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Abstract

Unemployment insurance is intended to reduce hardship by providing labor force members with partial wage replacement during periods of involuntary unemployment. However, in performing this income maintenance function, unemployment insurance may prolong spells of unemployment. Evidence from a field experiment conducted in Illinois in 1984 suggested that offering unemployment insurance claimants a modest cash bonus for rapid reemployment would increase the speed of return to work and reduce program costs. In 1988 a similar experiment, examining several different bonus offers, was conducted in Washington State. Evidence from the Washington experiment indicates that bonus offers do change job seeking behavior, but that only relatively generous bonus offers--about six times the weekly benefit amount--should be expected to significantly change the behavior of persons eligible for unemployment benefits.

Reemployment Incentives for Unemployment Insurance Beneficiaries: Results from the Washington Reemployment Bonus Experiment

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 the system has the potential of prolonging spells of unemployment. Indeed in the 1970s, economists led by Martin Feldstein (1975) 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 that 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 terms of carrot and stick incentives, these rules represent the stick.

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, lead 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, it 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 the UI trust fund through reduced benefit payouts, and increased revenues to governmental treasuries through increased personal income. Furthermore, the bonus offered the prospect of administrative cost savings through reduced UI work test monitoring--the administrative activity identified by Burgess and Kingston (1987) as having the highest error rate in the UI system. 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 in the field 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 by Woodbury and Spiegelman (1987) for the Illinois experiment, together with encouraging results from another bonus experiment conducted in New Jersey in 1986-87 and evaluated by Corson et al (1989), led the U.S. Department of Labor (DOL) 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 4 months. In 1987, DOL 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, DOL 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]

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 Washington Reemployment Bonus (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 below. 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 84 percent of the state's UI claims load. Among the ten JSCs omitted as enrollment sites, seven were particularly small and remote, one handled mostly interstate claims since it served the labor market which included Portland, Oregon, and two JSC (Lakewood and Tacoma) were host to another UI experiment (see Johnson and Klepinger, 1991).

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.

Enrollment rates were specified at the 21 selected JSCs to achieve a balancing 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 facing control group members thereby biasing 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

[1] Much, but not all, of the material reported here also appears in Spiegelman, O'Leary, and Kline (1992). Results from the Pennsylvania experiment are reported in Corson et al (1992).

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 enrollment rates permitted enrollment of the sample over an eight-month period with little chance of displacement, with sufficient volume to maintain claimstaker interest in the project.

There were effectively two data bases for the experiment. An operational data base was designed by DOL, utilizing the 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, and was provided by the state one year after completion of the benefit year for the last claimant enrolled into the experiment.

To place the findings from the WREB experiment in context, we now proceed to describe the economic environment in which the experiment was conducted, and summarize the composition of the population studied by examining the characteristics of the control group.

Economic Context and Client Population

The economic context of the experiment is summarized in Table 1. WREB was operated in 21 Job Service Centers (JSCs) across Washington state which serve 82 percent of the state's 4.2 million people, and handle 84 percent of the state's new UI claims. Racial minorities comprise nearly 14 percent of the Washington state population with the racial mix evenly distributed among black, hispanic and other non-white groups. Across the WREB enrollment sites, the racial mix is somewhat less even. The sample included a slightly smaller proportion of blacks than in the state because Tacoma, where the population is 10% black, was not a WREB enrollment site since another UI experiment was being run concurrently in that JSC.[2] Several JSCs which handle a large proportion of farm worker claims were included as enrollment sites, this resulted in Hispanics being slightly over-represented in the WREB sample compared to their share of the state population. The total unemployment rate across the WREB enrollment sites was very close to that statewide. With the exception of the government sector which is considerably under represented in the sample, the industrial mix of employment in the sample was very similar

[2] The UI experiment conducted in Tacoma was designed to evaluate alternative work search requirements, results are presented in Johnson and Klepinger (1991). In WREB, to compensate for the expected under-representation of minorities, enrollment was doubled at the Rainier site.

to that across the state.[3] Again, excluding the Tacoma area, which encompasses a major federal installation--Fort Lewis, is likely to have caused the discrepancy. There is no variable on which earnings across enrollment sites can be directly compared to earnings across the state. However, the statewide average for quarterly earnings in covered employment was just over $5000, while the base period average quarterly earnings for unemployed workers across WREB sites was just under $4000.[4] This difference is more likely due to the shorter hours experienced by the unemployed, rather than a lower hourly wage for this group.

Table 2 provides a description of the population studied in WREB by summarizing the characteristics of the control group. The WREB control group sample of insured unemployed includes relatively more females, young persons, and minorities than the state population as a whole, but as expected it is typical of the unemployed population. The sample also includes a large proportion of blue collar workers from non-manufacturing industries. Using the duration of previous job attachment to identify dislocated workers, the sample includes up to 36% who were dislocated--separated from continuous employment of three years or more, with smaller percentages being dislocated by narrower definitions of dislocation. The sample average weekly benefit amount was about $150. Claimants typically collected benefits for about half of their entitled weeks of benefits. Benefit entitlement averaged nearly 27 weeks. Nearly one-quarter of all claimants exhausted their benefit entitlement.

EXPERIMENTAL DESIGN

There are three important elements in the WREB experimental design: (1) the eligibility conditions, which delimit the target population; (2) the treatment design or structure of incentive offers; and (3) the sample design which includes determination of the appropriate sample size and enrollment site selection. In addition to these design matters, the degree of randomness achieved in WREB by following the enrollment design is reviewed in this section.

Eligibility Criteria for Enrollment

Since the experiment was intended to increase the job search effort of UI claimants thereby reducing unemployment and UI benefit payments, a bonus was only offered to those claimants eligible to receive UI benefits. This required claimants to have sufficient wage credits to establish

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[3] The under representation of government employees is probably due to the fact that former federal government employees qualifying for benefits under the UCFE program were excluded because only those who qualified for regular state UC were eligible for a bonus offer. Also, former state employees were generally excluded because their recent history of earnings from the state would not be in the computerized system at the time of filing.

[4] Base period earnings is the wage data used to determine UI eligibility. In Washington the base period usually is the first four of the five quarters prior to the quarter in which the claim is filed; however, some claimants use an alternate base period of the last four of the five quarters prior to the quarter of filing.

a monetarily valid claim, and no nonmonetary issues such as quit or discharge which would block UI payments.[5] Furthermore, the claimant must have been submitting an initial claim to start a new benefit year. If a bonus program were to be implemented it is likely that a bonus offer would be made only at the start of a new benefit year. Therefore, restricting eligibility to those filing initial claims is appropriate.

To avoid discouraging immediate job search, the bonus offer was not limited to claimants who actually received UI benefits. Since the state of Washington requires a waiting week before beginning payment of UI benefits, to accommodate the possibility of claimants taking jobs before receiving any benefits, claimants were eligible for the bonus without receiving any UI benefits provided they would have otherwise been eligible for UI.[6]

Claimants whose benefits were not chargeable to the Washington State UI trust fund could not participate in the experiment. These claimants included those filing interstate claims, recent federal employees (UCFE) or recently discharged veterans (UCX). Since these claimants could not establish a monetarily valid claim for regular Washington state UI benefits at the time of filing, they could not participate. Claimants filing combined wage claims could participate since claimstakers could identify Washington State wage credits at the time of filing using the computerized Benefits Automated System (BAS); however, only Washington wage credits were used to determine the size of the bonus offer.

Claimants who could not establish a monetarily valid claim at the time of filing for benefits were excluded from the experiment since the UI entitlement was needed to determine the size of the bonus offer and the length of the qualification period (the length of time the claimant would have to find employment in order to qualify for a bonus payment). This restriction assured that all claimants could be given exact information on the size of their bonus offer and their reemployment deadline at the time of filing, but it established some restrictions that probably would not exist if a bonus program were actually implemented.

In addition to having a monetarily valid claim, the claimant must not have been ineligible to receive benefits because of job separation issues (nonmonetary issues such as quit or discharge). Since these claimants were ineligible for benefits for the duration of their unemployment spell, they could not qualify for the bonus. However, some denials could be temporary as in the case of able-and-available (continuing eligibility) issues. Claimants who were sick or away on vacation did not receive benefits for those weeks since they were not searching for work, did not lose their bonus eligibility. These issues were no longer in effect once the

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[5] These are referred to as indefinite nonmonetary stops.

[6] An exception to the UI eligibility criteria occurred in the case of claimants who filed monetarily valid claims but did not claim waiting week credit. In cases where there were nonmonetary issues pending, the state of Washington did not adjudicate these issues unless the claimants filed for waiting week credit. Some of these claimants with pending nonmonetary issues may not have otherwise been eligible, but with no adjudication of the issue, these claimants were allowed to participate.

claimants returned to active job search and therefore the claimants could still qualify for the bonus.

There were two other groups of claimants who could not receive the bonus: (1) claimants recalled to their previous job by their terminating employer, or (2) claimants who found work through their union hiring hall. In these cases, since the bonus could not affect job search behavior because job acquisition was totally dependent upon the actions of the employer or the union, these claimants were not paid a bonus.

Note, however, that awaiting recall or being a member of a referral union did not exclude a claimant from participation in the experiment. The bonus offer could encourage a claimant to return to work more rapidly than if they simply waited for recall or placement by their union. A member of a referral union who obtained a job without union placement was eligible to receive a bonus as was a claimant on standby (awaiting recall) who obtained another job. A special provision of the experiment allowed claimants to remain bonus-eligible if, after working at least one week on a new job which was not a recall or a union referral, they returned to their previous job or accepted a union hiring hall placement.

Since the intent of the bonus offer was to encourage more vigorous job search, a claimant who obtained a new job with the previous employer, which was not a recall to the previous job, could 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, UCFE, or UCX claim.

In addition, to be eligible to receive a bonus, the 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;
(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.

Treatment Design

The WREB experiment had three components: (1) the bonus amount--in dollars; (2) the qualification period--the period of time during which the claimant must start a new job to remain eligible for a bonus; and (3) the reemployment period--the length of time the participant must have remained employed full time to receive the bonus.

Bonus Amount

Since a major goal of the WREB experiment was to determine an optimal bonus size, a variety of bonus amounts were examined. These were specified as two, four, and six times the claimant's weekly benefit amount (WBA). With the statutory minimum and maximum WBA set at $55 and $209 respectively, bonus offers ranged from $110 to $1254. The bonus offers within a given treatment varied across claimants because of different entitlement, but were constant in terms of opportunity cost of unemployment. That is, a reduction of one week in unemployment cost each fully unemployed claimant one week of compensation, so that within each treatment group the "price" of the bonus was the same for each individual in terms of this sacrifice. A monetary determination at the time of filing set the bonus offer, so that all claimants had full information regarding their bonus amount at the time of filing.

Qualification Period

The qualification period is the maximum duration of insured unemployment after filing for benefits that the participant could experience and still qualify for a bonus. The qualification period begins the week that the new initial claim for UI benefits is made, and ends on what is called the reemployment deadline.

The experiment tested two different qualification periods, 20 and 40 percent of the entitled duration of benefits, plus one week to cover the required waiting week. Since the minimum and maximum entitled duration of benefits in Washington are 10 and 30 weeks respectively, qualification period lengths ranged from 3 to 13 weeks. If the qualification period calculated using this formula included a fraction of a week, the computation algorithm rounded up the qualification period to the next whole week. Claimstakers communicated the qualification period to the claimants as a reemployment deadline by which the claimant must start full-time employment and stop receiving UI benefits in order to remain eligible for the bonus.

Reemployment Period

Once the treatment assigned claimant began full-time reemployment by the deadline, the start-date of the new job marked the beginning of a four month reemployment period during

which the claimant must have remained fully employed and must not have drawn UI compensation in order to be paid a bonus. The four month period was judged sufficient to avoid paying a bonus for return to temporary or seasonal work, and to reduce the tendency for a claimant to take a job strictly to obtain a bonus.

Sample Design

Randomization is at the heart of a field experiment. The key principle involved is that each member of the population has an equal chance of selection into the experiment and for assignment to any of the treatment cells. Most important, randomization avoids systematic self-selection into a treatment, which is the major pitfall of non-experimental program evaluation. In WREB, randomization was achieved by using the last two digits of each claimant's Social Security Number (SSN) for assignment to one of the six treatments or the control group. Since the last two digits of an individual's SSN are randomly generated, the expected result of the assignment process is that the average characteristics of individuals in each of the seven groups is the same.

The sample size for each treatment is set to achieve two goals: (1) to have a high degree of confidence that an ineffective policy is not accepted, and (2) to avoid rejecting an effective policy.

The following statistical standards were set to meet these goals: (1) the statistical significance level for hypothesis testing was set at 5 percent, which means that if a program is judged effective there is only a 5 percent chance of being wrong, and (2) the power of statistical tests was required to be at least 80 percent, which means that when a program is effective a hypothesis test will reveal it to be effective at least 80 percent of the time. Naturally, more powerful tests are more reliable, but they require substantially larger samples. The selected confidence levels are considered standard by most analysts and policy makers.

Using results of the Illinois claimant experiment as a guide to the magnitude of treatment effects, tables in Cohen (1977), a standard reference for power analysis in the behavioral sciences, were used to specify the sample size targets for each cell. That is, sample sizes were set to detect a reduction in the duration of insured unemployment of at least 1.15 weeks--the experimental impact in Illinois.[7] Since the bonus offer was not expected to cause an increase in

[7] Sample sizes were set to detect an average "effect size" equal to that observed in the Illinois participant experiment. The effect size, d, is the treatment effect on the outcome measure divided by the standard deviation of the outcome measure in the population. That is, $d = [m(c) - m(t)]/s$, where m(c) and m(t) are the mean values of the outcome in the control and treatment groups respectively, and s is the standard deviation of the outcome in the population. Using Illinois data, $d = (1.15/12)$, or about 0.1.

the duration of unemployment, sample sizes were set to conduct one-tail tests of significance.[8] To test hypotheses about treatment effects at the 5 percent significance level with 80 percent power, treatment cells averaging 2,000 claimants in size were specified.

Statistical requirements do not dictate that each cell receive the same share of the total sample. Considerations of cost per observation and policy relevance are also important factors in determining the allocation of the sample to the various treatments. The size of the control group is also important because the confidence in the result will be greater the larger the "effective sample size." Larger samples may be achieved by increasing both treatment and control groups equally, or by increasing one holding the other constant.[9]

On the basis of both cost and policy relevance, it was decided to enroll a larger proportion of the sample in less expensive treatments. Furthermore, since treatments with smaller bonus offers were expected to have somewhat smaller impacts, the larger sample allocation to these treatments would allow for tests of treatment effects with adequate significance and power. The designed and actual enrollment numbers are listed in Table 3.

By setting the control group size at 3,000 adequate significance and power is achieved for hypothesis tests with 1,500 claimants enrolled in the two high bonus offer treatments, treatments 3 and 6 (T3 and T6), and with 2,250 in each of the other four treatments. Assuming that 18.75 percent of claimants offered a bonus would cash one and that the average WBA was $148, a bonus payment budget of $1.25 million was requested from the U.S. Department of Labor.[10] A budget of $1.2 million was eventually set. The random assignment algorithm resulted in actual sample sizes which are not significantly different from the designed sizes.

Enrollment of the Final Analytic Sample

During the enrollment phase of the experiment, a simulation model was used to monitor the expected costs of bonus payments. The model was initialized with historical claims information for the enrollment sites, and was updated weekly to reflect experience during WREB. Information from this model was used to adjust the length of the enrollment period so as to

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[8] In the end, two-tail tests were used because of the possibility that the bonus offer, operating through an income effect, could cause an increase in the duration of unemployment.

[9] In this case the effective sample size is the harmonic mean, $n_h = [2n_t n_c/(n_t + n_c)]$, of the size of the treatment group, n_t, and the size of the control group, n_c. By this formula it can be seen that with $n_t = 1{,}500$ and $n_c = 3{,}000$, the effective sample size is $n_h = 2{,}000$.

[10] The proportion of claimants cashing bonuses was set at .1875 since the observed rate in the Illinois claimant experiment was .14 and because WREB used enhanced informational procedures. The initial bonus payment budget estimate of $1.25 million = [(4,500x2x$148) + (4,500x4x$148) + (3,000x6x$148)]x.1875.

maximize the sample size, given the bonus budget.[11] As expected the model was useful for guiding adjustments for differences from historical levels in the volume of claims (down 5.4 percent from the previous year), and in refining the assumption regarding the proportion of bonus offers converted into payments (14.58 percent instead of 18.75 percent). An unexpected benefit of the model was the ability to adjust for what might be called "the bonus upgrade" phenomenon. There is a bonus upgrade when the average bonus paid exceeds the average bonus offered. This was impossible with the $500 offer in the Illinois experiment, but it occurred in WREB because claimants offered larger bonuses were more likely to cash them. Indeed the average bonus offer was $567 and the average bonus paid was $653. The net result of these adjustments to the plan was that enrollment was ended after 37 weeks, and in the end $1.19 million of the $1.2 million bonus budget was spent.

Of 17,554 claimants assigned to treatment and control groups, 15,534 were ultimately eligible to participate in the bonus offer program. Claimants must have satisfied one of the following criteria for inclusion in the final analytic sample: (1) the claim must have been monetarily valid at filing and there must not have been any 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 analytic sample excluded 2,020 claimants who had indefinite, nonmonetary stops on their claim throughout their qualification period. These claimants were excluded since they were not eligible to receive UI compensation. Table 3 presents a summary of the designed enrollment and actual enrollment into the six treatment groups for the final analytic sample; the control group for the analytic sample included 3,082 claimants.

Tests for Randomization in Treatment Assignment

Randomization is at the heart of a field experiment. The key principle involved is that each member of the population has an equal chance of selection into the experiment and assignment to any of the treatment cells. In particular randomization avoids systematic self-selection into a treatment, which is the major pitfall of non-experimental program evaluation.

The procedure for random assignment in WREB used the fact that the last two digits of each claimant's Social Security Number are randomly assigned. However, even with an error-free assignment process there is no guarantee that homogeneity across the control and six treatment groups will result. Table 4 shows mean values across the control and treatment groups of a set of observable exogenous characteristics. Some of these variables, such as the weekly benefit amount and weeks of entitlement are parameters of the UI system, while others describe the socioeconomic characteristics of individual claimants.

Statistical tests using F-statistics indicated that the assignment process was random when considering the characteristics collectively; that is no more than the expected number of

[11] Details of this model are presented in Appendix D of Spiegelman, O'Leary, and Kline (1992).

characteristics differed significantly in tests at the 90 percent confidence level. However, t-tests revealed statistically significant variation across groups for some individual characteristics. The mean values of the weekly benefit amount (WBA) for T4 and T6 were significantly greater than the control group, and T5 and T6 had significantly higher base period earnings (BPE) than the control group. Unfortunately these variables affected the outcomes of interest--in particular dollars of UI compensation drawn. Because unadjusted treatment impacts may be biased, the analysis in subsequent sections will present results with and without control variables.

PARTICIPATION IN THE EXPERIMENT

For a claimant to have fully participated in the experiment (s)he must have collected a bonus after submitting a valid Notice of Hire (NOH)--the form claimants used to notify the Washington experiment staff of return to work. These are clear and objective criteria for identifying participation. There are several more limited, but somewhat subjective, measures which are also useful. These might more properly be labeled measures of qualification. A claimant has fully qualified for a bonus if (s)he has terminated benefits and returned to full time employment within his/her qualification period, drawn no UI and remained continuously employed during the subsequent four months, did not return to the previous job, and was not placed on the new job through a union hiring hall. Full qualification is defined as satisfying all the conditions to collect a bonus whether or not one was received, and partial qualification means meeting some of these criteria. It should be noted that performance of an observable action, such as filing a NOH, is an indicator of partial qualification in the experiment, however, such an action does not necessarily mean that job search behavior has been affected by a bonus offer. Participation in a theoretical sense means that job search effort was increased in response to the bonus offer. This behavioral response may or may not be associated with various observable degrees of qualification in the experiment such as filing a NOH or cashing a bonus voucher, but it is the source of observed reductions in UI benefit receipt which result from a bonus offer.

Measures of qualification are useful since they allow investigation of the "bonus take-up rate"--the proportion of fully qualified claimants who actually collect a bonus. The take-up rate is a conditional measure of participation which is useful in estimating the potential cost effectiveness of an actual program.

Table 5 presents two objective measures of participation based on records from the Washington experiment. Among the claimants offered a reemployment bonus, 18% filed a valid NOH. This percentage increases with both the level of the bonus and the length of the qualification period. A somewhat smaller proportion, 14.6%, of treatment assigned claimants fully participated in the experiment by actually collecting a bonus.

The pattern of bonus receipt is similar to the pattern for NOH filing. The percentage of treatment assigned claimants cashing a bonus increases with the level of the bonus and the length of the qualification period. However, these differing degrees of participation associated with

different bonus levels or qualification periods do not necessarily imply differences in labor market behavior. The expected progression could simply reflect differences in bonus take-up rate by claimants who qualified for the bonus without changing labor market behavior. Table 6 allows examination of some more subtle aspects of participation.

Among all treatment assigned claimants 55.5% terminated benefits by the end of their qualification period.[12] This number is over three times the fraction who submitted a valid NOH, but like the percent filing a valid NOH it increases with the bonus level and the length of the qualification period. A claimant will file a NOH only if (s)he expects to qualify for a bonus. There are several reasons why a claimant, who otherwise appears to qualify, might not file. A claimant may have returned to work at his/her previous job or been placed on a new job through a union hiring hall, events which would make filing a NOH futile. Additionally, a claimant may have stopped drawing UI benefits and appeared to qualify, when in actuality (s)he stopped seeking work and dropped out of the labor force.

Within the claimants assigned to treatments, 38% both terminated UI benefits by the end of their qualification period and drew no benefits in the subsequent four month period. Excluding from this group claimants who returned to their previous employer and claimants who declared membership in a full referral hiring hall at their time of filing for UI benefits, 25.1% of treatment assigned claimants appear to have qualified for a bonus.[13] This is probably the clearest indicator of participation in the experiment. Within the group of claimants who appear to have qualified for a bonus 55.8% ultimately received one--this is a measure of what is called the bonus "take up rate."[14] This estimate of the take up rate is somewhat downward biased since some of the claimants who stopped drawing benefits before their reemployment deadline may simply have dropped out of the labor force. If labor force drop outs could be excluded from the estimated number of bonus eligible claimants, the number of claimants cashing bonuses would be a larger fraction of those believed to be eligible. However, just like the bonus cashing rate, the bonus take up rate increases with the level of the bonus and with the length of the qualification period. The data does indicate a behavioral response to the bonus offer, since the percentage of claimants who appear to have qualified for a bonus increases with the level of the bonus and with the length of the qualification period.

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[12] Two consecutive weeks without a UI payment was used to identify the end of the first spell of unemployment compensation.

[13] This exclusion presumes that claimants who returned to their previous primary employer were recalled. Since a claimant could qualify for a bonus by returning to the previous employer at a different job, claimants who ultimately received a bonus were not excluded. A similar adjustment wasmade for union hiring hall members who were not placed on their new jobs by the union.

[14] The number listed in Table 6 for bonuses paid is less than the number listed in Table 5 since the number of claimants analyzed as having "partially qualified" for abonus examines only the first spell of collecting UI. In fact some claimants experienced more than one spell of insured unemployment before their reemployment deadline, and still qualified for a bonus.

Table 7 presents an analysis of the bonus take-up rate using four different models of the treatment effect on the probability that claimants who apparently qualified for a bonus will actually cash a bonus. For each of the models the sample is the 3,128 treatment assigned claimants who appear to have qualified for a bonus (as indicated in Table 6), with the dependent variable taking a value of one for claimants who cashed a bonus and zero otherwise.

Regression results for the Treatment Model, which used six treatment dummy variables, are reported in the first column of Table 7. The take-up rate estimates from a linear probability model controlling for demographic and program characteristics are very similar to the unadjusted rates reported for the six treatments in Table 6. All treatment impact estimates on the take up rate are statistically significant. With the exception of T2, the treatment impacts are each significantly different from the mean take up rate of 55.8 percent indicating that differences in the treatment parameters affected the take-up rate.[15] The second model is called the Price Model since it combines treatments with similar WBA multiples or prices--T1 and T4 with a multiple of two, W2 and T5 with a multiple of four, and T3 and T6 with a multiple of six. The estimates clearly show the progression in the take-up rate as the WBA multiple increases. All coefficients and differences from the mean treatment impact are statistically significant. The third model is called the Duration Model since it combines treatments with similar qualification period lengths; that is, similar in terms of the share of entitlement--20 or 40 percent of the entitled duration of benefits, plus one week. The take-up rates for both the short and long qualification periods are significantly different from the mean take-up rate. The results indicate that a longer search period does cause the take-up rate to increase.

Table 7 also reports on results of estimating what is called the Continuous Model, which uses the bonus amount and qualification period length as continuous variables--the dollar value of the bonus offer and the qualification period length in weeks. The parameter estimates are statistically significant, and consistent with results from the previous models suggesting that an increase in bonus take up will result from an increase in either the bonus amount or the qualification period. This model provides estimates of the impact on bonus take up of changes in the bonus parameters. Each $100 dollar increase in the bonus offer is estimated to increase the take-up rate by 2 percent, while each one week increase in the qualification period is estimated to increase the take up rate by about 1 percent.

Results from the preceding analysis of participation might be used to adjust estimates in a benefit-cost analysis of a reemployment bonus, but some caveats apply. A very conservative

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[15] Testing the difference between the individual treatment impacts and the mean treatment impact on the take up rate was done using the method of Kennedy (1986). The method involves imposing the restriction in estimation that the weighted sum of deviations from the mean treatment impact is zero. Control variables are entered as differences from their means, so that the intercept in the regression equations is the mean take up rate. To judge if variations in the parameters of the bonus offer affect the take up rate, tests of differences of the treatment impacts from the mean are preferred to tests between separate treatment impacts. The latter tests are bound to reveal some differences even when overall behavior is relatively unaltered, while the former test shows differences from mean behavior which is a much weaker test of response.

application of the findings suggests that bonus payments could increase by a factor equal to the reciprocal of the take-up rate.[16] If there were no corresponding decrease in UI compensation, this would lead to a proportionate drop in net benefits. However, a higher take-up rate could be associated with greater reduction in insured unemployment, if the additional bonus recipients responded to the offer by returning to work sooner. At any rate, such a large increase in the take up rate is unlikely, since no entitlement program experiences a take-up reaching one-hundred percent.[17]

TREATMENT IMPACTS ON INSURED UNEMPLOYMENT

The main objective of a bonus offer is to reduce the duration of insured unemployment by encouraging workers to return to employment more quickly, thereby also reducing the cost of UI compensation. The bonus offer is expected to operate by increasing the intensity of job search and thereby raising the probability of finding an acceptable job offer. The observed response to bonus offers is summarized using several measures reported in Tables 8 and 9. These measures are weeks of insured unemployment and dollars of compensation in the initial spell of unemployment and over the full benefit year, and the benefit exhaustion rate.[18] Table 8 presents unadjusted estimates of treatment impact, while Table 9 gives regression adjusted estimates.

Unadjusted Treatment Impacts

For a classically designed experiment involving random assignment and large sample sizes, treatment impact estimates may be computed as the simple difference between treatment and control group means on outcome measures of interest. The absence of constraints imposed by modelling of behavior, and the ease of understanding gained through this simplicity are the fundamental appeals of experiments for program evaluation. Table 8 presents unadjusted estimates of the response to the bonus offer. The mean unadjusted response across all treatments was a reduction of $22 in UI compensation and about a one-third week reduction in insured unemployment.

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[16] For example with a take-up rate of 55%, bonus payments to the remaining 45% of fully qualified claimants would increase costs by a factor of 1.82.

[17] Blank and Card (1991) have estimated that in the U.S. only about 70% of eligible claimants receive regular UI benefits.

[18] The end of the initial spell is a somewhat arbitrary concept. UI payments could stop 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. Ending a spell of unemployment in the experiment implies obtaining full-time work. Without precise information as to why there is a gap in the payment series, we have arbitrarily defined the end of the spell as occurring when the claim break is two weeks or longer. Requiring a three week interruption did not change the results appreciably.

For compensation received by claimants in the initial spell and benefit year, only T3, T4, and T6 have the expected negative sign; and only the high bonus multiple-long search period treatment (T6) shows a significant difference from the control group mean. Weeks of insured unemployment (weeks of receiving some compensation or waiting week credit) again show the strongest effects for the high bonus multiple treatments. In terms of weeks of insured unemployment, T6 induced a 0.82 week reduction during the initial spell and a 0.73 week reduction over the benefit year with both impacts being statistically significant at the 95 percent level. T3 induced a significant reduction in insured weeks over the benefit year at the 90 percent significance level; while T4 reduced weeks compensated in the benefit year and resulted in a reduced exhaustion rate. Neither in terms of compensation received nor weeks of insured unemployment are benefit year impacts significantly different from first spell impacts, which is consistent with the intent of the bonus offer to reduce the spell of unemployment immediately after filing for benefits.

Combining treatments with similar WBA multiples yields three groups that differ in the level of the bonus offer but have the same mean qualification period (T1,4, T2,5, T3,6); among these groups the high WBA multiple treatments (T3,6) show the greatest effects.

Adjusted Treatment Impacts

If treatment-control differences in outcome variables are due to factors other than the treatment, a simple comparison of means may not be adequate to identify treatment effects. As noted previously, for the Washington experiment there were no more differences between treatment and control groups in observable characteristics than would be expected to result from a random assignment process. Unfortunately the variables on which there were the most pronounced differences, WBA and BPE, may have an effect on the measurement of outcomes of interest--most importantly dollars of UI compensation drawn.

As stated above, unadjusted treatment impact estimates can be computed as a simple difference between treatment and control means on an outcome variable of interest. An alternative procedure which yields the same result involves estimating:

$$Y = a + TB + u, \tag{1}$$

by ordinary least squares regression. In this equation the intercept, a, is the mean value of the outcome variable, Y, for the control group. T is a matrix of dummy variables representing the treatments, and u is a normally distributed mean zero error term. The parameter vector B yields estimates of the simple differences between treatment and control means on the outcome variable.

The model used to estimate treatment impacts while controlling for other factors is a straightforward generalization of equation (5.1). The specification for computing adjusted

treatment impacts involves adding terms for control variables, and is referred to as a covariance model. In the present case it takes the form:

(2) $$Y = a + TB + ZC + u,$$

where, the introduction of control variables, *Z*, into the model reduces the experimental error caused by differences in the observable characteristics between the control and treatment groups.[19] So as to preserve interpretation of the intercept as the full sample mean value of the outcome variable Y, each control variable is included as a deviation from its own mean.[20] By this approach the vector B yields estimates of treatment impacts adjusted for differences across observations in the characteristics Z.

An investigation into the causes of the lack of complete random assignment was undertaken. The analysis indicated: (1) no systematic problem with the random assignment mechanism, (2) treatment impacts estimated without adjusting for heterogeneity in the characteristics of assigned sample observations are likely to be biased, and (3) variations in the weekly benefit amount (WBA) correct for most of the error in the estimates of experimental-control due to differences in the characteristics of the two samples.

Treatment impact estimates adjusted by including variables to control for demographic and program characteristics in the regression equation are given in Table 9. Comparing these estimates with the unadjusted estimates in Table 8, it is seen that introducing control variables increases the parameter estimates and reduces the standard errors. Furthermore, regression adjustment has a greater effect treatment impact estimates on dollars of UI compensation than on weeks of compensation or the exhaustion rate. The mean regression adjusted response across all treatments to a bonus offer was a reduction of $63 in UI compensation and 0.40 week of insured unemployment over the benefit year. The mean impact across all treatments on compensation in the benefit year is more than double the unadjusted estimate and is significant at the 90 percent confidence level. The mean reduction in weeks of insured unemployment estimated increases only slightly, but combined with a slight fall in the standard error it becomes statistically significant at the 90 percent confidence level. The increased accuracy in measuring the treatment effects is the result of the control variables reducing the differences between control and treatment groups that resulted from differences in the composition of the control and treatment groups instead of the treatment alone. After regression adjustment, three of the individual treatments--

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[19] See Netter and Wasserman (1974), Chapter 22, for a concise discussion of covariance analysis.

[20] The effect of centering around the mean is that the intercept takes the value of the outcome measure for a hypothetical person in the sample who was not exposed to the experimental treatment, and whose exogenous characteristics are at the mean value for each of the characteristics across the total sample (control and experimental groups combined). The treatment effect is the impact of the treatment on the outcome measure for that hypothetical individual. The full list of control variables used to control for demographic and program characteristics includes the following three continuous variables: weekly benefit amount, entitled duration of benefits, and base period earnings; and the following dummy variables: gender, age less than 35, age greater or equal to 55, black, hispanic, other non-white race, previous job in manufacturing, work search exempt, and 20 dummies for enrollment sites.

T3, T4, and T6--show statistically significant impacts on compensation in the benefit year at the 95 percent level of confidence, whereas no significant individual treatment impacts were estimated before adjustment.

With regression adjustment the combined treatments based on the bonus level and length of the qualification period, also revealed larger treatment impact estimates. The pattern of impact estimates is similar to the unadjusted impacts in that the biggest effect is for the high bonus level (T3 and T6). For both the unadjusted and the regression adjusted estimates, the large anomalous impact for T4 (low bonus multiple, long qualification period) tends to obscure the relationship among the individual bonus levels.[21]

Tobit and Probit Estimates of Treatment Impacts

It must be recognized that each beneficiary under unemployment insurance faces a limit on the total benefit payments which they may receive in a given benefit year. In Washington state during 1988 the maximum dollar value this limit could take was $6270, but the limit could vary across individuals depending on their pre-unemployment earnings. Furthermore, claimants must draw all benefits within 52 weeks of establishing filing for benefits or they will lose entitlement to them. Therefore in terms of the outcome measures examined in this study, there are limits on our ability to observe the complete extent of underlying behavior. That is, for a claimant who exhausts UI benefits the spell of unemployment does not necessarily end, but our ability to observe the spell duration stops. In this case our data is censored. To examine if censoring seriously affected our estimates of treatment effects, Tobit estimates where made.[22]

Results of the estimation are given in Table 10 in a format identical to that of Table 8 and 9. For the Tobit estimates of treatment impacts on dollars of UI compensation the upper limit was set for each beneficiary at their entitlement level, and for the estimates on weeks with some compensation an observation was classified as censored if the claimant exhausted benefits or drew compensation in the fifty-second week of the benefit year. For the exhaustion rate estimates, since the outcome is binary, Table 10 presents Probit estimates of treatment impacts.[23] In both Tobit and Probit analysis the effect estimates must be computed by appropriately weighting the underlying parameter estimates with statistical significance being judged on the basis of the parameter estimates. Following custom, Table 10 presents both the parameter estimates and the effect estimates.

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[21] The pattern among the treatments amounts to a violation of the law of demand. The smaller bonus offer generated a larger response. Either the treatment impact estimate for WT4 is too large, or impact estimates for WT1, WT2, and WT5 are too small. The former conclusion is more consistent with the evidence.

[22] The method used was popularized by Tobin (1958), a good summary of the technique is given in Chapter 6 of Maddala (1983).

[23] Probit was popularized by Finney (1947), a good summary is given in Chapter 2 of Maddala (1983).

Like the regression adjusted impact estimates reported in Table 9 the Tobit and Probit estimates given in Table 10 were computed while controlling for demographic and program characteristics. Further adjusting for censoring only slightly affects treatment impact estimates for both UI compensation and weeks of UI receipt, with some increasing and others decreasing. The probit estimates of treatment impact on the exhaustion rate are virtually identical to both the adjusted and the unadjusted impact estimates.

Price and Duration Effects

The two parameters of the reemployment bonus which vary across the individual offers made are the dollar amount and the duration of the qualification period. We refer to the response to changes in these two parameters as the price and duration effects. Where, the price effect is the response to varying the dollar bonus amount and the duration effect is the response to changes in the length of the qualification period. Two approaches are used to estimate price and duration effects. The first estimates presented were produced using discrete indicator variables, the second set of estimates were computed in a continuous variables model.

The estimates of the price effect presented in Table 11 involve a contrast in treatment response by claimants offered a bonus equal to the highest WBA multiple (T3 and T6) compared to claimants offered lower multiples. Computed in this way the price effect was statistically significant, being slightly stronger during the initial spell of unemployment than over the benefit year. An increase in the bonus amount to the highest WBA multiple induced a reduction of about $85 in compensation and one-half a week of insured unemployment over the benefit year. The price effect on exhaustion was insignificant. The duration effect is somewhat smaller, being significant in terms of dollars of compensation, but not weeks. Nearly doubling the qualification period reduced compensation only $59 over the benefit year. Paradoxically, the effect on exhaustion was large and significant. The longer qualification period induced a 2.1 percent decrease in exhaustion of UI benefits.

Table 12 presents estimates of price and duration effects from a linear continuous variable model. Using this model the marginal effects of a change in the dollar bonus amount and the weeks in the qualification period are estimated. The model defines the treatments as a bonus offer with a given dollar amount and a qualification period having a certain number of weeks. The model estimated has the general form:

$$(3) \qquad Y = a + b_1 B + b_2 Q + ZC + v,$$

where, B is the bonus amount in dollars, Q is the qualification period length in weeks, and Z is the set of control variables centered around their mean.[24]

[24] The only control variable included in computing the estimates reported in Table 12 is the weekly benefit amount.

The estimates presented in Table 12 indicate that the price effects of the experiment are small. A $1,000 increase in the bonus offer reduces UI compensation in the benefit year by $37, and weeks compensated in the benefit year by 0.43 weeks. Evaluated at the mean bonus offer of $575, the price effect reduces benefit year compensation by $21 and weeks compensated in the benefit year by 0.25 weeks.

In this model adding a week to the qualification period has insignificant effects, apparently reducing UI compensation in the benefit year by a mean value of $8. Evaluated at the mean qualification period length of 8.4 weeks, the duration effect reduces benefit year compensation by $67 and decreases weeks compensated in the benefit year by 0.20. As was estimated using the discrete model, there is no statistically significant price effect on benefit exhaustion, but the duration effect is a statistically significant reduction in exhaustion of UI benefits

For the continuous variable model, the combined price and duration effect shows a mean reduction of benefit year compensation of $69 and a mean reduction of weeks compensated of 0.3 weeks. It is worth noting that while not all of the individual price and duration effects have a significant impact on the outcome measure, the parameters for the bonus amount and qualification period are statistically significant jointly for all outcome measures.

In summary, the price and duration effect analysis of the bonus suggests that increases in the dollar dimension of the offer tend to reduce dollars and weeks of compensation, while increases in the weeks dimension of the offer tend to reduce benefit exhaustion.

The Timing of Treatment Impacts on Insured Unemployment

Up to this point analysis of treatment impacts has focused on the mean response by various treatment groups. In this section the time pattern of treatment impacts is examined using the methods of economic duration analysis.[25] The fundamental concept discussed here is the conditional UI exit rate. 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. The time frame for this analysis is the first spell of covered employment. The initial risk set (number of claimants eligible for WREB at filing) includes all claimants who served a waiting week within 3 weeks of opening a claim for benefits. It also excludes claimants who opened a claim but never filed for a waiting week or for compensation, this exclusion is made on the assumption that these claimants left insured 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 controls.

[25] Refer to Kiefer (1988) for a summary of methods of economic duration analysis which has been used by Decker (1992) and by Davidson and Woodbury (1991) to analyze UI bonus experiments.

If the bonus offer was effective, claimants in treatment groups 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 claimants attempting to qualify for the bonus, seven weeks (for the short qualification period) or thirteen weeks (for the long qualification period) is the longest period of unemployment claimants could experience and still qualify. For claimants with a short qualification period, exit rates should exceed those for controls by the greatest margin in weeks 1 to 7 after filing, while exit rates for claimants with a long qualification period should exceed those for controls by the greatest margin in weeks 1 to 13.

Table 13 presents conditional UI exit rates for the control group and differentials for the treatment groups.[26] For the short qualification period treatments (T1, T2 and T3), T2 and T3 show a positive differential from the control group for weeks 1-7; however, the coefficients are not significantly different from zero. The significant coefficients on T2 and T3 for weeks 14-31 are not consistent with expectations. However, for the long qualification period treatments (T4, T5 and T6) the results accord more closely with expectations. Strong, highly significant coefficients appear on T6 for weeks 1-7 and 8-13 indicating exit rates which are 3.4 and 4.4 percentage points higher than the control group, respectively.

Table 14 presents evidence of the permanence of the treatment effects on UI exit rates. The data are cumulative UI exit rates for controls and differentials for treatments. The first row in Table 14 is identical to the first row in Table 13; it is repeated to allow easy comparison. Through 13 weeks, the cumulative UI exit rates are generally diminished relative to week 7 for the short qualification period treatments, while they are increased for the long qualification period treatments. The bottom rows of Table 14 indicate the permanence of the treatment impact. For all treatments, the cumulative UI exit rate over the first 31 weeks of the benefit year is significantly greater than for the control group, with the increase averaging 0.7 percent.

The 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; that is, weeks 1-7 for T3 and weeks 1-13 for T6. By the time the maximum entitled duration of benefits in Washington had elapsed, 0.7 percent more treatments than controls had left UI.

Impacts of the Bonus Offer on Population Subgroups

There are two important reasons to examine treatment impacts by population subgroup. One is to provide information to policy makers who may consider targeting a reemployment

[26] The estimates given in Table 13 are not regression adjusted since introducing a control variable for the weekly benefit amount had virtually no effect on estimates of the timing of response to the bonus offer. To simplify the presentation, adjusted hazards are not reported.

bonus program to certain groups like dislocated workers or older UI claimants. Another is to identify any possible biases in the effects--a program that benefits only one gender or certain racial/ethnic groups may not be considered good policy even if it is cost effective. This section reports on treatment impacts for twelve subgroups defined by binary variables for the following six characteristics: gender, age, race, industry, area unemployment, and weekly benefit amount maximum. The dummy variables actually used were: a variable indicating if female, an age variable indicating if aged 45 years or over, a race variable indicating if black, an industry variable indicating if the previous job was in manufacturing, an unemployment variable indicating if the local total unemployment rate was below five percent, and a weekly benefit amount (WBA) variable indicating if the claimant qualified for the maximum WBA.

All subgroup treatment impacts were simultaneously estimated in a single regression model. The specification employed allows the treatment response for each subgroup to be estimated controlling for the influence of other subgroup characteristics. For example, the model allows estimation of treatment impacts associated with being black controlling for the fact that blacks are less likely to be at the maximum weekly benefit amount and less likely to claim benefits in low unemployment rate areas. The subgroup treatment impact estimates reported in Table 15 were computed while also controlling for differences in the level of the weekly benefit amount across claimants.[27] The regression equation estimated is a straight forward generalization of equation (2) above:

$$Y = a + TB + ZC + GD + GTE' + GZF' + ZTH' + u \tag{4}$$

where Y is the outcome measure, either UI compensation, weeks with some compensation or the exhaustion rate, a is the intercept, B, C, D, E, F, *and* H are conformable parameter vectors, T is the matrix of treatment dummies, Z is a matrix of control variables in deviation form, G is the matrix of dummy variables which code for membership in a subgroup, and u is a mean zero normally distributed random error term. Equation (4) specifies a complete one-way interaction model. It allows simultaneous estimation of all subgroup treatment impacts, but imposes linear restrictions on their estimates.[28]

The mean impact estimate across all treatments on three separate outcome measures is given in Table 15. When reviewing these results recall that the overall mean regression adjusted impact estimate across all treatments is a reduction of $63 in compensation and 0.40 weeks in insured unemployment over the benefit year.

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[27] The weekly benefit amount (WBA) was entered linearly and interacted with all subgroup dummy variables. The WBA was the only covariate included in the model.

[28] Treatment impacts for a particular subgroup are computed as the sum of the parameter estimate on the product of the subgroup dummy variable and the treatment indicator plus the sum ofparameter estimates on other subgroup dummies times the treatment indicator times their respective population shares. In each computation, parameter estimates for the complement to the subgroup of interest are omitted.

The subgroups for which there was a statistically significant response to the bonus offer all had a relatively larger reduction in insured unemployment than the complementary subgroup. These subgroups were: males, claimants aged 45 years and over, claimants filing in relatively low unemployment areas, and claimants with weekly benefit amounts below the maximum. Treatment impacts on weeks of benefits received in the benefit year were also were significantly different from the complementary subgroup for males, claimants filing in low unemployment areas, and claimants eligible for less than the maximum weekly benefit amount (WBA).

Claimants whose previous job was in manufacturing had relatively large treatment impacts, and while these impacts were larger than for those from non-manufacturing jobs the impacts were neither statistically significant nor different from each other. Combining the result that older workers respond to the bonus more than younger workers, with the result that workers whose previous job was in manufacturing respond to the bonus more than workers from other industries, suggests that a reemployment bonus may be a good candidate for a targeted program toward dislocated workers.

The estimate that claimants below the maximum WBA respond strongly and significantly more to the bonus than claimants at the maximum WBA is good evidence for the design of the bonus as a multiple of the WBA. In the whole sample, 34 percent of claimants were at the maximum WBA, some in this group had earnings just sufficient to qualify for the maximum WBA, others had earnings well in excess of that required. Between the minimum and maximum, the formula in Washington sets the WBA at approximately one-half of the weekly wage.[29] For those below the maximum WBA the bonus as a multiple of the WBA represents a substantial sum in comparison to their previous earnings experience, while for those with high earnings potential who qualify for the maximum WBA even six times the WBA may not amount to a strong financial incentive. It is true that setting the bonus as a fixed multiple of the WBA unifies treatments by presenting all claimants a bonus equal to the same number of weeks of jobless pay, however this design also has the benefit of making the bonus amount directly related to earnings potential for claimants below the WBA maximum. With a fixed bonus amount the relative incentive effect would be diminished for the 97 percent of eligible claimants who qualify for more than the minimum WBA.

The finding that the bonus response was bigger where the unemployment rate was lower is consistent with the view that the bonus would be most effective where increased search effort has a good chance of improving reemployment prospects. If good job prospects are available an unemployed person receiving compensation may defer job search, so that there may be more room for the bonus to stimulate job search. Using the subgroup treatment impact estimation methodology to isolate the effects of characteristics, we investigated if factors other than low unemployment may have contributed to the strong treatment response in low unemployment areas.

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[29] Between the minimum and maximum, the WBA is 1/25 of the average of earnings in the two quarters with the highest quarterly earnings in the base year.

Enrollment for WREB took place in 21 Job Service Centers (JSCs) throughout Washington; 6 JSCs were in areas with total unemployment rates (TURs) below 5 percent, with 5 of these being in the Seattle metropolitan area. To investigate if the result for low TUR areas was due to something in Seattle other than simply the low unemployment rate, a subgroup dummy variable for the 8 JSCs in the Seattle area was included in the subgroup regression. The result was that neither treatment response in nor out of Seattle was different from zero. Furthermore, inclusion of the Seattle dummy variable had virtually no affect on the estimated treatment response in low TUR areas--indeed the estimated response was slightly larger--suggesting that the treatment response by TUR category is independent of other factors present in the Seattle metropolitan area.

IMPACTS OF THE BONUS OFFER ON OTHER OUTCOMES OF INTEREST

The previous section examined impacts of the bonus offer on insured unemployment. In this section, we examine impacts of the bonus offer on three other outcomes of interest: (1) earnings in the benefit year, (2) the rate of earnings, and (3) employer attachment. Negative effects of a reemployment bonus offer would be indicated if the bonus offer caused wage rates or employer attachment to decrease.

Impacts of the Bonus Offer on Earnings

In this sub-section, we investigate if, and how, the bonus offer affected earnings during the year following claimants' benefit application. Since claimants reduced UI receipt in response to bonus offers it was expected that those offered a bonus would show an increase in post-application earnings. Earnings may not have increased if the reduction in weeks of UI compensation was spent out of the labor force instead of at work, or if reemployment occurred at lower wages.

Our analysis of impacts of the bonus offers on earnings was based on quarterly earnings of claimants recorded in UI wage records. We examined earnings in four calendar quarters beginning with quarter of benefit application. Earnings data for the first quarter partly reflect claimants' experience with pre-UI employers, but random assignment implies that pre-UI earnings during this quarter should not vary significantly across the treatment and control groups. Hence, any significant impact on earnings in the quarter of benefit application should be attributable to the bonus offer.

Tables 16 and 17 report on earnings in the quarter of benefit application and the subsequent three quarters, this information spans a period nearly coinciding with the benefit

year.[30] Table 16 reports the quarterly earnings for the control group and unadjusted estimates of treatment impacts on earnings. On average treatment and control earnings did not differ. The only significant difference over the four quarters was for claimants assigned to treatment six; they experienced higher earnings than the control group.

While controlling for differences in demographic and program characteristics, Table 17 repeats the analysis presented in Table 16. This second table includes at the bottom results from estimating a continuous variables model of the treatment impact on earnings. While earnings are seen to significantly increase in the third quarter after filing for T6, no treatment has a statistically significant impact on earnings over the total of the four quarters. In the continuous variables model the bonus amount had the effect of significantly increasing earnings in the third quarter after filing and the total of the four quarters, but the qualification period length did not significantly affect earnings.[31]

5.5.2 Impacts of the Bonus Offer on Job Quality

Search theory suggests that if job search had been optimal before the bonus offer, then speeding reemployment implies taking jobs that are somehow less than optimal. In this section we examine if the jobs taken by claimants offered a bonus in the Washington experiment are better or worse than jobs they would have accepted without the bonus offer. Jobs may be compared on many characteristics since 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 relevant data available from administrative records is data on total quarterly earnings.

A direct way to assess the impact of treatments on the quality of jobs is to compare earnings immediately before receiving unemployment compensation with earnings immediately after leaving unemployment compensation. Two versions of this comparison are presented in Tables 18 and 19. In Table 18 we compare earnings in the full quarter *before* the quarter in which the claimant filed for benefits with earnings in the first full quarter *after* the quarter in which the claimant terminated UI receipt.[32] The value of earnings in each of these quarters, and

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[30] Quarterly earnings impact estimates reported in Tables 16 and 17 were computed on a sample of 15,479 which is 55 smaller than the full analytic sample. Observations where reported earnings exceeded $100,000 in any of the four quarters were excluded.

[31] Since it was reported in the previous section that the bonus offer is estimated to result in a reduction in weeks of compensation, the absence of an impact on earnings might reflect a reduction in either the hourly wage rate or hours per week of work on the new job.

[32] Earnings impacts were estimated on a sample of 10,099 treatment and control group members, who served a waiting week within 3 weeks of filing for benefits, terminated benefits before exhaustion, and had wages in the UI Wage File in both the quarter before filing and the
quarter after termination of benefits. If no waiting week was served, the claimant was included in the sample if wages were in the UI Wage File for both the quarter before filing and the quarter after filing for benefits.

the change in earnings between these two quarters is presented in Table 18 for each treatment, for each bonus level, for the treatments combined, and for the controls. The change column shows that quarterly earnings declined significantly for every group except for T4 and T6, but the final column in the table shows that no treatment had a significantly greater decline in earnings than the control group. Indeed, for T4 and T6 quarterly earnings declined by less than for the control group. These results indicate that the bonus offer does not induce claimants to accept new jobs which are of lower quality than they would have accepted in the absence of the bonus.[33]

For a variety of reasons it is useful to examine job quality in terms of hourly wages instead of quarterly earnings. Within the sample reported on in Table 18 in addition to having earnings reported in both the quarter before filing and the quarter after termination of benefits, 8,915 treatments and controls had hours reported for the same quarters. For this sample, hourly wages were computed and the results of comparisons similar to those presented in Table 18 for quarterly earnings are reported in Table 19 for hourly wages. The results summarize essentially the same phenomenon as was observed using quarterly earnings. As measured by the hourly wage rate, new jobs accepted by treatment assigned claimants are not of lower quality than they would have accepted if the bonus had not been offered. In fact the mean reemployment wage for treatment assigned claimants was $10.32 per hour while that for control assigned claimants was $10.19. Average pre-claim hourly wage rates for treatment assigned claimants were 30 cents per hour higher than for controls, and some regression toward the mean was observed for both groups upon reemployment.

5.5.3 Impacts of the Bonus Offer on Employer Attachment

An original intent of the UI system was to help maintain employer-employee relationships in times of slack demand. A design element in the experiment that seems to run counter to this intent is the explicit prohibition against paying a bonus to a claimant who returns to his/her previous job.[34]

To examine the question of employer attachment we investigate if a claimant offered a bonus who returned to work before exhausting benefits was more or less likely than a control

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[33] To avoid a potential problem of selection bias, the change in earnings of treatment assigned claimants should be compared to that for a group similar in all respects except being offered a bonus. Since we only examine earnings of people who become reemployed, it might be argued that our usual control group is not the appropriate comparison group. Treatments returned to work at a rate which is slightly higher (65.2%) than controls (64.3%), but not statistically different from controls. The additional treatment assigned claimants returning to work are unlikely to have higher wages, therefore if there is any bias due to additional employment the wage of the experimentals is likely to be biased downward. So that correction for the bias would diminish the estimated decline in quarterly earnings of treatment assigned claimants, so that the methodology presented in the text exaggerates the treatment impact on earnings. Since no treatment impact is detected using this methodology, we can be assured that there was no treatment impact on reemployment earnings.

[34] Bonuses were only paid to participants hired by their previous employer if they began a different job.

group member to return to his/her previous employer.[35] Behavior was examined for the total sample, and for a subsample of claimants explicitly informed by their previous employer to expect recall to their previous job--a status designated standby in UI. 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.

Using the quarterly earnings history from the UI Wage File, 10,060 claimants were identified as having gone back to work prior to exhausting benefits.[36] In this group of treatment and control assigned claimants, 44 percent returned to their separating employer and 56 percent went to work for another employer. As reported in Table 20, for the treatments taken together, the fraction of claimants returning to their previous employer was 2.0 percentage points lower for treatments than for controls. The greatest effect was for bonus offers set at four times the WBA (T2 and T5) where the mean rate of returning to previous employer fell by 3.5 percent. However, this result is not consistent with the estimates for the impact of the mid level bonus on the use of UI. Furthermore, the absence of an effect on return to previous employer for the high WBA multiple treatments make the results suspect, since it was only the high bonus level treatments that induced a statistically significant response on use of UI. Therefore, the results do not provide strong evidence that the bonus affects the probability of returning to the previous primary employer.

Employers are most concerned about the attachment of laid off workers on standby awaiting recall. In the experiment 2,134 claimants entered the UI system on standby, of these 1,824 returned to work before exhausting benefits, with 1,436 returning to their previous primary employer.[37] The evidence given in the lower half of Table 21 strongly suggests that employers interested in retaining laid off workers for recall have nothing to fear from a bonus program. There was no statistically significant affect of the bonus offer to reduce the probability of standby claimants returning to their previous employer. Indeed, the treatment impact estimates are generally positive though statistically insignificant.

Taken together these results indicate that the bonus offer did not decrease the probability of returning to the previous employer for claimants explicitly on standby awaiting recall, although it may have reduced the probability of return to the previous employer for other claimants. A bonus program could induce wider use of UI standby provisions by employers.

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[35] The employer used for the analysis was the primary employer in each quarter. The primary employer is defined as the employer at which the largest share of wages was earned in the quarter as reported in the UI Wage File.

[36] These claimants either served a waiting week within three weeks of filing and terminated their initial spell of unemployment prior to exhaustion and had earnings in either (1) the quarter of benefit termination for claimants who terminated benefits after the quarter of filing, or (2) the quarter after the terminating quarter for claimants who filed and terminated benefits in the same quarter. Claimants who did not serve a waiting week are included if they had earnings in the quarter after the filing quarter.

[37] Standby is a designation made by the previous employer who declares an intention to recall a worker. It results in a fixed duration work search exemption for the claimant.

SUMMARY AND CONCLUSIONS

As part of an effort to find ways to reduce the cost of unemployment insurance (UI), the U.S. Department of Labor asked the Upjohn Institute to design an experiment using cash bonus offers to encourage more rapid return to work by UI beneficiaries. The motivation for this experiment came from two relatively successful previous experiments which tested the bonus offer approach in Illinois and New Jersey. In 1988, after six months of design work, an experiment involving over 12,000 UI claimants in the State of Washington, was undertaken.

The Washington Reemployment Bonus Experiment (WREB), was comprised of six different "treatments" involving offers of bonuses to unemployed workers filing initial claims for UI benefits. The six treatments were represented by all combinations of three different "bonus amounts" calculated as either 2, 4, or 6 times the claimant's weekly benefit amount, and two different "qualification periods," calculated as either 20 or 40 percent of a claimant's maximum entitled duration of benefits. To qualify for a bonus, all claimants were also required to remain fully employed for at least four months. The experiment examined a range of bonus offers in the hope that an optimally cost-effective combination of bonus amount and qualification period could be identified.

The experimental design sought to encourage individuals to either undertake a more active job search, or accept a given job offer more quickly. The desired aggregate result was a reduction in the amount of unemployment. In the experiment, the response of UI claimants to a bonus offer is inferred by measuring the difference in duration of unemployment between the group offered the bonus and the control group which was not offered a bonus. It is the existence of a control group, that has been randomly selected from those eligible to be offered a bonus, which makes the evaluation an experiment.

As expected, the strongest response to WREB bonus offers was exhibited by those made high bonus amount-long qualification period offers. For this treatment there was a statistically significant reduction in weeks of insured unemployment of 0.76 weeks over the benefit year (the 52 weeks from the date of filing for benefits), and an average reduction of $138, or 6.7 percent, in benefit payments over the benefit year. The weakest effect was observed in response to the low bonus amount-short duration offer. On average this group only reduced weeks of insured unemployment by 0.05, an impact that was not statistically different from zero. Overall, the estimated mean impact across the six treatment groups was a statistically significant 0.40 week reduction in insured unemployment over the benefit year, and a reduction in unemployment compensation paid of $63 over the benefit year.

If the offer of a reemployment bonus causes a change in a claimant's job search behavior, then the bonus offer has had an effect. However, when attempting to measure the impact of the bonus offer it is impossible to separate those claimants who changed their job search behavior from those who did not. Indeed even among claimants who collect a bonus, some have not changed their behavior. Nonetheless, the proportion of claimant's offered a bonus who actually

receive one is a measure of participation in the experiment. Among the individual treatments, the rate of bonus receipt ranged from 8.7 percent for the low bonus-short duration offer to 22.0 percent for the high bonus-long duration offer, with an average bonus receipt rate of 14.6 percent. The progressively greater proportion of claimants receiving bonuses as the bonus offer increases is consistent with the finding that the impact on unemployment and UI compensation generally increases with the size of the bonus offer.

The research results reported in this paper went beyond the simple comparison of experimental and control group means. Discrete analysis of the separate parts of the bonus offer indicated a strong response to additional dollars of bonus offer and a modest response to increases in the length of the qualification period. A hazard model was used to examine if reductions in insured unemployment occurred during the qualification period. Only for the long duration-high bonus offer treatment did the model show a statistically significant increase in the proportion of claimants leaving unemployment prior to the end of their qualification period.

As a further aid to policy making, the degree of response to the bonus offer was examined for some important subgroups within the sample. The findings indicated that a participant who was male, or a member of an ethnic/racial group other than black, or had a weekly benefit amount below the maximum, or lived in a low unemployment area (below 5 percent in 1988) was more likely than their counterparts to respond to the bonus offer by reducing the duration of unemployment, other characteristics held constant.

An important question is whether more rapid reemployment by those offered a bonus offer occurs at the expense of job quality. Analysis of claimant reemployment earnings indicated that the bonus offer did not result in reemployment at lower average earnings than would have been accepted if the a reemployment bonus was not offered. There was also concern that a bonus paid only for reemployment in a job different from the previous one would weaken employer attachment. There was no evidence that return to previous employer by persons on standby awaiting recall was less likely for those offered a bonus.

In all, the Washington Reemployment Bonus Experiment was a successful test of the reemployment bonus concept, in that it suffered from none of the problems that could have made the experiment internally invalid. The results suggest that the offer of a bonus does lead to a reduction in the length of unemployment; but that relatively generous bonus offers are needed to significantly change the behavior of persons eligible for unemployment benefits.

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Table 1
Characteristics of Washington State and Enrollment Sites

	Washington	Enrollment Sites
Population (thousands, 1986)	4,132	3,404
Population per square mile (1986)	67	90
Racial mix (percent, 1990)		
Black	3.1	2.7
Hispanic	4.4	6.5
Asian and Pacific Islander	4.3	2.1
Native American, Eskimo, Aleut	1.7	1.1
Unemployment Rate (percent, 1988)	6.2	6.7
Employment growth (%), 1988-89	6.8	
Labor force growth (%), 1988-89	6.8	
Industry Mix (Statewide, percent, 1988)		
Manufacturing	18	21
Wholesale and retail trade	25	23
Finance, insurance, real estate	6	5
Services	23	18
Government	19	2
New claims for UI (1988)	464,715	390,360
Quarterly earnings (1988)	5,087	3,940

Sources: County and City Data Book, 1988, U.S. Department of Commerce; Statistical Abstract of the United States, 1990 and 1991, U.S. Department of Commerce, UI Data Summary, various issues 1988 and 1989, U.S. Department of Labor. Data for enrollment sites on racial mix, industry mix, and quarterly earnings is mean values for treatments and controls in the WREB data base.

Table 2
Characteristics of Control Group Member

Characteristic	Mean
Total Members	3,082
Gender	
Male	60.5%
Female	39.5
Age	
Less than 35	52.2%
35 to 54	39.8
55 and above	8.0
Race	
White, Non-Hispanic	83.3%
Black	4.3
Hispanic	7.0
Other	5.4
Industry	
Manufacturing	23.1%
Non-manufacturing	76.9
Occupation	
White collar	34.2%
Other occupations	65.8
Dislocated Workers	
12 quarters same employer	14.5%
12 quarters same industry	19.8
12 quarters continuous employment	36.2
Weekly benefit amount ($)	150.51
Entitled duration (weeks)	26.9
Base period earnings	15,475
UI Benefits	
Weeks of insured unemployment	14.3
Dollars of benefits drawn	2066.0
Exhaustion rate (%)	23.9
Initial UI spell (weeks)	11.4

Table 3
Washington Reemployment Bonus Treatment Design
Treatment Number and Actual Enrollment
(Designed Enrollment in Parentheses)

		Qualification Period	
		.2 * Entitled Duration + 1 Week	.4 * Entitled Duration + 1 Week
Bonus Amount	2 x WBA	Treatment 1 (WT1) 2,246 (2,250)	Treatment 4 (WT4) 2,387 (2,250)
	4 x WBA	Treatment 2 (WT2) 2,348 (2,250)	Treatment 5 (WT5) 2,353 (2,250)
	6 x WBA	Treatment 3 (WT3) 1,583 (1,500)	Treatment 6 (WT6) 1,535 (1,500)

Table 4
Tests of Randomization
Control and Treatment Group Means
(t-statistic of difference from control group mean in parentheses)

	Control	1	2	3	4	5	6	All
Female (%)	39.5	38.9 (0.42)	39.3 (0.17)	38.7 (0.55)	38.5 (0.74)	39.8 (0.22)	38.0 (0.99)	38.9 (0.58)
Age less than 35 (%)	52.2	53.3 (0.84)	52.8 (0.43)	51.9 (0.20)	52.2 (0.02)	52.4 (0.14)	53.8 (1.01)	52.7 (0.51)
Age 55 and above (%)	8.0	7.7 (0.48)	7.8 (0.36)	8.0 (0.01)	8.2 (0.21)	8.4 (0.49)	6.8 (1.47)	7.8 (0.31)
Black (%)	4.3	5.2 (1.48)	4.7 (0.65)	4.0 (0.43)	4.7 (0.67)	4.1 (0.42)	4.2 (0.23)	4.5 (0.48)
Hispanic (%)	7.0	6.8 (0.40)	6.4 (0.96)	6.8 (0.28)	7.0 (0.00)	6.8 (0.42)	5.3** (2.18)	6.6 (0.92)
Other non-whites (%)	5.4	4.9 (0.76)	4.6 (1.27)	4.7 (0.92)	4.9 (0.84)	5.1 (0.36)	4.4 (1.47)	4.8 (1.29)
Manufacturing (%)	23.1	22.0 (0.92)	22.8 (0.28)	21.9 (0.91)	22.8 (0.24)	22.3 (0.69)	22.5 (0.43)	22.4 (0.80)
Weekly Benefit ($)	150.5	150.5 (0.02)	152.1 (1.12)	152.8 (1.40)	153.5** (2.11)	152.8 (1.58)	154.0** (2.12)	152.6* (1.93)
Entitlement (weeks)	26.9	26.7 (1.22)	27.0 (0.91)	26.8 (0.75)	26.9 (0.62)	26.8 (0.06)	27.0 (1.16)	26.9 (0.15)
Base earnings ($)	15,475	15,486 (0.03)	15,860 (1.22)	15,537 (0.17)	15,872 (1.27)	16,073* (1.90)	16,148* (1.88)	15,830 (1.54)
Maximum WBA (%)	33.0	32.7 (0.18)	33.5 (0.43)	33.5 (0.40)	34.8 (1.40)	34.0 (0.83)	36.0** (2.03)	34.0 (1.11)
Search exempt (%)	22.5	21.7 (0.66)	21.8 (0.64)	20.9 (1.23)	22.3 (0.18)	20.7 (1.58)	22.7 (0.14)	21.7 (0.99)
White collar (%)	34.2	33.3 (0.70)	35.6 (1.05)	34.8 (0.39)	35.3 (0.83)	36.7* (1.87)	35.6 (0.94)	35.2 (1.05)
Years of education	12.3	12.3 (0.34)	12.4* (1.80)	12.3 (0.43)	12.3 (0.34)	12.4 (1.00)	12.4 (1.61)	12.4 (1.28)

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

Table 5
Direct Measures of Participation in the WREB Experiment

	T1	T2	T3	T4	T5	T6	All T's
Total Treatments	2,246	2,348	1,583	2,387	2,353	1,535	12,452
Submitted Valid NOH	249	389	295	413	500	395	2,241
Percent of Total Treatments	11.1	16.6	18.6	17.3	21.2	25.7	18.0
Collected Bonus	196	292	237	332	419	337	1,813
Percent of Total Treatments	8.7	12.4	15.0	13.9	17.8	22.0	14.6

Table 6
Indirect Measures of Participation in the WREB Experiment

	T1	T2	T3	T4	T5	T6	All Ts
Total Treatments	2,246	2,348	1,583	2,387	2,353	1,535	12,452
Terminated Benefits by Qualification Deadline	1,051	1,128	790	1,502	1,436	1,006	6,913
Percent of Total Treatments	46.8	48.0	49.9	62.9	61.0	65.6	55.5
Terminated Benefits by Qualification Deadline and No UI in 4-Month Period	686	762	559	1,013	1,007	703	4,730
Percent of Total Treatments	30.5	32.5	35.3	42.4	42.8	45.8	38.0
Terminated Benefits by Qualification Deadline and No UI in 4-Month Period and Not Recalled or Union Placed	433	511	384	637	673	490	3,128
Percent of Total Treatments	19.3	21.8	24.3	26.7	28.6	31.9	25.1
Collected a Bonus	190	286	229	323	401	316	1,745
Collected a Bonus as a Percent of Terminated Benefits by Qualification Deadline and No UI in 4-Month Period and Not Recalled or Union Placed	43.9	56.0	59.6	50.7	59.6	64.5	55.8

Table 7
Linear Estimates of the Probability of Cashing a Bonus
Controlling for Demographic and Program Characteristics
(Standard errors in parentheses)

Treatment Model		Price Model		Duration Model		Continuous Model	
Variable	Estimate	Variable	Estimate	Variable	Estimate	Variable	Estimate
T1	0.452**## (0.0220)	T1,T4	0.488**## (0.0140)	T1,T2,T3	0.534**## (0.0126)	BONUSAMT	0.000199** (0.000034)
T2	0.552** (0.0202)	T2,T5	0.576**# (0.0133)	T4,T5,T6	0.575**## (0.0108)	QUAL	0.0062** (0.0030)
T3	0.602**## (0.0233)	T3,T6	0.619**## (0.0155)				
T4	0.512**## (0.0181)						
T5	0.595**## (0.0176)						
T6	0.631**## (0.0207)						

* Coefficient significant at the 90 percent level for a two-tail test.
** Coefficient significant at the 95 percent level for a two-tail test.
\# Significantly different from the mean take up rate (.558) at the 90 percent confidence level for a two-tail test.
Significantly different from the mean take up rate (.558) at the 95 percent confidence level for a two-tail test.

Table 8
Unadjusted Differences Between Experimental and Control Group Means
(standard errors in parentheses)

	Treatment group difference from control											
Compensation Received:	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	T1,2,3	T4,5,6	All Ts
Initial spell	34.80	20.33	-49.99	-28.78	41.18	-105.64*	2.04	30.77	-77.39	7.57	-21.35	-7.00
	(49.69)	(49.06)	(55.38)	(48.84)	(49.03)	(55.95)	(41.63)	(41.51)	(45.49)	(39.51)	(39.40)	(36.04)
Benefit year	29.83	5.08	-68.87	-58.21	12.46	-86.30	-15.53	8.78	-77.45	-4.87	-38.58	-21.86
	(51.87)	(51.21)	(57.81)	(50.98)	(51.18)	(58.41)	(43.46)	(43.33)	(47.49)	(41.23)	(41.13)	(37.62)
Weeks of insured unemployment:												
Initial spell	0.10	0.00	-0.27	-0.28	0.22	-0.82**	-0.09	0.11	-0.54**	-0.03	-0.22	-0.13
	(0.297)	(0.293)	(0.331)	(0.292)	(0.293)	(0.334)	(0.249)	(0.248)	(0.272)	(0.236)	(0.235)	(0.215)
Benefit year	-0.06	-0.17	-0.61*	-0.50*	-0.13	-0.73**	-0.29	-0.15	-0.67**	-0.24	-0.42*	-0.33
	(0.302)	(0.299)	(0.337)	(0.297)	(0.298)	(0.340)	(0.253)	(0.253)	(0.277)	(0.240)	(0.240)	(0.219)
Proportion of claimants who exhausted benefits	0.011	-0.004	-0.012	-0.030**	-0.014	-0.023*	-0.010	-0.009	-0.018	-0.00	-0.022**	-0.01
	(0.012)	(0.012)	(0.013)	(0.011)	(0.012)	(0.013)	(0.010)	(0.010)	(0.011)	(0.009)	(0.009)	(0.009)

*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 9
Adjusted Differences Between Experimental and Control Group Means
Controlling for Demographic and Program Characteristics
(standard errors in parentheses)

	Treatment group difference from control											
Compensation Received:	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	T1,2,3	T4,5,6	All Ts
Initial spell	22.59	-15.15	-95.71*	-75.67*	-19.04	-138.37**	-28.04	-17.09	-116.70**	-22.09	-69.80**	-46.11
	(44.52)	(43.96)	(49.62)	(43.75)	(43.94)	(50.16)	(37.31)	(37.21)	(40.77)	(35.40)	(35.32)	(32.30)
Benefit year	21.64	-28.76	-117.08**	-111.95**	-44.02	-135.82**	-47.19	-36.41	-126.32**	-33.10	-92.36**	-62.94*
	(45.95)	(45.36)	(51.21)	(45.15)	(45.35)	(51.77)	(38.51)	(38.40)	(42.08)	(36.53)	(36.44)	(33.34)
Weeks of insured unemployment:												
Initial spell	0.06	-0.11	-0.42	-0.39	0.01	-0.81**	-0.17	-0.05	-0.61**	-0.13	-0.34	-0.24
	(0.282)	(0.279)	(0.315)	(0.277)	(0.279)	(0.318)	(0.237)	(0.236)	(0.258)	(0.224)	(0.224)	(0.205)
Benefit year	-0.05	-0.22	-0.71**	-0.58**	-0.27	-0.76**	-0.32	-0.24	-0.74**	-0.28	-0.51**	-0.40*
	(0.293)	(0.270)	(0.327)	(0.288)	(0.290)	(0.331)	(0.246)	(0.245)	(0.269)	(0.233)	(0.233)	(0.213)
Proportion of claimants who exhausted benefits	0.008	-0.005	-0.017	-0.032**	-0.019*	-0.020	-0.013	-0.012	-0.019*	-0.003	-0.024**	-0.014*
	(0.011)	(0.011)	(0.013)	(0.011)	(0.011)	(0.013)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.008)

*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 10
Adjusted Differences Between Experimental and Control Group Means
Controlling for All Control Differences and Censoring in the Observed Outcome
(Standard errors in parentheses)

	Treatment group difference from control											
Compensation received:	T1	T2	T3	T4	T5	T6	T1,4	T2,5	T3,6	T1,2,3	T4,5,6	All Ts
Initial spell parameter[1]	28.16	-8.34	-92.48	-91.65*	-23.55	-158.57**	-33.72	-15.94	-125.13**	-16.64	-82.63**	-49.94
	(52.44)	(51.76)	(58.44)	(51.44)	(51.72)	(58.92)	(43.91)	(43.80)	(47.97)	(41.68)	(41.55)	(38.02)
Effect estimate	23.00	-6.81	-75.55	-74.88*	-19.24	-129.55**	-27.55	-13.02	-102.22**	-13.59	-67.50	-40.79
Benefit year parameter[1]	33.52	-29.80	-133.89**	-158.09**	-70.19	-163.81**	-65.83	-50.06	-148.68**	-33.60	-126.69**	-80.66*
	(58.38)	(57.56)	(64.95)	(57.13)	(57.48)	(65.51)	(48.84)	(48.71)	(53.34)	(46.37)	(46.20)	(42.30)
Effect estimate	29.29	-26.04	-117.02**	-138.17**	-61.34	-143.17**	-57.53	-43.75	-129.94**	-29.37	-110.72**	-70.49*
Weeks of insured unemployment:												
Initial spell parameter[1]	0.05	-0.12	-0.41	-0.48	-0.04	-0.90**	-0.22	-0.08	-0.65**	-0.13	-0.42	-0.28
	(0.322)	(0.318)	(0.359)	(0.316)	(0.318)	(0.363)	(0.270)	(0.269)	(0.295)	(0.256)	(0.256)	(0.23)
Effect estimate	0.04	-0.10	-0.35	-0.41	-0.04	-0.77**	-0.19	-0.07	-0.55**	-0.11	-0.36	-0.24
Benefit year parameter[1]	0.00	-0.24	-0.76*	-0.83**	-0.38	-0.86**	-0.43	-0.31	-0.81**	-0.29	-0.67**	-0.48*
	(0.350)	(0.345)	(0.390)	(0.343)	(0.345)	(0.394)	(0.293)	(0.292)	(0.320)	(0.278)	(0.277)	(0.254)
Effect estimate	0.00	-0.22	-0.69*	-0.75**	-0.34	-0.77**	-0.39	-0.28	-0.73**	-0.26	-0.60**	-0.43*
Exhaustion rate paramete[2]	0.024	-0.019	-0.065	-0.109**	-0.062	-0.072	-0.043	-0.041	-0.068*	-0.014	-0.082**	-0.048*
	(0.039)	(0.039)	(0.044)	(0.039)	(0.039)	(0.045)	(0.033)	(0.033)	(0.036)	(0.031)	(0.031)	(0.029)
Effect estimate	0.007	-0.005	-0.019	-0.032**	-0.018	-0.021	-0.013	-0.012	-0.020	-0.004	-0.024**	-0.014*

[1] For this outcome a Tobit model was used.
[2] For this outcome a Probit model was used.
* Statistically significant at the 90 percent confidence level using a Chi Square test.
** Statistically significant at the 95 percent confidence level using a Chi Square test.

Table 11
Price and Duration Effects Using a Discrete Variable Model
Controlling for Demographic and Program Characteristics
(Standard errors in parentheses)

	Price Effect	Duration Effect
Compensation Received:	Parameter Estimate	Parameter Estimate
Initial spell	-94.2**	-47.7*
	(33.2)	(28.8)
Benefit Year	-84.6**	-59.3**
	(34.3)	(29.7)
Weeks of insured unemployment:		
Initial spell	-0.51**	-0.22
	(0.21)	(0.18)
Benefit year	-0.45**	-0.22
	(0.22)	(0.19)
Proportion of claimants who exhausted benefits	-0.006	-0.210**
	(0.008)	(0.007)

Table 12
Price and Duration Effects Using a Continuous Variable Model
Controlling for Demographic and Program Characteristics
(Standard errors in parentheses)

	Price Effect		Duration Effect		
Compensation Received:	Parameter Estimate	Effect at Mean	Parameter Estimate	Effect at Mean	Total at Mean
Initial spell	-0.0572 (0.0470)	-32.85	-5.327 (3.698)	-44.75	-77.60
Benefit Year	-0.0371 (0.0485)	-21.32	-7.951** (3.816)	-66.79	-88.11
Weeks of insured unemployment:					
Initial spell	-0.00040 (0.00030)	-0.23	-0.0184 (0.0234)	-0.16	-0.38
Benefit year	-0.00043 (0.00031)	-0.25	-0.0233 (0.0244)	-0.20	-0.44
Proportion of claimants who exhausted benefits	-0.0000056 (0.000012)	-0.003	-0.0021** (0.00094)	-0.017	-0.021

Table 13
Unadjusted 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	49.2	-0.6	0.7	2.4	0.8	-2.1	3.4**	0.5
		(1.4)	(1.4)	(1.5)	(1.4)	(1.4)	(1.6)	(1.0)
8-13	29.1	-0.2	-2.5	-2.0	2.5	2.8	4.4**	0.8
		(1.8)	(1.8)	(2.0)	(1.8)	(1.8)	(2.1)	(1.3)
14-31	87.9	0.6	3.0**	2.7*	2.1	0.9	0.9	1.7
		(1.4)	(1.4)	(1.6)	(1.4)	(1.4)	(1.7)	(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.

Table 14
Unadjusted 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	49.2	-0.6	0.7	2.4	0.8	-2.1	3.4**	0.5
		(1.4)	(1.4)	(1.5)	(1.4)	(1.4)	(1.6)	(1.0)
13	64.0	-0.5	-0.8	0.7	1.8	-0.0	4.4**	0.7
		(1.3)	(1.3)	(1.5)	(1.3)	(1.3)	(1.5)	(1.0)
31	95.7	0.1	1.0*	1.0*	0.9*	0.3	0.6	0.7**
		(0.5)	(0.5)	(0.6)	(0.5)	(0.5)	(0.6)	(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.

Table 15
Impacts of the Treatments on UI Receipt by Subgroup
(Standard errors in parentheses)

Subgroup	Dollars of Benefits Received in Benefit Year	Weeks of Benefits Received in Benefit Year	Exhaustion Rate	Sample Size
Males	-94.3**	-0.67**	-0.011	9471
	(43.6)	(0.28)	(0.011)	
Females	15.7	0.10#	-0.019	6063
	(55.3)	(0.35)	(0.014)	
Age 45 years and over	-135.4*	-0.60	-0.017	3332
	(71.8)	(0.46)	(0.017)	
Age less than 45 years	-28.4	-0.31	-0.013	12202
	(37.4)	(0.24)	(0.010)	
Non-black	-55.4	-0.35	-0.014*	14839
	(33.7)	(0.21)	(0.008)	
Black	35.9	-0.71	-0.015	695
	(159.8)	(1.02)	(0.040)	
Manufacturing	-98.4	-0.65	0.001	3505
	(70.6)	(0.45)	(0.022)	
Non-manufacturing	-37.7	-0.29	-0.018	12029
	(37.9)	(0.24)	(0.010)	
Low Unemployment	-195.3**	-0.96**	-0.048**	5328
	(58.2)	(0.38)	(0.014)	
High Unemployment	23.8##	-0.06#	0.004##	10206
	(41.3)	(0.25)	(0.011)	
Below maximum WBA	-143.8**	-1.03**	-0.020	10282
	(51.0)	(0.33)	(0.013)	
Maximum WBA	129.6##	0.93*##	-0.003	5252
	(82.5)	(0.53)	(0.020)	

* Statistically significant at the 90 percent confidence level for a two-tail test.
** Statistically significant at the 95 percent confidence level for a two-tail test.
\# Statistically different from the complementary subgroup at the 90 percent confidence level for a two-tail test.
Statistically different from the complementary subgroup at the 95 percent confidence level for a two-tail test.

Table 16
Unadjusted Impacts of the Treatments on Earnings
(Standard errors in parentheses)

Treatment	Period of Observation: Quarter of Benefit Application	Quarter 1	Quarter 2	Quarter 3	Total of the Four Quarters
T1	8	-232**	-69	-3	-297
	(106)	(103)	(103)	(104)	(313)
T2	25	-78	-112	139	-25
	(104)	(102)	(101)	(102)	(309)
T3	-21	8	-20	185	152
	(118)	(115)	(114)	(116)	(348)
T4	-4	39	-82	54	8
	(104)	(101)	(101)	(102)	(307)
T5	8	-83	-38	60	-52
	(104)	(102)	(101)	(102)	(308)
T6	35	145	262**	295**	736**
	(119)	(116)	(116)	(117)	(352)
T1,4	2	-92	-76	26	-140
	(88)	(87)	(86)	(87)	(262)
T2,5	17	-80	-75	99	-39
	(88)	(86)	(86)	(87)	(261)
T3,6	7	75	119	239**	440
	(97)	(95)	(94)	(95)	(286)
All Ts	9	-46	-27	107	44
	(77)	(75)	(74)	(75)	(227)
Control Mean	$3,059	$2,614	$3,120	$3,271	$12,063

Note: Observations with earnings greater than $100,000 in any quarter are excluded. Quarters 1, 2, and 3 are the first, second, and third full calendar quarters after benefit application.

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

Table 17
Adjusted Impacts of the Treatments on Earnings
(Standard errors in parentheses)

Treatment	Quarter of Benefit Application	Period of Observation: Quarter 1	Quarter 2	Quarter 3	Total of the Four Quarters
T1	18	-212**	-56	9	-241
	(93)	(95)	(93)	(93)	(258)
T2	-26	-92	-132	109	-142
	(92)	(94)	(92)	(92)	(254)
T3	-35	22	-17	181*	151
	(103)	(106)	(104)	(104)	(287)
T4	-74	3	-133	-2	-206
	(91)	(93)	(92)	(92)	(254)
T5	-65	-83	-65	34	-180
	(91)	(94)	(92)	(92)	(254)
T6	-78	58	165	196*	340
	(104)	(107)	(105)	(105)	(290)
T1,4	-29	-102	-96	3	-223
	(78)	(80)	(78)	(78)	(216)
T2,5	-45	-87	-99	71	-161
	(77)	(79)	(78)	(78)	(215)
T3,6	-56	40	72	188**	244
	(85)	(87)	(85)	(85)	(236)
All Ts	-42	-61	-55	75	-83
	(67)	(69)	(68)	(68)	(187)
Bonus Amount	-0.000	0.047	0.122	0.319**	0.487*
	(0.098)	(0.100)	(0.098)	(0.098)	(0.272)
Effect at Mean	-00.02	27.03	70.15	183.43	280.03
Qualification Period Length	8.16	-2.93	-7.57	-11.40	-30.06
	(7.70)	(7.89)	(7.75)	(7.75)	(21.42)
Effect at Mean	68.54	-24.61	-63.59	-95.76	-252.50

Note: Observations with earnings greater than $100,000 in any quarter are excluded. Quarters 1, 2, and 3 are the first, second, and third full calendar quarters after benefit application.

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

Table 18
Quarterly Wages Before Filing and After Reemployment
(Standard errors in parentheses)

Treatment Group	N	Before Filing	After Reemployment	Change	Treatment vs. Control Difference in Change
T1	1,388	4,656	4,395	-262**	-89
		(101)	(104)	(98)	(140)
T2	1,432	4,779	4,434	-344**	-172
		(122)	(101)	(130)	(138)
T3	999	4,574	4,312	-262**	-89
		(95)	(108)	(101)	(155)
T4	1,519	4,569	4,428	-141	32
		(82)	(101)	(95)	(136)
T5	1,493	4,567	4,364	-203**	-30
		(90)	(96)	(95)	(137)
T6	1,007	4,587	4,508	-80	93
		(115)	(135)	(102)	(154)
T1,4	2,907	4,611	4,412	-199**	-26
		(65)	(72)	(68)	(116)
T2,5	2,925	4,671	4,399	-272**	-99
		(75)	(70)	(80)	(116)
T3,6	2,006	4,580	4,410	-170**	3
		(75)	(86)	(72)	(126)
All Ts	7,838	4,625	4,407	-219**	-46
		(42)	(43)	(43)	(101)
Control	1,930	4,575	4,402	-173**	
		(86)	(97)	(102)	

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

Table 19
Hourly Wage Rates Before Filing and After Reemployment
(Standard errors in parentheses)

Treatment Group	N	Before Filing	After Reemployment	Change	Treatment vs. Control Difference in change
T1	1,230	10.37	10.71	0.35*	0.15
		(0.18)	(0.21)	(0.20)	(0.23)
T2	1,259	10.26	10.22	-0.04	-0.24
		(0.19)	(0.17)	(0.18)	(0.23)
T3	881	10.25	10.16	-0.10	-0.29
		(0.21)	(0.21)	(0.20)	(0.26)
T4	1,331	10.24	10.24	0.00	-0.19
		(0.18)	(0.17)	(0.18)	(0.22)
T5	1,317	10.19	10.27	0.08	-0.12
		(0.17)	(0.17)	(0.16)	(0.23)
T6	898	10.48	10.25	-0.23	-0.43*
		(0.23)	(0.20)	(0.19)	(0.25)
T1,4	2,561	10.30	10.47	0.17	-0.03
		(0.12)	(0.14)	(0.13)	(0.19)
T2,5	2,576	10.22	10.24	0.02	-0.18
		(0.13)	(0.12)	(0.12)	(0.19)
T3,6	1,779	10.37	10.20	-0.17	-0.36*
		(0.16)	(0.15)	(0.14)	(0.21)
All Ts	6,916	10.29	10.32	0.03	-0.17
		(0.08)	(0.08)	(0.08)	(0.17)
Controls	1,717	9.99	10.19	0.20	
		(0.14)	(0.15)	(0.13)	

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

Table 20
Treatment Impacts on the Probability of Return to Previous Employer for All Claimants Who Became Reemployed (n = 10,060)
(Standard errors in parentheses)

Variable	Unadjusted Estimate	Adjusted Estimate
T1	-0.028	-0.017
	(0.017)	(0.016)
T2	-0.033*	-0.025
	(0.017)	(0.016)
T3	0.005	0.014
	(0.019)	(0.018)
T4	0.004	0.013
	(0.017)	(0.015)
T5	-0.036**	-0.023
	(0.017)	(0.016)
T6	-0.027	-0.020
	(0.019)	(0.018)
T1,4	-0.011	-0.002
	(0.014)	(0.013)
T2,5	-0.035**	-0.024*
	(0.014)	(0.013)
T3,6	-0.011	-0.003
	(0.016)	(0.014)
All Ts	-0.020	-0.010
	(0.012)	(0.011)
Control mean	0.455	0.421

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

Table 21
Treatment Impacts on the Probability of Return to Previous Employer for All Claimants Who Became Reemployed and Were on Standby (n = 1,824)
(Standard errors in parentheses)

Variable	Unadjusted Estimate	Adjusted Estimate
T1	-0.020	-0.010
	(0.032)	(0.032)
T2	0.032	0.033
	(0.033)	(0.032)
T3	0.062*	0.051
	(0.036)	(0.036)
T4	0.035	0.021
	(0.032)	(0.031)
T5	-0.018	-0.018
	(0.033)	(0.033)
T6	0.011	0.011
	(0.038)	(0.037)
T1,4	0.009	0.006
	(0.027)	(0.027)
T2,5	0.007	0.009
	(0.028)	(0.027)
T3,6	0.038	0.032
	(0.030)	(0.030)
All Ts	-0.015	0.014
	(0.024)	(0.023)
Control mean	0.775	0.771

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