for a two-tail test.

claimant with earnings over $2000 in all four quarters prior to filing for benefits was 25 percentage points more likely to submit a NOH (if qualified) than a claimant with no or one quarter with earnings of $2000 or more.

The experimental effects were also large. A claimant exposed to the highest bonus multiplier had a participation rate 14.8 percentage points greater than did a claimant with the lowest bonus multiplier. On the other hand, the length of the qualification period had no effect on the participation rate of eligibles. These results are as should be expected. The richer reward presented by the higher bonus multiplier would be expected to increase the rate of participation. On the other hand, while a longer qualification period was expected to increase the proportion of claimants who qualify for the bonus, there was no reason for the longer period to increase participation.

Overall, the R^2 of 0.12 indicates that most of the reasons for differences among eligible claimants in participation are not explained by this regression. Most of the explanation lies in the personal reasons expressed in the survey responses and discussed in Section 8.2.

CHAPTER 9

BENEFIT-COST ANALYSIS OF A BONUS OFFER PROGRAM

Benefit-Cost Analysis (BCA) is a process of assessing the favorable and unfavorable effects of a program, in which favorable effects are defined as benefits and unfavorable effects as costs. BCA can be used whenever these effects can be measured in monetary terms. Mark Thompson presented the following eight-step methodology for undertaking BCA (1980, pp. 47-49):

1. Identify the decisionmakers and their values
2. Identify alternatives (what options are to be compared?)
3. Identify benefits
4. Identify costs (program expenses or lost benefits)
5. Value all the effects monetarily
6. Discount all effects to present value
7. Take distributional effects into account
8. Aggregate the effects and interpret the results

In the remainder of this section, we use this eight-step methodology as a framework for undertaking the BCA for a bonus offer program. The program alternatives considered in this chapter are the same as the treatments in the WREB experiment. However, the benefit and cost calculations assume that it is an ongoing program, and not an experiment, a demonstration, or a start-up program. Following is a discussion of how the eight-point methodology is applied to the bonus-offer program.

1. Identify decisionmakers: The items to be included as benefits or costs depend critically on the identification of the decisionmaker and its perspective. A government agency, for instance, may take the view that optimization should be considered solely from the perspective of its agency, or may take a larger view and consider optimization from the perspective of the total society. Mathematica, in its study of the New Jersey UI

Reemployment Demonstration (Corson et al. 1989), did the BCA from the following perspectives: Society, Employers, Claimants, and the Government (separately for the Labor Department and the rest of the federal government). Our analysis follows this design, with three modifications regarding employer, claimant and government benefits.

Employers would probably gain from a reemployment bonus program because of more rapid filling of job vacancies. These gains would be due to increased profits resulting from increased production that follows from more rapid filling of job vacancies. In addition, if laid-off workers become reemployed faster there will be a saving in UI taxes because of experience rating. Estimation of both of these effects is beyond the scope of this project, therefore we do not consider employer benefits of a bonus program further.

Claimants can be assumed to benefit, or they would not respond to the bonus offer. However, the extent to which they benefit is unknown. In a competitive equilibrium environment with perfect knowledge, we could assume that the value of forgone earnings just equals the value of leisure. However, in the real world with imperfect information, the individual claimant may derive net benefits from increased employment. At a minimum, claimant benefits equal the value of the bonus offer, and they may be greater.

With regard to government, we assume that bonus costs will be borne directly by the unemployment insurance system. However, we will not distinguish between state and federal governmental units, because the allocation of costs and benefits between these two levels of government is a political decision, beyond the scope of this project to evaluate. Thus, our BCA will be from the following perspectives: society, the UI system, and total government.

2. Identify alternatives (what options are to be compared?): Six different bonus offer programs, defined by the amount of the bonus offer and the length of the qualification period, are evaluated. In the real world, there are many alternative ways the government could spend money. Even within the confines of programs to encourage more rapid reemployment of UI claimants, there are options other than the bonus offer, but none of these are considered here.

3. Identify benefits (either direct or indirect): Benefits need to be defined in terms of the decisionmaker's perspective, as benefits to one group are costs to another. For instance, transfer payments, such as UI compensation, are costs to the government, benefits to the recipients, and neither costs nor benefits from the total society perspective. Benefits accrue to the total society only if real income is increased. Although indirect benefits, such as increased psychological and physical well-being or improved ability to provide health and education benefits for children, may accrue due to additional work effort, we have not attempted to ascertain their existence or to estimate their value in the context of this program. It may, however, be assumed that most, if not all, of the indirect effects from reduced unemployment provide positive benefits to the claimants and to society, making the measured direct effects an understatement of the total benefits.

4. Identify costs (program expenses or lost benefits): The same issues of identification and association exist for costs as for benefits. Costs accrue only if real resources are used. Some costs are direct expenses of the program, while others may be indirect, such as the cost associated with "displacement" of nonparticipants by participants (discussed more fully below). The costs to be considered in this BCA are the bonus payments, the administrative costs of a program (not the experiment), and costs associated with displacement effects.

5. Value all the effects monetarily: To conduct a BCA, it is necessary that all the effect measures be in the same value units so that all the benefits can be added together

and all the costs added together, and the two compared. Since money is the one value measure that ideally meets the additivity criteria, all the benefits and costs are expressed in dollars.

6. Discount all effects to present value: In order to add together and compare benefits and costs that occur over time, or that occur in different years, it is necessary to discount future effects. In the BCA for WREB, however, discounting is not necessary because all the benefits and costs are assumed to occur in a single benefit year.

7. Take distributional effects into account: The effects on income distribution are not considered in this study.

8. Aggregate the effects and interpret the results: The two standard methods for aggregating benefits and costs for use in decisionmaking are (1) calculate Net Benefits, defined as the difference between all benefits (B) and all costs (C), i.e., B - C; and (2) calculate a benefit/cost ratio, i.e., B/C (Thompson 1980, p. 72). For society as a whole, the Net Benefit measure is most appropriate, because the Kaldor compensation criterion[1] suggests that all projects should be undertaken that have positive net benefits, since the losers (those bearing the costs) can be paid off by the winners (those obtaining the benefits) and leave a net surplus. However, governmental agencies operating with fixed budgets would tend to use the benefit/cost ratio. Proposed projects would be placed in a queue in descending order of their benefit/cost ratios. The project with the highest ratio would be the first chosen, and so on down the line of projects with decreasing benefit/cost ratios (greater than one) until the budget is exhausted. Both calculations are made in our BCA.

[1] As defined in Henderson and Quandt (1971).

9.1 Benefit-Cost Analysis

A BCA is conducted for six alternative bonus offer programs that replicate the bonus offer treatments in WREB. The BCA is conducted from the perspective of the UI system, the government as a whole, and total society.

9.1.1 Benefit-Cost Analysis from the Perspective of the UI System

The benefits to the system are the reductions in UI compensation payments that result from the more rapid reemployment of claimants offered the bonus. The costs are the direct costs of bonuses paid to claimants and the administrative costs of the program. It is assumed that in a real program, bonus payments would be costs to the UI system, although in the experiment, bonus costs were borne by the U. S. Department of Labor as part of their research program. Thus, the Net Benefit equation is:

B = change in compensation per eligible claimant

C = bonus cost plus administrative costs per eligible claimant, and

$B - C$ = Net Benefit per eligible claimant, and

B / C = the ratio of benefits to costs, or the dollar value of benefits per dollar of cost.

Table 9-1 shows the components of the formula. The compensation data are taken directly from the estimates of compensation change in Table 5-4. The bonus cost calculations are shown in Table 9-2. For each bonus program option, the bonus cost is the product of the average value of the bonus paid in each program (treatment in the experiment) times the proportion of eligible claimants who receive bonuses.

Table 9-1

Benefit-Cost Comparison of the Six Bonus Offer Programs
(dollars per eligible claimant)

	T1	T2	T3	T4	T5	T6	All T's
Society							
Earnings[a]	-3	93	269	311	91	352	172
Admin Costs[b]	-3	-3	-3	-3	-3	-3	-3
Net Benefits	-6	90	266	308	88	349	169
B/C Ratio	N.C.	31	90	104	30	117	57
UI System							
UI Compensation[c]	-19	41	107	117	40	141	65
Bonus Payments[d]	-29	-80	-142	-46	-114	-215	-95
Admin Costs	-3	-3	-3	-3	-3	-3	-3
Net Benefits	-51	-42	-38	68	-77	-77	-33
B/C Ratio	N.C.	.49	.74	2.39	.34	.65	.66
All Government							
Tax Revenues	Neg	14	40	47	14	53	26
UI Compensation	-19	41	107	117	40	141	65
Bonus Payments	-29	-80	-142	-46	-114	-215	-95
Admin Costs	-3	-3	-3	-3	-3	-3	-3
Net Benefits	-51	-28	2	115	-63	-24	-7
B/C Ratio	N.C.	.66	1.01	3.35	.46	.89	.93

[a] Earnings are calculated as the bonus induced change in full time equivalent weeks of compensation times the average weekly earnings of eligible claimants. (See Table 9-2.)
[b] See Table 9-3.
[c] Change in Benefit Payments over the Benefit Year, from Table 5-4.
[d] Bonus payments are the product of the average value of the bonuses paid in the given treatment group times the proportion of eligible claimants receiving a bonus payment. These numbers are taken from Table 9-2.

The calculation of administrative costs per eligible claimant is shown in Table 9-3. These costs were prepared in cooperation with Ms. Patricia Remy, Employment Security Department, State of Washington. They represent a best guess as to the administrative costs of an ongoing bonus offer program. Following is a brief sketch of the structure of the ongoing program that underlies the cost estimate.

Table 9-2A

Benefit and Cost Components for
Benefit-Cost Analysis

All Claimants

	(1) Average Value of Bonuses Paid	(2) Proportion Collecting Bonus	(3) Bonus Cost (1) x (2)	(4) Weekly Wage	(5) Change in Weeks Compensated	(6) Change in Earnings 4 x (-5)	(7) Change in Taxes 0.15 x (6)
T1	$328.14	.087	$ 28.64	$426.29	.008	-3.41	-0.51
T2	645.79	.124	80.31	409.51	-.227	92.96	13.94
T3	949.75	.150	142.19	403.95	-.667	269.43	40.41
T4	327.26	.139	45.52	415.05	-.749	310.87	46.63
T5	638.89	.178	113.77	412.74	-.220	90.80	13.62
T6	980.46	.220	215.25	415.94	-.847	352.30	52.85
T1,4	327.59	.114	37.33	420.47	-.382	160.62	24.09
T2,5	641.72	.151	97.06	411.15	-.224	92.10	13.81
T3,6	967.78	.184	178.16	410.02	-.756	309.98	46.50
T	653.47	.146	95.14	414.32	-.416	172.36	25.85

Table 9-2B

Benefit and Cost Components for Benefit-Cost Analysis

All Except Union Hiring Hall Members and Claimants on Standby Awaiting Recall

	(1) Average Value of Bonuses Paid	(2) Proportion Collecting Bonus	(3) Bonus Cost (1) x (2)	(4) Weekly Wage	(5) Change in Weeks Compensated	(6) Change in Earnings 4 x (-5)	(7) Change in Taxes 0.15 x (6)
T1	324.32	.094	30.43	417.53	.150	-62.63	-9.39
T2	641.78	.131	83.82	401.36	-.166	66.63	9.99
T3	950.28	.163	154.42	386.94	-.512	198.11	29.72
T4	326.87	.153	49.98	402.98	-.560	225.67	33.85
T5	635.75	.189	120.17	408.33	-.188	76.77	11.51
T6	973.33	.233	226.92	409.22	-.731	299.14	44.87
T1,4	325.94	.124	40.53	409.95	-.217	88.96	13.34
T2,5	638.19	.160	102.12	404.92	-.177	71.67	10.75
T3,6	963.76	.197	190.39	398.34	-.620	246.97	37.05
T	649.09	.156	101.37	405.08	-.303	122.74	18.41

Table 9-2C

Benefit and Cost Components for Benefit-Cost Analysis

Dislocated Workers
(12 quarters continuous employment)

	(1) Average Value of Bonuses Paid	(2) Proportion Collecting Bonus	(3) Bonus Cost (1) x (2)	(4) Weekly Wage	(5) Change in Weeks Compensated	(6) Change in Earnings 4 x (-5)	(7) Change in Taxes 0.15 x (6)
T1	333.96	.120	40.125	455.47	.40	-182.19	-27.33
T2	682.62	.151	103.05	429.32	-.29	124.50	18.68
T3	1,051.07	.193	202.97	422.23	-.95	401.12	60.17
T4	354.97	.165	58.68	440.09	-.73	321.27	48.19
T5	686.62	.211	144.91	442.69	-.29	128.38	19.26
T6	1,066.79	.280	299.07	436.43	-1.21	528.08	79.21
T1,4	346.53	.143	49.77	447.50	-.19	85.03	12.75
T2,5	685.04	.182	124.91	436.37	-.29	126.55	18.98
T3,6	1,060.41	.237	251.22	429.48	-1.08	463.84	69.58
T	712.35	.182	129.76	438.55	-.45	197.35	29.60

It is assumed that all UI claimants with valid new Washington State claims would be offered the bonus, and that the offer would be made at the time the claim is filed, thereby adding an estimated two minutes per claim for the claimstaker to present the bonus offer to the claimant. The other costs that would impact the Job Service Centers (JSC) are estimated by equating bonus program operations with existing operations that appear most similar. These are the following: the time to process a Bonus Voucher is assumed to be the same as processing a Continued Claim Form; the cost of handling a bonus claim denial is assumed to be the same as cost of handling a nonseparation denial or allowance; and the cost of handling appeals in the bonus program are considered the same as handling an appeal to a nonseparation denial. The frequency of bonus payments and denials per claim have been estimated using the ratios of total bonuses to initial claims, and the ratio of both NOH and Bonus Voucher denials to initial claims that were found in the experiment averaged over all six treatments. The ratio of appeals to denials was estimated using State of Washington ratios of nonseparation appeals to initial claims. The experiment could provide no information on this issue, since appeals were not allowed in the experiment.

Central office costs are based on the assumption that one program administrator and one clerical assistant could handle the central office chores for the entire state in an ongoing program. Administration of the bonus offer would be similar to that of any other UI payment, and would also be subject to recovery for fraud or overpayment. The process would be automated. Claimants would notify the Employment Security Department about acquisition of a full-time job on the claim form, which would be automatically entered into the system. The Notice of Hire form would not be used. The system could track the claim history for four months, automatically sending a Bonus Voucher and a notice to participants to get employer verification of employment. The claimant would mail the Voucher and employment verification to the central office. Operations at the JSC would be limited to informing the claimant about the bonus offer, and handling denials and appeals.

As shown in Table 9-3, this highly automated system would result in a program with very low administrative costs, about $3 per eligible claimant. These costs are added to the bonus cost for each program to yield the estimate of total cost per claimant from the perspective of the UI system.

The net benefit of each program option is the difference between the reduction in UI compensation and the bonus costs. We use the parameter estimate from the OLS regression as the best estimate of program effect, regardless of statistical significance. Table 9-1 shows that only treatment 4 shows positive net benefits (or Benefit/Cost Ratios over 1). The next best program is treatment 3 (high bonus, short qualification period). This treatment option appears to be better than treatment 6 (high bonus, long qualification period), because the shorter qualification period results in a lower take-up rate and thus lower bonus costs, which more than compensates for the slightly lower benefit value (reduction in compensation payments).

9.1.2 Benefit-Cost Analysis from the Perspective of All Government

As can be seen in Table 9-1, the net benefits for all government units are more positive than for the UI system alone, because the government has another source of benefits, namely, the additional taxes collected because of the additional earnings by the claimants responding to the bonus offer. We have used an estimate of 15 percent of the earnings as the tax return to government. This is the low federal level, and seems appropriate for the group involved here. We do not include other payroll taxes, such as FICA, because these represent obligations on the part of the government for future payments, and therefore do not constitute unencumbered new revenue. For all of government, treatment 3 now becomes essentially a break-even proposition. All others, except treatment 4, still show negative net benefits.

Table 9-3

Administrative Costs for an Ongoing Bonus Offer Program

Central Office Cost	1988 Dollars
1 Program Administrator & 1 clerical assistant	$ 45,900
Fringe Benefits @ 28.3%	12,990
Nonpersonnel services @ 16% of salary	7,344
Administrative, Staff and Technical Cost @ 16.35%	7,505
	$ 73,739
Total new intra-state claims, FY90	227,484
Central Office cost per new claim	$ .32

Job Service Center Costs

Costs per minute: JSC Specialist II @ 1,776 per month, plus fringe benefits @ 28.3%, non-personnel services @ 16%, and AST costs @ 16.35% of salary = $1,776 x 1.6065 = $3,075 per mont/9600 minutes per month = $.32 per minute

	Time per Operation	Units per Claim	$ per Claim
Additional time for the initial claim	2 minutes	1	$ .64
Processing Bonus payments	4.5 minutes[a]	.129[b]	.19
Allowance	27 minutes[c]	.129[b]	1.11
Denials	27 minutes[c]	.029[d]	.25
Appeals-- Lower level	34 minutes		
Higher level	20 minutes[e]	.002[f]	.01
			$2.20
Total Cost per claimant			$2.52
Total Cost per eligible claimant (add 16%)			$2.92

[a] Allowed time for processing a Continued Claim Form.
[b] Ratio of total bonuses to initial claims in experiment, i.e., 1,816/14,080 (see Table 3-1, Chapter 3).
[c] Time allowed for a nonseparation denial or allowance.
[d] Ratio of NOH and bonus denials to initial claims in experiment, i.e., (278 + 130)/14,080 (see Table 3-1, Chapter 3).
[e] Time allowed for lower and higher level appeals.
[f] Proportion of nonseparation appeals to initial claims: .06 x .029 (lower) + .01 x .029 (higher) = .07 x .029 = .002.

9.1.3 Benefit-Cost Analysis from the Perspective of Total Society

The net benefit to total society is the sum of all benefits and costs to the individuals who comprise the society. In computing net benefits to total society, transfer payments net out to zero, since they are benefits to the recipients and costs to the taxpayers. Thus, neither the savings in UI compensation nor the payment of bonuses become net benefits or costs to total society. For there to be a net benefit to total society, there must be an increase in real income greater than the real costs incurred to produce that income.

For the bonus offer program, the increases in real income are represented by the increase in earnings of those responding to the bonus offer. The real costs incurred to produce that additional income are the administrative costs of the program and the value of leisure forgone by the claimants. As we noted above, however, we have no means for placing a value on the forgone leisure. Since our calculation of society benefits do not include an estimate of this value, our estimates must be regarded as an overstatement of true societal benefits.

In Table 9-1, the earnings are calculated as the product of the change in covered unemployment caused by the bonus offer (see Table 5-4) and the average full-time equivalent weekly wages of eligible claimants.[2] Costs are merely the $3 per claimant administrative costs of the program. The estimated net benefit is the difference between

[2] Full-time equivalent wages were calculated from the data for 9,907 of the 12,452 eligible treatment-assigned claimants. To be included in the calculation, the claimant had to have served a waiting week within three weeks of filing, terminated benefits before exhaustion, and have wages in the full quarter after termination; claimants who served no waiting week are included if they have wages in the full quarter after filing. Sixty-one of those who qualify by this definition are excluded because of extreme values--hourly wages averaging less than $2.35 or more than $100 for the quarter. The full-time equivalent weekly wage was calculated for this group as follows: hourly wage rate = total wages in quarter minus any wages where the hours are 0 minus wages where the hours are blank divided by the reported hours; weekly wage = hourly wage rate x 40 hours.

the change in earnings and the administrative costs, resulting in a surprisingly large net societal benefit for all of these program options, and enormously large B/C ratios.

There are three simplifying assumptions in these calculations. The first is that the value of forgone leisure is zero. Since this is unlikely to be the case, our estimate of net benefits is overstated by an amount which may be as large as the full amount of benefits shown.[3] Second, it is assumed that all the change in compensated unemployment represents increased employment. Since reemployment was a condition for obtaining a bonus, the assumption that the change in unemployment causally connected to the bonus offer represents additional employment is reasonable.

The third simplifying assumption is that there is no displacement of nonparticipants. Any reduction in earnings by nonparticipants due to displacement would have to be deducted from the gain in earnings as a cost of the program. Although we have no direct evidence regarding displacement at this time, the theoretical work of Davidson and Woodbury suggest that on balance there shouldn't be any displacement. They write: ". . . as a result of the bonus program, any worker is more likely to be employed at a given time . . . We conclude that reductions in covered (program participant) unemployment do not come at the expense of increased uncovered (nonparticipant) unemployment, and in this sense the bonus program entails no displacement effect" (Davidson and Woodbury 1990, p. 18). The reasons the authors believe there is no displacement is that increased search effort by covered workers improves the performance of the economy by creating <u>new</u> jobs, and these new jobs eventually produce vacancies for other workers. Second, increased search effort by the

[3] The benefit-cost analysis presented considers only values which can be objectively measured. Early return to work involves a loss of leisure time, but it also involves a gain in psychic benefits associated with being employed. Separately it is difficult to assign economic value to these quantities, and on net it is hard to say which dominates. By excluding these factors from the cost-benefit analysis, we implicitly assume they cancel each other out. This approach is not without precedent. A well-regarded text on benefit-cost analysis by Edward Gramlich, <u>A Guide to Benefit-Cost Analysis</u> (1990), does not include the value of lost leisure in the discussion of benefits to work-generating programs.

participating workers triggers an increase in search effort by nonparticipating workers, and this rivalry effect also tends to neutralize the displacement effect.

Thus, from the perspective of total society, the bonus offer appears to be a very cheap means for increasing total employment and earned income, and most likely, net benefits. Since, however, the program represents a net cost to the UI system and to government as a whole, additional revenue sources would need to be tapped in order to finance any of the six bonus offer programs tested in WREB.[4]

9.1.4 The Benefit-Cost Analysis Reconfigured as a Dollar Bonus and Weeks for Qualification

In Chapter 5, the impacts of a bonus program reconfigured to measure the impacts of programs that differed by the dollar value of the bonus offer and by the length of the qualification period in weeks were measured using a linear regression model. The predicted values from that regression are displayed in Figure 5.1. In addition, a cross-hatched area is shown that represents the range of bonus offers in which the benefit/cost ratio from the perspective of the UI system is 1 or greater.[5] The B/C ratios are numerically presented in Table 9-4.

[4] This is technically not the case for treatment 4, which showed large positive net benefits; but the lack of symmetry with the results for the other five treatments makes the results for treatment 4 too suspect to be considered as representing a true program impact.

[5] The benefit/cost ratios are calculated using the following model to calculate benefits and costs:

Benefits = -0.065 * \$ Bonus Offer - 5.48 * Qualification Period
Costs = \$ Bonus Offer * (9.989×10^{-5} * \$ Bonus Offer + .00997 * Qualification Period).

The benefit equation provides OLS estimates of the change in UI compensation as a function of the bonus offer level and weeks of qualification (described in Section 5.3). The part of the cost equation within the parentheses is the OLS estimate of the probability of a treatment-assigned claimant receiving a bonus as a function of the bonus offer level and weeks of qualification; i.e., probability of receiving a bonus = f(bonus offer, qualification period, control variables). The cost is the product of this estimated probability times the value of the bonus offer.

Table 9-4

Simulated Benefit/Cost Ratio[a]
Based on Continuous Model Estimated on Treatments

Bonus Amount	Weeks in the Qualification Period										
	3	4	5	6	7	8	9	10	11	12	13
110	5.2	5.2	5.2	5.1	5.1	5.1	5.1	5.1	5.1	5.1	5.1
160	3.7	3.6	3.6	3.6	3.6	3.5	3.5	3.5	3.5	3.5	3.5
210	2.8	2.8	2.8	2.7	2.7	2.7	2.7	2.7	2.7	2.7	2.7
260	2.3	2.3	2.2	2.2	2.2	2.2	2.2	2.2	2.2	2.2	2.2
310	1.9	1.9	1.9	1.9	1.9	1.9	1.9	1.9	1.8	1.8	1.8
360	1.7	1.7	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.6	1.6
410	1.5	1.5	1.5	1.4	1.4	1.4	1.4	1.4	1.4	1.4	1.4
460	1.3	1.3	1.3	1.3	1.3	1.3	1.3	1.3	1.3	1.3	1.3
510	1.2	1.2	1.2	1.2	1.2	1.2	1.2	1.1	1.1	1.1	1.1
560	1.1	1.1	1.1	1.1	1.1	1.1	1.1	1.0	1.0	1.0	1.0
610	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0
660	0.9	0.9	0.9	0.9	0.9	0.9	0.9	0.9	0.9	0.9	0.9
710	0.9	0.9	0.9	0.9	0.8	0.8	0.8	0.8	0.8	0.8	0.8
760	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8
810	0.8	0.8	0.8	0.8	0.7	0.7	0.7	0.7	0.7	0.7	0.7
860	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7
910	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7	0.7
960	0.7	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6
1010	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6
1060	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6	0.6
1110	0.6	0.6	0.6	0.6	0.6	0.5	0.5	0.5	0.5	0.5	0.5
1160	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5
1210	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5
1254	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5

[a]The benefit cost ratio, B/C, is a function of the parameters of the bonus offer: the dollar bonus amount (BONUS), and the number of weeks in the qualification period (WEEKS). It is computed using the results of models estimated including the control variables given in Appendix A. The numerator is the reduction in UI compensation estimated in the continuous model: B = (0.065 * BONUS) + (5.48 * WEEKS). The denominator, C, is the product of the dollar bonus amount offered and the take-up rate for that offer. The take-up rate is R = (0.00009986 * BONUS) + (0.009967 * WEEKS), so that the equation for the cost of a bonus offer is: C = BONUS * R. The take-up rate equation uses estimates from a linear probability model of bonus receipt.

The area in which the B/C ratio exceeds unity comprises all of the bonuses less than $600 over the whole range of qualification periods. Remember that none of the six observed treatments had benefit/cost ratios greater than one (see Table 9-1). The greater benefits shown for small bonuses derives from the nature of the linear model that forces the bonus effects to be proportionate to the size of the bonus, thereby raising the estimated effects for small bonuses above that which we actually observed. The linearization of the cost model does not distort reality to the same degree. As a result of the distortion of benefits for smaller bonuses in the linear model, bonuses under $600 appear to have net positive benefits from the UI system perspective.

9.2 Benefit-Cost Analysis of a Bonus Offer Program for Selected Population Subgroups

Two subgroups emerge as possible candidates for a more specifically targeted bonus offer program. These groups are older workers and dislocated workers (defined in terms of the longevity of their work history). In this section, we estimate the societal and governmental net benefits from programs directed at these two groups. Because in each case the analysis must be conducted on a relatively small sample, we use the three treatment combination rather than the six individual treatments, thereby ignoring differences in the possible effects of the qualification period on net benefits.

9.2.1 Benefit-Cost Analysis of a Bonus Offer Program for Older Workers

One possible target group is older workers, who may be particularly good candidates for a bonus offer program because they may experience more voluntary unemployment than younger workers. This would be the case if older workers were more discouraged than younger workers about their job prospects, or if they had higher reservation wages than younger workers because of more assets or outside income. If this is the case, a bonus offer may succeed in increasing voluntary job search by older workers. Defining older workers as being 45 years of age or older, we do find a larger impact of the bonus offer on their compensation and weeks unemployed than for younger workers (see Table 6-12). We saw in Chapter 6 that the difference was particularly strong for older males (See Tables 6-24 and 6-25). The larger impact translates into larger net benefits than for the sample as a whole. (See Table 9-5.)

Looking at a bonus offer program equivalent to the average of the six experimental treatments (WBA multiplier of 3.75 and Qualification period of .3 times entitlement duration plus one week), we see that an older worker program has a B/C ratio for the UI system of 1.78 and for government as a whole of 2.40. Given the large societal benefits, an older worker program has promise. However, as noted in Chapter 6, the additional effectiveness of the bonus offer for older workers is concentrated among males. Unless that concentration was also consistent with some other policy objective, such as additional incentives for dislocated workers, an older worker bonus policy only effective for males would have problems.

9.2.2 Benefit-Cost Analysis of a Bonus Offer Program for Dislocated Workers

In Chapter 6, evaluations were conducted for dislocated workers using three different definitions of dislocation, all related to the claimant's work history. The largest impact of the bonus offer was found for claimants defined as dislocated by having been employed steadily for 12 consecutive quarters. The largest impact translates into the largest value of net program benefits, and our discussion of the net benefits of a bonus offer program targeted on dislocated workers is confined to this group.

From the perspective of the UI system, or the government as a whole, none of the alternative bonus offer programs look particularly attractive as a dislocated worker program. In Table 9-5, the B/C ratios are greater than plus one only for the high bonus offer from the perspective of all government. In general, we conclude that the BCA did not disclose the bonus offer program as particularly viable for dislocated workers.

Table 9-5

Benefit-Cost Analysis of a Bonus Offer Program for Selected Subpopulation Groups

	T1,4	T2,5	T3,6	All T's
Older Workers				
Net Benefits (Dollars per Claimant):				
Society	416	221	367	329
UI System	115	11	45	59
All Government	178	45	100	109
Benefit/Cost Ratio:				
Society	139	75	123	111
UI System	4.12	1.13	1.36	1.78
All Government	5.66	1.52	1.78	2.40
Dislocated Workers				
Net Benefits (Dollars per Claimant):				
Society	82	124	461	194
UI System	-29	-92	-36	-53
All Government	-16	-73	34	-23
Benefit/Cost Ratio:				
Society	28	42	155	66
UI System	.44	.31	.86	.60
All Government	.66	.46	1.13	.81

Note: For Benefit/Cost Ratios, the table presents the numerators; the denominators are always equal to 1.

However, dislocated workers did respond to the bonus to a somewhat greater extent than nondislocated workers, according to the analysis in Chapter 6. Since the high bonus offer could provide net benefits to the government, the UI system could be compensated for its losses by transfers from other government agencies, and society as a whole would appear to benefit from such a program.

CHAPTER 10
POLICY IMPLICATIONS AND CONCLUSIONS

In this final chapter, we present conclusions about what has been learned from WREB and what the experimental results suggest for unemployment insurance (UI) policy. Before finally reviewing the results, we compare WREB findings with those of two other bonus offer experiments. This comparison will help to evaluate the usefulness of WREB results for national UI policy.

10.1 Comparison of Results from WREB with those from Other Experiments

In this section, the impacts of the bonus offer on UI compensation and weeks of insured unemployment in WREB are compared with those from the Pennsylvania and Illinois Bonus experiments. (See Corson et al, 1991; and Davidson and Woodbury, 1991.) A fourth experiment conducted in New Jersey in 1986-87 offered job search assistance combined with a reemployment bonus. (See Corson et al, 1989.) Although the bonus offer in the New Jersey experiment appeared to have effects similar to those in WREB, the experimental results cannot be compared, because the bonus offer in the New Jersey experiment was made seven weeks after filing. Since the bonus offer in WREB was made at the time of filing, no WREB subsample is comparable to the population eligible for the New Jersey experiment. Before comparing results across experiments, it is necessary to briefly describe the designs of the other two experiments and to compare the eligibility criteria used to select the claimants for the experiments.

10.1.1 Comparison of Designs Across Three Bonus Offer Experiments

Following is a brief description of the experimental designs of the Illinois and Pennsylvania experiments:

The Illinois Reemployment Bonus Experiment:

The Illinois Reemployment Bonus Experiment was conducted in 20 sites in central and northern Illinois in 1984-85. The experiment consisted of two treatments. In the claimant treatment, a single bonus of $500 was offered to eligible claimants for obtaining full-time work within 11 weeks of filing for benefits and remaining employed for four months. In the employer treatment, an employer hiring a qualified claimant received a bonus of $500 if the claimant met the same conditions as prescribed for the claimant treatment.

The Pennsylvania Reemployment Bonus Demonstration:

The treatments in Pennsylvania were:

	Bonus Amount	Qualification Period
Treatment 1	3 x WBA	6 weeks (short)
Treatment 2	3 x WBA	12 weeks (long)
Treatment 3	6 x WBA	6 weeks
Treatments 4 and 6 *	6 x WBA	12 weeks
Treatment 5	6 x WBA/declining	12 weeks

* No voluntary job search workshop.

All treatments, except treatment 6, had a voluntary job search workshop (JSW) component that was so little used as to be considered irrelevant. In the Pennsylvania evaluation, treatments 4 and 6 were combined and considered a single treatment, equivalent to T6 in WREB. Pennsylvania treatments 1 and 2 have bonus levels half way between the low and middle bonus levels in WREB. The Pennsylvania qualification period lengths were close to the mean lengths for the short and long qualification periods in WREB. The reemployment period of 16 weeks was virtually the same as that required by WREB.

10.1.2 Comparison of Eligibility Requirements Across the Three Experiments

Eligibility criteria differed somewhat across the three experiments, as can be seen in Table 10-1. All three experiments required claimants to be monetarily eligible for UI benefits. All three also had some requirements regarding nonmonetary eligibility, but the requirements differed across the experiments as follows:

* Pennsylvania required that there be no non-monetary disqualification for the duration of the spell.
* Illinois required that there be no such stop at the time of qualifying for the bonus.
* WREB required that there be at least one week in which there was no benefit payment stop for non-monetary reasons prior to reemployment or prior to the end of the qualification period, whichever came first.

Waiting week requirements differed across the experiments, as follows:

* In Illinois, filing a claim automatically involved claiming a waiting week.
* Pennsylvania required that a waiting week be served.
* WREB had no waiting week requirements.

Claimants exempt from work search due to standby status or being a member of a full-referral union were treated differently across experiments, as follows:

* Pennsylvania required that the claimant not be exempt from work search.
* In Illinois, there was no formal exclusion. However, since enrollment occurred at the time of registering for work with the Job

Service, there was a de facto exclusion of most work search exempt claimants.

* In the WREB experiment bonus offers were made to claimants exempt from work search, but bonuses were not paid to any claimants who were recalled to their previous job or were placed on their new job through a union hiring hall.

Table 10-1

Eligibility Requirements for Participation in Four Experiments

Is This a Requirement?	Washington	Illinois	Pennsylvania
Mon. Elig. at Filing	Y	N	N
Monetarily Eligible	Y	Y	Y
No Indef. Non-Mon. Stops			
Full Period	N	N	Y
Some Part	Y	Y	Y
Claim a Waiting Week	N	Y	Y
Serve a Waiting Week	N	N	Y
Not Work Search Exempt	N	N	Y
Claim not Interstate, UCX, UCFE	Y	Y	Y
Claim not Backdated over two weeks	N	N	Y

Y = yes, a requirement to participate in experiment
N = no, not a requirement to participate in experiment

None of the experiments enrolled claimants who were exclusively inter-state claimants (earned qualifying wages in another state), UCX (ex-military), or UCFE (recent federal employee) claimants, since these payments are not charged against the host state's UI trust fund. Among the experiments, only WREB had the requirement that a WBA be established at the time of filing, thereby eliminating all claimants whose base period wages were not known at the time of filing, e.g., state employees. Pennsylvania was unique in excluding claimants who backdated their claims more than two weeks.

Creating a sample of claimants who met the joint eligibility criteria for all three experiments might eliminate 20 percent of the WREB sample. For WREB, omitting the no-waiting-week group reduces the sample of eligible claimants by 1,233, and omitting the work-search-exempt group eliminates about 1,800 claimants from the sample of 15,534.[1]

10.1.3 Comparison of Experimental Impacts on Compensation and Weeks of UI Benefit Payments Across the Three Experiments

Table 10-2 compares the estimated effects on compensation in the benefit year in Pennsylvania and WREB. Two sets of results for WREB are presented: (1) the mean effect of specific treatments, with results from the low and middle bonus treatments averaged to create a simulated treatment equivalent to the low treatment in Pennsylvania; (2) the WREB response from the continuous variable model evaluated at a bonus value equal to the WREB sample mean values of the WBA multiplied by the bonus multipliers used in the Pennsylvania treatments, and qualification periods equal to that in the Pennsylvania treatments. The WREB results were made to be comparable to those from Pennsylvania by using the sample excluding claimants who did not serve a waiting week. Except for the weakest treatment (low bonus, short qualification period), WREB results are somewhat stronger than those for Pennsylvania.

Table 10-3 shows the same comparisons for weeks with some compensation. These results indicate that WREB and Pennsylvania results were very similar. The short

[1] Eliminating both groups from the WREB sample would probably reduce the total sample by less than the sum of the two groups because of overlap.

qualification, low bonus results are somewhat stronger in Pennsylvania, and the short qualification, high bonus results are somewhat stronger in WREB. The results for the long qualification period are very close in the two experiments, for both low and high bonuses.

Table 10-2

Experimental Effects on Compensation in Pennsylvania and WREB

	Pennsylvania [a]	WREB [b]	
		Mean Treatment	Regression Estimate [d]
Short Qual Low Bonus	$104	$ 37 [c]	$ 74
Short Qual High Bonus	97	134	119
Long Qual Low Bonus	67	96 [c]	104
Long Qual High Bonus	129[e]	157	149

[a] As reported in Corson et al (1991).
[b] The sample used for WREB excludes those not serving a waiting week.
[c] A simple mean of the low (2xWBA) and middle (4xWBA) bonus multipliers was used for WREB to replicate the low bonus multiplier of 3xWBA in Pennsylvania.
[d] Computed from the continuous variable model, with the following OLS equation: $Y = a + bB + cQ + D'Z + u$, where $b = -0.1, c = -4.89$, and Z is a set of control variables.
[e] This represents a simple averaging of the results of Pennsylvania treatments 4 and 6.

Table 10-3

Experimental Effects on Weeks with Some Compensation in Pennsylvania and WREB

	Pennsylvania[a]	WREB[b]	
		Mean Treatment	Regression Estimate[d]
Short Qual Low Bonus	.65	.27[c]	.38
Short Qual High Bonus	.43	.86	.69
Long Qual Low Bonus	.35	.48[c]	.44
Long Qual High Bonus	.82[e]	.80	.75

[a] As reported in Corson et al (1991).
[b] The sample used for WREB excludes those not serving a waiting week.
[c] A simple mean of the low (2xWBA) and middle (4xWBA) bonus multipliers was used for WREB to replicate the low bonus multiplier of 3xWBA in Pennsylvania.
[d] Computed from the continuous variable model, with the following OLS equation: $Y = a + bB + cQ + D'Z + u$, where $b = -0.0007$, $c = -0.01$, and Z is a set of control variables.
[e] This represents a simple averaging of the results of Pennsylvania treatments 4 and 6.

To compare WREB and Illinois, it is necessary to update the value of the Illinois bonus offer of \$500. In that experiment, the \$500 offer was equivalent to a bonus offer at 3.85 times the average WBA of the claimants in the sample. Using the Washington average WBA of \$151, an equivalent bonus at 3.85 times the WBA would equal \$580. Table 10-4 shows the comparison of WREB and Illinois results. For this comparison, we use the sample of WREB claimants who were not exempt from work search, and the sample of Illinois claimants who were not eligible for the Federal Supplemental

Compensation (FSC) extended benefit program.[2] The WREB impacts for treatment 5, were much smaller than the Illinois treatment impact. However, when estimated using the continuous variable model, the impact estimates were quite similar.

Table 10-4

Experimental Results of Illinois and WREB Compared

	Illinois	WREB	
		Mean Effects Treatment 5	Regression Estimates
Compensation	$94	$43	$98
Weeks	.71	.21	.44

The main story emerging from the comparisons is that despite differences in treatment design and eligibility criteria, there is a great deal of consistency in the magnitudes of the experimental response. A middle level bonus of about $600, or 4xWBA, causes a decline in UI compensation of somewhat under $100 and a decrease in duration of insured unemployment of a little more than one-half week. High multiplier bonuses have proportionately larger effects. Results of the treatments with middle and high level bonuses and long qualification periods are the most consistent across the experiments. The low bonus, short qualification period treatment in Pennsylvania and the low bonus, long qualification period treatment in WREB fall outside the patterns of the other treatments. Of course, Illinois is not subject to such inconsistencies, because there is only one treatment.

[2] In the Illinois experiment claimants eligible for FSC were entitled to 38 weeks of benefits while non-FSC claimants were entitled to 26 weeks; the latter group is more comparable to the Washington sample.

10.2 Policy Implications and Conclusions

10.2.1 Internal and External Validity

Internal validity is achieved if the results are unbiased estimates of the effects of the experimental treatments on outcomes of interest. We identify five potential problems as possibly affecting internal validity: (1) administrative modification of the experimental design, (2) learning effects, (3) Hawthorne effects, (4) selective attrition, and (5) displacement.

Intended or unintended deviations from the design could easily occur in a field experiment operated as an added feature of an ongoing program. We are confident that this did not occur in WREB. The latitude available for altering either the assignment of claimants or the process of enrollment in the 21 Job Service Centers (JSCs) was very small. Assignment was done by computer using Social Security numbers; the enrollment process was explicitly laid out in a claimstaker Desk Aid; a concise WREB information sheet was issued to treatment-assigned claimants; and the field staff was consistently trained to provide precise and similar information to all assigned claimants. The entire procedure was monitored through personal visits by staff of the WREB central office, USDOL, and the Upjohn Institute, and by computer checks on local office operations using the Participant Tracking System (PTS).

Ex post checks indicated that appropriate assignment occurred. Demographic and other characteristics did not differ across treatments from what would be expected from random assignment. A competent and diligent central office staff, led by Patricia Remy, and armed with an accessible and up-to-date PTS, assured that assigned claimants met the criteria for eligibility at each of the three stages in WREB's operations: enrollment, NOH filing, and bonus payment. Ex post, only 3 of the 1,816 bonuses paid may have been issued in error. The bonuses paid conformed precisely to the expected multipliers of the WBA. We therefore have good reason to believe that WREB administration did not reduce internal validity.

The second potential problem affecting internal validity is the presence of learning effects. Learning effects are changes in the behavior of experimental participants that occur over time for at least two reasons:
(1) participants increase their understanding of the consequences of their behavior as the experiment progresses; and (2) it takes time for participants to adjust their behavior in response to experimental incentives.

Increased understanding could be manifest in increased participation over time, but such an increase was not evidenced in WREB. In fact, NOH filing declined in the later months. Learning effects could also occur if there was a serious problem of communicating the procedures for participating in the experiment. We believe that this problem is likely to have been very small. Simple one-page instructions were given both orally and in writing, with consideration given to any language problems that could have prevented understanding (e.g, written material was provided in both English and Spanish, and special translators were available for some Asian languages). Since job search is expected to begin immediately after filing for benefits under present law, no special behavior was required for the claimant to respond to the experiment. Therefore, no elapsed time was needed for participants to adjust their behavior.

The third potential problem is the well-known Hawthorne effect, i.e., the participant responds to an unintended treatment rather than to the designed treatment. Since the bonus offer was presented as simply an added reward for accomplishing goals that should already have been set, it should not have required new actions on the claimant's part (e.g., such as attending job search workshops). Furthermore, the experiment did not establish any new systems of monitoring claimant actions. Thus, we believe that the possibility of Hawthorne effects is lower in the bonus offer experiment than in other more invasive programs to stimulate job acquisition. Since the impact results are estimated over all eligible claimants assigned to the experiment, the experimental-control comparisons are devoid of any attrition, thereby eliminating the possibility that selective attrition reduced internal validity.

The last internal validity problem is displacement. Displacement occurs if the duration of insured unemployment of control group members was increased by treatment-assigned claimants filling available job slots more rapidly. Although displacement could be a problem of external validity, it is unlikely to have been a problem of internal validity for two reasons. (1) Only 16 percent of new filers in all but one of the JSCs (32 percent in Rainier) were treatment-assigned, and they constituted only a tiny fraction of the number of unemployed seeking work in the state; therefore, even if some displacement occurred, it could have only an insignificant effect on the reemployment probability of the control group. (2) Theoretical work by Davidson and Woodbury (1990) leads us to conclude that increased job search encouraged by the bonus offer tended to make job matching more efficient, thereby reducing overall unemployment.

There would not appear to be a concern about the internal validity of the experiment.[3] We now consider the question of external validity. External validity relates to the ability to transfer the results of the experiment to a different population and environment, most specifically to the larger population and environment envisioned in a state or national program. An experiment may be externally valid under certain circumstances and not others. For instance, if the experiment was operated at times of

[3] However, the need for control variables to reduce heterogeneity and the absence of logical progression in response across treatments of increasing bonus offers raise questions about the robustness of the experiment. The introduction of control variables in the equation to reduce experimental error resulted in large increases in the estimates of treatment impact on UI compensation. This suggests that despite random assignment, there was unintentional sample selection bias that could have affected the results. Differences among the treatment groups in the size of the WBA affected the outcome measures that required adjustment. The absence of positive correlation between bonus size and impact--mainly due to the large anomalous treatment 4 impact--does not conform to expectations and appears in most of the subgroup analyses. Our inability to explain the strong impacts of treatment 4 relative to the treatments with higher WBA multipliers leaves us puzzled.

particularly low or high unemployment it may be relevant for policy only when the labor market conditions are replicated.

A subtle problem that we tried particularly hard to avoid in WREB was providing information that would be different from that provided in an ongoing bonus program. For instance, if the bonus was only available to claimants who had already experienced a certain length of insured unemployment, and the availability of the bonus offer was not known until the time of eligibility in the experiment, there is clearly an external validity problem. That information will be known to everyone once there is a regular program, and behavior will be affected by that information. External validity is reduced if some group is excluded from the bonus offer who, by a readily available change in behavior, could make themselves eligible. For instance, excluding persons on standby could result in claimants not declaring standby status when otherwise eligible. This problem was avoided in WREB, since every claimant filing a new claim who could possibly be eligible, or become eligible, was given a bonus offer at the date of filing. Thus, if WREB were to become an actual program, all persons filing new initial claims would receive exactly the same information as treatment-assigned claimants received under WREB.

A potential problem of external validity existed because of a large percentage of claimants assigned to the experiment who appeared to be eligible for a bonus but did not participate in the experiment. Survey data indicated that about 66 percent of eligible claimants actually participated in the experiment. This implies that the cost of the bonus program could increase by about 50 percent in a fully implemented program simply because more eligible claimants would collect bonuses. Based on the less precise data from administrative records, the WREB participation rate may have been even lower--perhaps as low as 53 percent. Before extrapolating from these findings, two caveats should be entered. First, 100 percent participation does not occur in any government program, therefore it is unlikely that the number of eligible claimants who state, "I do not understand," "I forgot," or "I don't want to participate," would decline to zero or even close to zero. Second, to the extent that claimants do not participate, they also do not change behavior. Thus, increased participation may also mean increased behavioral change, which would increase the benefits of the program. We conclude that there is a participation problem that could increase the cost of a bonus program, but the potential amount of the increase and the effect on net benefits is difficult to predict.

Another external validity problem arises because a program like WREB could increase job-leaving by workers wanting to take advantage of the bonus offer (if the separation did not result in a nonmonetary denial), or cause some of those who become unemployed and currently do not file for benefits to do so.[4] The resulting increased rate of filing could increase costs above those estimated in the experiment.

Overall, we believe that because of broad eligibility criteria and the high degree of operational integrity, WREB had a high level of external validity, but may still understate the costs (and perhaps the benefits) of a fully operational and ongoing program.

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[4] A recent study by Wayne Vroman (1991), sponsored by the National Foundation for U.C. and W.C. and the A.F.L.-C.I.O., used the results of a series of supplementary questions asked the unemployed in the BLS survey. The study found the following:

> "Almost two thirds of the unemployed surveyed did not apply for UI benefits. Their reasons--over half of them believed they were not eligible, another 14 percent already had another job awaiting them, and another 5.3 percent said it was too much hassle or to much like charity. Fewer than 3 percent said they did not know about the program, and only 2 percent had previously exhausted benefits." (The Advisor, a UBA Publication, January 31, 1991)

Assuming that those who believed they were ineligible actually were ineligible, then over 20 percent of those who do not apply could apply if a bonus offer program was a sufficient enticement. However, requiring the claimant to claim a waiting week may eliminate a large proportion of those who did not apply because they had another job waiting for them, reducing to a relatively small percent the number of additional eligible UI claimants from among the newly unemployed.

10.2.2 Secondary Effects of a Bonus Offer Program

Interest by government in a bonus offer program may depend heavily on the positive and negative secondary effects that such a program would generate. Many of these were investigated in Chapter 7. The design of the experiment gave rise to concerns that the experiment was (1) anti-union, because it denied bonuses for placement in a first job through a union hiring hall, and (2) anti-employer, because it denied bonuses to claimants recalled to their previous job.

Data from 1,900 completed responses to the follow-up survey contained no evidence that the experiment caused union members to switch to nonunion jobs. While the sample may be too small for reliability, survey results did indicate that the bonus offer reduced the probability of being placed on a job through a union hiring hall.[5]

Since one purpose of the UI system is to assist employers in retaining their workforce during brief periods of economic downturn, evidence that the experiment reduced employer attachment would be troublesome. Results on this matter are mixed. Based on administrative data for the whole sample, there is no support for the hypothesis that a claimant who is offered a bonus and returns to work will be less likely than a claimant in the control group to return to the primary employer. This is true also for the smaller group of claimants on standby.

The survey data tell a different story. Twenty-nine percent of claimants who received a bonus offer and returned to work returned to their previous employer, whereas 35 percent of the control group became reemployed with their previous employer. The survey results were verified by using administrative data (Table 7-6) on the entire sample. Therefore, we cannot dismiss the possibility that the experiment

[5] Statistical evidence on this point is inconclusive. Note that, after reemployment, subsequent jobs could be a recall or a union placement without voiding bonus eligibility. Out of 1,187 full-referral claimants in the PTS, only 12 were denied bonus eligibility upon filing a NOH due to union placement.

reduced employer attachment, although that conclusion rests on the validity of the survey data.

Another undesirable side effect of the experiment would be a decline in job quality resulting from more rapid reemployment. If job matching had been optimal before the bonus offer, then more rapid reemployment may have resulted in claimants accepting less satisfactory jobs. Job quality is represented by quarterly earnings in the first full calendar quarter after ending a spell of UI benefits, or the first full calendar quarter after filing for benefits if no benefits had been received (differences in quarterly earnings could result from differences in hourly wage rates or number of hours worked per quarter).

Table 7-1 shows that for the total sample, the experiment had no discernible effect on earnings. The treatment-control difference of $70 was not statistically significant. However, for some population subgroups there were large differences, some of which were statistically significant. Of particular interest was the large negative difference of about $300 in quarterly earnings experienced by treatment-assigned claimants who were dislocated--having previously worked 12 consecutive quarters for the same employer or in the same industry. If this estimate of earnings decline is correct, then any net benefit to claimants or society from a bonus offer program aimed at these workers would be negated. Using a broader definition of displaced workers--continuous employment for the 12 quarters prior to filing for benefits--the experimental-control earnings difference declines to about $140 and becomes statistically insignificant. The bonus offer had no effects on earnings for another possible target group--older workers.

Also studied were the effects on use of the Employment Service (ES) for work search assistance and intensity of job search. There was no evidence of increased use of the ES (see Table 7-7), but there was evidence that job search intensity increased. The number of employer contacts per week for the claimants offered a bonus was considerably greater, and different from the control group, i.e., there were about 2 contacts per week for the treatment group and only 1.3 contacts for the control group (see Table 7-10). Furthermore, the number of employer contacts appeared to increase with the generosity of the treatment.

The experiment appeared to improve the claimant's share of household earnings. The control group experienced an 8.5 percent decline in their share of household income between the period before filing and the time of the interview (about a year later), whereas the treatment group, on average, experienced a decline of only 5.2 percent with the difference being statistically significant (see Table 7-13).

10.2.3 The Benefits of a Bonus Offer Program

From a policy perspective, there can be no reason for considering a new program unless it generates net benefits. However, the net benefits of a program depend upon the policy perspective, as benefits to one group are often costs to another. A bonus offer program appears to generate large net benefits and extremely high benefit/cost ratios from society's perspective, because of the high value of earnings gains and the very low administrative costs of the program--only $3 per eligible claimant. Unfortunately, no bonus offer configuration is a winner from the perspective of the UI system, although some treatment configurations are close to a break-even proposition for government as a whole (see Table 9-1). The negative net benefits calculated for the UI system are a result of the small effect on compensation relative to the cost of paying bonuses. The government as a whole does somewhat better, because added tax revenues result from the increased earnings of claimants.

As a program for older workers, the reemployment bonus should be seriously considered. It showed large gains for society, and positive net benefits to the UI system and to government as a whole. In fact, the high bonus programs have a positive rate of return of about 35 percent for the UI system and 66 percent for all government. Real consideration should be given to this option.

As a program for dislocated workers, it looks less appealing. For dislocated workers, the societal benefits are quite large (see Table 9-5), especially for treatments at the middle and high WBA multiples. The approximately one-week decline in insured unemployment, when translated into an additional week of earnings, generated over $450 dollars in net benefits. Unfortunately, the treatments with a high WBA multiplier produced negative benefits for the UI system because of the high bonus costs. The program does appear to be beneficial from the perspective of the government as a whole, but there would need to be some transfer of tax revenues from other governmental units to the UI system. All these calculations ignore the negative earnings effects. Even for the dislocated workers defined most broadly as claimants with 12 quarters of continuous employment prior to filing for UI benefits, the negative (though not statistically significant) effect on earnings (-$127 for the high multiplier programs seen in Table 7-1) would cost the government in taxes ($19), almost wiping out the benefits of reduced UI compensation. However even after factoring in the earnings decline, the societal benefits, though reduced from $449 to $322, are still quite large.

Any program expected to decrease the length of unemployment of a claimant by one-half week at a cost to society of $3 should be considered seriously. However, to the extent that the government regards transfer payments as real costs (because government prefers to avoid taxes), then from the government perspective the reemployment bonus program for all claimants does not look appealing. It may, however, deserve consideration as a special incentive program for older workers.

10.2.4 Conclusions

A valid test of a bonus offer program has been conducted. WREB has a high degree of internal validity, implying that experimental-control comparisons are reliable. The experiment also has a high degree of external validity, in that it could be replicated in a full program with reasonable expectations that the results would be similar to those in the experiment. The exception is the likelihood of some increase in participation, with unknown implications for program outcomes.

Internally, the experimental response was weak. The results for treatments with bonus offers at low and middle WBA multipliers basically show no statistically significant effects except for the inconsistently large effect for treatment 4 (low WBA multiplier, long qualification period). However, representing the bonus offer as continuous in the dollar bonus amount and weeks in the qualification period demonstrates that programs with low and middle level bonuses (up to about $600) could produce programs that show net benefits to the UI system. It is possible, though by no means assured, that after a bonus offer program has been in effect for a number of years, the responses will become more like those predicted from the continuous variable model, and a bonus offer program with bonuses under $600, or WBA multipliers of 4 or less, could become cost-effective from the perspective of government. Overall, the length of the qualification period did not effect the amount of net benefits received, as both the effectiveness and the costs increased similarly with the length of the qualification period.

A bonus offer program does appear to have potential for some population subgroups. The strongest showing was for a bonus offer program aimed specifically at older workers (workers over age 45). Benefit/cost ratios for older workers were higher than for younger workers, especially for programs with the high WBA multiples (6xWBA). For dislocated workers, defined as having been continuously employed for 12 quarters prior to filing, bonus offers with high multiples of the WBA have some promise. These programs had benefit/cost ratios somewhat less than one for the UI system, but greater than one for all government, which argues for a funds transfer to the UI system.

The results are not clear cut. Because of very low administrative costs, the net social benefits of a bonus offer program appear to be quite high. Thus, it might be a cost-effective means for increasing employment. However, no configuration of a bonus offer program for all claimants displayed benefit/cost ratios greater than one for the UI system. Treatments with a high WBA multiplier had benefit/cost ratios of about one for all government, which suggests that a bonus offer at a high multiple of the WBA might be a good way to increase employment, but would be implemented only by transferring some funds to the unemployment insurance system. Overall, the results from WREB suggest that more investigation of the bonus offer, and other program modifications, should be undertaken before implementing any new reemployment incentive programs in the unemployment insurance system.

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THE WASHINGTON REEMPLOYMENT BONUS EXPERIMENT

APPENDICES

APPENDIX A

Forms, Letters, and Instructions

KEY TO LETTER DESIGNATIONS

F1	Information sheet
F2b	Invalid claim denial
F2a	Enrollment letter
F2c	Nonmonetary denial
F3	Notice of Hire form
F4a	Valid Notice of Hire
F4b	UI drawn after reemployment deadline or start date or work
F4c	New job a recall
F4d	Job found through union hiring hall
F4e	Job started after reemployment deadline
F4f	Notice of self employment inquiry
F4g	Job change inquiry
F4h	Job not full time
F4i	Union inquiry
F5	Bonus voucher form
F5a	Four month elapsed letter
F6a	UI drawn after reemployment deadline or start date of work
F6b	New job a recall
F6c	Job found through union hiring hall
F6d	Job not full-time
F6e	Employment validation request
F6f	Employment not verified
F6g	Self employment not verified
F6h	Nonmonetary denial
F6i	Union inquiry during bonus voucher process

BONUS ELIGIBILITY CODES

Payment Denial Codes		Payment Allowance Codes	
A	Recalled to prior job	W	Verified by phone
B	First job union placement	X	Verified self employment
C	Can't verify employment	Y	Verified by claimant
D	Can't verify self employment	Z	Pay, no apparent conflict
E	Work not full time		
F	UI drawn after start date of work or reemployment deadline		
G	Nonmonetary denial		
H	State date of work after deadline		
I	Not continuous employment		

The participant tracking system used the above letter designations to track all Notice of Hire and Bonus Voucher activity. The Bonus eligibility codes were used to track the bonus verification method, type of allowance, and the specific reason for a denial of a bonus.

UI Program Analysis
Washington State
Employment Security
Department

Washington Reemployment Bonus Demonstration
UI Program Analysis Unit
Employment Security Department
Olympia, Washington 98504

WASHINGTON REEMPLOYMENT BONUS (WREB) DEMONSTRATION

INFORMATION SHEET

______________________________, you have been randomly selected to take part in the Washington Reemployment Bonus Demonstration. This is part of a national demonstration to find ways to improve the unemployment system for both workers and employers. The State of Washington will pay you a ***bonus*** if you become employed under the conditions described below.

To Receive the Bonus, You Must:

- **Be fully eligible** to receive unemployment insurance
- **Start full time work** before the date set by the program as shown on your monetary determination
- **Not be recalled** to your previous job
- **Not be placed on the job** through a union hiring hall
- **Stay fully employed** for at least 4 months

The amount of your ***bonus*** and the ***date*** by which you must start a job is shown on your monetary determination. Receipt of a bonus will not affect your claim for unemployment benefits. Like unemployment insurance, the bonus is subject to Federal Income Tax.

This is How it Works:

• **Enrollment Letter:** You will receive a letter in the mail verifying the amount of your bonus and the last date you can start work to qualify. ***Do not wait for the letter to accept a job.*** If you are denied unemployment benefits, you are not eligible for the bonus.

• **Notice of Hire:** Send in the Notice of Hire if you obtain a new job, or start a business, before the last date you can qualify for the bonus. Mail it to the Washington Reemployment Bonus Unit at the above address. For you to qualify for the bonus, you must work ***full time***. If you change jobs send another Notice of Hire.

• **Voucher:** If you remain employed 4 months on that or other jobs, and do not receive any unemployment insurance during that time, you need to submit a ***Bonus Voucher for Payment.*** The Washington Reemployment Bonus Unit will send you a Voucher form and will authorize payment. The unit may verify your employment status with your employer.

QUESTIONS? CALL toll free 1-800-782-9099.

Demostración del Bono de Reempleo
de Washington (WREB)
Departamento de Seguridad de Empleo
Unidad de Análisis del Programa UI
Olympia, Washington 98504
800-782-9099

DEMOSTRACION DEL BONO DE REEMPLEO DE WASHINGTON (WREB)
PLANILLA DE INFORMACION

___________________________________, Ud. ha sido seleccionado a la suerte para que participe en la Demostración del Bono de Reempleo de Washington. Esta es parte de una demostración nacional para mejorar de algún modo el sistema de desemplo tanto para los trabajadores como para los empleadores. El estado de Washington le pagará un ***bono*** si Ud. ha sido empleado de nuevo de acuerdo con las condiciones que siguen.

Para Recibir el Bono, Ud. Deberiá:

- **Ser elegible del todo** para recibir aseguranza de desempleo
- **Empezar de tiempo entero en el nuevo trabajo** antes de la fecha establecida por el programa según se muestra en su determinación monetaria
- **No ser llamado** de nuevo a su empleo anterior
- **No ser colocado en el trabajo** a través de un registro de una unión
- **Permanecer empleado de tiempo entero** por lo menos cuatro meses

La cantidad de su ***bono*** y la ***fecha*** en la cual Ud. deberá empesar de trabajo se muestran en su determinación monetaria. El recibo de un bono no afecta su derecho a reclamar beneficios de desempleo. Al igual que la aseguranza de desempleo, el bono está sujeto al Impuesto Federal sobre los Ingresos.

He Aquí Como Funciona:

- **Carta de Matriculación:** Ud. habrá de recibir una carta en el correo verificando la cantidad de su bono y el último día en que Ud. puede comenzar a trabajar para tener derecho. ***No aguarde por la carta para aceptar un trabajo.*** Si se le nigan los beneficios de desempleo, Ud. no tendrá derecho al bono.

- **Notificación de Empleo:** Envíe la Notificación de Empleo si Ud. obtiene un nuevo trabajo, o inicia un negocio, antes del último día en que Ud. tiene derecho al bono. Remítalo a la Unidad del Bono de Reempleo de Washington a la dirección que se indica arriba. Ud. deberá estar trabajando de ***tiempo entero*** para tener derecho al bono. Si cambia de trabajo, envíe otra Notificación de Empleo.

- **Voucher (Comprobante):** Si Ud. permanece empleado por cuatro (4) meses en ése u otros trabajos, y no recibe aseguranza de desempleo durante ese tiempo, Ud. tiene que someter un **voucher del bono para pago**. La Unidad del Bono de Reempleo de Washington le remitirá una planilla del voucher y dará autorización para el pago. La unidad puede verificar el estado de su empleo con su empleador.

¿PREGUNTAS ? LLAME GRATIS AL 1-800-782-9099.

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

ENROLLMENT LETTER

You are eligible to participate in the BONUS DEMONSTRATION. The Employment Security Department will pay you a bonus of $XXX.00 if you start work on a new, full-time job before XX/XX/XX and continue in that job or other jobs for at least four months. You may be eligible for the bonus even if you started work before receiving this letter.

If you start a new, full-time job, not a recall by your last employer or placement through a union hiring hall, complete the Notice of Hire and mail it in the enclosed postage paid envelope.

Upon receipt of your Notice of Hire, the Employment Security Department will verify that you have stopped filing for unemployment benefits and send you an acknowledgement letter or a denial letter. If you switch jobs during the four months, send another Notice of Hire.

At the end of the four months, if you have remained fully employed and have not filed for unemployment benefits during the entire four months, you will receive a bonus voucher which you should complete and return. Your bonus check will then be mailed to you.

If you need further information about the BONUS DEMONSTRATION call the WREB Unit at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F2a

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE, ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

INVALID CLAIM DENIAL

The Washington Reemployment Bonus (WREB) Demonstration Unit has received information that your unemployment claim has become invalid. To be eligible for the bonus, you must be eligible for unemployment benefits. The Bonus offer presented to you is no longer in effect. If this claim becomes valid at a future date, you may still be bonus eligible if you fulfill all the requirements.

If you believe that the reason above for your being ineligible to receive the bonus is incorrect, please call the Washington Reemployment Bonus Demonstration Unit in Olympia at 1-800-782-9099. They will review your situation with you and try to resolve the problem.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F2b

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

NONMONETARY DENIAL

The Washington Reemployment Bonus (WREB) Demonstration Unit has received information that you have been denied benefits on your unemployment claim. To be eligible for the bonus, you must be eligible for unemployment benefits. The Bonus offer presented to you is no longer in effect.

If you believe that the reason above for your being ineligible to receive the bonus is incorrect, please call the Washington Reemployment Bonus Demonstration Unit in Olympia at 1-800-782-9099. They will review your situation with you and try to resolve the problem.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F2c

Name ______________________________

SSN ______________________________

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504
800-782-9099

Notice of Hire

I wish to notify the Washington Employment Security Department that I have been hired on a new full time job, that is not a recall to my previous job, and was not obtained through a union hiring hall.

I started work on __________ - __________ -1988.

The following information may be used to contact my new and prior employers:

New Job Information

Employer ______________________________
Occupation ______________________________
Street ______________________________
City ______________________________
State ______ Zip ______________________________
Phone ( ) ______________________________
Average Weekly Hours ______________
Average Weekly Earnings ______________

Prior Job Information

Employer ______________________________
Occupation ______________________________
Street ______________________________
City ______________________________
State ______ Zip ______________________________
Phone ( ) ______________________________
Average Weekly Hours ______________
Average Weekly Earnings ______________

I will be eligible for a cash bonus if I meet all eligibility requirements. I must remain employed and not draw unemployment insurance for the next four months. Please send me a Bonus Voucher which I will complete and return four months after the date I became reemployed.

Please answer the following questions:

- Do you currently hold more than one job? Yes ____________ No ____________
- Do you own the business where you now work? Yes ____________ No ________
- If you are a union member, answer the following:

  I am a member of ______________, local __________________________

  Were you placed on your new job by a union? Yes ______ No ________

Signature ______________________________ Date ____________________

Phone ( ) ______________________________

EMSX WREB.F3

Nombre ____________________________

SSN ____________________________

Demostración del Bono de Reempleo de Washington (WREB)
Departamento de Seguridad de Empleo
Unidad de Análisis del Programa UI
Olympia, Washington 98504
800-782-9099

Noticia de Empleo

Deseo notificar al Departamento de Seguridad de Empleo de Washington que he sido contratado de tiempo entero para un nuevo trabajo, que ésto no es un regreso a un empleo anterior, y que no fue obtenido a través de un registro de empleo de una unión.

Comencé a trabajar en ________________-1988.

La siguiente información puede ser usada para contactar a mis patrones presente y pasado:

Información sobre el nuevo trabajo	**Información sobre trabajo Anterior**
Patron____________________	Patron____________________
Ocupación____________________	Ocupación____________________
Calle____________________	Calle____________________
Ciudad ____________________	Ciudad____________________
Estado _________ Zip____________	Estado_________ Zip____________
Teléf. ( )____________________	Teléf. ( )____________________
Promedio de Horas Semanales__________	Promedio de Horas Semanales__________
Promedio de Ingreso Semanal__________	Promedio de Ingreso Semanal__________

Tendré derecho a un bono en efectivo si cumplo todos los requisitos de elegibilidad. Deberé pemanecer empleado sin recibir aseguranza de desempleo por al menos cuatro (4) meses. Por favor envíenme un voucher (comprobante) del bono que he de completar y entregar cuatro meses después de la fecha en que fui empleado.

Por favor responda a las siguientes preguntas:

- ¿Tiene Ud. en la actualidad más de un trabajo? Sí__________ No__________
- ¿Es Ud. dueño del negocio en que trabaja ahora? Sí__________ No __________
- Si Ud. es miembro de una unión, responda a lo que sigue:

  Soy miembro de____________________, local______________

  ¿Fue Ud. colocado en su nuevo trabajo por una unión? Sí ________ No__________

Firma______________________________ Fecha____________________

Teléf. (   )____

EMSX WREB.F3S

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504
(800) 782-9099

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

SSN: XXX- XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

Notice of Hire

wish to notify the Washington Employment Security Department that I have een hired on a new full time job, that is not a recall to my previous job, nd was not obtained through a union hiring hall.

I started work on ____________-________-____________.

he following information may be used to contact my new and prior employers:

New Job Information	Prior Job Information
Employer____________________	Employer____________________
Occupation____________________	Occupation____________________
Street____________________	Street____________________
City____________________	City____________________
State______Zip____________	State______Zip____________
Phone (____)____________	Phone (____)____________
Average Weekly Hours__________	Average Weekly Hours__________
Average Weekly Earnings________	Average Weekly Earnings________

will be eligible for a cash bonus if I meet all eligibility requirements. must remain employed and not draw unemployment insurance for the next four onths. Please send me a Bonus Voucher which I will complete and return our months after the date I became reemployed.

Please answer the following questions:

- Do you currently hold more than one job? Yes____ No____
- Do you own the business where you now work? Yes____ No____
- If you are a union member, answer the following:
  - I am a member of __________________, Local__________________
  - Were you placed on your new job by a union? Yes________ No________

ignature ________________________________ Date ____________

hone (______) __________________________

EMSX WREB. F3I

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

VALID NOTICE OF HIRE

The Notice of Hire you submitted has been received. It has been verified that you are not currently receiving Unemployment Insurance benefits. You have become reemployed on a new full time job within the period necessary to qualify for a bonus. It is understood that the job was not acquired through a union hiring hall, and was not a recall to your prior employment.

If you remain employed for four months , until XX/XX/XX and d that period, you should complete and return the enclosed Bonus Voucher. The Employment Security department may verify your employment status prior to authorizing payment of a bonus to you.

If during the four month reemployment period you change jobs, be sure to submit another Notice of Hire form (a copy of which is enclosed) indicating your new employer. You may still be eligible for a bonus payment, if you do not file for Unemployment Insurance benefits during that period.

Please feel free to call and direct any questions you may have to the Washington Reemployment Bonus Unit at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F4a

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXXX.00

UI DRAWN AFTER DEADLINE OR
UI DRAWN AFTER START OF WORK

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Notice of Hire. Although you have obtained new employment, an investigation of your records shows that you continued to receive unemployment benefits either after the Reemployment Deadline or after your start date of work. Your having received Unemployment Benefits makes you ineligible for the bonus.

If you believe that the reason shown above for your being ineligible to receive the bonus is incorrect, please call the Washington Reemployment Bonus Demonstration Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F4b

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

JOB STARTED AFTER REEMPLOYMENT DEADLINE

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Notice of Hire. The new job listed on your Notice of Hire does not make you eligible for the bonus because the job reported on your Notice of Hire started after your reemployment deadline.

If you believe that the reason stated above for your being ineligible to receive the bonus is incorrect, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F4e

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

NEW JOB A RECALL

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Notice of Hire. An investigation of your records shows that the new job listed on your Notice of Hire does not make you eligible for the bonus because it is considered a recall to your previous job.

If you believe that the reason checked above for your being ineligible to receive the bonus is incorrect, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F4c

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program An alysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

JOB FOUND THROUGH UNION HIRING HALL

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Notice of Hire. The new job listed on your Notice of Hire does not make you eligible for the bonus because you were placed on your new job through your union hiring hall.

If you believe that the reason shown above for your being ineligible to receive the bonus is incorrect, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F4d

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

JOB CHANGE INQUIRY

The information on the Notice of Hire that you recently submitted shows that you have returned to work with your former employer. You may still be eligible for the reemployment bonus if this job is not a recall. Information to determine if this is a new job or a recall is needed.

Please complete the information requested below, and return it in the enclosed postage paid and self-addressed envelope.

	NEW JOB	OLD JOB
Job Title	______________	______________
Pay Rate	______________	______________
Geographic Location	______________	______________
Division/ Department	______________	______________
Job Duties	______________	______________
Employers Name	______________	______________
Phone Number	______________	______________

Additional information explaining how the two jobs differ.

____

____

Signature: ______________________ Date: ______________

Phone Number: (_______) ______________________

EMSX WREB. F4g

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

NOTICE OF SELF-EMPLOYMENT

Please complete the information requested and return it in the enclosed postage paid and self-addressed envelope.

I, ____ wish to notify the Washington Employment Security Department that I have become self-employed, and am no longer receiving unemployment insurance benefits.

I started self-employment on _______________ - ________ - 1988

I work an average of _________hours per week.

The name of my business is: ____

The address of my business is: Street ______________________________

City ______________________________

State ___________ Zip ______________

Phone (_____) ______________________

My State of Washington Business License Number is: ___________________

A copy of my quarterly business income tax form is enclosed, or will be sent as soon as it is filed.

Claimant Signature _________________________________ Date ___________

Phone Number (____) _______________________

EMSX WREB. F4f

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline XX/XX/XX
Bonus Amount $XXX.00

UNION INQUIRY

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Notice of Hire. You state you are a member of a Union, or your Job service Center indicates you are a union member. Further information is needed, please answer the following questions:

1. Did you acquire the job yourself? ______ How? ________

2. What is your Union's name and number? ____________
   Phone number and contact person ____________

3. Is your new employer a Union employer? ____________

4. Did your Union place you on the job? ____________

To be eligible for the bonus, your first new job can't be a placement through your Union Hiring Hall. If you contacted your new employer and acquired the job on your own, you are bonus eligible. The back of this letter may be used to explain how you got the job on your own and were not placed on the job through the Union.

Please answer the questions above and return this in the enclosed postage paid envelope. Call 1-800-782-9099 with any questions.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F4i

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline XX/XX/XX
Bonus Amount $XXXX.00

JOB NOT FULL TIME

The information on the Notice of Hire that you recently submitted shows that you have returned to less than full time work with your new employer. To be eligible for the Bonus you have to have been working full time. The job needed to be an average of 34 hours per week, or have enough weekly earnings to prevent eligibility for unemployment insurance benefits. Since your job(s) did not fulfill this criteria, you are not eligible for the Bonus.

If you believe that the reason shown above for your being ineligible to receive the bonus is incorrect, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F4h

Demostración del Bono de Reempleo de Washington (WREB)
Departamento de Seguridad de Emple
Unidad de Análisis del Programa UI
Olympia, Washington 98504
800-782-9099

SSN____________________

Fecha____________________

VOUCHER (COMPROBANTE) DEL BONO

Yo ____, deseo notificar a la unidad del Bono de Reempleo de Washington que soy eligible para recibir un bono de reempleo. He cumplido con los siquientes requisitos:

- He estado trabajando de nuevo de tiempo entero por al menos cuatro meses,
- Volví a trabajar antes de la fecha de vencimiento de mi reempleo,
- No me colocaron a través de un registro de empleo de una unión en el primer trabajo que he tenido después de aplicar para beneficios de desempleo,
- Este primer trabajo que he tenido no es un regreso a ningún trabajo anterior,
- No he recibido beneficios de desempleo desde la fecha de vencimiento de mi reempleo.

Doy permiso para que se contacte a mi patrón para verificación de mi empleo:

Persona a contactar____

Nombre del Patrón__________________Dirección____________________________

Ciudad__________________ Estado_____________ Zip_____ Telef. (   ) ____________

Mi presente dirección postal es la siguiente:

Calle____

Ciudad ____

Estado ____________ Zip ____________________Telef. (   ) ______________________

Yo certifico que la información es correcta:

Firmado ____Fecha __________________

**Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504
800-782-9099**

SSN___________________

Date___________________

BONUS VOUCHER

I ____, wish to notify the Washington Reemployment Bonus Unit that I am eligible to receive a reemployment bonus. I have fulfilled the following requirements:

- I have been back at work full-time for at least four months,
- I went back to work before my reemployment deadline,
- I was not placed on the first job held after filing for unemployment benefits through a union hiring hall,
- The first job held after filing was not a recall to a previous job, and
- I have not drawn unemployment benefits since my reemployment deadline.

You may contact my employer to verify my employment:

Contact Person____

Employer Name____________________Address__________________________

City__________________ State_______________ Zip_____ Phone (   ) ___________

My current mailing address is:

Street ____

City ____

State ____________ Zip ____________________Phone (   ) ______________

I certify to the above statements:

Signed ____Date ____________________

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline XX/XX/XX
Bonus Amount $XXX.00

INVALID VOUCHER: UI DRAWN AFTER REEMPLOYMENT DEADLINE OR UI DRAWN AFTER START OF WORK

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Bonus Voucher. An investigation of our records, however, shows that you are not eligible for the bonus because you continued to receive unemployment benefits after the reemployment deadline or after the start date of work.

If you believe that the reason shown above for your being ineligible to receive the bonus is incorrect, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6a

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504
1-800-782-9099

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline XX/XX/XX
Bonus Amount $XXX.00

REEMPLOYMENT TIME PERIOD ELAPSED

The Washington Reemployment Bonus (WREB) Demonstration Unit records show that you are potentially eligible to receive a bonus. You acquired a job before the reemployment deadline. A Voucher has not been received from you to claim the Bonus. If you have worked for four months and feel you are otherwise eligible to receive the bonus, complete the Voucher and send it in the enclosed postage paid envelope.

If you have any questions call the Washington Reemployment Bonus Demonstration Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB.F5a

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

INVALID VOUCHER: JOB FOUND THROUGH UNION HIRING HALL

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Bonus Voucher. An investigation of your records, however, shows that the new job listed on your Bonus Voucher, Notice of Hire and/or Union Inquiry was acquired through the Union. You are not eligible for the bonus, because you were placed on your new job through your union hiring hall.

If you believe that the reason stated above for your being ineligible to receive the bonus is incorrect, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6c

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $352.00

INVALID VOUCHER: NEW JOB A RECALL

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Bonus Voucher and your Job Change Inquiry response. An investigation of your records, shows that the new job is a recall to the employer you worked for prior to filing your unemployment claim. Because the job that you now hold is a recall to your previous job, you are not eligible for the bonus.

If you believe that the reason stated above for your being ineligible to receive the bonus is incorrect, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6b

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXXX.00

EMPLOYMENT VALIDATION

The information on the Bonus Voucher that you recently submitted shows employment that we are unable to verify. Please send verification of this employment to the address shown above. Inadequate documentation or no response will cause a denial of Bonus Payment.

If you have any questions, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6e

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

INVALID VOUCHER: JOB(S) NOT FULLTIME

The information on the Bonus Voucher that you recently submitted shows that you have returned to less than full-time work with your new employer. To be eligible for the Bonus you must be working full-time. Your job or jobs should average 34 hours per week or result in sufficient earnings to prevent eligibility for unemployment insurance benefits. Since your job(s) do not fulfill this criteria, you are not eligible for the Bonus.

If you believe that the reason stated above for your being ineligible to receive the bonus is incorrect, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6d

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline XX/XX/XX
Bonus Amount $XXX.00

SELF-EMPLOYMENT NOT VERIFIED

The information on the Bonus Voucher that you recently submitted shows self-employment that we are unable to verify. The documentation you provided is inadequate to verify that you were employed for four months after the reemployment deadline. You are not Bonus eligible.

If you have any questions, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6g

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

EMPLOYMENT NOT VERIFIED

The information on the Bonus Voucher that you recently submitted shows employment that we are unable to verify. The documentation you provided is inadequate to verify that you worked for this employer or employers for four months after the reemployment deadline. You are not Bonus eligible.

If you have any questions, please call the WREB Reemployment Bonus Unit in Olympia at 1-800-782-9099.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6f

STATE OF WASHINGTON

EMPLOYMENT SECURITY DEPARTMENT

Olympia, Washington 98504

LAST FIRST INITIAL
ADDRESS
CITY STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline XX/XX/XX
Bonus Amount $XXX.XX

UNION INQUIRY

The Washington Reemployment Bonus (WREB) Demonstration Unit has received your Bonus Voucher. You state you are a member of a Union, or your Job service Center indicates you are a union member. Further information is needed, please answer the following questions:

1. Did you acquire the job yourself? ______ How? ________

____

____

____

2. What is your Union's name and number? ______________
   Phone number and contact person ______________________

3. Is your new employer a Union employer? ______________

4. Did your Union place you on the job? ________________

To be eligible for the bonus, your new job can not be a placement through your Union Hiring Hall. If you contacted your new employer and acquired the job on your own, you are bonus eligible. The back of this letter may be used to explain how you got the job on your own and were not placed on the job through the Union.

Please answer the questions above and return this in the enclosed postage paid envelope. Call 1-800-782-9099 with any questions.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6i

STATE OF WASHINGTON
EMPLOYMENT SECURITY DEPARTMENT
Olympia, Washington 98504

LAST, FIRST INITIAL
ADDRESS
CITY, STATE ZIP

Washington Reemployment
Bonus (WREB) Demonstration
Employment Security Department
UI Program Analysis Unit
Olympia, Washington 98504

Date: XX/XX/XX
SSN: XXX - XX - XXXX
Reemployment Deadline: XX/XX/XX
Bonus Amount: $XXX.00

NONMONETARY DENIAL

The Washington Reemployment Bonus (WREB) Demonstration Unit has received information that you have been denied benefits on your unemployment claim. To be eligible for the bonus, you must be eligible for unemployment benefits. The Bonus offer presented to you is no longer in effect.

If you believe that the reason above for your being ineligible to receive the bonus is incorrect, please call the Washington Reemployment Bonus Demonstration Unit in Olympia at 1-800-782-9099. They will review your situation with you and try to resolve the problem.

Sincerely,

P.J. Remy
Project Coordinator

EMSX WREB. F6h

APPENDIX B

Effects of the Experiment on Backdating Claims

APPENDIX B

Effects of the Experiment on Backdating Claims

Prior to May 9, 1988, a treatment-assigned claimant's qualification deadline was determined from the Effective Date of the Claim. Beginning May 9th, the deadline was determined from the date of Greenbar printing. Therefore, prior to May 9th, treatment-assigned claimants had an incentive not to backdate their claim, because backdating the claim would reduce the time available for obtaining a job that qualified the claimant for a bonus.

We found that the proportion of claimants backdating claims increased dramatically after May 9th. However, this phenomenon did not seem to be related to the experiment, since the control group's behavior in this regard changed in precisely the same way, as shown in Table B-1.

Table B-1

Backdating Claims Before and After May 9, 1988

	Before 5/9	After 5/9
Control Group Proportion Backdating	0.114	0.224
Treatment Group Prop. Backdating	0.100	0.212

We tested the hypothesis that controls and experimentals differed in the tendency to backdate and that this difference was related to the change in practice that occured on May 9th. In an OLS regression, we found that May 9th had a strong effect on backdating, with claimants filing before that date having an 11 percentage point greater probability of backdating the claim than would claimants filing after that date. The treatment coefficient indicated a possible 1.4 percentage point negative effect on backdating, but the coefficient had a *t*-value of 1.61, which was not significant at the 90 percent confidence level. The May 9th date did not influence the treatment impact, as the coefficient on the variable "treatment impact before 5/9" was very small and statistically insignificant. Thus, we concluded that the change in policy regarding dating the start of the qualification period did not effect the tendency for treatment-assigned claimants to backdate claims.

APPENDIX C

The Effect of Monetary Redetermination on the Value of the Bonus Offer

APPENDIX C

The Effect of Monetary Redetermination on the Value of the Bonus Offer

The size of the bonus offer was based on the value of the Weekly Benefit Amount (WBA) on the date that the claimant filed his/her claim. If wages were missing, or extraneous wages were in the claim, the value of the WBA would change subsequent to Greenbar printing. Thus, the claimant's bonus offer, which had been assigned on the basis of being 2, 4, or 6 times the WBA at the time of filing, may not necessarily have held that ratio to the final WBA. The following table presents some statistics on the amount of changing and its effects on the bonus multiplier.

Table C-1

Effects of Monetary Redetermination

Experiment Group	Proportion Redetermed	Change in WBA	Multiplier at Filing	Multiplier at BYE
Control	0.062	$ 9.02	NA	NA
1	0.074	15.99	1.999	1.985
2	0.067	16.79	4.000	3.973
3	0.0635	7.85	6.003	5.986
4	0.067	10.02	1.999	1.994
5	0.061	11.87	4.002	3.985
6	0.061	16.08	5.995	5.961

NA: Not available.
BYE: Benefit year end.

A claim is defined as having a monetary redetermination if either the maximum benefits payable (MBP) or the WBA "at filing" (i.e., when initial claim was keyed into the computer) is not equal to the MBP and the WBA at the benefit year end (BYE).

Table C-1 shows that the redetermination process had little effect on the bonus offer multiplier, primarily because redetermination occurs for only a small proportion of claimants. The change in WBA represents about 10 percent of the WBA for those who experience redetermination. However, since only 7 percent of claimants have redeterminations, the effect on the bonus multiplier is less than 1 percent.

An ordinary least squares (OLS) regression confirmed what is suggested in the table, that there was no difference between treatment and control claimants in the probability of a redetermination.[1] We also tested to determine if the multiplier at the BYE differed significantly from the designed multiplier for each of the six treatments. An OLS regression confirmed that the mean difference in the bonus multiplier of -0.018 did not represent a statistically significant difference from the overall weighted design mean of 3.76.

[1] Treatment 1 did show a statistically significant difference in the probability of redetermination at the 90 percent confidence level.

APPENDIX D

The WREB Enrollment Monitoring Simulation Model

TABLE OF CONTENTS

The WREB Enrollment Monitoring Simulation Model

May, 1988

Prepared by:

Robert Spiegelman and Christopher O'Leary
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The Upjohn Institute for Employment Research
300 South Westnedge Avenue
Kalamazoo, Michigan 49007

Prepared for:

The Unemployment Insurance Service
Employment and Training Administration
United States Department of Labor
200 Constitution Avenue
Washington, DC 20210

and

The Employment Security Department
State of Washington
Olympia, WA 98504

I. Introduction

To get the richest statistical results possible from the experiment, it is necessary to enroll the largest possible sample of claimants given the bonus payments budget. A simulation model has been developed to guide the timing and rate of enrollment into the WREB demonstration. This paper documents that model.

In the following section, a summary of the data and variables involved in running the WREB enrollment monitoring simulation model are described. A concise sketch of the model is presented as Section III. In Section IV the method used to compute preliminary updates of the model is given along with a strategy for a completely revising estimates. Section V presents, compares, and examines baseline simulation estimates and two preliminary updates based on observed WREB demonstration data in a form which serves as an example of a weekly WREB enrollment monitoring report.

II. The Data and Variables

The WREB enrollment monitoring simulation model uses data on the experience of claimants to form point estimates of group behavior. Quarterly data from the CWBH records for 1986 and 1987 in Washington State is used to make baseline estimates. Data on new initial claims, weekly benefit amounts, the duration of benefits claimed, and the length of benefit entitlement for claimants at each of the 21 Job Service Centers (JSCs) selected is used.

The simulation model is based on four quantities:

I_{jt} = The inflow of new claimants who are monetarily valid at the time of filing at JSC j in t of the demonstration (a basic datum).

B_{jt} = The average dollar bonus offer made to claimants enrolled into WREB at JSC j in week t of the demonstration (based on the average WBA and treatment assignment).

volume of 1988 UI claims will exceed those for the period one year earlier by 9.5 percent. By examining the data in Table D1 it is obvious that use of monthly average data is superior to using annual average data since it captures seasonal trends and new quarter effects. These features are particularly useful if the experiment lasts less than 12 months. The weekly inflow numbers for each JSC are arrived at by dividing the tabulated monthly values by the appropriate number of weeks for that month as listed on the WSESD claim calendar.

Table D1
Estimated monthly flow of monetary valid initial claims (I)
March 1988 - February 1989

	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb
Aberdeen	307	372	263	230	175	296	230	350	339	767	526	361
Auburn	734	767	920	515	723	1007	602	920	1205	591	1599	1007
Bellevue	734	777	734	975	821	843	690	515	996	569	1292	712
Bellingham	405	482	526	350	482	350	274	296	350	339	712	372
Bremerton	515	405	350	438	350	438	307	230	613	350	602	372
Cowlitz Cnty	427	274	526	482	537	504	197	350	482	493	745	394
Everett	690	810	701	569	635	668	504	734	701	734	1533	756
Lewis Cnty	208	219	219	307	285	383	219	252	526	328	591	164
Lynnwood	668	558	602	471	690	657	602	668	657	339	1084	657
Moses Lake	230	197	285	230	526	219	142	208	405	328	646	175
Mount Vernon	318	526	504	274	493	460	296	394	558	405	1139	328
North Seattle	1040	909	854	668	1161	1018	887	1150	1292	986	1785	1062
Olympia	493	526	449	537	548	624	383	558	701	734	843	51
Rainier	756	799	723	449	569	602	537	580	93	1117	2026	931
Renton	1007	953	799	788	777	865	690	777	975	504	1467	887
Spokane	1062	996	942	887	975	1205	931	996	1982	1413	2562	1248
Sunnyside	274	252	296	307	504	285	142	756	788	580	690	372
Tri-Cities	350	493	438	569	624	449	361	427	865	1040	1183	252
Walla Walla	208	208	110	164	142	164	175	263	405	230	482	186
Wenatchee	350	548	887	328	580	361	219	164	427	405	679	318
Yakima	591	887	701	602	931	646	307	493	1117	1248	1643	537

p_{jtik} = The proportion of WREB treatment assigned claimants at JSC *j* who will become reemployed in the *i*th week after filing an initial claim in program week *t* which occurs in quarter *k* of the benefit year (based on weeks of benefits claimed).

q_{jtik} = The proportion of WREB treatment assigned claimants at JSC *j* whose reemployment deadline will arrive in the *i*th week after filing an initial claim in program week *t* which occurs in quarter *k* of the benefit year (based on duration of entitlement and experimental design).

The data and method used to form these concepts is discussed below, each in turn. An example of the raw data used is presented for each concept. Also explained are the assumptions involved in relating these variables to the program concepts.

I_{jt} - Monetarily valid initial claimants.

An estimate of the number of new, monetarily valid initial claimants, I_{jt}, is used to get at a preliminary estimate of the number of claimants who will be WREB treatment assigned at each JSC in a particular week, T_{jt}. Estimates of I_{jt} were provided by the Washington State Employment Security Department (WSESD) for the period March, 1988 to February, 1989, from their Continuous Wage and Benefit History (CWBH) records, which are based on a ten percent random sampling of Unemployment Insurance (UI) administrative records. For the 20 JSCs where WREB is tracking 20% of the monetarily valid claimants, we estimate T_{jt} to be equal to $(.16)*I_{jt}$, in an analogous fashion for Rainier we have $T_{jt} = (.32)*I_{jt}$ in the simulation 1. These relations are appropriate since the last 2 digits of the SSN are randomly assigned. Sixteen and thirty-two percent of claims that are monetarily valid at the time of filing are currently being assigned to bonus offer treatments.

Monthly baseline data on I_{jt} used in the simulations is presented on the next page for a possible 12-month enrollment period, March, 1988 through February, 1989. This data reflects the judgement of the UI Program Analysis Unit of the WSESD that the

Table D2
(Continued)

	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb
Lynnwood	616.27	585.63	656.68	610.63	572.34	590.63	620.89	596.28	650.63	630.94	619.87	651.61
Moses Lake	585.63	481.46	517.36	523.13	502.81	575.63	629.86	406.68	495.76	621.11	498.34	476.25
Mount Vernon	525.63	582.05	554.10	611.63	632.29	612.95	543.96	538.91	592.36	491.39	562.07	518.13
North Seattle	601.25	633.86	605.25	605.86	641.93	632.50	646.60	608.74	641.77	654.17	631.19	618.61
Okanogan	631.21	448.13	530.63	549.91	392.63	442.77	560.63	463.13	410.63	508.70	472.23	683.35
Olympia	575.30	579.38	588.06	608.63	606.21	575.10	597.11	523.86	545.63	664.30	601.01	571.56
Port Angeles	555.63	470.63	564.20	543.32	591.88	623.53	527.29	598.13	613.85	650.63	615.17	621.74
Pullman	496.34	410.63	635.63	635.63	635.63	660.63	560.63	616.88	598.13	610.62	650.63	560.63
Rainier	583.86	540.63	544.08	551.48	573.61	580.71	592.98	570.35	592.89	587.57	589.00	573.71
Raymond	560.63	641.88	598.13	643.13	549.09	560.63	528.48	600.63	573.13	615.43	521.34	543.96
Renton	611.69	548.69	653.63	640.83	647.24	654.83	616.01	611.33	609.50	589.47	623.03	638.40
Spokane	525.02	529.65	545.11	557.85	472.98	588.58	556.31	530.13	555.27	587.33	568.50	567.74
Sunnyside	430.82	470.63	509.73	464.20	485.63	455.07	614.20	389.05	418.84	482.99	408.32	434.20
Tacoma	604.05	557.36	569.55	606.40	601.07	601.53	581.17	536.55	592.17	627.21	545.21	588.89
Tri-Cities	605.16	588.96	517.50	530.91	539.57	547.82	571.99	495.00	563.47	597.73	574.94	573.67
Vancouver	573.13	541.13	635.63	585.17	551.25	558.99	657.05	571.51	554.67	609.06	589.87	596.00
Walla Walla	521.15	501.41	493.13	499.69	387.55	523.13	457.50	485.63	479.54	507.05	525.63	401.80
Wenatchee	621.99	407.74	658.49	521.92	450.90	509.89	650.63	475.63	491.39	540.35	500.14	470.11
Yakima	555.07	462.76	489.09	493.81	433.57	476.88	515.09	505.63	489.95	568.96	467.74	525.42

p_{jtik} - The proportion who become reemployed.

The CWBH data also contains the information necessary to construct a distribution of the duration of UI benefits. Duration ranges from less than 1 week up to 17 or more weeks. Dividing by the total number of people in this distribution produces a proxy relative frequency distribution for the proportion of claimants expected to find a job in the ith week after enrollment in week t. These proportions, p_{jtik}, are computed for each JSC, j, for different weeks, i, relative to different weeks, t, since the start of the bonus experiment. Furthermore, since the duration of UI benefits drawn varies seasonally, distributions have been constructed for each of the four different quarters of the year, k. The simulation selects the appropriate distribution for a week's WREB enrolled cohort depending on the season of the year.

B_{jt} - The dollar bonus cost.

Under the experimental design, bonus amounts is either 2, 4, and 6 times the Weekly Benefit Amount (WBA): 3/8 of the people will receive 2 x WBA, 3/8 will receive 4 x WBA and 1/4 will receive 6 x WBA. This means that the average bonus amount is 15/4 x WBA = [(3/8 x 2 x WBA) + (3/8 x 4 x WBA) + (1/4 x 6 x WBA)]. The WBAs used to calculate the bonus offer is based on the wage record available at the time the claimant files his/her claim. Based on the CWBH, data are available to compute the expected average WBA to be paid at each of the JSCs in Washington State during each month from March, 1988 through February, 1989. Multiplying the average bonus amount by the number of people who are expected to receive a bonus yields an estimate of the dollar bonus cost of enrolling claimants into the experiment for that week, B_{jt}.

Table D2 shows the average expected WREB bonus offer used in the baseline data for the period March, 1988 through February, 1989 by JSC. Since this data is a constant multiple of the average WBA the variation in the numbers listed may reflect seasonal and industry mix patterns across JSCs.

Table D2
Estimated Monthly Average WREB Bonus Amounts (B)
March 1988 - February 1989

	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb
Aberdeen	627.87	578.27	635.63	671.34	602.81	641.18	660.63	635.63	602.81	691.10	562.19	656.08
Auburn	645.70	529.80	595.45	593.44	590.85	585.90	571.53	579.38	592.67	629.48	616.75	594.86
Bellevue	651.30	590.20	604.28	695.46	621.63	666.79	626.10	621.26	686.72	645.72	650.12	641.39
Bellingham	524.09	560.63	591.24	581.08	564.03	562.90	595.24	468.96	544.72	544.22	546.78	534.15
Bingen	627.29	466.88	485.63	616.88	635.63	685.63	610.62	539.20	546.34	685.62	638.30	773.13
Bremerton	554.24	534.27	515.17	481.88	588.75	500.26	582.05	532.05	627.73	595.78	560.63	562.83
Colville	718.96	564.79	628.81	437.10	433.13	496.34	539.20	500.63	523.13	560.63	591.66	576.41
Cowlitz Cnty	576.01	614.63	654.38	668.96	647.87	619.32	514.79	532.50	584.49	659.56	580.98	635.63
Ellensburg	549.91	553.13	530.63	585.63	517.77	535.63	665.63	335.63	571.34	571.34	521.74	568.13
Everett	567.66	555.63	572.34	615.81	617.52	587.67	557.43	558.39	623.13	601.25	591.35	589.55
Lakewood	600.77	499.02	613.30	655.41	623.99	596.25	615.25	566.79	657.62	650.99	580.62	590.63
Lewis County	469.84	500.63	635.63	560.63	560.63	585.63	605.63	570.41	621.56	640.46	588.40	551.25

An estimate of the experimental effect of a reemployment bonus offer is factored into the distribution of unemployment duration. Woodbury and Spiegelman (1987) report that in the Illinois Job Search Incentive Experiment the average duration of the first spell of unemployment fell 7.1 percent for claimants who were offered the reemployment bonus. Since the dollar bonus offered in the Illinois JSIE is about equal to the mean bonus to be offered in the WREB, a 7.1 percent treatment effect is assumed for the WREB simulations. The distribution of unemployment duration is modified by evenly increasing the proportion of persons who stop collecting benefits before 13 weeks, and decreasing the proportion who stop after 13 weeks such that the mean duration falls by 7.1 percent.

Table D3a summarizes the proportion of WREB assigned claimants expected to return to work i weeks after filing an initial claim in some week in March of 1988 (the only simulation relevant month from the first quarter of 1988), $k = 1$. The table has 21 rows for the JSCs, and 14 columns for the 13 weeks in which successful job finding is relevant to WREB bonus eligibility plus a column for the residual probability. Three other matrices are presented on the subsequent pages in Table D3b, D3c and D3d; one for each of the remaining quarters in 1988, $k = 2,3,4$.

Table D3a

Estimated proportion of WREB assigned claimants who return to work
in 1..30 weeks since Effective Date of Claim (P)
First quarter 1988 estimates

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Aberdeen	0.052	0.080	0.101	0.047	0.014	0.041	0.047	0.074	0.058	0.052	0.041	0.036	0.025	0.3
Auburn	0.109	0.031	0.119	0.045	0.055	0.043	0.049	0.031	0.043	0.024	0.045	0.029	0.043	0.3
Bellevue	0.074	0.063	0.100	0.056	0.047	0.058	0.052	0.047	0.040	0.040	0.036	0.033	0.026	0.3
Bellingham	0.085	0.049	0.081	0.089	0.040	0.032	0.032	0.049	0.049	0.036	0.028	0.036	0.020	0.4
Bremerton	0.090	0.054	0.081	0.036	0.049	0.040	0.045	0.027	0.036	0.031	0.027	0.040	0.049	0.4
Cowlitz	0.095	0.081	0.049	0.053	0.042	0.067	0.046	0.053	0.046	0.042	0.042	0.028	0.021	0.3
Everett	0.053	0.063	0.097	0.055	0.053	0.057	0.076	0.029	0.042	0.023	0.025	0.031	0.031	0.3
Lewis County	0.084	0.072	0.061	0.072	0.072	0.044	0.044	0.055	0.038	0.032	0.026	0.021	0.032	0.3
Lynnwood	0.068	0.099	0.084	0.066	0.079	0.037	0.042	0.047	0.037	0.035	0.042	0.027	0.024	0.3

Table D3a
(Continued)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14+
Moses Lake	0.068	0.043	0.075	0.043	0.043	0.049	0.055	0.023	0.036	0.017	0.049	0.030	0.049	0.451
Mount Vernon	0.071	0.037	0.061	0.044	0.041	0.037	0.024	0.034	0.047	0.041	0.034	0.041	0.044	0.467
North Seattle	0.068	0.047	0.064	0.054	0.070	0.039	0.045	0.043	0.029	0.032	0.045	0.018	0.039	0.426
Olympia	0.086	0.083	0.086	0.060	0.049	0.043	0.032	0.049	0.055	0.029	0.032	0.024	0.026	0.371
Rainier	0.075	0.046	0.079	0.033	0.069	0.052	0.043	0.027	0.039	0.025	0.035	0.014	0.033	0.449
Renton	0.076	0.047	0.091	0.049	0.064	0.027	0.047	0.041	0.052	0.021	0.047	0.039	0.025	0.402
Spokane	0.061	0.031	0.084	0.038	0.054	0.031	0.049	0.040	0.039	0.038	0.042	0.036	0.043	0.447
Sunnyside	0.027	0.032	0.032	0.023	0.027	0.032	0.023	0.014	0.045	0.023	0.054	0.027	0.054	0.613
Tri-Cities	0.061	0.064	0.071	0.041	0.054	0.041	0.031	0.034	0.037	0.037	0.044	0.041	0.044	0.439
Walla Walla	0.051	0.022	0.039	0.034	0.039	0.028	0.034	0.028	0.039	0.022	0.063	0.028	0.051	0.551
Wenatchee	0.034	0.039	0.043	0.039	0.043	0.064	0.064	0.043	0.056	0.039	0.052	0.013	0.030	0.476
Yakima	0.067	0.034	0.073	0.058	0.039	0.023	0.062	0.032	0.058	0.028	0.039	0.032	0.049	0.438

Table D3b

Estimated proportion of WREB assigned claimants who return to work
in 1..30 weeks since Effective Date of Claim (P)
Second quarter 1988 estimates

	1	2	3	4	5	6	7	8	9	10	11	12	13	14+
Aberdeen	0.050	0.141	0.056	0.063	0.036	0.063	0.030	0.030	0.030	0.043	0.063	0.030	0.023	0.362
Auburn	0.082	0.057	0.082	0.040	0.079	0.033	0.065	0.026	0.038	0.035	0.057	0.021	0.035	0.375
Bellevue	0.060	0.082	0.055	0.039	0.077	0.044	0.087	0.052	0.041	0.017	0.047	0.023	0.041	0.367
Bellingham	0.050	0.059	0.084	0.038	0.042	0.072	0.042	0.046	0.050	0.029	0.038	0.046	0.029	0.412
Bremerton	0.087	0.061	0.087	0.061	0.056	0.035	0.040	0.020	0.020	0.030	0.035	0.020	0.051	0.417
Cowlitz	0.104	0.083	0.088	0.072	0.067	0.040	0.056	0.046	0.040	0.019	0.014	0.046	0.040	0.309
Everett	0.059	0.080	0.086	0.051	0.057	0.030	0.062	0.022	0.033	0.046	0.044	0.022	0.028	0.402
Lewis County	0.012	0.114	0.080	0.080	0.029	0.021	0.105	0.029	0.054	0.029	0.021	0.012	0.012	0.431
Lynnwood	0.073	0.122	0.108	0.052	0.038	0.052	0.031	0.035	0.045	0.010	0.049	0.035	0.038	0.349
Moses Lake	0.043	0.019	0.081	0.027	0.066	0.058	0.112	0.058	0.050	0.035	0.012	0.012	0.050	0.421
Mount Vernon	0.094	0.086	0.047	0.034	0.030	0.022	0.022	0.030	0.039	0.026	0.026	0.030	0.039	0.505
North Seattle	0.080	0.063	0.092	0.030	0.058	0.044	0.054	0.013	0.070	0.025	0.039	0.035	0.025	0.396
Olympia	0.077	0.077	0.126	0.044	0.065	0.057	0.049	0.028	0.024	0.024	0.012	0.020	0.036	0.381
Rainier	0.099	0.033	0.111	0.023	0.047	0.035	0.045	0.033	0.035	0.033	0.045	0.028	0.025	0.440
Renton	0.096	0.045	0.115	0.057	0.069	0.035	0.055	0.021	0.035	0.030	0.023	0.028	0.035	0.380
Spokane	0.081	0.034	0.107	0.057	0.066	0.036	0.045	0.038	0.034	0.017	0.030	0.030	0.040	0.414
Sunnyside	0.026	0.041	0.055	0.019	0.062	0.055	0.033	0.019	0.055	0.012	0.033	0.012	0.019	0.621
Tri-Cities	0.066	0.049	0.075	0.031	0.071	0.062	0.058	0.040	0.049	0.026	0.044	0.035	0.022	0.396
Walla Walla	0.059	0.059	0.086	0.059	0.018	0.004	0.100	0.059	0.059	0.031	0.004	0.045	0.018	0.403
Wenatchee	0.065	0.034	0.057	0.042	0.042	0.084	0.084	0.053	0.046	0.046	0.031	0.027	0.031	0.383
Yakima	0.051	0.076	0.051	0.043	0.040	0.037	0.051	0.037	0.046	0.040	0.026	0.062	0.040	0.444

Table D3c

Estimated proportion of WREB assigned claimants who return to work
in 1..30 weeks since Effective Date of Claim (P)
Third quarter 1988 estimates

	1	2	3	4	5	6	7	8	9	10	11	12	13	14
Aberdeen	0.068	0.074	0.116	0.074	0.092	0.074	0.063	0.039	0.039	0.021	0.027	0.033	0.021	0.
Auburn	0.082	0.028	0.136	0.045	0.048	0.040	0.070	0.031	0.065	0.016	0.038	0.018	0.040	0.
Bellevue	0.058	0.078	0.094	0.081	0.073	0.032	0.048	0.032	0.040	0.027	0.027	0.030	0.022	0.
Bellingham	0.083	0.092	0.074	0.047	0.070	0.056	0.078	0.021	0.039	0.021	0.030	0.043	0.030	0.
Bremerton	0.044	0.094	0.108	0.039	0.029	0.014	0.029	0.064	0.074	0.034	0.044	0.009	0.029	0.
Cowlitz	0.076	0.096	0.132	0.076	0.088	0.059	0.051	0.043	0.031	0.027	0.023	0.011	0.027	0.
Everett	0.047	0.069	0.110	0.069	0.072	0.039	0.042	0.036	0.031	0.031	0.026	0.023	0.039	0.
Lewis County	0.074	0.055	0.119	0.061	0.048	0.042	0.055	0.036	0.048	0.023	0.029	0.016	0.048	0.
Lynnwood	0.080	0.084	0.178	0.089	0.087	0.036	0.044	0.024	0.029	0.024	0.029	0.027	0.015	0.
Moses Lake	0.055	0.029	0.061	0.080	0.080	0.067	0.048	0.061	0.055	0.023	0.023	0.035	0.042	0.
Mount Vernon	0.101	0.066	0.066	0.030	0.035	0.039	0.026	0.044	0.048	0.026	0.035	0.009	0.039	0.
North Seattle	0.068	0.045	0.063	0.043	0.070	0.034	0.041	0.034	0.050	0.032	0.041	0.025	0.041	0.
Olympia	0.107	0.061	0.076	0.061	0.080	0.019	0.049	0.023	0.030	0.030	0.030	0.023	0.030	0.
Rainier	0.103	0.029	0.096	0.041	0.057	0.034	0.043	0.027	0.050	0.032	0.027	0.022	0.020	0.
Renton	0.090	0.057	0.105	0.067	0.042	0.034	0.049	0.034	0.032	0.024	0.034	0.024	0.027	0.
Spokane	0.085	0.027	0.106	0.021	0.075	0.035	0.060	0.041	0.041	0.019	0.041	0.021	0.035	0.
Sunnyside	0.058	0.028	0.088	0.070	0.022	0.028	0.016	0.052	0.022	0.058	0.034	0.016	0.034	0.
Tri-Cities	0.073	0.039	0.096	0.035	0.046	0.035	0.043	0.027	0.039	0.039	0.043	0.023	0.066	0.4
Walla Walla	0.122	0.033	0.063	0.063	0.019	0.033	0.048	0.004	0.063	0.019	0.004	0.019	0.063	0.4
Wenatchee	0.040	0.040	0.107	0.061	0.081	0.055	0.066	0.035	0.055	0.019	0.030	0.061	0.035	0.3
Yakima	0.068	0.062	0.091	0.074	0.059	0.050	0.039	0.065	0.059	0.033	0.036	0.033	0.039	0.3

Table D3d

Estimated proportion of WREB assigned claimants who return to work in 1..30 weeks since Effective Date of Claim (P)
Fourth quarter 1988 estimates

	1	2	3	4	5	6	7	8	9	10	11	12	13	14+
Aberdeen	0.072	0.128	0.103	0.062	0.065	0.041	0.051	0.027	0.055	0.045	0.051	0.010	0.027	0.290
Auburn	0.063	0.042	0.123	0.042	0.072	0.038	0.029	0.034	0.038	0.034	0.042	0.025	0.025	0.414
Bellevue	0.034	0.063	0.067	0.038	0.030	0.042	0.030	0.026	0.050	0.034	0.017	0.026	0.026	0.537
Bellingham	0.089	0.114	0.089	0.053	0.053	0.016	0.028	0.028	0.065	0.016	0.016	0.016	0.004	0.418
Bremerton	0.108	0.056	0.111	0.022	0.082	0.052	0.063	0.029	0.048	0.022	0.059	0.011	0.041	0.311
Cowlitz	0.089	0.123	0.082	0.075	0.085	0.041	0.034	0.041	0.037	0.020	0.031	0.017	0.024	0.319
Everett	0.032	0.059	0.074	0.059	0.024	0.043	0.032	0.024	0.032	0.028	0.047	0.012	0.032	0.524
Lewis County	0.054	0.088	0.165	0.058	0.058	0.037	0.054	0.041	0.037	0.041	0.054	0.037	0.020	0.270
Lynnwood	0.079	0.042	0.069	0.069	0.031	0.063	0.021	0.026	0.021	0.037	0.021	0.021	0.021	0.513
Moses Lake	0.040	0.040	0.049	0.014	0.040	0.032	0.049	0.014	0.067	0.032	0.023	0.023	0.032	0.569
Mount Vernon	0.091	0.024	0.058	0.071	0.031	0.024	0.085	0.031	0.038	0.031	0.018	0.024	0.024	0.468
North Seattle	0.080	0.045	0.135	0.043	0.096	0.037	0.064	0.029	0.060	0.030	0.063	0.018	0.043	0.276
Olympia	0.096	0.070	0.129	0.101	0.078	0.034	0.041	0.034	0.036	0.036	0.047	0.012	0.023	0.285
Rainier	0.082	0.052	0.123	0.036	0.106	0.027	0.059	0.029	0.038	0.018	0.057	0.029	0.027	0.331
Renton	0.100	0.057	0.062	0.073	0.057	0.052	0.036	0.036	0.052	0.025	0.046	0.052	0.025	0.357
Spokane	0.080	0.032	0.125	0.039	0.093	0.031	0.074	0.034	0.056	0.025	0.030	0.020	0.034	0.345
Sunnyside	0.043	0.036	0.094	0.029	0.111	0.018	0.053	0.018	0.087	0.025	0.059	0.023	0.050	0.369
Tri-Cities	0.066	0.072	0.088	0.040	0.052	0.030	0.056	0.034	0.062	0.040	0.060	0.028	0.034	0.350
Walla Walla	0.036	0.020	0.098	0.010	0.051	0.036	0.104	0.036	0.041	0.030	0.062	0.010	0.020	0.479
Wenatchee	0.028	0.013	0.049	0.057	0.013	0.042	0.020	0.020	0.020	0.028	0.035	0.020	0.035	0.648
Yakima	0.067	0.058	0.136	0.045	0.094	0.035	0.052	0.042	0.045	0.025	0.042	0.014	0.028	0.331

q_{jtik} - The proportion whose reemployment deadline arrives.

Using data on benefit entitlement duration and the experimental design, a proxy distribution of the proportion of WREB treatment assigned claimants whose qualification period will expire *i* weeks after enrollment in week *t* was developed for each JSC in the State of Washington. These proportions, q_{jtik}, are computed for each JSC, *j*, for different weeks, i, relative to different weeks, *t*, since the start of the bonus experiment, because they have been computed for the four different quarters of the year, *k*.

Half of the claimants were assigned to a qualification period of 2/10 of their entitlement (potential duration) plus a waiting week, rounded up to the next whole week.

The other half was assigned to a qualification period of 4/10 of their entitlement plus a waiting week, rounded up to the next whole week. For example, if 100 people were entitled to receive benefits for 30 weeks, 50 were assigned to a 7 week qualification period (0.2 x 30 weeks + 1 waiting week) and 50 were assigned to a 13 week qualification period (0.4 x 30 weeks + 1 waiting week). Given minimum and maximum potential duration of 16 and 30 weeks, qualification periods in the simulation model range from a minimum of 4 weeks to a maximum of 13 weeks. Taking the distribution of individuals with various qualification periods and dividing by the total number of people produces a distribution of the proportion of people who have qualification periods of 4 weeks, 5 weeks and so on up to 13 weeks. This distribution was developed by combining quarterly CWBH data on the distribution of the duration of entitlement for the years 1986 and 1987. If 5 percent of the people have a 4 week qualification period, at the beginning of the fifth week of the experiment, that entire 5 percent group could be eligible for the bonus, assuming all found jobs. Of course, less than the whole 5 percent group will be bonus eligible because the probability of finding a job within 4 weeks is less than one.

The following four tables, one for each of the four calendar quarters of 1988, show the proportion of WREB assigned claimants whose qualification period is expected to expire i weeks after filing an initial claim.

Table D4a

Estimated proportion of WREB assigned claimants whose Qualification Period expires in 3..13 weeks of their Effective Date of Claim - first quarter 1988 (Q)

	3	4	5	6	7	8	9	10	11	12	13
Aberdeen	0.000	0.003	0.076	0.082	0.342	0.027	0.049	0.024	0.057	0.038	0.302
Auburn	0.000	0.004	0.060	0.101	0.339	0.014	0.046	0.042	0.060	0.040	0.295
Bellevue	0.000	0.000	0.050	0.068	0.381	0.023	0.027	0.025	0.043	0.041	0.340
Bellingham	0.000	0.002	0.114	0.100	0.287	0.047	0.067	0.045	0.055	0.045	0.240
Bremerton	0.000	0.005	0.072	0.115	0.313	0.034	0.038	0.052	0.059	0.034	0.279
Cowlitz	0.000	0.002	0.060	0.099	0.341	0.018	0.042	0.044	0.053	0.064	0.277
Everett	0.000	0.001	0.091	0.098	0.311	0.037	0.054	0.041	0.056	0.042	0.268
Lewis County	0.000	0.000	0.091	0.114	0.294	0.023	0.069	0.037	0.077	0.046	0.249
Lynnwood	0.000	0.001	0.053	0.088	0.359	0.017	0.036	0.023	0.065	0.044	0.313
Moses Lake	0.000	0.010	0.138	0.128	0.234	0.042	0.096	0.058	0.067	0.058	0.170
Mount Vernon	0.000	0.000	0.123	0.093	0.283	0.052	0.072	0.038	0.055	0.043	0.240
North Seattle	0.000	0.001	0.073	0.095	0.331	0.026	0.048	0.033	0.063	0.029	0.301
Olympia	0.000	0.003	0.088	0.084	0.328	0.033	0.055	0.033	0.050	0.051	0.276
Rainier	0.000	0.002	0.080	0.112	0.308	0.028	0.052	0.043	0.067	0.040	0.268
Renton	0.000	0.001	0.052	0.085	0.362	0.013	0.040	0.032	0.053	0.043	0.319
Spokane	0.000	0.003	0.078	0.096	0.326	0.038	0.040	0.028	0.068	0.055	0.269
Sunnyside	0.000	0.000	0.173	0.173	0.155	0.073	0.100	0.061	0.111	0.052	0.102
Tri-Cities	0.000	0.003	0.106	0.131	0.262	0.035	0.071	0.053	0.078	0.040	0.219
Walla Walla	0.000	0.009	0.087	0.198	0.215	0.029	0.058	0.076	0.116	0.047	0.166
Wenatchee	0.000	0.000	0.062	0.139	0.298	0.026	0.036	0.067	0.073	0.060	0.238
Yakima	0.000	0.003	0.109	0.124	0.267	0.039	0.070	0.041	0.082	0.060	0.204

Table D4b

Estimated proportion of WREB assigned claimants whose Qualification Period expires in 3..13 weeks of their Effective Date of Claim - second quarter 1988 (Q)

	3	4	5	6	7	8	9	10	11	12	13
Aberdeen	0.000	0.007	0.056	0.105	0.340	0.013	0.042	0.036	0.065	0.042	0.294
Auburn	0.000	0.004	0.059	0.120	0.322	0.021	0.038	0.041	0.077	0.051	0.268
Bellevue	0.000	0.003	0.048	0.080	0.372	0.017	0.031	0.027	0.052	0.036	0.334
Bellingham	0.000	0.002	0.093	0.142	0.265	0.042	0.051	0.047	0.095	0.042	0.220
Bremerton	0.000	0.005	0.078	0.112	0.310	0.034	0.044	0.036	0.073	0.047	0.260
Cowlitz	0.000	0.000	0.061	0.088	0.351	0.029	0.032	0.027	0.061	0.027	0.324
Everett	0.000	0.000	0.074	0.107	0.319	0.024	0.050	0.032	0.075	0.045	0.274
Lewis County	0.000	0.000	0.072	0.114	0.314	0.030	0.042	0.047	0.068	0.047	0.267
Lynnwood	0.000	0.000	0.064	0.078	0.357	0.026	0.038	0.021	0.057	0.023	0.334
Moses Lake	0.000	0.000	0.093	0.143	0.264	0.031	0.062	0.062	0.081	0.027	0.236
Mount Vernon	0.000	0.002	0.099	0.144	0.258	0.026	0.073	0.054	0.090	0.028	0.227
North Seattle	0.000	0.001	0.051	0.107	0.342	0.020	0.031	0.039	0.068	0.037	0.304
Olympia	0.000	0.002	0.082	0.102	0.316	0.039	0.043	0.045	0.057	0.055	0.259
Rainier	0.000	0.002	0.079	0.108	0.313	0.031	0.048	0.045	0.062	0.042	0.270
Renton	0.000	0.001	0.070	0.099	0.331	0.023	0.047	0.048	0.051	0.034	0.295
Spokane	0.000	0.006	0.066	0.105	0.329	0.019	0.047	0.037	0.066	0.044	0.279
Sunnyside	0.000	0.000	0.105	0.109	0.286	0.029	0.076	0.033	0.076	0.062	0.225
Tri-Cities	0.000	0.000	0.067	0.109	0.324	0.029	0.038	0.045	0.065	0.038	0.286
Walla Walla	0.000	0.000	0.104	0.132	0.264	0.056	0.049	0.076	0.056	0.028	0.236
Wenatchee	0.000	0.000	0.073	0.151	0.277	0.015	0.057	0.073	0.078	0.046	0.231
Yakima	0.000	0.000	0.098	0.169	0.233	0.032	0.066	0.055	0.114	0.058	0.175

Table D4c

Estimated proportion of WREB assigned claimants whose Qualification Period expires in 3..13 weeks of their Effective Date of Claim - third quarter 1988 (Q)

	3	4	5	6	7	8	9	10	11	12	13
Aberdeen	0.000	0.003	0.067	0.098	0.335	0.034	0.034	0.027	0.067	0.030	0.305
Auburn	0.000	0.001	0.060	0.107	0.332	0.031	0.029	0.045	0.060	0.029	0.304
Bellevue	0.000	0.004	0.063	0.085	0.352	0.028	0.035	0.036	0.048	0.031	0.317
Bellingham	0.000	0.000	0.070	0.098	0.332	0.030	0.041	0.041	0.057	0.043	0.289
Bremerton	0.000	0.003	0.075	0.101	0.325	0.018	0.057	0.034	0.064	0.034	0.291
Cowlitz	0.000	0.006	0.081	0.091	0.329	0.025	0.056	0.037	0.052	0.033	0.291
Everett	0.000	0.003	0.078	0.089	0.332	0.031	0.047	0.041	0.049	0.041	0.289
Lewis County	0.000	0.003	0.070	0.080	0.350	0.023	0.047	0.030	0.050	0.060	0.287
Lynnwood	0.000	0.001	0.053	0.096	0.352	0.016	0.037	0.029	0.065	0.050	0.301
Moses Lake	0.000	0.003	0.043	0.135	0.322	0.016	0.026	0.043	0.092	0.043	0.276
Mount Vernon	0.000	0.000	0.078	0.106	0.316	0.035	0.044	0.037	0.069	0.025	0.290
North Seattle	0.000	0.000	0.061	0.089	0.350	0.021	0.040	0.026	0.063	0.044	0.306
Olympia	0.000	0.006	0.075	0.089	0.335	0.034	0.042	0.040	0.050	0.050	0.280
Rainier	0.000	0.002	0.079	0.102	0.319	0.028	0.051	0.040	0.060	0.030	0.288
Renton	0.000	0.000	0.064	0.088	0.349	0.028	0.036	0.027	0.061	0.033	0.316
Spokane	0.000	0.001	0.074	0.084	0.343	0.028	0.046	0.034	0.050	0.049	0.293
Sunnyside	0.000	0.000	0.081	0.152	0.267	0.022	0.059	0.065	0.087	0.031	0.236
Tri-Cities	0.000	0.008	0.089	0.117	0.294	0.016	0.073	0.032	0.083	0.048	0.240
Walla Walla	0.000	0.008	0.094	0.188	0.219	0.031	0.063	0.031	0.156	0.023	0.188
Wenatchee	0.000	0.003	0.077	0.170	0.253	0.011	0.066	0.051	0.120	0.072	0.178
Yakima	0.000	0.003	0.067	0.162	0.271	0.021	0.046	0.055	0.107	0.067	0.201

Table D4d

Estimated proportion of WREB assigned claimants whose Qualification Period expires in 3..13 weeks of their Effective Date of Claim - fourth quarter 1988 (Q)

	3	4	5	6	7	8	9	10	11	12	13
Aberdeen	0.000	0.002	0.042	0.079	0.379	0.008	0.034	0.028	0.051	0.026	0.352
Auburn	0.000	0.002	0.060	0.095	0.345	0.024	0.036	0.032	0.062	0.036	0.308
Bellevue	0.000	0.000	0.037	0.092	0.371	0.014	0.023	0.032	0.060	0.020	0.351
Bellingham	0.000	0.000	0.086	0.117	0.296	0.031	0.056	0.037	0.080	0.031	0.265
Bremerton	0.000	0.002	0.065	0.089	0.346	0.029	0.036	0.033	0.056	0.040	0.304
Cowlitz	0.000	0.000	0.081	0.109	0.310	0.022	0.059	0.032	0.077	0.049	0.261
Everett	0.000	0.005	0.068	0.086	0.346	0.021	0.047	0.037	0.050	0.031	0.309
Lewis County	0.000	0.002	0.067	0.123	0.310	0.022	0.044	0.032	0.091	0.039	0.268
Lynnwood	0.000	0.000	0.057	0.079	0.364	0.029	0.029	0.039	0.039	0.039	0.325
Moses Lake	0.000	0.000	0.088	0.125	0.288	0.056	0.031	0.050	0.075	0.081	0.206
Mount Vernon	0.000	0.000	0.070	0.117	0.313	0.030	0.039	0.048	0.070	0.035	0.278
North Seattle	0.000	0.001	0.057	0.090	0.353	0.025	0.032	0.032	0.058	0.035	0.317
Olympia	0.000	0.001	0.074	0.085	0.341	0.023	0.051	0.028	0.057	0.040	0.300
Rainier	0.000	0.002	0.070	0.117	0.313	0.026	0.044	0.044	0.072	0.046	0.265
Renton	0.000	0.005	0.056	0.088	0.356	0.016	0.040	0.035	0.053	0.043	0.307
Spokane	0.000	0.003	0.076	0.124	0.299	0.026	0.050	0.042	0.082	0.044	0.253
Sunnyside	0.000	0.000	0.199	0.167	0.133	0.065	0.135	0.080	0.088	0.039	0.094
Tri-Cities	0.000	0.000	0.068	0.124	0.308	0.025	0.043	0.055	0.069	0.036	0.273
Walla Walla	0.003	0.003	0.128	0.148	0.224	0.034	0.090	0.066	0.083	0.031	0.190
Wenatchee	0.000	0.000	0.054	0.147	0.299	0.016	0.038	0.054	0.092	0.049	0.250
Yakima	0.000	0.001	0.092	0.132	0.275	0.030	0.063	0.057	0.076	0.053	0.221

III. An Outline of the WREB Simulation Model

In general terms, the model may be described as iterative. Given an inflow of eligible claimants each week, a certain proportion are selected as treatment subjects, among these a still smaller group become bonus eligible by getting reemployed before their qualification period expires. And finally, among the group eligible to do so, a certain proportion actually submit bonus vouchers for payment. Given the WREB bonus budget, this final estimate of dollar bonuses paid per offer made--the take up rate--provides the basis for regulating enrollment into the WREB demonstration.

Given the above discussion on the data an outline of the model may be meaningfully sketched. Taking the initial inflow of monetarily valid claimants as given, the treatment assignment algorithm guarantees that on average sixteen percent of I_{jt} will be given an offer at all sites except Rainier.

$$T_{jt} = (0.16)*I_{jt} \;;\; j = 1,\ldots,21 \text{ excluding Rainier}$$
$$T_{jt} = (0.32)*I_{jt} \;;\; \text{for } j = \text{Rainier}$$

In the simulation model it is assumed that, among those who are given a WREB bonus offer ten percent are later found to be non-monetarily ineligible for UI and therefore bonus ineligible. In the WREB demonstration the people who are sent enrollment letters, E_{jt}, have no indefinite non-monetary stops.

$$E_{jt} = (0.9)*T_{jt}$$

This is the group on which the reemployment probabilities, p_{jtik}, and the qualification period expiration probabilities, q_{jtik}, are applied to determine the group expected to qualify for the bonus.

The way in which the simulation model arrives at this estimate may be explained in two simple equations once the model has been initialized. To reduce clutter in the exposition we suppress subscripts and examine the experience of a group of enrollees, E, at a particular JSC, j, who are enrolled in a particular program week, t, which occurs in a particular quarter, k. During the week of enrollment a certain fraction, p_1, of this cohort becomes reemployed, J,

$$J_1 = p_1{*}E.$$

The residual stock of bonus eligible claimants still searching for this cohort at the end of the first week is:

$$S_1 = (1 - p_1){*}E.$$

Since the subscript i indicates the number of weeks since enrollment in week t, the general relation between the stock of searchers, S, and the number of bonus eligible claimants who find jobs, J, is:

(1) $$J_i = p_i{*}S_{i-1} \; ; i = 1, \ldots, 13,$$

where; $S_0 = E$. This process continues for thirteen weeks, with the stock of bonus eligible searchers decreasing each week by the proportion p_i until the fourth week after enrollment, $i = 4$. In the fourth week the stock of searchers also decreases because the bonus qualification period for some searchers expires at the end of the week. A general expression describing how the stock of bonus eligible claimants changes in the relevant weeks following treatment assignment is:

(2) $$S_i = S_{i-1}{*}(1 - p_i){*}(1 - q_i) \; ; i = 1, \ldots, 13,$$

where; $q_i = 0$; $i = 1,2,3$ and $S_0 = E$.

To get at an estimate of how long to continue enrolling claimants into the WREB demonstration, we first add up all those who became reemployed before their WREB deadline by JSC. The simulation model then assumes that because of things like recall and union hiring hall referral, 50 percent of such claimants will file a valid notice of hire, N_{jt}. Restoring the JSC and enrollment week subscripts this is:

$N_{jt} = 0.5*[Sum_i(J_{jti})]$; $i = 1, \ldots, 13$.

It is expected that among this number, 15 percent will fail to remain employed full time for the required four month period or will simply fail to file a valid voucher for bonus payment. The resulting number of bonuses paid to the WREB enrollees for a given week t is:

$V_{jt} = 0.85*(N_{jt})$.

By using the data available on average weekly benefits paid by JSC relative to different reference weeks, an estimate of the dollar cost of bonuses for claimants enrolled into WREB in a particular week by JSC is:

$B_{jt} = (15/4)*(WBA_{jt})*V_{jt}$.

And summing these figures over all of the 21 JSCs yields an estimate of the dollar bonus cost of enrolling claimants into WREB in a particular week t,

$B_t = Sum_j(B_{jt})$; $j = 1, \ldots, 21$.

The final step in the simulation model is to estimate how long enrollment should continue. After the expected bonus qualification success of each week's enrollment has been evaluated, a check is done to see if the bonus payment budget has been exceeded. The following check is performed:

if $[(Budget - Sum_t B_t),\ t = 1,2,3,...] > 0$: then continue enrolling,

otherwise: suspend enrollment.

If enrollment is suspended in week T, the cost of enrolling for (T - 1) weeks is estimated to be: $Sum_t B_t$, $t = 1, \ldots, (T\text{-}1)$, and the maximum enrollment period estimate is between (T - 1) and T weeks.

IV. Methods for Preliminary and Revised Update Estimates

In order to execute the simulation model, data for each of the 21 WREB JSCs is needed on the following four concepts: 1) new, monetarily valid claims, 2) weekly benefit amounts, 3) the duration of benefits claimed, and 4) the length of benefit entitlement. While there is immediate potential for updating the model components related to the first two of these, updates of items based on the last two must be deferred. Hence we will have, enrollment simulation model updates which are either preliminary or completely revised.

Information on new, monetarily valid claims, I_{jt}, (or treatment assignments, T_{jt}), and actual weekly benefit amounts (or actual dollar bonus offers, B_{jt}) is available weekly beginning at the start of the program. This data may be directly used to update the model to arrive at preliminary updates. Further updates of B_{jt} will be made as Notices of Hire (NOH) and vouchers are submitted. These values of B will be effected by no expected tendency for those with larger bonus offers to take jobs sooner.

The information on the duration of benefits claimed and the length of benefit entitlement must be accumulated for some time before meaningful estimates of the distribution of the timing of return to work proportions (p_{jt}) and the distribution of the timing of qualification period expiration proportions (q_{jt}) can be formed. In particular, relevant data on the acutal duration of benefits calimed will begin to be completely available for a particular week's WREB enrollees thirteen weeks after enrollment. Therefore preparation of revised enrollment estimates must be deferred until that time.

In Section V three sets of simulation results are presented and discussed. The results are referred to as: Baseline (BL), Observed Program/Baseline (O/BL), and Observed Program/Adjusted Baseline (O/ABL). The last two are preliminary update results. They incorporate actual WREB demonstration information on treatment assignments (T_{jt}) and dollar bonus offers (B_{jt}).

Preliminary Updates

The BL results are for the data and model discussed in Sections II and III. For the O/BL results actual program data replaces the baseline data for the weeks observed, but the weeks in the simulation beyond the observed period remain at baseline levels. To understand the O/ABL update process, suppose that WREB has been operating for w weeks, actual program data replaces the baseline data on T_{jt} and B_{jt} for these weeks but, the information for the (T - w) weeks remaining in the simulation is updated by the average error between the program experience and the baseline value of the variables. The formulae for computing adjustment factors to be used on T_{jt} and B_{jt} are:

$$a_j = \{Sum_t(T'_{jt} - T_{jt})\}/Sum_t T_{jt} \quad ; t = 1, \ldots, w \, ; j = 1, \ldots, 21, \text{and}$$
$$b_j = \{Sum_t(B'_{jt} - B_{jt})\}/Sum_t B_{jt} \quad ; t = 1, \ldots, w \, ; j = 1 \, , \ldots, 21,$$

where the apostrophe denotes the observed program values of the variables. If the observed rate of treatment assignment is on average higher, lower, or the same as the baseline expectation a_j will be positive, negative, or zero respectively. Similarly for b_j. Naturally, the adjustment factors are applied to simulation data beyond the observed period as follows:

$T''_{jt} = T_{jt}*(1 + a_j)$; $t = (w + 1), \ldots, T$, and

$B''_{jt} = B_{jt}*(1 + b_j)$; $t = (w + 1), \ldots, T$,

where T'' and B'' are the adjusted baselind (ABL) values of T and B.

By using the program data in the above way to make preliminary updates to the simulation model, the seasonality in the initial claims load and the intertemporal variation in the size of the bonus offers within JSCs will be preserved.

The preliminary updates are therefore able to capture expected enrollment timing changes related to claimant load changes and average weekly benefit changes. It should be noted however that the preliminary updates of the enrollment period forecast include no change in the implicit take up rate embodied in the p_{jt} and q_{jt} matrices.

Revised Estimates

Updating the matrices of p_{jt} and q_{jt} to get completely revised enrollment period estimates for the simulation model is a more involved process. Data becomes available on the actual reemployment deadlines of treatment assigned claimants weekly. However, the matrix of q_{jt}, which contains a row for each WREB JSC and a column for each potential qualification period, cannot be meaningfully updated each week. The elements of q_{jt} are relative frequency estimates, and it may take several weeks before any observations fall into certain cells, let alone enough to give statistical significance to the distribution. Similar problems of sample size apply to updating the p_{jt} matrix, but the situation is even more difficult since the necessary data become available only at the end of a completed spell of unemployment or the end of the qualification period, whichever comes first.

It is expected that by 16 weeks after full start-up of the demonstration, i.e. 16 weeks after WREB enrollment was occurring at all twenty-one sites, in July, 1988 adequate information will be available to produce enrollment period estimates based on completely revised information. We plan to directly update the net result J_{jt}, rather than updating each matrix p_{jt} and q_{jt} separately. From Section III substituting from (2) into (1) and summing over i the implicit dependence of J_{jti} on p_{jti} and q_{jti} is clear,

$$Sum_i\{J_{jti}\} = Sum_i\{[S_{jti}/(1 - p_i)*(1 - q_i)][p_i]\} \ ; i = 1, \ldots, 13.$$

Around the beginning of July information on J_{jt} for the first week of enrolling at all 21 sites should be available. Using this information two types of revised estimates, analogous to those for T_{jt} and B_{jt} will be prepared. That is, one revision simply replaces baseline values of J_{jt} with observed values (O/BL), and another also updates terminal job finding success rates for other weeks by the average percentage error observed (O/ABL).

Further Updates

The remaining steps in the simulation all amount to expectations for aggregate average behavior. We anticipate updating these proportions, as program information becomes available, by using an average of the observed and baseline rates weighted by the applicable fractions of program enrollment, i.e. (observed enrollment/expected enrollment) = (oe/ee) and [1 - (oe/ee)].

Specifically, one baseline assumption is that fifty percent of claimants who become reemployed before their qualification period ends will file valid notices of hire,

$$N_{jt} = 0.5*[Sum_i(J_{jti})] \ ; i = 1, \ldots, 13 \ ; \ j = 1, \ldots, 21.$$

This assumption embodies the expectation that many claimants will either return to their previous job, be placed on their new job by a union hiring hall, or simply fail to submit a Notice of Hire form. About fourteen weeks after enrollment in week t, nearly complete data should be available to compute an observed rate of Notice of Hire filing for that enrollment week, t, by JSC, $e_{jt} = [N_{jt}]/[Sum_i(J_{jti})]$, and across JSCs, $e_t = \{Sum_j(e_{jt})/21\}$.

If t was the first week of an expected 34 weeks of enrollment then the updated Notice of Hire filing rate would be equal to: $\{[(1/34)*e_t] + [(33/34)*0.5]\}$. And for n weeks of observed Notice of Hire filing: $\{[(n/34)*(Sum_t e_t/n)] + [((34-n)/34)*0.5]\}$.

Another baseline assumption is that eighty-five percent of those who file a valid Notice of Hire (NOH) will file a valid bonus voucher,

$$V_{jt} = 0.85*(N_{jt}) \quad ; j = 1,\ldots,21,$$

Adjustments to this proportion similar to those for the NOH can be carried out. However, the information necessary to update this factor will only become available very late in the program. The next section presents, compres, and examines baseline simulation estimates and two preliminary updates based on observed WREB demonstration data.

V. Enrollment Monitoring Simulation Report - Preliminary Update

In Tables D6, D7, and D8 complete enrollment simulation model results are presented for the cases summarized in Table D5. Before presenting these tables, a concise review of the differences between and meaning of the three different sets of simulation results is given.

The Baseline (BL) results provide the basic estimates for the characteristics of the enrollment process. They are based on quarterly data from the Washington CWBH for 1986 and 1987 on new initial claims, weekly benefit amounts, the duration of benefits, and the length of benefit entitlement for claimants at each of the 21 Job Service Centers (JSCs) selected. This data was provided by the UI Program Analysis Unit of the Washington State Employment Security Department and reflects the judgement of that unit that the volume of 1988 UI claims will exceed those for the period one year earlier by 9.5 percent.

Cumulative Data through 04/09/88
2 Weeks of Operation at all 21 Sites
Including Yakima Pilot Data

Table D5
A Summary of the Significant Results of the Simulations

Characteristic	BL	O/BL	O/ABL
Maximum weeks of enrollment is between:	32 - 33	33 - 34	37 - 38
Bonus cost for maximum enrollment is:	$ 1.242	$ 1.244	$ 1.210
Associated number of treatment assigned:	13,827	13,845	13,549
Expected number of bonuses paid:	2,162	2,166	2,097
The implicit bonus "take-up rate" is:	.156	.156	.155

BL - Baseline
O/BL - Observed Program/Baseline
O/ABL - Observed Program/ Adjusted Baseline

The Observed Program/Baseline (O/BL) results are based on a preliminary update of baseline data wherein actual program data replaces the baseline data for the weeks observed, but the weeks in the simulation beyond the observed period remain at baseline levels. The variables updated are the number of claimants treatment assigned (T) and the average bonus offer (B) by JSC.

The Observed Program/Adjusted Baseline (O/ABL) results are based on a preliminary update of baseline data wherein actual program data replaces the baseline data for observed weeks, and data for the remaining weeks in the simulation is updated by the average error between the program experience and the baseline value of the variables. As in O/BL, only T and B are updated.

Definitions of the table column headings are as follows:

I - The number of new initial claims by week by JSC.

T - The number of treatment assignments ($T = .16*I$).

E - The number of enrollment letters sent ($E = .9*T$).

J - The number reemployed in their qualification period ($J(E,p,q)$).

N - The number of valid Notices of Hire filed ($N = .5*J$).

V - The number of valid Bonus Vouchers filed ($V = .85*N$).

B - The average bonus offer by JSC ($B = (15/4)*WBA*V$).

Table D6
BASELINE CASE
BONUS PAYMENTS ASSUMING 10% NONMON INELIGIBLE,
50% RECALL AND UNION,
AND 15% NO VOUCHER AND 4-MTH INVALID REEMPLOYMENT PERIOD

	EXPECTED TREATMENT ASSIGNED (T)	ENROLL LETTERS SENT (E)	JOB INQUAL PERIOD (J)	VALID NOH (N)	BONUSES PAID (V)	AVERAGE BONUS COST (B)	TOTAL COST OF ENROLLMENT
Aberdeen	308.71	277.84	125.12	62.56	53.18	629.54	33475.99
Auburn	872.32	785.09	330.86	165.43	140.62	582.92	81968.09
Bellevue	911.84	820.66	346.04	173.02	147.07	638.04	93835.11
Bellingham	471.20	424.08	175.66	87.83	74.66	563.28	42052.15
Bremerton	429.42	386.48	153.85	76.93	65.39	531.68	34765.12
Cowlitz County	470.76	423.69	199.46	99.73	84.77	622.19	52742.52
Everett	775.18	697.66	283.34	141.67	120.42	579.82	69822.04
Lewis County	301.88	271.69	114.59	57.30	48.70	568.58	27691.38
Lynnwood	717.18	645.46	299.30	149.65	127.20	605.14	76975.55
Moses Lake	284.98	256.48	101.35	50.68	43.07	522.96	22525.86
Mount Vernon	484.18	435.76	157.13	78.57	66.78	581.85	38856.72
North Seattle	1114.72	1003.25	400.56	200.28	170.24	623.69	106174.90
Olympia	583.58	525.23	226.56	113.28	96.29	583.81	56212.50
Rainier	1470.09	1323.08	520.27	260.13	221.11	565.32	124999.00
Renton	934.87	841.38	355.20	177.60	150.96	622.50	93972.41
Spokane	1087.74	978.96	395.96	197.98	168.28	541.27	91085.86
Sunnyside	369.08	332.17	111.29	55.64	47.30	465.50	22017.12
Tri-Cities	524.56	472.10	185.42	92.71	78.80	545.19	42962.60
Walla Walla	187.73	168.95	62.05	31.02	26.37	481.71	12702.88
Wenatchee	497.58	447.82	180.73	90.37	76.81	536.17	41183.31
Yakima	1029.02	926.11	363.31	181.65	154.41	497.00	76739.05
Total	13826.60	12443.94	5088.03	2544.02	2162.41	574.71	1242760.00

THE COST OF ENROLLING 32 WEEKS IS 1242760.00
MAX ENROLLMENT PERIOD IS BETWEEN 32 AND 33 WEEKS

Table D7

Cumulative Data Through 04/09/88
2 Weeks of Operation at all 21 Sites
Including Yakima Pilot Data

OBSERVED DATA AND BASELINE
BONUS PAYMENTS ASSUMING 10% NONMON INELIGIBLE,
50% RECALL AND UNION, AND 15% NO VOUCHER AND 4-MTH INVALID
REEMPLOYMENT PERIOD

	EXPECTED TREATMENT ASSIGNED	ENROLL LETTERS SENT	JOB INQUAL PERIOD	VALID NOH	BONUSES PAID	AVERAGE BONUS COST	TOTAL COST OF ENROLLMENT
	(T)	(E)	(J)	(N)	(V)	(B)	
Aberdeen	302.50	272.25	123.42	61.71	52.45	623.57	32707.28
Auburn	907.40	816.66	343.70	171.85	146.07	587.46	85812.95
Bellevue	906.17	815.56	342.33	171.16	145.49	635.79	92500.72
Bellingham	461.77	415.60	172.16	86.08	73.17	564.54	41307.14
Bremerton	405.40	364.86	145.75	72.88	61.94	530.99	32892.34
Cowlitz County	450.86	405.78	192.12	96.06	81.65	617.38	50410.70
Everett	782.46	704.21	284.42	142.21	120.88	589.80	71293.63
Lewis County	308.54	277.68	117.49	58.74	49.93	575.51	28737.00
Lynnwood	698.06	628.26	290.02	145.01	123.26	601.06	74084.52
Moses Lake	291.55	262.40	103.15	51.58	43.84	518.70	22739.04
Mount Vernon	493.67	444.30	160.35	80.18	68.15	584.20	39812.72
North Seattle	1096.33	986.69	396.81	198.41	168.65	620.99	104727.60
Olympia	578.01	520.21	225.32	112.66	95.76	586.50	56163.89
Rainier	1523.96	1371.56	541.12	270.56	229.97	564.14	129738.60
Renton	878.05	790.25	334.69	167.34	142.24	622.90	88602.13
Spokane	1110.28	999.25	405.60	202.80	172.38	545.98	94115.44
Sunnyside	403.23	362.90	122.39	61.20	52.02	460.09	23932.58
Tri-Cities	534.50	481.05	189.23	94.61	80.42	546.81	43975.76
Walla Walla	190.49	171.44	63.08	31.54	26.81	474.87	12730.86
Wenatchee	506.92	456.22	183.46	91.73	77.47	536.82	41855.45
Yakima	1015.35	913.82	359.84	179.92	152.93	494.12	75567.32
Total	13845.48	12460.93	5096.45	2548.22	2165.99	574.20	1243708.00

THE COST OF ENROLLING 33 WEEKS IS 1243708.00
MAX ENROLLMENT PERIOD IS BETWEEN 33 AND 34 WEEKS

Table D8

Cumulative Data Through 04/09/88
2 Weeks of Operation at all 21 Sites
Including Yakima Pilot Data

OBSERVED DATA AND ADJUSTED BASELINE
BONUS PAYMENTS ASSUMING 10% NONMON INELIGIBLE,
50% RECALL AND UNION, AND 15% NO VOUCHER AND 4-MTH INVALID
REEMPLOYMENT PERIOD

	EXPECTED TREATMENT ASSIGNED (T)	ENROLL LETTERS SENT (E)	JOB IN QUAL PERIOD (J)	VALID NOH (N)	BONUSES PAID (V)	AVERAGE BONUS COST (B)	TOTAL COST OF ENROLLMENT
Aberdeen	207.52	186.77	84.92	42.46	36.09	513.91	18547.51
Auburn	1143.73	1029.36	430.21	215.11	182.84	625.54	114373.70
Bellevue	866.59	779.93	318.12	159.06	135.20	622.95	84223.13
Bellingham	384.10	345.69	142.87	71.43	60.72	577.26	35050.26
Bremerton	234.60	211.14	86.05	43.03	36.57	556.35	20346.89
Cowlitz County	170.99	153.89	72.45	36.23	30.79	444.26	13680.06
Everett	778.82	700.94	277.42	138.71	117.90	663.95	78282.59
Lewis County	360.01	324.01	140.14	70.07	59.56	679.55	40474.33
Lynnwood	507.39	456.65	205.08	102.54	87.16	566.18	49348.89
Moses Lake	345.23	310.71	117.24	58.62	49.83	475.46	23689.85
Mount Vernon	545.90	491.31	177.89	88.94	75.60	612.81	46330.16
North Seattle	883.42	795.08	326.82	163.41	138.90	594.51	82577.73
Olympia	430.59	387.53	170.61	85.31	72.51	645.53	46807.90
Rainier	1888.29	1699.46	683.28	341.64	290.39	561.08	162934.50
Renton	343.38	309.04	130.96	65.48	55.66	564.48	31416.53
Spokane	1231.67	1108.50	456.58	228.29	194.05	624.65	121211.90
Sunnyside	754.94	679.45	237.90	118.95	101.11	454.39	45942.95
Tri-Cities	583.26	524.93	208.22	104.11	88.49	598.98	53005.63
Walla Walla	167.32	150.59	54.62	27.31	23.22	370.97	8612.28
Wenatchee	622.92	560.63	217.31	108.65	92.35	546.83	50502.32
Yakima	1098.37	988.54	395.10	197.55	167.92	491.18	82477.14
Total	13549.03	12194.13	4933.79	2466.90	2096.86	576.97	1209836.00

THE COST OF ENROLLING 37 WEEKS IS 1209836.00
MAX ENROLLMENT PERIOD IS BETWEEN 37 AND 38 WEEKS

The differences in these simulation results are clear. The net effect of using actual program data to update the model is to lengthen the expected enrollment period. Furthermore, the O/ABL update magnifies this expectation over the O/BL update. The causes for these changes can be clearly sorted out.

The preliminary revisions of the simulation model data involve updates only of claimant load and bonus size. The claimant load updates skip over (I) directly to (T), and the bonus updates average the actual bonus offers rather than using the (15/4) factor. Both of these updates replace the assumption implicit in the baseline case that the random assignment process is exact. Divergence between the three sets of simulation results can be attributed to:

1. True differences from WSESD projections in the number of new initial claims. Simply put, when the claims load is lower (higher) than expected, the enrollment period is longer (shorter) than expected.

2. True differences in the average weekly benefit amount. If the average WBA is down (up) the enrollment period should be longer (shorter) than baseline projections.

3. Errors in assignment. If the proportion of people given offers does not bear the expected relation to the claims load there will be a change in the forecast. The error could be due simply to chance with the figures converging to expected levels as enrollment continues, or could be due to systematic error which will not correc itself such as failure to properly produce green bars.

4. The update methods differ. As is obvious from the graphic on the first page, divergence between the baseline and observed data results in a shift of the bonus for the O/BL case but a pivot in the case of O/ABL. For the latter case the divergence is magnified. Which update is appropriate depends on whether the change is permanent or temporary.

Tables D9 and D10 summarize the expected and actual levels of treatment assignments and bonus offers. Using this data the accuracy of baseline estimates can be assessed. Soon data on the actual volume of new, monetarily valid initial claims will be available to assess the accuracy of assignment. Since the update data used here was gathered in weeks at the end of a benefit quarter and in the first week of a new benefit quarter, attaching particular significance to trends is risky. Both the O/BL and O/ABL are useful estimates subject to the caveats suggested in point 4.

Table D9

WREB Program Data on Actual and Expected Treatment Assignment
Cumulative Data Through 04/09/88
2 Weeks of Operation at all 21 Sites
Including Yakima Pilot Data

Site	Actual	Expect	% Dif	Dif	Sigma	Dif/Sigma
Aberdeen	22	39	-44.2	-17	5.8	-3.0
Auburn	95	89	6.3	6	8.7	.7
Bellevue	97	119	-18.6	-22	10.0	-2.2
Bellingham	49	68	-27.8	-19	7.6	-2.5
Bremerton	26	57	-54.7	-31	6.9	-4.5
Cowlitz County	14	45	-69.0	-31	6.2	-5.1
Everett	99	115	-14.1	-16	9.8	-1.6
Lewis County	24	25	-5.5	-1	4.6	-.3
Lynnwood	62	102	-39.5	-40	9.3	-4.4
Moses Lake	17	17	-.5	0	3.8	.0
Mount Vernon	56	59	-5.3	-3	7.0	-.4
North Seattle	106	161	-34.2	-55	11.6	-4.7
Olympia	37	60	-38.8	-23	7.1	-3.3
Rainier	262	245	6.8	17	7.3	2.3
Renton	37	119	-68.8	-82	10.0	-8.2
Spokane	73	82	-11.3	-9	8.3	-1.1
Sunnyside	31	21	47.4	10	4.2	2.4
Tri-Cities	30	34	-11.0	-4	5.3	-.7
Walla Walla	11	17	-33.9	-6	3.7	-1.5
Wenatchee	40	36	11.4	4	5.5	.7
Yakima	336	365	-8.1	-29	17.5	-1.7
Column Total	1524	1878	-18.9	-354		
Overall Total					39.72	-8.91

Table D10

WREB Program Data on Actual and Expected Bonus Offers
Cumulative Data Through 04/09/88
2 Weeks of Operation at all 21 Sites
Including Yakima Pilot Data

Site	Actual	Expect	% Dif	Dif
Aberdeen	499.36	609.13	-18.0	-109.77
Auburn	649.12	605.93	7.1	43.19
Bellevue	616.52	635.35	-3.0	-18.84
Bellingham	552.16	534.46	3.3	17.70
Bremerton	558.15	548.60	1.7	9.55
Cowlitz County	423.57	585.39	-27.6	-161.81
Everett	643.47	564.28	14.0	79.20
Lewis County	564.58	480.46	17.5	84.12
Lynnwood	567.45	609.59	-6.9	-42.14
Moses Lake	494.70	537.55	-8.0	-42.85
Mount Vernon	575.39	545.69	5.4	29.70
North Seattle	578.81	608.60	-4.9	-29.79
Olympia	646.92	576.72	12.2	70.20
Rainier	564.21	572.59	-1.5	-8.38
Renton	537.84	591.46	-9.1	-53.62
Spokane	606.33	527.26	15.0	79.06
Sunnyside	456.52	449.90	1.5	6.62
Tri-Cities	653.40	595.69	9.7	57.71
Walla Walla	393.64	511.28	-23.0	-117.64
Wenatchee	504.00	491.35	2.6	12.65
Yakima	537.57	543.24	-1.0	-5.67
Total	570.32	569.88	.1	.43

The above figures indicate that treatment assignment is significantly below the expected level (18.9 percent below) and average bonus offers are modestly larger (0.1 percent above) than expected. The net affect of these countervailing changes is to increase the expected enrollment duration. Whether these results are an aberration is unclear at this point. If this is a temporary change the O/BL results (33.5 weeks) are appropriate, if this is a permanent difference from the baseline the O/ABL results (37.5 weeks) should be planned for. The likely situation is that a combination of temporary and permanent changes is at work. Assuming that the baseline estimate of new initial claims was too high, perhaps WSESDs load increase estimate of 9.5 percent was too high, there remains a handful of particular sites (Renton, Cowlitz, Bremmerton, Lynnwood, North Seattle) where the short fall from expected treatment assignment remains outside the range of likely occurrence. This is a useful guide for on site monitoring, and is a foundation for future updates of the enrollment monitoring simulation model.

After data on the actual volume of new, monetarily valid initial claims is available the accuracy of treatment assignment may be assessed. After some on site monitoring has been done, systematic errors in local JSC procedures may be corrected. After a few more weeks of data is gathered, stronger expectations about claims load trends can be formed. At this point a conservative estimate is that the baseline enrollment period estimate should be increased by ten percent. However, with the drought in agricultural areas in Washington, and the expected Hanford layoffs, perhaps this 10 percent is a good cushion to have built into the planning model.

Furthermore, as is obvious from the front page of this section a take-up rate of about .156 is implicit in the enrollment monitoring simulation model. If measures taken to maximize this bonus pay rate have an affect, this could shorten the period during which WREB offers should continue to be made at the current rate in the 21 participating JSCs. When complete revisions of the simulation model are made, including bonus pay rate updates, the take-up rate estimate may change. If WREB enrollment continues for 40 weeks, planning for exact bonus budget exhaustion will be very precise.

Reference

Woodbury, Stephen A., and Robert G. Spiegelman (1987a), "Bonuses to Workers and Employers To Reduce Unemployment: Randomized Trials in Illinois," <u>The American Economic Review</u>, 77: 513-30 (September).

APPENDIX E

The WREB Follow-Up Survey

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Between November 1989 and May 1990, the Social and Economic Sciences Research Center (SESRC) at Washington State University conducted a follow-up telephone survey on a randomly selected subsample of unemployment insurance claimants studied in the Washington Reemployment Bonus (WREB) demonstration. The survey was designed to solicit information not available from either the WREB project data system or the Washington State Employment Security Department's (WSESD) administrative records. The survey is the sole source of information on the effects of the bonus offer on union membership, union hiring hall placement, the claimant's contribution to household income, and reasons for nonparticipation. The follow-up survey may also corroborate the basic WREB data with more precise information about dislocated workers, return to previous employer, intensity of job search, the use of various job search methods, reemployment job stability, and self-employment.

We would like to use results from the follow-up survey to make statements about the full population of claimants studied in WREB. The ability to generalize from results of the survey may be limited if there is evidence of "nonresponse bias." That is, if the respondents to the survey systematically differ from nonrespondents in important ways, then analysis of behavior based on the survey information may be misleading.

Because of the extensive information available on claimants monitored for the WREB experiment, we are in an unusually favorable position to test for the existence of nonresponse bias. In this appendix, we examine the problem of nonresponse using the basic treatment impact estimation as an example. For the estimation of treatment impact we have full information on all survey respondents and nonrespondents. After considering various nonresponse bias adjustments, we examine their usefulness by reevaluating the impact of the treatment on return to the previous employer.

We first report overall survey response rates, then provide a comparison of the full WREB sample (n = 15,534) and the survey sample (n = 3,851) to survey respondents (n = 1,900) and nonrespondents (n = 1,951) on exogenous and endogenous characteristics, and finally investigate whether the treatment and control groups who responded to the survey differ on observed and unobserved characteristics. This latter review includes a report by call attempt. In Section E.2 we review methods for dealing with the problem of nonresponse bias, along with an example representing the pattern of nonresponse observed in the WREB follow-up survey. Section E.3 reports on the results of applying nonresponse bias adjustments when using the follow-up survey data to estimate treatment impacts. Finally, Section E.4 reports the results of using the best adjustment methods identified to estimate the impact of the treatment on placement of survey respondents by union hiring halls.

E.1 The Analytic Sample and the Problem of Nonresponse

This section documents the analytic sample criteria (ASC) and the characteristics of the sample of respondents, which resulted from the follow-up survey. Some seeming inconsistencies in sample size counts associated with the follow-up survey are also explained. This clarification is necessary since the original subsample for the follow-up survey was selected in February 1989, which was before final data summarizing behavior over the whole benefit year were available for many experimental subjects.

The criteria for selection into the final sample for analysis are that the claim must have been monetarily valid and that there were no nonmonetary issues on the claim during at least one week in the qualification period. In WREB, 17,578 claimants were tracked as either a treatment or control subject. The ASC was met for 15,454 of these as of February 1989, but this was based on preliminary information; the final count was 15,534 meeting the ASC. In February 1989, when the sample to be interviewed was selected, the ASC required that the claim was monetarily valid and that there were no nonmonetary issues on the claim during the week four months after the reemployment deadline. The final ASC used is that the claim be monetarily valid and that there was at least one week during the qualification period in which there were no nonmonetary issues on the claim. Groups satisfying the ASCs differ because: (1) the ASCs differ, and (2) in February 1989, benefit years were not yet completed for some claimants studied, so that assignment to the analytic sample was based on incomplete information.

In February 1989, information on 4,019 subjects of the 15,454 meeting the ASC was presented to the WSESD to get current telephone number and mailing address. WSESD was unable to get any information to help in contacting 108 persons. The SESRC at Washington State University attempted to administer the follow-up survey to 3,911 subjects.

Of the 4,019 subjects selected in February, it turned out that 63 failed to meet the final ASC when full benefit year data became available. Of the 3,911 that SESRC tried to contact, 60 failed to meet the ASC. Ultimately, SESRC attempted to contact 3,851 persons who met the ASC. They succeeded in completing interviews with 1,900 claimants, and failed to get complete interviews with 1,951. The impact on sample size of the various stages of gathering the sample information is summarized in Table E-1.

Table E-2 gives breakdowns of respondents by treatment and control. Among the 1,900 respondents meeting the ASC, 1,518 (79.9 percent) were treatments, while among the 1,951 nonrespondents (80.6 percent) were treatments. Neither of these response rates is statistically significantly different from the designed proportion. Response rates by treatment group are investigated below.

Table E-1

Summary of Sample Sizes

	Total WREB Sample
17,578	Total treatment and controls followed in WREB
15,454	Met ASC in February 1989
15,534	Met final ASC = 12,452 (treatments) + 3,082 (controls)
	Follow-up Survey Sample
4,019	Selected from 15,454 in February for SESRC interview
3,956	Of the 4,019 ultimately met the ASC; a subset of the 15,534
3,911	SESRC attempted to contact
3,851	Of The 3,911 SESRC attempted to contact finally met the ASC
1,929	Of the 3,911 had complete interviews
1,900	Of the 1,929 completed interviews met the ASC
1,982	Of the 3,911 did not have complete interviews
1,951	Of the 1,982 without completed interviews met the ASC

Table E-2

Groups Meeting the Analytic Sample Criteria

	Total	Follow-up	Respondent	Nonrespondent
Combined	15,534	3,851	1,900	1,951
Treatment	12,452	3,091	1,518	1,573
Control	3,082	760	382	378

Basic survey completion statistics are summarized in Table G-3. In the sample of 3,851 meeting the final ASC, interviews were completed with 1,900 (49.3 percent) claimants. Among claimants in the ASC, 294 (7.6 percent) personally refused to answer the survey and refusal was made for 35 (0.9 percent) claimants by another person. There was a failure to contact 1,067 (27.8 percent) claimants because of telephone listing problems (358 disconnected numbers, 604 wrong numbers, 105 no listing available). A total of seven telephone contacts were attempted to complete an interview. Up to four call attempts were made between the hours of 6:00 and 9:00 on the evenings of Sunday through Thursday. If the evening calls failed, two additional attempts were made on the same days of the week in the afternoon. Finally, the seventh call attempt was made in the morning.[1] After the seven attempts at contact by telephone, 72 (1.9 percent) claimants were never available during the interview times and 277 (7.2 percent) claimants could not be reached. For a variety of other reasons--such as language problems, answering machines, and death--interviews were not completed for 206 (5.3 percent) claimants meeting the ASC who were selected for the survey.

A variety of steps were taken to enhance the survey response rate. In addition to the strategy of a six callbacks, advance letters were mailed to inform claimants that a representative of SESRC would be telephoning to ask survey questions about their experience with unemployment compensation.[2] Another effort involved postcards being sent to 1,023 claimants who met the ASC, requesting new telephone information.

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[1] While it was not intended by the survey managers, enthusiastic survey workers sometimes made up to 12 telephone call attempts to contact claimants. Indeed, more than seven call attempts were involved in 55 (2.9 percent) of the 1,900 interviews completed.

[2] Daniel (1975) called this strategy to increase survey response "Dillman's Personalization Technique." See Dillman and Frey (1974).

Table E-3

Completion Rate Statistics[a]

Disposition of Calls Made	Codes	N	Category Percent	Cumulative Percent for Categories
A. Completed Interviews	CM	1900	49.3	49.3
B. Partial Completes	PC	38	1.0	50.3
C. Refusals				
- By Respondent	RF	294	7.6	57.9
- By Another Person	RP	35	0.9	58.8
D. Respondent Not Available During Interview Period	RN	72	1.9	60.7
E. Unable to Reach (7 Attempts)				
- Call Backs Not Reached	CB	225	5.8	66.5
- Busy Numbers Not Reached	BZ	16	0.4	66.9
- No Answer	NA	36	0.9	67.8
F. Answering Machines	AM	68	1.8	69.6
G. Communication Problems				
- Handicapped	HC	1	0.0	69.6
- Deaf or Language Problems[b]	DL	38	1.0	70.6
H. Nonworking Telephone Numbers				
- Disconnected Numbers	DS	358	9.3	79.9
- Wrong Numbers	WN	604	15.7	95.6
I. No Telephone Listing Available	NL	105	2.7	98.3
J. Reached Business or Government Office	BG	10	0.3	98.6
K. Other (i.e., Fax Machine)	OT	11	0.3	98.9
L. R claimed they were NEVER Unemployed or in Program	IE	6	0.2	99.1
M. Deceased	DD	9	0.2	99.3
N. Respondents Never Contacted	NC	25	0.7	100.0
	TOTAL	3851	100.0	100.0

[a] Because of the nature of this survey and the population sampled, all members of the sample were considered to be potential respondents and eligible to participate in this study.
[b] While efforts were made to contact Spanish speaking claimants, no similar efforts were made with regard to other non-English-speaking claimants.

From the full analytic sample of WREB treatment and control claimants, a random sample amounting to 25 percent of the total was drawn by the Upjohn Institute and presented the SESRC for the follow-up survey. Each claimant meeting the ASC had a one in four chance of being drawn. Out of 15,534 claimants in the analytic sample, 3,851 were drawn. If sample sizes are large enough, the two samples should be similar in terms of both observable and unobservable characteristics. Among the 3,851 drawn 1,900 responded completely to the survey and 1,951 did not. Significant and systematic differences in the observed characteristics of these groups would suggest that nonresponse bias is a problem with the survey data.

The full survey sample, the respondents, and the nonrespondents were compared to the full WREB sample in terms of observable exogenous characteristics. The characteristics used to compare the samples are the control variables, which are discussed in Appendix F and used throughout the report to correct for heterogeneity across treatment and control groups, and demographic and regional characteristics. Out of the 26 characteristics on which the samples are compared, there is a statistically significant difference between the full sample (15,534) and the survey sample (3,851) in only one characteristic--32 percent of the survey sample filed for benefits in eastern Washington, compared to 30 percent in the full sample. This result suggests that a random draw was successfully completed. Conversely, there is a statistically significant difference between the full sample and both the respondents (1,900) and nonrespondents (1,951) in nearly every characteristic. The exceptions are the proportion who worked in white- collar occupations, the proportion in some of the race/gender subgroups, the proportion at the minimum weekly benefit amount (WBA), and dislocated worker status for nonrespondents.[3]

[3] The definition of dislocated worker status used is 12 quarters employment with the same employer prior to filing for UI and not on stand-by. This is definition 1 discussed in Chapter 6, the strictest definition of worker dislocation considered.

The mean values of exogenous characteristics were used to compare treatment and controls within the respondent and nonrespondent samples. Among the 3,851 claimants selected to be interviewed, 80.3 percent had been treatment-assigned; this proportion is not statistically significantly different from the designed fraction of 80 percent assigned to treatments. Among the 1,900 respondents, there were 1,518 or 79.9 percent treatments, while among the 1,951 nonrespondents 1,573 or 80.6 percent were treatments. Among neither respondents nor nonrespondents is there a statistically significant difference from the 80 percent designed fraction of the sample assigned to treatment status. Based on exogenous characteristics within the groups of respondents and nonrespondents, there is virtually no difference between treatment- and control-assigned claimants. The single difference is that among nonrespondents, a slightly greater proportion of treatment- assigned claimants worked in white-collar occupations compared to controls.

Taken together, the results of comparing the samples on exogenous characteristics indicate the following: (1) the survey attempted to contact a random subsample (3,851) of the full sample (15,534); (2) the survey respondents (1,900) systematically differed from the nonrespondents (1,951); and (3) within the respondent (1,900) and nonrespondent groups (1,951), there was no appreciable difference between treatment and control claimants.

The groups considered have been compared in terms of exogenous or "observable" characteristics; however, differences in "unobservable" characteristics should also be considered. In terms of the observable characteristics, while respondents exhibit a statistically significant difference from nonrespondents, treatments do not differ from controls within the two response groups. To get an indication about whether unobservable characteristics are an important source of differences, the groups were also compared in terms of endogenous, or outcome variables. The samples were compared in terms of the following five outcome variables: (1) total UI compensation in the benefit year, (2) weeks with some compensation in the benefit year, (3) total UI compensation in the initial spell of unemployment, (4) weeks with some compensation in the initial spell of unemployment, and (5) the UI benefit exhaustion rate.

In tests contrasting the full WREB sample of controls (3,082) to the group selected as the follow-up sample controls (760), the respondent controls (382) and the nonrespondent controls (378) are given. This analysis revealed the essence of a problem of nonresponse in the WREB follow-up survey. The controls in the follow-up sample have a statistically significantly higher number of weeks with some compensation in the benefit year compared to controls in the full WREB sample, and the sample of respondents who are controls drew significantly more weeks with some compensation and more total compensation in the benefit year than did controls in the full WREB sample. None of the other contrasts on endogenous variables showed a significant difference. That is, control respondents and nonrespondents are not different from all controls selected for the survey, and neither treatment respondents nor nonrespondents are different from either treatments in the full sample or treatments selected for the survey.

We also compared unadjusted treatment and control means on benefit year UI compensation and benefit year weeks with some compensation for the four different groups: (1) the full WREB sample (15,534), (2) the group selected for the follow-up (3,851), (3) the respondents (1,900), and (4) the nonrespondents (1,951). While the full sample showed no significant impacts either for the six treatments individually or when all treatments were grouped together, for the sample of respondents the aggregate treatment impact is large and significant, and, with the exception of treatment five on compensation and treatments four and five for weeks, the individual treatment impacts are large and significant for respondents. This outcome is a result of the earlier finding that survey respondents who are control group members had more weeks with some compensation and drew more dollars of UI compensation during their benefit years.

In Chapter 5 and throughout the final report, treatment impacts are estimated in regression models that include control variables to correct for heterogeneity between the controls and various treatment groups in observable characteristics. The difference in unadjusted treatment impacts for the various groups examined here is not pursued; rather, judgement on the question of nonresponse bias is reserved for comparison of adjusted means between treatments and controls for the various groups.

Before turning to a comparison of adjusted treatment impacts for the various groups, we reconsider means of exogenous and endogenous characteristics of the groups, comparing them by the telephone call on which the survey was completed. Remember that in order to enhance the survey response rate SESRC policy for the WREB follow-up survey was six call backs, for a total of seven telephone calls attempted, before a claimant was judged impossible to contact and dropped from the survey. Since we have not identified evidence of heterogeneity due to survey nonresponse on observable characteristics, evidence on how the bias changes with the number of calls on which contact is made would be useful if there is evidence of bias in impact estimates due to differences in unobservable characteristics.

Table E-4 summarizes the distribution of follow-up survey interviews completed by call attempts. Out of the 1,900 interviews completed, over 40 percent were completed on the first attempt, an additional 23 percent were completed on the second attempt, and 15 percent more were completed on the third call. The proportion completed on each subsequent call declines with only 2.2 percent of the final total being contacted on the seventh call. As Table E-4 shows, an additional 55 claimants, or 3 percent of those ultimately contacted, were reached after more than six call back attempts. While SESRC policy for the WREB follow-up survey was six call backs, for a variety of reasons including interviewer enthusiasm more call backs were sometimes made.

The extra effort after the third call back only generated about 15 percent of the total respondents, but these data points may be particularly useful in improving how well the sample represents the population. In their study of the effect of call backs on convergence to the population distribution in observable characteristics, Dunkelberg and

Day (1973, p. 161) report that "as the number of calls is increased and hard to reach respondents are contacted and added to completions, the distributions of these characteristics converge on the final distributions." They offer a way to estimate the rate of convergence in characteristics when the process is "stable" or monotone--i.e., each successive call attempt moves the respondent group characteristic mean closer to the population mean. The tables that follow allow us to examine the process of convergence in characteristics. This provides a useful guide to adjust for nonresponse bias.

A natural procedure to correct for nonresponse bias when there are several call attempts and convergence in characteristics is stable is to use extrapolation methods. Armstrong and Overton (1977) say that "extrapolation methods are based on the assumption that subjects who respond less rapidly are more like non-respondents." One of the most successful reweighting methods we employ below involves a geometric system of reweighting where respondents on the later calls are given the most weight.

Table E-4

Distribution of Completed Calls by Attempt

Completed on attempt	Number	Percent	Cumulative Total	Cumulative Total
1	765	40.3	765	40.3
2	445	23.4	1210	63.7
3	281	14.8	1491	78.5
4	134	7.1	1625	85.5
5	119	6.3	1744	91.8
6	60	3.2	1804	94.9
7	41	2.2	1845	97.1
8	27	1.4	1872	98.5
9	16	0.8	1888	99.4
10	9	0.5	1897	99.8
11	2	0.1	1899	99.9
12	1	0.1	1900	100.0

Frequency Missing = 1951

Dunkelberg and Day (1973, p. 161) report that "a sample of respondents consisting only of interviews completed on the first and second calls would contain considerable bias in the distributions of age, income, and city size." Furthermore, they say that, "the most important characteristic explaining differences in the number of calls required for [survey] completions was city size of residence. Respondents living in the ...largest...cities were the most difficult to reach." Consistent with this and other studies of nonresponse bias (see also Hawkins 1975 and O'Neil 1979), if the WREB follow-up survey had involved only one call attempt, only 42 percent of the sample would have been from the Seattle metropolitan area. This proportion is statistically significantly less than for the full WREB sample (51 percent) and the follow-up sample (50 percent). By the process of repeated call attempts, the proportion of Seattle area claimants rises to 46 percent in the final respondent sample. And the trend toward the population value is monotonic in the number of call attempts.

If the follow-up survey had been limited to a single call attempt, the sample of respondents would be relatively older, more highly educated, and have had higher base period earnings, in addition to overrepresenting areas outside Seattle. It would also have a greater proportion of claimants who are white, female, and employed in white-collar occupations, compared to the full WREB sample and the follow-up survey sample. While there appears to be some oscillating in the mean values of some characteristics as the number of call backs is increased, statistically, convergence on all characteristics is monotone.

To provide an indication about how unobservable characteristics may affect the representativeness of the sample of survey respondents, we compared the mean values of endogenous or outcome variables by call attempt.

On the benefit year basis of comparison, the means of neither UI compensation nor weeks with some compensation are statistically significantly different for <u>treatment-</u>

assigned respondents after the first call from the full WREB sample or the follow-up survey sample of treatment- assigned claimants. Furthermore, after the call attempts are completed, the mean values of both total UI compensation and weeks with some compensation are closer to the mean value in the full WREB sample than after the first call attempt. This results in spite of the fact that the mean value of total UI compensation in the sample of treatment claimants selected for the survey is larger than for the full WREB sample of treatments, which may explain why convergence is not monotonic in this case. For the initial spell concepts there is no statistically significant difference between the means for respondents and the full sample and the survey sample means, regardless of the number of call attempts. However, perhaps due to the definition of these variables, the changes are unstable and generally diverge from the full sample values as the number of call attempts is increased.

A finding suggestive of a nonresponse problem is the persistent statistical difference in the exhaustion rates between treatment-assigned claimants in the final sample of respondents and those in the full WREB sample and the sample selected for the follow-up survey. Respondents have a lower exhaustion rate. It may be the case that, for treatment-assigned claimants, an unobserved factor such as "claimants relocating to accept a new job," which affects both response and outcomes, is being picked up by the exhaustion rate.

A similar comparison of groups on endogenous variables by call attempt was done for claimants assigned to control status. In this case, the results of the comparison are quite different from those found for treatment-assigned claimants. Benefit year UI compensation is higher for respondents and statistically different from compensation for the full WREB sample. While the mean value among respondents who are control-assigned converges toward the full sample mean for controls, the terminal value remains $275 or 13.3 percent greater. A similar pattern is exhibited for benefit year weeks with some compensation, again the mean value for control-assigned respondents remains statistically significantly greater than the mean for control-assigned claimants in the full sample even after all call attempts have been made.

Taken together, the analysis of mean endogenous outcomes by call attempt compared to the full WREB sample and the sample selected for the survey indicates that among claimants assigned to the control group, i.e., not given the bonus offer, claimants who drew more UI compensation are more likely to respond, while among treatment-assigned claimants those less likely to exhaust benefits are more likely to respond. It could be the case that the lower exhaustion rate reflects the impact of the bonus offer among treatment-assigned claimants, and that the bonus offer increased the response rate sufficiently to make up for lower response due to lower UI compensation received by treatments to result in an overall response rate of 50 percent for treatments, which is nearly identical to the response rate for controls.

Because of heterogeneity between the six treatment groups and the control group in terms of observable characteristics, throughout this report treatment impacts were estimated with control variables. To compare to the results presented in the body of this report on an equal footing with results for the two response groups, and to test for differences in estimated treatment effects due to possible heterogeneity between the respondent and nonrespondent groups, Table E-5 presents the regression adjusted treatment impacts for the various sample groups.

The treatment impact estimates reflect the observed pattern of outcomes; they are generally larger for the survey sample than for the total sample, but because the survey sample is much smaller the standard errors on the impact estimates are about double those on the impacts for the total sample. Therefore, the differences across the two groups in the estimated treatment impacts are not statistically significant.

To test if the treatment impact estimates for the survey respondents are different from survey nonrespondents and the full survey sample, an F-test comparing the sets of

Table E-5

A Comparison of Treatment Impact Estimates on Benefit Year UI Compensation
Total Sample, Survey Sample, Survey Respondents, and Survey Nonrespondents
Controlling for Population and Program Characteristics
(standard errors in parentheses)

Treatment	Total Sample	Survey Sample	Survey Respondents	Survey Nonrespondents
T1	18.7	7.0	-198.8	217.2*
	(45.7)	(92.8)	(131.9)	(130.2)
T2	-40.7	-138.6	-243.6*	-50.0
	(45.2)	(91.4)	(133.8)	(125.1)
T3	-106.9**	-213.6**	-345.7**	-79.6
	(51.0)	(103.2)	(149.3)	(142.5)
T4	-117.2**	-142.9	-278.5**	1.8
	(45.0)	(90.7)	(129.7)	(126.5)
T5	-39.8	46.1	-6.3	87.7
	(45.1)	(91.6)	(130.7)	(128.0)
T6	-140.5**	-176.6	-404.4**	47.7
	(51.5)	(104.5)	(148.4)	(146.9)
TREATMENT	-65.2**	-92.8	-229.9**	39.6
	(33.2)	(67.3)	(96.1)	(93.9)
Sample Size	15,534	3,851	1,900	1,951

* Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

regression parameters from the equations was conducted.[4] Tests were run for both the six-treatment model and the model where the six treatments were combined into a single one. In both cases, the F-test rejects the hypothesis of a common set of regression parameters across the groups at the 5 percent level of significance.

In the next section, we start by giving an example that reflects the pattern of nonresponse observed in the WREB follow-up survey. We then describe various methods to adjust for nonresponse bias.

E.2 Approaches to Adjusting for Nonresponse Bias

Significant difference in the mean values of observable exogenous characteristics between the respondent and nonrespondent groups is evidence that a problem of nonresponse bias may exist. However, if respondents and nonrespondents do not differ in unobservable characteristics, then regression methods can be used to get unbiased estimates of program impacts. The procedure is to simply include the variables on which the respondent groups differ as control variables in the regression equations used to estimate treatment impacts.

Another indication of potential nonresponse bias would be if different proportions of treatment and controls responded to the survey. For the WREB follow-up survey, the same rate of response among treatment and controls was observed, but while the treatments who respond have the same average level of UI compensation as the treatments not responding, the controls who respond have a much higher level of UI compensation than the nonresponding controls. The net result of this pattern of nonresponse is that treatment impacts among the follow-up survey respondents are very large--indeed, larger than the impacts estimated on the full WREB population.

[4] The F-test involved is commonly referred to as a "Chow Test." An exposition is in Kennedy (1979, p. 70).

After controlling for observable characteristics, Brown et al. (1986, paraphrased from p. 40) say that "two conditions are necessary for impact estimates to be biased: (1) nonresponse is affected by the outcome measure (e.g., level of UI compensation), or by some unobserved factor (e.g., having relocated for a new job) that affects both response and the outcome measure; and (2) the pattern of nonresponse differs for treatment and control groups." The degree to which these conditions are met, and methods for dealing with any existing bias, are reviewed in this appendix.

The detailed analysis of outcome variables given in the previous section revealed that, among claimants assigned to the control group, those who drew more UI were more likely to be survey respondents. And while treatment-assigned claimants on average drew less benefits, they had the same overall survey response rate.

Nonresponse bias in treatment impact estimates can occur in association with differential response rates between treatments and controls, but this is not necessary. What is necessary for nonresponse bias to occur is that the pattern of response within groups differ. Nonresponse bias occurs in our survey in spite of the fact that both treatment and control claimants respond at a rate of 50 percent. The overall response rate for treatments equals that for controls because treatment-assigned claimants who reduced UI as a result of the bonus offer (low UI compensation) also tend to respond to the survey, while treatment-assigned claimants who were not affected by the bonus offer (high UI compensation) are unlikely to respond to the survey.

Next we review methods for estimating treatment impacts that involve adjustments for nonresponse bias. These methods can be used to account for both observable and unobservable factors.

E.2.1 Reweighting by Observable Characteristics

In several microeconomic household surveys such as the Current Population Survey (CPS) and the Panel Study of Income Dynamics (PSID), a series of weights is available to improve the representativeness of the data collected in the survey. In both the CPS and the PSID, the adjustment is intended to offset differences in sampling and response rates.[5] Throughout this report, we present treatment impact estimates computed in regression models which include exogenous variables to correct for sample heterogeneity. This procedure allows for unbiased estimation of treatment effects when there are differences in observable exogenous characteristics between the respondent and nonrespondent groups. However, in a sample of respondents these variables can only help adjust the mean impact estimate toward the mean for the sample of respondents. Application of weights to adjust the sample for nonresponse bias on observable variables may still improve treatment impact estimates in the sense of moving them toward the population values. We have developed and applied several series of weights based on observable characteristics to adjust for nonresponse bias.

In Table E-6 the proportions for the survey sample and the respondent sample on each of 13 exogenous characteristics are listed. These are the characteristics on which the survey respondents differed most from the survey sample. The characteristics are treated as categorical variables, 12 are coded as binary variables and one, base period earnings (BPE), has three categories. The weights for each characteristic are simply the ratio of the proportion for the survey sample in each category of the characteristic divided by the proportion in the response group for that category. The weights for each characteristic sum to one. For example, the nonresponse adjustment weight for age amounts to (.784/.747) for claimants less than 45 years of age and (.216/.253) for claimants 45 or older. Estimates using these weights are presented below.

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[5] For a discussion of the CPS methodology, see U.S. Department of Commerce (1978) pp. 56-63. The PSID weights are defined in Duncan and Morgan (1978) p. 496.

Table E-6

Sample Proportions by Characteristics
Fractions used to Correct for Nonresponse by Reweighting

Characteristic	Survey Sample (3,851)	Respondents (1,900)
Age < 45 years	0.784	0.747
Age ≥ 45 years	0.216	0.253
Male	0.610	0.579
Female	0.390	0.421
White	0.836	0.869
Nonwhite	0.164	0.131
Black	0.040	0.019
Nonblack	0.960	0.981
Seattle	0.501	0.457
non-Seattle	0.499	0.543
Eastern Washington	0.320	0.352
Western Washington	0.680	0.648
Education < 12 years	0.183	0.153
Education ≥ 12 years	0.817	0.847
BPE < $10,000	0.352	0.303
$10,000 ≤ BPE < $20,000	0.383	0.396
BPE > $20,000	0.266	0.301
WBA < $150	0.447	0.396
WBA ≥ $150	0.553	0.604
UI Entitlement ≤ 27 Weeks	0.386	0.354
UI Entitlement > 27 Weeks	0.614	0.646
Work Search Exempt	0.217	0.246
not Work Search Exempt	0.783	0.754
BPE/High Quarter < 2.0	0.139	0.115
BPE/High Quarter ≥ 2.0	0.861	0.885
CV in BP Quarterly Wages < 0.7	0.596	0.653
CV in BP Quarterly Wages ≥ 0.7	0.404	0.347

E.2.2 Reweighting for Unobservable Characteristics

As discussed in Section E.1, the literature on nonresponse bias reports that increasing the number of call attempts increases the number of hard-to-reach respondents who are contacted. In this section, we state a reweighting scheme which adjusts for both observable and unobservable characteristics. The proposed method assumes nonrespondents are similar to claimants who respond on later call attempts. This assumption is common in the literature on extrapolation methods for dealing with nonresponse bias.[6]

Our reweighting method involves a geometric system of weights where respondents on the later calls are given more weight than respondents on earlier calls, and the sum of the weights is constrained to be the reciprocal of the overall response rate. In the present survey, since the overall response rate is 50 percent, the weights employed sum to 2. Furthermore, our method is a maximum likelihood method in that the geometric system of weights chosen is the geometric system most likely to result in parameter estimates on weighted response sample, which is most similar to those that result when using the full survey sample.[7] The formula for the set of weights is:

$$w_t = [(k)^{T-t}]/\{R[Sum_{t=1}^{T}(k)^{T-t}]\}$$

where, w_t is the weight for observations for which the interview was completed on the tth call, R is the overall response rate, T is the maximum number of calls, and $0 < k < 1$. For the WREB follow-up survey, $R = 0.5$ and $T = 7$. After trying various values of k, it was determined that a value of 0.74 resulted in an average treatment impact estimated

[6] See the papers by Dunkelberg and Day (1973), Armstrong and Overton (1977), and O'Neil (1979).

[7] This type of maximum likelihood approach is possible here since we have extensive information on nonrespondents. Such an approach is possible in any situation where there is prior information on the population.

on the weighted sample of respondents closest to the average treatment impact estimated for the full survey sample. Results from using this reweighting scheme are presented in the next section.

E.2.3 The Heckman Method of Adjusting for Unobservable Differences.

Throughout the WREB final report, treatment impacts of the bonus offer presented were estimated in regression models that included control variables to correct for heterogeneity across treatment and control groups in observed characteristics. The typical regression equation had the form:

$$\text{(E.1)} \quad \begin{aligned} Y_1 &= a + B'T + C'_1 Z_1 + u_1, \\ &= XG + u_1, \end{aligned}$$

where, Y_1 is the outcome of interest, either dollars of UI compensation or weeks with some UI compensation, T is a matrix of treatment dummy variables, Z_1 is a matrix of control variables, a, B, and C_1 represent parameters to be estimated, and u_1 is a normally distributed mean zero error term. The second line of (G.1) is a more concise statement of the equation, with X representing the data matrix, which includes T, Z'_1, and the constant, and G being the expanded parameter vector.

The condition for unbiased estimation of regression coefficients is $E(Y_1|X) = XG$. However, according to Heckman (1976), if there has been sample truncation on some selection rule, then $E(Y_1|X, \textit{Selection Rule}) = XG + E(u_1|\textit{Selection Rule})$. And ordinary least squares (OLS) estimation on the selected sample yields an unbiased estimate of G only if $E(u_1|\textit{Selection Rule}) = 0$. This would occur if the control variables, Z_1, fully explained any difference between respondents and nonrespondents in the outcome variable, Y_1. However, if there are unmeasured characteristics that affect both the probability of survey response and the outcome variable, treatment impact estimates will be biased.

The following, which is adapted from Brown et al. (1986), describes the mechanism by which nonresponse bias may occur. As a complement to equation (E.1) consider the following two equations, which describe the process of nonresponse.

(E.2) $\quad Y^*_2 = Z_2C_2 + u_2$, and

(E.3) $$Y_2 = \begin{cases} 1 \text{ if } Y^*_2 > 0 & \text{(A survey respondent)} \\ 0 \text{ if } Y^*_2 \leq 0 & \text{(A survey nonrespondent).} \end{cases}$$

The dependent variable in equation (E.2), Y^*_2, is an unobserved continuous variable representing the sample member's propensity to respond to the survey. The propensity to respond depends on observable characteristics, Z_2, which include the treatment dummies, T, and control variables in Z_1 as well as other variables and interactions. Survey response also depends on unobservable characteristics represented by the error term, u_2, which is assumed to have a standard normal distribution. Equation (E.3) is what Heckman (1976) called the selection rule, and with the threshold for survey response normalized to zero, we have $(u_2 > -Z_2C_2)$ as the selection rule for survey respondents.

Because of the structure of the selection rule, OLS estimates of the treatment impacts will be biased if the unobserved factors that affect survey response, u_2, are correlated with the unobserved factors, u_1, that affect the outcome measure, Y_1. That is, if the last term in the equation

(E.4) $$E(Y_1|X, u_2 > -Z_2C_2) = XG + E(u_1|u_2 > -Z_2C_2),$$

is not zero. Denoting *g* as the parameter vector estimated by OLS on the selected sample, the form of the bias--or deviation from the true parameter vector G--can be seen in the following:

$$\text{(E.5)} \qquad E(g \,|\, A\ survey\ respondent) = G + (X'X)^{-1}X'E(u_1 \,|\, u_2 > -Z_2C_2).$$

In general, the size and direction of these biases are unknown.

Heckman (1976) showed that bias in OLS estimates of the type that may be experienced here due to survey nonresponse is analogous to the bias that results from omitting an important explanatory variable. He also recommended a statistical correction for this type of problem that relies on the assumption that u_1 and u_2 have a bivariate normal distribution. The procedure amounts to forming a proxy for the second term on the right-hand side of equation (E.4). Heckman showed that

$$\text{(E.5)} \qquad E(u_1|u_2 > -Z_2C_2) = \frac{s_{12}}{s_2}\frac{f(Z_2C_2/s_2)}{F(Z_2C_2/s_2)} = (s_{12}/s_2)M$$

where, s_{12} is the covariance of u_1 and u_2, s_2 is the standard deviation of u_2, *f(.)* the standard normal density and *F(.)* is the standard normal distribution function, and C_2 is the vector of estimated coefficients from a survey response equation. The parameters needed to form *M,* the inverse Mill's ratio, can be consistently estimated by maximum likelihood Probit. Inclusion of this variable in the regression eliminates it from the error term and therefore eliminates the correlation between *X* and the error term in equation (E.4), thereby eliminating nonresponse bias in estimates of *G.*

The steps in the procedure developed by Heckman and used here to adjust for nonresponse bias are as follows:

1. Using data on both respondents and nonrespondents, estimate the parameters of the response model, C_2, stated in equation (E.2) and equation (E.3) using maximum likelihood probit.

2. Using the estimated probit coefficients and the data on Z_2, form the inverse Mill's ratio *M*.

3. Include the new variable *M* in equation (E.1) and estimate the parameters by OLS on a sample restricted to respondents only.

That is, estimate:

$$\text{(G.6)} \quad Y_1 = a + B'T + C'_1 Z_1 + dM + u_1^*,$$

on the sample of respondents. This is the same as equation (E.1) where *dM* replaces the non-zero conditional expectation of the old disturbance, *u1,* and the new disturbance term, u_1^*, satisfies all the standard least squares assumptions. The statistical significance of *d,* the coefficient on *M,* is an indication of whether there are unobserved factors affecting both survey response and Y_1, a necessary condition for treatment impact estimates to be biased.

E.3 Results of Applying the Non-Response Bias Adjustment Methods

In Table E-7, average treatment impact estimates computed for various reweighted versions of the data on the sample of respondents are given. For purposes of comparison, the impact estimates for various unweighted samples are listed near the bottom of the table. The impact estimated on the respondent sample is $137 more than that estimated on the full survey sample. By an F-test for differences in parameter estimates across subsamples, this bias is judged significant at the 95 percent confidence level.

Among the exogenous characteristics on which the respondent sample was weighted, no individual weight reduces the nonresponse bias by much; indeed, some of the weights even magnify the bias. The biggest reduction in bias is associated with BPE and age.

The second panel in Table E-7 reports on the results of applying weights that are products of the various individual weights. The combination that yields the best adjustment is the product of age and BPE, which reduces the bias to $17. A problem with this and all the reweighted estimates is, however, the lack of precision.

In the third panel of Table E-7, we present average treatment impact estimates computed on the sample of nonrespondents using corrections based on unobservable characteristics. Applying the scheme for geometrically reweighting the data on respondents with a different weight for the number of the call attempt on which the interview was completed, the average treatment response estimated on the respondents becomes indistinguishable from that estimated on the full survey sample. However, just as for the full survey sample, the impact estimate is statistically insignificant.

The effectiveness of the Heckman (1976) correction procedure--which we call Heckit--does not appreciably adjust for the nonresponse bias in the average bonus impact estimate. The first stage Probit included the six treatment dummies, the standard set of 13 control variables, plus variables for industry (equal to one for mining or manufacturing, otherwise zero) and outside Seattle (equal to one if outside Seattle). The most significant predictors of survey response were age, gender, and outside Seattle. Claimants were more likely to respond if they were older, female, and lived outside Seattle. The Probit equation also included all interactions of the 15 control variables with the six treatment variables.

The average treatment effect estimated on the response sample in the second stage of the Heckit procedure has a bias only 2 percent smaller than that estimated

Table E-7

Average Treatment Impacts for Survey Respondents
Estimated with Correction for Nonresponse Bias

Weight	Impact Estimate	Standard Error	t-Stat
	Weights Based on Observable Characteristics		
Age	-216.45	95.70	2.26
Male	-285.23	94.00	3.03
White	-232.70	96.45	2.41
Black	-225.25	96.18	2.34
Seattle	-272.14	98.08	2.77
East	-227.36	93.63	2.43
Education	-227.82	95.25	2.39
Base Period Earnings	-133.57	109.00	1.23
Weekly Benefit Amount	-226.46	98.88	2.29
Entitled Benefit Duration	-232.93	93.58	2.49
Work Search Exempt	-234.34	96.13	2.44
BPE/High Quarter Earnings	-230.29	96.66	2.38
CV in BP Quarterly Earnings	-229.40	96.16	2.39
Product of All Above Weights	-249.83	107.68	2.32
Age*BPE*White*Seattle*Male	-250.33	108.88	2.30
Age*BPE*White*Seattle	-171.83	110.57	1.55
Age*BPE*White	-113.10	108.96	1.04
Age*BPE	-109.92	108.70	1.01
	Adjustments Based on Unobservable Characteristics		
Call Attempt	-92.30	99.88	0.92
Heckit	-232.19	99.27	2.34
	Unadjusted Average Impact Estimates		
Respondents (1,900)	-229.89	96.09	2.39(Respondents)
Survey Sample (3,851)	-92.80	67.29	1.38(Survey Sample)
Full Sample (15,534)	-65.18	33.18	1.96(Total Sample)

without any corrections for nonresponse bias. Again, the impact is estimated with a large degree of imprecision. Not reported in Table E-8, but of importance, is the coefficient on the inverse Mill's ratio (the Heckman correction term). As mentioned in Section E.2.3, statistical significance of this parameter is evidence of nonresponse bias. The parameter estimate (standard error) is 455.3 (298.6) with a t-ratio of 1.52. This does not provide strong support for nonresponse bias, but it indicates that unobserved factors which increase the likelihood of being a survey respondent on average also increase benefit year UI compensation by $455. It is interesting that while the parameter estimate on the correction factor is large and almost statistically significant, it does not have a big effect on the treatment impact estimate. This is probably because the WREB treatment impacts are not particularly strong in general.

Table E-8 presents estimates from reweighted data on survey respondents for the six WREB treatments. Results are presented for the same series of corrections for nonresponse bias reported in Table E-9. Few of the adjustments substantially reduce the nonresponse bias, and some even increase it. Just as when estimating the average treatment effect, the scheme for a geometric reweighting of observations by call attempt on which the survey was completed is the best adjustment for the disaggregated treatment effects.

E.4 A Reexamination of Return to Previous Employer

Among the many outcomes on which the follow-up survey was designed to provide information, estimates using the sample of respondents indicate significant treatment effects for only a few. The survey response data indicate no statistically significant treatment effects on union membership, union hiring hall placement, the use of the job service, reemployment job stability, self-employment, or different treatment impacts for dislocated as compared to other unemployed workers. Statistically significant treatment impacts were found for return to previous employer, the contribution to household

Table E-8

Treatment Impacts for Survey Respondents
Estimated with Correction for Nonresponse Bias
(standard errors in parentheses)

Weight	T1	T2	T3	T4	T5	T6
Age	-188.03 (131.32)	-222.71 (132.70)	-352.53 (149.20)	-263.49 (129.16)	29.37 (130.65)	-410.61 (148.09)
Gender	-235.61 (129.51)	-307.67 (131.12)	-436.4 (146.51)	-339.24 (126.63)	-65.14 (127.69)	-428.52 (145.30)
Race-White	-207.50 (132.35)	-244.82 (134.22)	-350.27 (149.77)	-281.92 (130.25)	-6.58 (131.00)	-404.68 (149.03)
Race-Black	-197.92 (131.90)	-242.29 (133.81)	-341.87 (149.12)	-272.45 (129.86)	1.61 (130.82)	-396.48 (148.59)
Seattle	-246.24 (134.64)	-327.19 (136.95)	-401.69 (152.60)	-311.95 (131.83)	-8.87 (133.43)	-446.83 (152.54)
Eastern Washington	-185.28 (128.92)	-212.42 (130.05)	-352.57 (145.44)	-276.84 (126.78)	-18.17 (127.70)	-418.36 (143.69)
Education	-213.22 (130.75)	-228.31 (132.55)	-341.46 (147.68)	-265.92 (128.66)	-5.91 (130.04)	-408.72 (147.36)
Base Period Earnings (BPE)	-66.18 (149.39)	-117.14 (153.88)	-277.29 (169.86)	-264.22 (148.51)	151.70 (149.25)	-333.58 (166.07)
Weekly Benefit Amount	-186.85 (136.08)	-246.21 (138.16)	-361.36 (153.86)	-275.41 (133.48)	17.00 (134.20)	-418.59 (152.42)
Entitled Benefit Duration	-220.34 (128.28)	-234.12 (130.40)	-318.53 (144.99)	-275.51 (126.00)	-29.50 (127.46)	-404.96 (144.69)
Work Search Exempt	-184.04 (132.24)	-242.24 (133.97)	-379.44 (149.32)	-296.68 (129.61)	1.57 (130.69)	-413.72 (148.58)
BPE/High Quarter Earnings	-195.9 (132.77)	-241.25 (134.51)	-364.07 (150.25)	-275.20 (130.46)	-1.53 (131.45)	-409.05 (148.98)
Coeff. of Var. in BP Earnings	-198.64 (131.98)	-242.32 (133.87)	-347.36 (149.41)	-277.27 (129.79)	-5.30 (130.83)	-404.15 (148.43)

Table E-8

(Continued)

Weight	T1	T2	T3	T4	T5	T6
			Reweighted on Observable Characteristics			
Product of All Weights	-173.42 (149.29)	-242.85 (151.90)	-533.26 (169.47)	-372.85 (147.11)	135.72 (147.87)	-475.13 (164.71)
Age, BPE, White Seattle, Male	-191.80 (149.99)	-296.39 (153.43)	-469.47 (170.42)	-367.37 (148.44)	123.97 (149.19)	-444.48 (168.09)
Age, BPE, White Seattle	-159.00 (150.70)	-185.95 (154.70)	-349.63 (172.04)	-283.04 (150.55)	212.13 (151.69)	-395.35 (169.98)
Age, BPE, White	-84.13 (148.64)	-75.21 (152.59)	-272.86 (169.81)	-241.93 (148.62)	223.73 (149.34)	-346.67 (165.86)
Age, BPE	-71.64 (148.44)	-77.42 (152.32)	-267.82 (169.74)	-233.72 (148.16)	19.45 (149.16)	-344.88 (165.29)
			Adjusted for Unobservable Characteristics			
Unobservables Heckit	-166.92 (135.73)	-280.08 (147.16)	-322.96 (154.49)	-321.55 (133.46)	-25.42 (133.78)	-364.05 (152.11)
Unobservables Call Attempts	48.83 (134.88)	-117.08 (133.27)	-302.68 (161.07)	-22.57 (127.59)	-10.26 (128.02)	-347.61 (148.31)
			Estimates for Various Samples--Not Reweighted			
Full Sample (15,534)	18.66 (45.74)	-40.70 (45.16)	-106.92 (50.98)	-117.15 (44.95)	-39.79 (45.14)	-140.53 (51.52)
Survey Sample (3,851)	7.05 (92.77)	-138.60 (91.43)	-213.55 (103.22)	-142.94 (90.73)	46.14 (91.58)	-176.65 (104.52)
Respondents (1,900) (148.38)	-198.83 (131.87)	-243.64 (133.76)	-345.74 (149.25)	-278.48 (129.69)	-6.33 (130.72)	-404.38

income, and the intensity of job search as measured by the number of contacts during the period of compensated unemployment (there was no statistically significant difference between treatments and controls for two other measures of job search intensity).

From the questions examined using follow-up survey data, we selected the return to previous employer as the example to evaluate the effectiveness of the nonresponse bias correction methods. Among the survey outcomes with statistically significant treatment effects, return to previous employer has the most importance for policy about a reemployment bonus. From this exercise, we would like to generalize about the effect of nonresponse bias on impact estimates.

In Table E-9, we present impact estimates of the treatment on return to the prior employer. These estimates were computed on a sample of 1,459 survey respondents who returned to work prior to being interviewed for the follow-up survey. The impact estimates were computed by OLS in equations where the dependent variable is one if returned to prior employer, and was zero otherwise, i.e., the linear probability model was used. This was done to evenly assess the effects of three methods of adjusting for nonresponse bias: (1) a geometric reweighting by call attempt, (2) the Heckman (1976) or Heckit procedure, and (3) reweighting by age and BPE.

The unweighted results are statistically significant and indicate that, on average, treatment-assigned claimants were 6 percent less likely to return to their prior employer. Each of the adjustments for nonresponse bias reduces the estimated treatment impact, with the adjusted impact estimates ranging from -3 percent to -5.6 percent. The direction of the average impact estimate after adjustment remains the same as the unweighted result, but because the estimates are smaller, statistical significance is diminished. The adjustment by call attempt has the biggest effect on the parameter estimates. Effects of the Heckit and age-BPE adjustments are nearly identical and very small.

Table E-9

Impacts for Survey Respondents on Return to Previous Employer Estimated with Correction for Nonresponse Bias (standard errors in parentheses)

Weight	T1	T2	T3	T4	T5	T6
Call Attempts	0.005 (0.039)	0.040 (0.039)	-0.017 (0.046)	-0.103** (0.037)	-0.081** (0.037)	0.039 (0.045)
Heckit	-0.098** (0.039)	-0.006 (0.041)	-0.059 (0.044)	-0.014 (0.038)	-0.080** (0.038)	-0.075* (0.043)
Age*BPE	-0.093** (0.039)	-0.045 (0.039)	-0.033 (0.044)	-0.032 (0.038)	-0.075* (0.039)	-0.051 (0.044)
Respondents Unweighted	-0.087** (0.039)	-0.048 (0.039)	-0.051 (0.044)	-0.033 (0.038)	-0.078** (0.038)	-0.062 (0.044)

Weight	Treatment
Call Attempts	-0.030 (0.029)
Heckit	-0.055 (0.029)
Age*BPE	-0.056* (0.029)
Respondents Unweighted	-0.060** (0.029)

* Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

The effect of adjusting for nonresponse bias is much less straightforward when attempting to estimate the six treatment effects individually. Relative to the unweighted estimates, some of the adjusted estimates increase and some decrease. There is little effect on the standard errors of any of the particular treatment effect estimates resulting from the adjustments, but because the pattern of treatment impacts is affected, so is the pattern of statistical significance.

Table E-10 presents a similar analysis for return to work at the principal employer over the past five years. The average unweighted treatment impact for this concept is slightly larger at -6.9 percent. However, the relative effect of adjusting for nonresponse bias is similar. After the adjustments, the treatment impacts range from -3.3 percent to -6.9 percent. Again the biggest change in the impact estimate results from the reweighting by call attempt and the smallest impact is for the age-BPE reweighting.

Generalizing from these findings is risky because of the lack of strong statistical significance, however it appears that the treatment tended to affect the behavior of respondents relatively more than it affected nonrespondents. The implication is that in making inferences for the full sample, impacts estimated on the sample of respondents provide a guide to the direction of the impact, but overstate the magnitude and significance of the impact.

Table E-10

Impacts for Survey Respondents on Return to Major Employer
Estimated with Correction for Nonresponse Bias
(standard errors in parentheses)

Weight	T1	T2	T3	T4	T5	T6
Call Attempts	-0.032 (0.038)	-0.037 (0.038)	-0.042 (0.045)	-0.114** (0.036)	-0.071** (0.036)	0.028 (0.043)
Heckit	-0.090** (0.039)	-0.048 (0.041)	-0.030 (0.045)	-0.025 (0.038)	-0.099** (0.038)	-0.092* (0.043)
Age*BPE	-0.096** (0.039)	-0.088 (0.039)	-0.001 (0.044)	-0.043 (0.038)	-0.087* (0.038)	-0.078 (0.044)
Respondents Unweighted	-0.088** (0.039)	-0.076 (0.039)	-0.019 (0.044)	-0.040 (0.038)	-0.093** (0.038)	-0.085 (0.043)

Weight	Treatment
Call Attempts	-0.033 (0.029)
Heckit	-0.065** (0.029)
Age*BPE	-0.069** (0.029)
Respondents Unweighted	-0.069** (0.029)

* Coefficient significant at the 90 percent confidence level for a two-tail test.
**Coefficient significant at the 95 percent confidence level for a two-tail test.

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APPENDIX F

The Control Regression Equation

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The Control Regression Equation

Because the random assignment process did not result in ideal homogeneity between the treatment and control groups, regression adjusted treatment effects are discussed throughout this paper. The specification used to correct for nonhomogeneity was developed for the control group and then included when the combined control and treatment sample was used to assess treatment impacts. Table F-1 shows the variables used in the control equation, and their parameter estimates in a regression used to predict compensation and weeks compensated for the control group. The control set, however, is entered in a differed fashion when used as control variables in the impact equations. In that mode, the control variables are all redefined as differences from the mean calculated over treatment and control set. This procedure is adopted so that the INTERCEPT in the impact regressions represents the mean value of the outcome variable for a member of the control group who possesses mean characteristics for the full sample. In equations for testing the treatment impact of more than a single population group, the control set is interacted with each population set. Following is an OLS equation to compute the impacts on outcome, Y, of the treatment T applied to two population groups, N and M. The equation is structured as follows:

$$Y = a + bT + cZ + dN + eT{*}N + fZ{*}N + u, \qquad \text{(F.1)}$$

where Z is the set of control variables, with each variable in the set defined as the difference in the value of the variable from its mean. In this manner, a represents the control value of Y for the M population group, d is the additional control set value for the N population group, b is the treatment effect for the M population group, and e is the additional effect for the N group, so that $b + e$ is the total impact for the N group.

Table F-1

OLS Parameter Estimates from Estimating the Control Specification on the WREB Control Group.
(Standard Error in Parentheses)

	Benefit Year UI Compensation	Weeks of Insured Unemployment	Sample Means
INTERCEPT	-1677.37 (323.71)	0.19 (2.06)	
EDUCATION	17.77 (13.63)	0.11 (0.09)	12.36 (2.67)
WHITECOLLAR	296.24 (76.56)	1.15 (0.49)	0.35 (0.48)
AGE	15.44 (2.76)	0.14 (0.02)	36.25 (11.26)
MALE	-10.73 (70.19)	-0.45 (0.45)	0.61 (0.49)
BLACK	325.35 (147.50)	2.33 (0.94)	0.04 (0.21)
OTHERRACE	114.13 (100.04)	0.90 (0.64)	0.12 (0.32)
WBA	16.40 (1.04)	0.03 (0.01)	153.24 (51.92)
ENTITLEDUR	21.48 (9.57)	0.22 (0.06)	26.86 (4.17)
WBAMAX	-230.57 (106.01)	-2.54 (0.68)	0.34 (0.47)
WBAMIN	60.15 (179.07)	-2.08 (1.14)	0.03 (0.18)
BPE x (10^{-4})	-4.23 (5.16)	-0.06 (0.03)	15.76 (11.48)
SDBPE x (10^{-4})	44.27 (17.23)	0.32 (0.11)	1.44 (2.24)
SEARCHEX	-628.72 (78.38)	-3.19 (0.50)	0.22 (0.41)
R^2 F	.227 69.17	.067 16.81	Sample Size n = 3,082

The control equation explaining the amount of unemployment compensation received in the benefit year by control group members includes variables that capture human capital, demographic, region, and UI program structure factors. So that the constant in the equation may be interpreted as the mean response for the control group, continuously measured variables are included in deviation form.

The variables used to capture the various dimensions of the control specification are now discussed in groups. The same exact control specification is used in equations which explain the number of weeks in which compensation was received in the benefit year.

Human Capital Variables

Variables are included to capture differences due to education, occupation, and labor market experience. Education is included as a continuous variable representing years of formal schooling completed; it ranges in value from 1 to 19. Because of the high degree of multicollinearity among one-digit DOT code dummy variables, a single dummy variable having the value of one for white-collar workers and zero otherwise is included.[1] Labor market experience is captured in the control set by the demographic variable age; it is continuously measured in years.

The occupation dummy and the variable AGE are strongly significant in both the COMPENSATION and the WEEKS regression, while EDUCATION is significant in neither. Everything else constant, being a white-collar worker increases benefit year compensation by $296, and each year of age is associated with $15 more in benefits.

[1] The variable is based on the one-digit DOT code. If the DOT code is 0, 1, or 2, an individual is classified as being a white-collar worker. DOT 0 and 1 include professional, technical, and managerial occupations. DOT 2 represents clerical and sales workers.

Demographic Variables

The demographic variables control for sex, race, and age. The sex variables take on values of one if BLACK or OTHERRACE (meaning not white (non-Hispanic)). Three race categories, white, black, and other are considered in the control specification, with the omitted dummy being white. Age is continuously measured in years.

Along with AGE, the dummy variable BLACK is significant in both the COMPENSATION and the WEEKS regression. Everything else constant, being black increases benefit year compensation by $325.

UI Program Structure Variables

The unemployment insurance benefit entitlement in Washington depends on the level and variability of claimant earnings in the first four of the last five calendar quarters completed immediately prior to the first day of an individual's benefit year.[2] The entitlement is presented as a weekly benefit amount (WBA), which will be paid for a week of complete unemployment, and a maximum duration of weekly full benefit payments called the entitlement period (ENTITLEDUR). These are included as variables in the control specification. To completely capture the structure of the benefit entitlement, we follow the general approach of Classen (1979) and include dummy variables for the maximum and minimum state WBA, the level of base period earnings (BPE), and a measure of the variability of BPE which influences the length of the entitlement period. As a measure of the variability of BPE, we include the standard deviation of earnings in the four quarters of the base period, SDBPE.

WBA, ENTITLEDUR, and WBAMAX, are all strongly significant in both the COMPENSATION and the WEEKS regression, while WBAMIN is significant at the 10 percent level in the WEEKS regression, and SDBPE is significant at the 5 percent level

[2] "If a benefit year is not established using the first four of the last five calendar quarters as the base year, the department shall use the last four completed calendar quarters as the base year." (Revised Code of Washington 50.04.020) This is called the alternative base year in the revised code of Washington.

in the Compensation equation. Everything else constant, a dollar higher WBA means $16 dollars more in benefit year compensation, another week of entitled duration of benefits means another $23 in compensation, and being at MAXWBA means $233 less in compensation.

SEARCHEX is a dummy variable equal to one if the claimant was in a full-referral union or on standby and thereby exempt from work search requirements. Exemption from work search does appear to be justified, as such claimants receive $629 less compensation than a nonexempt claimant. The variable BWAGES defines the base-year wages used to calculate entitlement, which averaged $15,475 for the control group. In the control equation, this variable had no effect on compensation or weeks compensated, probably because of high correlation with WBA and ENTITLEDUR. However, in the impact regressions, it was often significant because of the different structure of the impact equations, as noted above.

APPENDIX G

Effects of Quarter of Filing on Outcomes

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Effects of Quarter of Filing on Outcomes

Since enrollment in the experiment took place over a period of less than one year, there is insufficient information to determine any seasonal variation in the effects of the bonus offer. However, there were surprisingly large differences in program impact in different quarters. These differences are shown by looking separately at the treatment effects in each of the four callendar quarters of 1988. Enrollment actually started with the pilot project in Yakima in February, proceeded with full enrollment of all sites in March, and then terminated in the middle of November, so that some enrollment occurred in each of the four quarters. The following model was estimated:

$$Y = a + bT + C'Z + D'Q + E'ZQ' + F'Q\,T' + u,$$

where,

Y = The outcome variable, i.e., UI compensation in the benefit year, or weeks in the benefit year with some UI compensation,

T = A single dummy variable representing the combined treatment,

Z = A matrix of control variables (as listed in Appendix F) included as differences from the mean of the total sample,

Q = A (3 x n) matrix of three quarterly dummy variables representing the quarter of enrollment into the experiment with the first quarter omitted,

a = The intercept of the equation, which is interpreted as the mean value of the outcome for the individual represented by the mean of the control variables,

b = The treatment impact for quarter one,

C = A (k x 1) vector of coefficients indicating how the k control variables impact the dependent variable for claimants enrolled in the first quarter,

D = A (3 x 1) vector of coefficients indicating how the mean equation intercept varies in quarters 2, 3, and 4,

E = A (k x 3) matrix of coefficients indicating how the k control variables impact the dependent variable for claimants enrolled in quarters 2, 3, and 4,

F = A (3 x 1) vector of treatment impact estimates for quarters 2, 3, and 4, $F' = [f_1\ f_2\ f_3]$, and,

u = a normally distributed error term, having mean zero.

In the regression, *Q1* is the omitted variable, so that "*b*" is the estimated value of the effect of the treatment in the first quarter, and *TQ* represents the set of three interaction terms between each quarter and treatment. Thus, the impact of *T* in the second quarter is given by $b + f2$, etc. The control variables, *Z*, are entered alone and interacted with the three *Q*s to remove all experimental error. Equation (F1) is the equivalent of running four separate equations, one for each of the four quarters. Table G-1 shows the estimated treatment coefficients for equation F1.

Table G-1

Impact on Compensation and Weeks Compensated of the Average Treatment in Each of Four Quarters

	Total Compensation		Weeks Compensated	
	Parameter Estimates	Standard Error	Parameter Estimates	Standard Error
T (1st Q)	-311.45**	134.82	-1.60*	.86
TQ2	393.82**	146.60	1.91**	.94
TQ3	139.73	145.56	0.52	.93
TQ4	260.99*	150.71	1.45	.96

* Statistically significant at the 90 percent confidence level.
** Statistically significant at the 95 percent confidence level.

Note: Total effect of the average treatment in each quarter is estimated as follows: *T(1st Q) + TQn*, where $n = 2, 3,$ or 4.

APPENDIX H

Length of Time Before Serving Waiting Week

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Length of Time Before Serving Waiting Week

In the analysis of experimental impacts, we omit claimants who served a waiting week more than 30 days after filing for benefits. It was assumed that a waiting week served this long after filing was unrelated to the employment condition that generated the filing for benefits. Table H-1 shows the distribution of claimants according to when they served the waiting week associated with the date of filing that initiated the bonus offer.

Table H-1

Weeks After Filing in Which Waiting Week is Served

Weeks	Frequency	Percent	Cumulative Frequency	Cumulative Percent
0	1265	8.2	1265	8.2
1	12552	81.4	13817	89.6
2	706	4.6	14523	94.2
3	149	1.0	14672	95.2
4	69	0.4	14741	95.6
5	60	0.4	14801	96.0
6	47	0.3	14848	96.3
7	29	0.2	14877	96.5
8	38	0.2	14915	96.7
9	33	0.2	14948	96.9
10	41	0.3	14989	97.2
11	57	0.4	15046	97.6
12-26	267	1.7	15313	99.3
27-52	100	0.6	15414	100.0

Table H-1 shows that 8.2 percent of the filing claimants never served a waiting week. These claimants were accorded special consideration in the experiment in that separation issues did not prevent their receiving a bonus. For those serving a waiting week, it is clear that some cut-off was necessary, because there were claimants serving waiting weeks 52 weeks after filing.

Waiting weeks several months to a year after the date of filing were probably unrelated to the conditions that existed at the time of filing. A cut-off after three weeks seems reasonable for several reasons: first, there is a substantial decline in the rate of serving a waiting week after the third week, creating a natural break at that point; second, the number serving a waiting week in any particular week after week 3 remains fairly constant until after week 11, providing no other natural break until that time. We regarded it more likely that those postponing the waiting week that long were serving waiting weeks related to a spell of unemployment which started long after filing for benefits; and last, the potential for error was small, because use of the third week encompased over 95 percent of all claimants.

APPENDIX I

Effect on Outcomes of Eliminating from the Analysis Those Claimants Whose Claim File Showed No Post-Filing Wages

APPENDIX I

EFFECT ON OUTCOMES OF ELIMINATING FROM THE ANALYSIS THOSE CLAIMANTS WHOSE CLAIM FILE SHOWED NO POST-FILING WAGES

Of the 1,816 claimants who collected bonuses, 178 had no wage data in the quarter following benefit termination. For most cases, we can identify why the data are missing. The following reasons have been identified:

Became self-employed (according to NOH)	36	
Became employed out of the state (from NOH)	56	
Had late wage entries, not in the analysis	17	(est.)[1]
Total accounted for	109	
Total not accounted for	69	
Total receiving bonus without post-filing wages	178	

In analyzing the effect of including or excluding claimants without post-termination wages in the file, we tested the impact on qualifying for a bonus (i.e., monetarily eligible, no unresolved separation issues on the claim, and either served no waiting week or served a waiting week within three weeks of filing for benefits and terminated benefits within the qualification period). The control group in the set without the wage constraint had qualifying rates about 4 percent above the set with the wage constraint. The treatment impacts on qualifying were slightly larger for the set without the wage constraint, and the t-values were also somewhat larger. However, in both sets of results, T3 and T6 were statistically significant at the 95 percent confidence level, and no other treatments were statistically significant. Thus, excluding claimants without post-filing wages does not materially affect impact estimates.

[1] Based on a sample of 10 of the remaining 86 claimants. Of these, 2 had wages in the quarter filing benefits due mostly to the filing of late wages. The remaining 8 claimants showed no wages, and could have been in uncovered employment or otherwise employed by an employer with no employer account number.

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