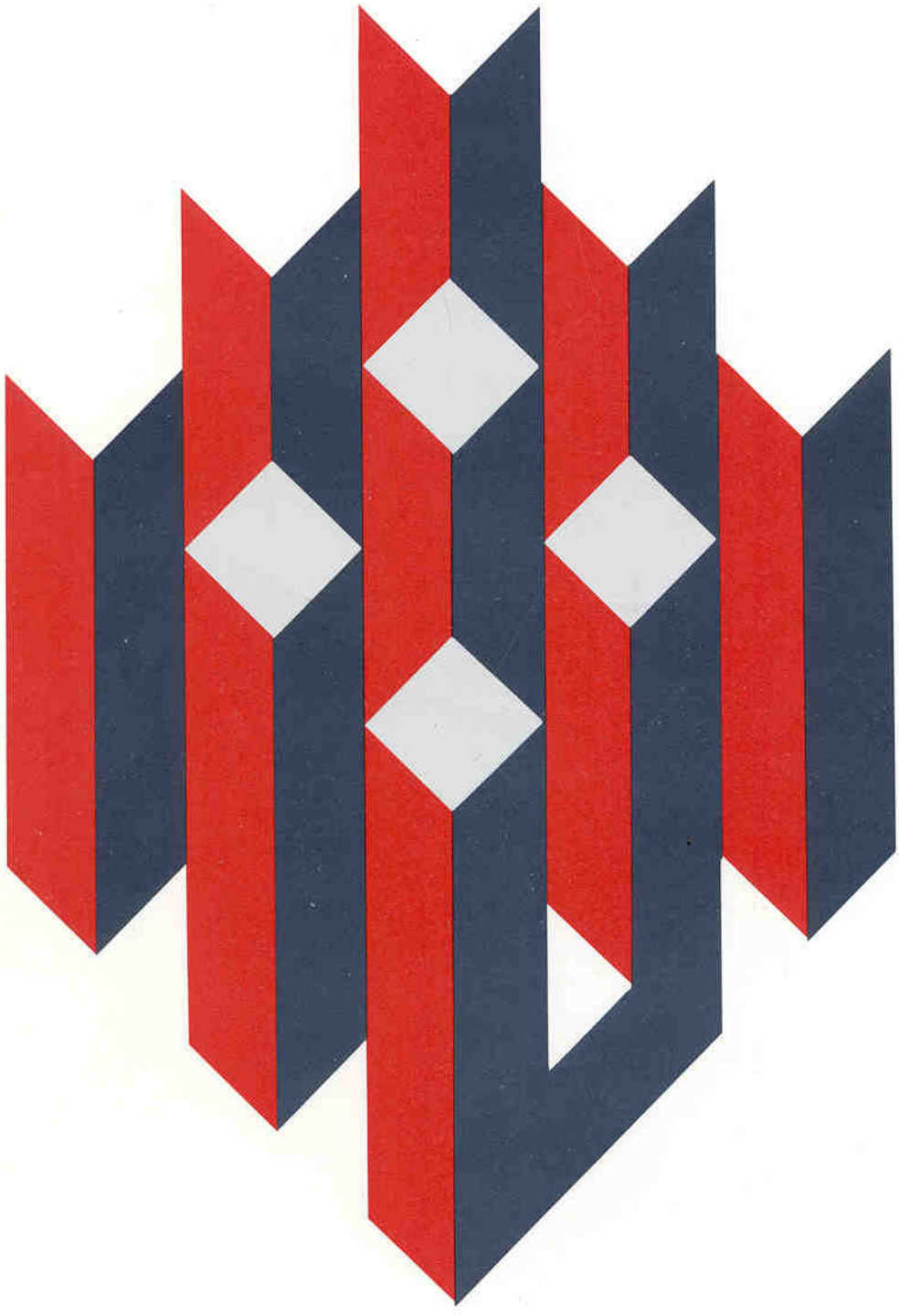
Evaluation of the Impacts of the Washington Alternative Work Search Experiment

Unemployment Insurance
Occasional Paper 91-4

U.S. Department of Labor
Employment and Training Administration



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U.S. Department of Labor
Lynn Martin, Secretary

Employment and Training Administration
Roberts T. Jones
Assistant Secretary of Labor

Unemployment Insurance Service
Mary Ann Wyrsch, Director

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Evaluation of the Impacts of the Washington Alternative Work Search Experiment

January, 1991

Prepared by:

Terry R. Johnson
Daniel H. Klepinger
Battelle Human Affairs
Research Centers

EVALUATION OF THE IMPACTS OF THE WASHINGTON ALTERNATIVE WORK SEARCH EXPERIMENT

Final Report

January 1991

Prepared by:

Terry R. Johnson
Daniel H. Klepinger
Battelle Human Affairs Research Centers
4000 N.E. 41st Street
Seattle, Washington 98105

Prepared for:

Washington State Employment Security Department
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TABLE OF CONTENTS

TABLE OF TABLES, FIGURES AND EXHIBITS

TABLES

APPENDIX TABLES

EXHIBIT

FIGURES

ACKNOWLEDGMENTS

The design, implementation and evaluation of the Washington Alternative Work Search Experiment was a cooperative effort, and involved individuals from several organizations. This includes individuals from the Washington State Employment Security Department (ESD), the W.E. Upjohn Institute for Employment Research and the Battelle Human Affairs Research Centers. In addition, valuable financial support was provided by the U.S. Department of Labor.

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Terry R. Johnson
Project Director

ABSTRACT

This report describes findings from an experimental evaluation of the effectiveness of alternative work search policies in the Unemployment Insurance (UI) program. The work search experiment was conducted in Tacoma, Washington and tested four work search approaches that ranged in philosophy from an "exception-reporting" approach with no specific work search directives or monitoring to an approach that involved intensive reemployment assistance early in the unemployment spell. Approximately 10,000 new UI claimants were randomly assigned to one of the four treatment groups during the July 1986 to August 1987 enrollment period. The results indicate that different work search policies have different and important consequences for the UI Trust Fund. For example, relative to the standard work search policy, we find that the more intensive reemployment services treatment reduces UI payments on average by about one-half of a week or about $70 per claimant. This reduction is considerably larger than the increased administrative costs associated with this treatment. It appears that the impacts of this treatment in reducing the UI spell are primarily due to raising the costs of remaining on UI, rather than enhanced job search abilities. There is, however, no evidence that the relatively rapid reemployment of claimants in this group occurs at the cost of lower earnings or hourly wage rates. We also find that the exception-reporting approach significantly increases UI outlays relative to the standard work search approach by approximately 3.3 weeks and $265 per claimant. Given that the costs of monitoring work search activities are relatively modest, these results taken together indicate that it would be prudent for states to maintain an active work search policy.

I BACKGROUND

The Unemployment Insurance (UI) system was designed to provide temporary income support to involuntarily unemployed individuals while they search for work. It is the responsibility of each State Employment Security Agency (SESA) to administer the UI program and pay benefits to eligible claimants in a timely manner. Each SESA is also responsible for implementing a work search policy that monitors the work-search efforts of claimants and provides job-search assistance. Different SESA's follow very different work search policies, which in turn affect claimants' duration of unemployment and the UI Trust Fund balance.

The traditional work search policy followed in most States requires that individuals receiving UI benefits contact at least three new employers each week in search of full-time work in their customary occupation.[1] Although a policy that treats each claimant similarly has certain advantages from an administrative standpoint, it does not take into account the unique circumstances of certain claimant groups or differences in customary local hiring practices or labor market conditions. In addition, it does not include a provision for increased work search requirements --either in terms of increased number of contacts or required employer contacts in a secondary occupation -- as the period of unemployment lengthens.

In recognition of the limitations of the traditional approach to work search, in the mid 1980s the Washington State Employment Security Department (ESD) developed a new policy that tailored the work search requirements to the characteristics of specific claimant groups. At the same time that this new policy was being designed, the W. E. Upjohn Institute for Employment Research was soliciting proposals from organizations interested in

[1]Claimants who are employer-attached or who are union members and obtain jobs through a union hiring hall are typically excluded from such a work-search requirement.

conducting a study to examine the effectiveness of alternative work search policies. Because of their interest in better understanding the impacts of alternative work search policies, ESD submitted a proposal to Upjohn to conduct an experimental evaluation of different work search approaches, including their new work search policy. Upjohn selected the ESD proposal for funding and additional funds were obtained from the U.S. Department of Labor to support a contract with Battelle Human Affairs Research Centers for assistance in designing, monitoring and evaluating the work search experiment.

The final report of the Washington Alternative Work Search Experiment is a joint effort involving the Upjohn Institute, ESD and Battelle. In the pages that follow, we provide Battelle's contribution to the final report, which focuses primarily on describing the impacts of the different work search policies tested on key outcome measures. Upjohn and ESD are preparing chapters on other issues that will provide a context for understanding the impact results, including a description of previous studies of UI work search policies, and a detailed description of the administrative processes and costs associated with the alternative work search policies tested. The joint final report will be published as a monograph by the Upjohn Institute.

The remainder of this report is organized as follows. In Section II, we describe the design of the work search experiment, including the alternative work search approaches that were tested, how claimants were assigned to each approach, the sample design, and data sources. In Section III, we describe the population of claimants included in the experiment, assess the comparability of the claimants across treatment groups, and present information on program participation and the services received. In Section IV, we present the impact results on several UI and employment and earnings outcome measures overall and for selected subgroups. In Section V, we conclude by briefly comparing our impact results with the findings from other studies and present some simple cost information to provide a rough indication of the cost-effectiveness of the different treatments.

II THE DESIGN OF THE WORK SEARCH EXPERIMENT

The Washington Alternative Work Search Experiment was undertaken to provide reliable information on the cost-effectiveness of various work-search policies for UI claimants. To meet this goal, the demonstration was designed as a classic experiment in which claimants were randomly assigned to one of four different treatment groups, each representing a different work search policy. The services were delivered to claimants in each of the treatment groups by local Job Service (ES and UI) staff. The experiment was implemented in the Tacoma Job Service Center (JSC) in July, 1986 and over 10,000 new UI claimants were assigned to one of the treatment groups during the one-year enrollment period.

In this section we describe the design of the work search experiment. We begin with an overview of the four work-search treatments tested. We then describe the specific elements of each of the treatments. The next subsection briefly describes the analytic design, including site selection and sample design issues, and the random assignment procedures. The section concludes with a description of the data sources for the evaluation.

Overview of Treatment Design

The experiment tested four approaches to work search policy. The four treatments ranged in philosophy from a completely "hands-off" approach that relied on claimants to follow the honor system and report relevant changes in circumstances that affect UI benefits to a "hands-on" approach that involved intensive reemployment assistance early in the unemployment spell. The specific work-search treatments tested and their underlying philosophies can be summarized as follows:

A. Exception Reporting is based on the honor system, and UI payments were automatically sent to claimants in an amount equal to the weekly benefit amount unless the claimant called the JSC to report changes in circumstances (e.g., earnings, returned to work) that affected the benefit amount. Although

claimants were initially instructed to make an active search for work, no work-search services were provided during the claim and claimants were not required to report work-search contacts. Because this treatment eliminated the normal continued-claims process and UI warrant checks were generated automatically, this treatment was also referred to as the "auto-warrant" treatment.

B. Standard Work Search, which has traditionally been used in most states, requires all claimants -- regardless of their job prospects and experience -- to make (at least) three employer contacts per week. A fixed minimum number of weekly contacts has been used because it is easier from a monitoring and administrative perspective. This is particularly important given the limited resources usually available for monitoring continuing eligibility for UI. Although some states choose not to use their resources to review the work-search efforts of claimants later in the claim -- and consider broadening or increasing claimants' work-search requirements -- the experiment followed the more typical approach of conducting eligibility review interviews (ERIs) about 13 - 15 weeks after filing the claim. Specifically, a group ERI that lasted about 1 hour was held, after which individuals were scheduled for brief individual follow-up ERIs that lasted about 15 minutes and focused on eligibility issues. Because the standard work search treatment is the traditional approach used in most states, we refer to this as the control group.

C. The New Work Search Policy is the approach developed by Washington State ESD that recognizes that different individuals need different types of assistance and tailors claimants' work-search requirements to their individual circumstances and the conditions of the local labor market.[2] The philosophy of this approach is to give claimants a reasonable time to find work and then to systematically increase work-search requirements and provide services to assist claimants to return to work. Work-search requirements were tailored for certain claimant groups, including targeted claimants, union members, claimants on standby (i.e., on temporary layoff), partially unemployed workers, permanent part-time workers, and seasonal workers during the off-season. Individuals in these groups who were still unemployed after a specific number of weeks into the claim were instructed to come to the local office for a group ERI, with in-depth individual follow-up ERIs that focused on employability development planning rather than UI eligibility issues.[3] Additional details on the timing of the ERI for these groups is provided below.

D. Intensive Services integrate work-search-technique assistance early in the unemployment spell with the employability development focus of the New Work Search Policy treatment. The philosophy of this approach is based on the view that many claimants have sufficient job skills to find work immediately, and those that do not find jobs within a few weeks are likely to be in need of

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[2]To ensure the work-search requirements are flexible and meet the needs of the claimants served and the economic conditions in the area, each JSC develops its own work search plan within certain broad guidelines. Plans are reviewed by State ESD staff for compliance with overall policies and to be sure they meet the needs of the claimants served.

[3]Because of the different emphases of the ERIs, separate group ERI sessions were held for claimants in Treatment Group B and those in Treatment Groups C/D.

assistance in job-finding techniques. To meet this need, all claimants -- except for union members and those who are employer-attached -- who were still unemployed after four weeks were directed to attend a two-day job search workshop. The workshop included training on skills assessment, interview and marketing techniques, telephone canvassing, completing applications, and preparing resumes. Approximately 10 hours of follow-up phone room activity was also provided for claimants to contact prospective employers to set up job interviews. Individuals in Group D who were still unemployed after 12 weeks were instructed to come in for a group ERI and an individual follow-up session that emphasized employability development planning.[4]

Treatment Services/Activities

To test the effectiveness of the four work-search policies required that certain changes be made in the way claimants were initially informed of their UI rights and responsibilities and that a process be developed to inform them of any new work search directives. In addition, certain services were modified to fit the objectives of the demonstration. An overview of how the services and activities differed by treatment group is provided in Exhibit 1. Below we describe these activities and services.

Presentation of Benefits Rights. Individuals filing new claims were given an application package and instructed to complete the forms to the extent possible and to return to the office for a group application interview. This group interview, called a Presentation of Benefits Rights (PBR), focused on the proper completion of the application forms and on informing claimants of their rights and responsibilities under the law. This included reviewing monetary determinations, instructing claimants on how to file continued claim forms to receive UI benefits, providing information concerning job search responsibilities, and giving each individual an "Information for Claimants" booklet.

Because different information needed to be provided to claimants in certain treatment groups, two different group PBRs were held: one for those in Treatment Group A and one

[4]Although the timing of the group ERI for Treatment Group D followed the same schedule as for Treatment Group B, the emphasis/content of the sessions were different as described earlier.

Exhibit 1

Overview of Activities/Services by Treatment Group

	Treatment Group			
Activity/Service	A	B	C	D
Presentation of Benefits Rights	Special Group Interview	Regular Group Interview	Regular Group Interview	Regular Group Interview
Continued-Claims Process	By Phone, As Necessary	Submit Forms Bi-Weekly	Submit Forms Bi-Weekly	Submit Forms Bi-Weekly
Initial Work Search Directive	Active Search for Work	Active Search for Work	Active Search for Work	Active Search for Work
Subsequent Work Search Directives	None	3 Contacts Per Week; Later to Report for ERI	Directed to Report for ERI	Directed to Attend Job Search Workshop After About 4 Weeks; Later to Report for ERI
Eligibility Review Interview	None	13-15 Weeks After Filing Claim; Focus on UI eligibility	Individualized Timing of ERI With Possible Increase in Work Search Requirements	13-15 Weeks After Filing Claim; Focus on Employability Development

for those in Treatment Groups B, C or D.[5] As indicated in Exhibit 1, individuals in both group interviews were initially directed to "make an active search for work," but were not told how many contacts to make. Claimants in the combined PBR for Treatment Groups B, C and D, were instructed to follow any work-search directives they later received from the JSC. As described below, many of these individuals shortly received letters informing them of specific work-search directives.

The major difference in the content of the group application interviews concerned the continued claims process. Individuals in Treatment Group A did not have to submit continued claims forms. Rather, they were instructed to report changes in circumstances that affected their eligibility for UI payments (e.g., earnings, returned to work, not available for work) by calling a specific telephone number in the JSC. If there were no changes in circumstances, no contact with the office was required, and benefit payments continued to be sent on a bi-weekly basis until claimants requested otherwise. In contrast, the PBR for claimants in Treatment Groups B, C and D instructed individuals to submit continued claims forms bi-weekly and to report work-search contacts on the forms.[6]

Work Search Directives. Claimants in Treatment Groups B, C and D were sent letters containing different work search directives. Individuals in Group B received a written work search advisory within about a week after the PBR instructing them to make three employer contacts each week to remain eligible for UI benefits. The reported contacts were reviewed to determine whether three contacts were listed, but in general were not closely monitored to assess work-search efforts. No other directives -- except to report for an ERI much later in the claim as discussed below -- were issued to Treatment Group B claimants

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[5]Different "Information for Claimants" booklets, reflecting the different continued claims processes, were also prepared and given to individuals in the two different group interviews.

[6]The additional time required to explain how to complete the bi-weekly continued-claims forms resulted in the PBRs for Treatment Groups B, C and D averaging about 80 minutes, as compared to 60 minutes for Treatment Group A.

during the benefit year. In contrast, individuals in Treatment Groups C and D received letters advising them to report to an ERI or a job search workshop as described below.

Job Finders Workshop. Claimants in Treatment Group D who did not find work in the first few weeks after filing their UI claim were sent a letter at Week 4 instructing them to attend a two-day workshop that would assist them in work search techniques.[7] The two-day workshop included training on skills assessment, methods of self-marketing, job-interview techniques, and resume preparation. In addition, these individuals were scheduled for a total of 10 hours of supervised telephone room activity over the following three weeks, calling employers and trying to set up job interviews. A follow-up meeting to the workshop and the phone bank activities was held each Friday during this period. Job Finders was responsible for claimants' work search during the first 12 weeks of the claim. After Week 12, these claimants were set a letter instructing them to report for a group ERI during the next two weeks.

Eligibility Review Interview. The major component of the individualized work-search requirements for claimants in Treatment Group C involved the directive to report for an ERI. The treatment was individualized in that the timing of the directive was different depending on the claimant's characteristics. For example, a letter was sent to seasonal workers (during the off season) in the second week of the claim instructing them to come in for an ERI during the next two weeks to discuss their work search. Targeted claimants were sent a letter in the fourth week of the claim; non-targeted workers were sent a letter in Week 12; those initially on standby were sent a letter one week after the standby indicator was removed; and union members were sent a letter instructing them to come in

[7]Union members and employer-attached claimants were not referred to the job search workshop. Individuals who later were removed from standby status were referred to the workshop one week after the change in status.

for an ERI at Week 16.[8] In contrast, all claimants in Treatment Groups B and D (except full-referral union members) were sent a letter at Week 12 directing them to come in for an ERI during the following two-week period.

These directives were to a group ERI session that usually lasted about 1 hour, after which claimants were scheduled for an individual follow-up ERI during the next 2-3 weeks. As indicated above, the emphasis of the ERIs was different for Treatment Groups B and C/D.[9] For example, although the ERI sessions for Group B included reviewing the claimant's employability development plan and providing assistance as necessary to revise the plan, the focus of the ERI was on issues related to UI eligibility and whether they were contacting the required three employers each week. On the other hand, the ERI for claimants in Treatment Groups C and D focused on activities related to increasing work search effort. Following the review of a more detailed employability development plan, individuals in these groups were generally directed to increase their work search intensity. This increase in work search activity could take the form of increases in the number of employer contacts (e.g., at least four employer contacts per week), as well as instructions to expand the search to include secondary occupations or a broader geographical area.

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[8]Washington State defines targeted claimants to include those with no employer or referral-union attachment who need reemployment assistance. This is a heterogeneous group and includes claimants in both demand and non-demand occupations. For example, claimants in demand occupations who are unemployed longer than usual, as well as applicants with poor prospects for returning to work in the local labor market are likely to need work search assistance and are thus considered targeted claimants. Non-targeted claimants include those in professional occupations (who are likely to benefit more from professional contacts within their field) and those in declining occupations who are more likely to benefit from other training programs.

[9]In addition, the forms used to develop employability plans and the length of the follow-up sessions were more extensive for Treatment Groups C and D than for Treatment Group B.

Site and Sample Selection

The work-search experiment was implemented in the Tacoma JSC in July 1986.[10] The Tacoma JSC is the primary office that serves job seekers and employers in Pierce County, the second most populated county in Washington State. At the time the experiment was implemented, the population of Pierce County was slightly over 500,000, of which about 160,000 lived in Tacoma. The unemployment rate for the area was similar to the state average of 8.6% in 1985 and followed the overall state pattern of steady decline throughout the evaluation period.

The Tacoma JSC was in part selected to operate the experiment because it had a sufficiently large claims load to provide the necessary sample of claimants for the experiment within approximately a one-year enrollment period.[11] It was also selected because of the diversity of its labor market and of the characteristics of individuals served. For example, of the 160,000 nonagricultural wage and salary workers employed in Pierce County in early 1986, 25 percent were employed in either wholesale or retail trade, 24 percent were employed in the services sector, 22 percent were employed by the government sectors and 14 percent were employed in manufacturing industries. The racial/ethnic composition of the population in Pierce County is also reasonably representative of the state as a whole, although minorities (and African Americans in particular) are over-represented

[10]Although the initial design considered implementing the experiment in three sites throughout the state, it was determined that there were not enough resources available to support the additional training and monitoring costs that would be incurred.

[11]A one-year enrollment period was chosen to ensure that the results would not be affected by seasonal differences in the characteristics of claimants or the hiring practices of employers in different industries. Based on information provided by ESD, we anticipated that the Tacoma JSC would process roughly 10,000 new initial UI claims in the 12-month period beginning July 1986. With such a sample, and given the allocation of the sample to the treatment groups described below, it was estimated that we would be able to detect a 1-week impact in UI benefits at the 95 percent significance level with from 94-99 percent statistical power and a .75-week impact at from 77-90 percent power depending on which treatment groups are being compared. Such impacts correspond to roughly 5-7 percent of the mean number of weeks of UI payments.

in Pierce county. Specifically, 86.5 percent of the population in Pierce County are white (and 6.8 percent were African American), as compared to the statewide distribution of 90.5 percent white and 2.9 percent African American.

All individuals who filed a valid new initial Washington State UI claim in the Tacoma JSC during the July 1986 to August 1987 period were randomly assigned to one of the four treatment groups described above based on the last digit of the person's Social Security Account (SSA) Number. Specifically, all claimants with SSA ending in 0, 1, or 2 were assigned to Treatment Group A; those with SSA ending in 3, 4, or 5 were assigned to Group B; those with SSA ending in 6 or 7 were assigned to Group C; and claimants with SSA ending in 8 or 9 were assigned to Treatment Group D.[12] Because the last two digits of the SSA are random numbers, this ensures that the characteristics of individuals in each of the four groups will be similar on average, and that observed differences in outcomes of the groups can be reliably attributed to the experimental treatment.

Data Sources

The evaluation of the work-search experiment was designed to rely primarily on information from various state administrative data systems. A brief baseline survey was also developed to obtain supplemental information on claimant characteristics at application. In addition, certain logs were kept at various times during the demonstration

[12]Allocating relatively more observations to Treatment Groups A and B follows the general philosophy of the optimal sample design rule: given a fixed budget, observations should be allocated among groups in proportion to the inverse of the square root of the relative cost per observation in each group. Although the sample sizes chosen for each group are not strictly based on the expected relative costs of each treatment, we did allocate less of the cases to the relatively more expensive treatments. The final distribution of claimants by treatment group differs somewhat from the expected 30-30-20-20 distribution because ESD stopped assigning claimants to Treatment Group A in May 1987 when preliminary evidence indicated that the costs of this treatment to the UI Trust Fund were substantial. For the remainder of the study, these claimants were assigned to Group D.

to monitor specific activities. Below we briefly describe the data sources that contributed to the evaluation.

Administrative Data Systems. The primary data for the evaluation of the work-search experiment were obtained from three different Washington State Employment Security Department data bases. These three data bases include the Benefits Automated System (BAS), the Tax Information System (TAXIS), and the Employment Security Automated Reporting System (ESARS). BAS is the data base maintained by ESD to store information on UI claimants and the benefits they receive. This data base contains individual demographic information (e.g., age, race, sex, education, occupation), UI eligibility information (e.g., claim type, weekly benefit amount, maximum benefits payable), UI benefits received in the prior year, work-search requirements and services, and detailed information on experiences with the UI system during the benefit year of the experiment. In addition to summary measures of numerous indicators of UI outcomes (e.g., total weeks paid, total conditional payments, total overpayments), claims information was provided for each of the 52 weeks of the benefit year in order to develop reliable measures of spells of UI benefit receipt during the experiment.

From the TAXIS data base, we obtained quarterly information on the total wages and hours worked of employees in covered employment.[13] It should be noted that because Washington is the only state that collects hours worked data, previous evaluations based on agency records in other states have been limited to examine impacts on total earnings, and

[13]Although the use of UI wage records has a number of advantages for the evaluation, it must be recognized that they are limited in that they only include wages on covered employment and do not include wages from other states. For example, in some states, UI wage records do not include earnings for the following types of employees: certain corporate officers, church employees, individuals paid exclusively on commissions, domestics with very low earnings, railroad employees, employees of small agricultural firms, and certain barbers or cosmetologists. These coverage gaps are relatively small, however, and result in nearly 90 percent of all state wages included in UI wage records. Also, although the exclusion of earnings from other states is a problem for individuals who live near state borders, or if the program is likely to have a large impact on migration, this is not likely to present problems for this analysis.

have been unable to examine effects on employment intensity or the average hourly wage rate. The availability of hours worked data are a unique element of the work search evaluation.

We obtained information on wages in covered employment and hours worked for the 12 quarters prior to the quarter that each person filed the claim and entered the experiment, and through the eight quarters after the quarter in which the UI claim was filed. These data were used to construct key outcome measures of employment and earnings experiences, as well as control variables.[14] In addition to providing information on wages and hours worked, data from the TAXIS system enabled us to determine the Standard Industrial Classification (SIC) code of the employer, as well as whether the claimant returned to work for the same employer after the demonstration.

The other administrative data base that contributed to the evaluation of the work-search experiment is ESARS (later replaced by JOBNET). This is the data base used to maintain information on employment services provided by local JSCs to ES/UI applicants. To understand the types of employment assistance provided to claimants in the experiment, and whether the services differed by treatment group, we obtained information from ESARS on the number of job referrals, job placements, and the number of other services such as counseling sessions, job developments, and any tests given.

Other Data Sources. To supplement information available in the administrative data systems, a self-completed survey was attached to the packet of materials that claimants were given at application. The Baseline Survey included questions on the reason for job

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[14]In each quarter, approximately 5-10 percent of the claimants had missing data on hours worked. In these cases, hours worked were imputed following a two-step procedure. For cases with missing data in one of the 12 pre-experimental quarters, an average (real) hourly wage rate was computed during the entire pre-experimental period and applied to the total wage data in the specific period to impute hours worked for that quarter. A similar procedure was used for the eight experimental periods. For the few claimants for whom we could not compute an average wage (i.e., always had missing hours worked data), we estimated quarter-specific hourly wage regression equations and applied the predicted values to total wages in the quarter to impute hours worked.

separation, marital status, spouse's employment status, presence of children under the age of six, number of children/dependents, and whether the claimant was a homeowner. In addition, several logs were used to record receipt of specific services and to document the content of specific services. For example, logs were completed to document the length of the PBR group interviews, to record attendance at the job search workshop, and to indicate whether the claimant showed up for a scheduled ERI and the outcome of the ERI (e.g., whether the work search requirement was modified and how).

III SAMPLE CHARACTERISTICS

During the period from July 1986 through August 1987 a total of 9,634 monetarily and non-monetarily eligible new UI claimants were enrolled in the Washington Alternative Work Search Experiment and randomly assigned to one of the four treatment groups.[15] In this section, we provide some background information on the characteristics of these new claimants and on the work search and employment services they received. This information is useful in understanding the population of claimants served in the demonstration and provides a context for understanding the net impact results described in later sections.

Background Characteristics of Experimental Sample

In Table 1 we present data on the background characteristics of the experimental sample by treatment group. Data are displayed for three different categories of factors: demographic characteristics, prior work experience and UI entitlement. As this table indicates, 70 percent of the sample were male and over 80 percent were white. The claimants enrolled in the experiment were fairly well educated, with 85 percent or more in each treatment group having at least a high school diploma. The claimants in the sample averaged approximately 34 years of age, with only one-sixth being 45 years of age or older.

In terms of prior work experience, the claimants enrolled in the experiment earned approximately $14,500 during the four complete quarters prior to filing their claim. Nearly one-fifth of the sample were full-referral union members -- indicating that they were required to seek employment through their union hiring hall -- and fewer than one-tenth of

[15]An additional 1,042 individuals applied for UI benefits at the Tacoma Job Service Center but were determined to be monetarily ineligible and 1,001 other claimants were judged to be non-monetarily ineligible due to job-separation issues. Although random assignment ensures that the general nature of the results would not be affected by including these claimants in the analysis, they were excluded from the analysis so that we could focus on the effects of the treatments on the subgroup of new claimants eligible for benefits.

Table 1

Selected Claimant Characteristics by Treatment Group

	Treatment Groups			Control Group	Overall
	A	C	D		
Sample Size	2,246	1,964	2,553	2,871	9,634
Demographics					
Percent male	71.0	70.1	70.1	69.5	70.1
Percent white	82.6	82.4	84.1	82.7	83.0
Percent African American	10.6	9.7	9.2	9.9	9.8
Percent high school graduate only	54.9	52.6	54.4	52.4	53.6
Percent some college	24.6	25.2	24.2	24.0	24.4
Percent college graduate	8.4	8.6	8.7	8.8	8.6
Mean education (in years)	12.5	12.5	12.5	12.5	12.5
Percent age $\leq$ 24	20.3	20.3	20.9	20.9	20.6
Percent age $\geq$ 45	17.7	18.1	16.5	17.6	17.4
Mean age (in years)	34.0	34.2	33.9	34.2	34.1
Prior Work Experience					
Mean earnings in prior year ($)	14,760	14,240	14,340	14,650	14,500
Mean hours worked in prior year	1,560	1,520	1,540	1,540	1,540
Percent union hiring hall member	19.4	19.9	18.1	19.2	19.1
Percent on standby	6.2	9.5	10.3	8.7	8.7
Percent professional/technical/managerial	11.7	11.6	13.3	12.5	12.4
Percent clerical	13.9	12.8	13.1	13.9	13.5
Percent in extractive and transformative sector	44.2	43.7	40.9	43.5	43.0
UI Entitlement					
Mean weekly benefit amount ($)	147	145	147	145	146
Mean maximum benefits payable ($)	3,890	3,820	3,810	3,860	3,850
Mean wage replacement ratio	0.63	0.63	0.63	0.63	0.63

the sample were employer-attached (i.e., on standby). Approximately one-sixth of the sample received some UI payments in the prior year. About one-eighth of the sample were in professional, technical or managerial occupations, and a slightly greater proportion of claimants were in clerical occupations. Over 40 percent of the claimants previously held jobs in the extractive or transformative sectors (i.e., agriculture, mining, construction or manufacturing).

Table 1 also provides data on claimants' UI entitlements. In Washington, claimants are entitled to up to 30 weeks of UI benefits at their weekly benefit amount (WBA). The mean WBA for our sample was $146 and the average maximum benefits payable was $3,850. The wage-replacement ratio -- calculated as the proportion of the base-period earnings that would be replaced at the annualized value of the WBA -- was slightly less than two-thirds for all four treatment groups.

An important analysis issue concerns the comparability of the individuals among the four treatment groups. As expected, the results in Table 1 indicate that the random assignment process yielded four subgroups of claimants that were remarkably similar on virtually all characteristics. The only significant difference concerns the percent of claimants who were employer-attached. As Table 1 indicates, according to agency records, individuals in Treatment Group A were considerably less likely to be "on standby" than claimants assigned to the other three groups. However, because claimants in Treatment Group A did not submit continued claims forms to document their work search activity, there was no incentive for UI staff to consistently record standby status in the system for these claimants. The likelihood that this difference is the result of different UI interviewer behavior in recording standby status for Treatment Group A than for the other three groups, and not a fundamental difference in the characteristics between the groups, is further supported by information obtained from the Baseline Survey. Specifically, based on self-reported information, there were no differences among treatment groups in terms of

the proportion who were temporarily laid off and expected to be recalled within 30 days.[16] Thus, it appears that the four treatment groups can be treated as four random subsamples of UI claimants.

Other Claimant Characteristics from Baseline Survey

As described in Section II, we supplemented the standard personal characteristics information available in the UI automated data system with a brief survey. Of the 9,634 eligible new claimants enrolled in the demonstration, 7,088 (73.6 percent) completed the Baseline Survey.[17] Consistent with the results described above, there were no significant differences in these other characteristics by treatment group. To provide further context to interpret the results described in subsequent sections, below we briefly describe the overall composition of the study sample on these additional characteristics.

The Baseline Survey obtained additional information concerning household composition and reason for job separation. Based on the survey results, 47.4 percent of the claimants in the study sample were married, 16.5 percent were divorced and 29.5 percent had never been married. Slightly over one-fourth of the claimants (26.7 percent) had a working spouse, and three-fourths of the working spouses had full-time jobs. About one-fourth (24.1 percent) of the claimants had a child under six years old.

Nearly three-fourths (71.6 percent) of the sample indicated that the reason for separation was due to "lack of work." Relatively few claimants (5.4 percent) reported that

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[16]The proportion of all claimants who reported they were temporarily laid off and expected to be recalled within 30 days ranged from 10.1 percent for Treatment Group B to 11.7 percent for Treatment Group C. The value of 11.4 percent for Treatment Group A was slightly higher than the overall mean of 11.1 percent.

[17]Regression analysis of the probability of completing the survey indicated that although there were some minor differences in the likelihood of responding to the survey among certain demographic groups, there were no differences by treatment groups. As such, any analysis restricted to the subsample of claimants who completed the Baseline Survey should also provide unbiased estimates of the treatment effects.

the lack of work was due to a permanent plant or company closure and another 28 percent reported that the lack of work was the result of a permanent layoff or job/contract completion. A substantial number of claimants (30 percent) reported that they were on temporary layoff. Although the majority of these claimants indicated they had a known recall date (11.1 percent had a recall date within 30 days and another 5.5 percent reported a known recall date beyond 30 days), over 13 percent considered themselves to be on temporary layoff but did not have a specific recall date.

Work Search and Employment Services Received

In interpreting the impacts of the various treatments it is important to understand the extent to which the claimants received specific work search and employment services. In particular, certain experimental treatments were expected to increase the level of reemployment services and/or provide these services earlier in the unemployment spell, whereas no specific assistance was offered to members of Treatment Group A. Because of the lack of job search assistance targeted to Treatment Group A, it is possible that local office staff might compensate by providing them with additional regular ES services. In Table 2 we provide information on the services received by claimants in the different treatment groups.

The results in Table 2 confirm that claimants in Treatment Group A participated in virtually no work search activities. Specifically, only 8 of the 2,246 claimants (0.4%) received an employability review interview and none participated in the job search workshop. Examination of the ES services data indicate that JSC staff did not compensate for the lack of work search assistance by providing additional ES services to members of Group A. In general, claimants in Treatment Group A were slightly less likely to receive any specific ES service than claimants in other groups.

Table 2

Services Received by Treatment

	Treatment Groups			Control Group	Overall
	A	C	D		
Work Search Services					
Percent received an ERI	0.4	33.2	19.3	24.6	19.3
Mean number of ERIs	0.0	0.5	0.2	0.4	0.3
Percent participated in job search workshop	0.0	0.1	15.2	0.1	4.1
ES Services					
Percent received job placement	5.7	7.2	6.0	6.8	6.4
Percent received job referral	14.8	16.9	15.7	17.9	16.4
Percent received job counseling	1.3	1.2	1.6	1.3	1.3
Percent received testing	1.2	0.8	0.7	0.9	0.9
Percent referred to training	0.4	0.3	0.4	0.3	0.3
Percent received employability development plan assistance	1.0	16.9	5.4	13.9	9.2
Percent received job development	1.9	2.0	1.6	1.7	1.8

Table 2 also indicates that the job search workshop was almost exclusively provided to claimants in Treatment Group D, as intended. That is, only 0.1 percent of the claimants in Treatment Groups B and C managed to somehow bypass the random assignment process and participate in the job search workshop. Approximately 15 percent of the claimants in Treatment Group D completed the workshop and another 3.3 percent reported for the workshop but did not complete it. Although this may seem like a relatively low participation rate, it must be recognized that full-referral union members and those on standby were not invited to the workshop and that invitation letters were not sent until after the fourth week of benefits. After adjusting for these factors, it appears that the workshop completion rate is approximately 40 percent among eligibles. Those who did not attend the workshop were potentially denied benefits during these weeks.

The remaining work search activity involves the employability review interview (ERI). As indicated in Table 2, a somewhat higher proportion of claimants received an ERI in Treatment Group C than in Group B (33 percent as compared to 25 percent). This difference is primarily due to the earlier scheduling of ERIs for a large portion of the claimants in Treatment Group C. Although it is difficult to precisely determine the percentage of eligible claimants in the different treatment groups who receive an ERI given available data, it appears that over 50 percent of the claimants eligible for an ERI in Treatment Groups B and C received an ERI.

IV TREATMENT IMPACTS ON UI AND EMPLOYMENT OUTCOMES

The work search treatments were expected to affect the receipt of UI benefits by eligible claimants in different ways. For example, the new ESD work search policy and intensive service treatments were expected to help claimants become reemployed more rapidly, reducing both the length of their unemployment spell and the amount of UI benefits they received relative to claimants in the control group. Moreover, the intensive service treatment (Group D) was expected to have a larger impact on spell length and UI benefits received because of the additional work search assistance provided. In contrast, the exception-reporting treatment (Group A) was expected to increase unemployment spell length and UI benefits paid because claimants in this treatment group were not required to demonstrate that they were actively seeking employment.

The treatments were also expected to affect certain UI continuing claims eligibility processes and administrative costs. For example, although the exception-reporting treatment was expected to result in greater UI benefits paid relative to controls, administrative costs were expected to be lower because of reduced work-search monitoring. For identical reasons, claimants in Treatment Groups C and D were expected to incur slightly higher administrative costs per week claimed. Moreover, the greater scrutinizing of claimants' work search efforts in Treatment Groups C and D was expected to increase the frequency with which the benefit eligibility issues were identified. Specifically, we expected the likelihood of conditional payments to be greater for claimants in Treatment Groups C and D and lower for claimants in Treatment Group A, relative to controls. Further, because of differential reporting and monitoring of work-search efforts, we expected the likelihood of overpayments to be higher in Treatment Group A and lower in Groups C and D relative to the control group.

In addition to examining the impacts of the treatments on UI outcomes, it is also important to consider employment and earnings impacts. To the extent that certain treatments affect claimants' job search intensity and result in them finding a job sooner than they otherwise would, this would affect not only UI benefits paid but could also affect the quality of the job obtained. Of particular concern here is whether any gains to the UI system from reduced UI payments that may arise from the more intensive services treatment would be offset by losses to claimants in the form of lower wage rates or earnings on the job obtained.

To examine the hypotheses described above, one could rely on the random assignment process -- which implicitly controls for any differences between the groups in observed or unobserved characteristics -- and compare simple differences in mean outcomes between the three treatment groups and the control group. To improve the efficiency of the estimated effects, however, we estimated the impact of the treatments on the outcome measures using regression and probit models that contained dummy variables for the three treatment groups and that controlled for individual claimant characteristics and employment conditions.[18] The models controlled for differences in demographic characteristics, prior work experience and UI entitlement. We also included a set of seasonal dummy variables to control for possible seasonal variations in earnings and UI benefit payments and to control for the change that was made in the assignment of sample members to the different treatment groups late in the study. As described above, beginning in May 1987, after about 80 percent of the total sample was enrolled in the experiment, no additional claimants were assigned to Treatment Group A; those with SSAs that would have been assigned to Group A

[18]Because ordinary least squares techniques are not generally appropriate for binary outcome variables (i.e., due to heteroscedasticity and the possibility that predicted probabilities fall outside the unit interval), a probit procedure was used to estimate models for all binary outcome variables. All other results are based on ordinary least squares regression models.

were instead assigned to Group D.[19] The seasonal dummy variables were constructed to correspond to the timing of this modification. Specifically, four dummy variables were added to the model to roughly correspond to the periods of Summer 1986, Fall 1986, Winter 1987 and Spring 1987 before the assignment change; the remaining period after the assignment change served as the left-out category.[20] The Appendix includes a complete listing of the control variables and presents their estimated coefficients for the main outcome measures.

For the most part, the results described below are based on the total sample of monetarily and non-monetarily eligible new claimants who were enrolled in the demonstration. As such, the effects can be interpreted as the average impacts over all eligible claimants, regardless of whether they actually received all of the specific services available as part of the treatment. To examine whether there were any differences in impacts among certain demographic groups (e.g., age, race/ethnicity, sex), we estimated models that included interaction terms that are the product of the treatment dummy variables and these claimant characteristics. Because certain variables included in the interaction analysis were obtained from the Baseline Survey, these analyses are restricted to claimants who completed the Baseline Survey.[21] In some cases, the analysis is restricted to

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[19]This was because preliminary information indicated that the costs to the UI Trust Fund for Treatment Group A were much higher than anticipated. Of course, all individuals who had previously been assigned to Treatment Group A remained in that experimental condition throughout the rest of their benefit year.

[20]These four seasonal variables were included because preliminary analysis indicated that the estimated treatment group impacts were sensitive to the inclusion of cases following the modification to the assignment procedure. Specifically, we find that point estimates obtained using the full sample are somewhat larger than those obtained using the subsample of cases assigned before May 1987, particularly for Treatment Group D. However, when the seasonal dummy variables are included, point estimates obtained from both samples are virtually indistinguishable.

[21]Although the results based on the full sample and the sample of respondents to the Baseline Survey are quite similar, we present the results for the full sample (except for certain interaction effects) because they provide more efficient estimates of experimental impacts.

certain sub-populations (e.g., only claimants who become re-employed). In analyzing the impacts of the treatments on such outcomes (e.g., hourly wage rate), we incorporate adjustments for the potential selection bias that arises from restricting the analysis to the sub-sample of workers following procedures developed by Heckman (1976, 1979) to derive impact estimates for the overall claimant population.

In the remainder of this section we examine the impacts of the treatments on several different UI payment measures, UI administrative outcomes, and employment and earnings measures. In each subsection we first describe the specific outcome measures used and then present the estimated overall treatment impacts. We also discuss information on impacts for selected demographic subgroups of interest. Additional details concerning the subgroup impacts are provided in the Appendix.

Impacts on UI Benefit Receipt and Duration of UI Spell By Treatment Group

In examining the impacts of the treatments on UI benefits, three types of measures of benefit receipt are used. The first is based on the entire 52-week benefit year, which is the relevant period for capturing the total effects of the treatments on UI payments. The three measures we examine based on the entire benefit year include: (1) the total dollar amount of UI benefits paid to claimants; (2) the number of weeks for which a payment was issued; and (3) whether claimants exhausted their benefits.

The other two types of measures are specific to the first two spells of receipt of UI. These measures are: (1) whether the (first or second) spell of UI receipt occurs; (2) length (i.e., number of weeks) of the spell; and (3) total UI benefits received during the spell. It should be emphasized that these spells correspond to consecutive weeks of receipt of UI payments, and not to spells of unemployment, as claimants can work part time and still receive benefits and for those who exhaust their benefits without working, the number of weeks of insured unemployment is a censored measure of total unemployment. We consider

measures of both total receipt and of the first two spells of UI receipt to help distinguish between treatment impacts that lead to temporary withdrawal from the UI rolls from impacts that lead to longer-term effects.

The impact of each treatment on UI benefits, relative to the control group, are presented in Table 3. As indicated in the last column of this table, claimants in the control group received an average of $2,030 in total UI benefits during the benefit year. On average, these benefits were received over 14.5 weeks of payments, with about two-thirds of the payments received during the first UI spell. Approximately one-fourth of the claimants in the control group exhausted the UI benefits available to them during the benefit year.

As expected, claimants in the exception-reporting treatment group (Group A) received greater UI benefits on average than did claimants in the control group. As indicated in the first column of Table 3, claimants in Group A received an average of $265 more in UI benefits during the benefit year than claimants in the control group. Our results also indicate that claimants in the exception-reporting treatment received UI benefits for about 3.3 weeks longer than claimants in the control group. Consistent with this finding, claimants in this treatment group were also much more likely to exhaust their benefits than claimants in the control group. After converting the probit estimates in Table 3 to percentage terms following Maddala (1983), we find that claimants in this treatment group are about 13 percentage points more likely to exhaust their benefits than those in Treatment Group B. Evaluated at the mean for the control group, this corresponds to about a 50 percent higher likelihood of exhaustion for claimants in Group A. Each of the estimated impacts for this treatment are statistically significant at the .05 level.

The intensive work search treatment (D) also had the expected negative impact on total UI benefits paid. Overall, claimants in Group D received $68 less in total UI benefits and received payments for .5 fewer weeks than did claimants in the control group. The impact of this treatment is statistically significant at conventional levels for the number of

Table 3

Treatment Impacts on UI Receipt
(Standard Errors in Parentheses)

Outcome Measures	Treatment Groups			Control Group Means
	A	C	D	
Full Benefit Year:				
Total UI benefits paid ($)	265**	5	-68	$2,030
	(44.2)	(45.0)	(42.5)	
Number of weeks of UI payments received	3.34**	0.17	-0.47*	14.48
	(0.29)	(0.30)	(0.28)	
Percent exhausted benefits[a]	37.9**	3.1	-2.0	25.7
	(3.9)	(4.1)	(3.9)	
First Spell:				
Percent who received at least one UI payment[a]	40.7**	2.9	-1.5	88.6
	(5.7)	(5.1)	(4.7)	
Number of weeks of first UI spell	2.65**	0.08	-0.66**	9.69
	(0.27)	(0.28)	(0.26)	
Total benefits paid in first spell ($)	330**	3	-101**	$1,413
	(42.8)	(43.6)	(41.2)	
Second Spell:				
Percent with a second spell of UI[ab]	-21.9**	-0.4	4.3	43.7
	(3.9)	(4.0)	(3.8)	
Number of weeks of second UI spell[b]	-0.39**	0.15	0.26	2.56
	(0.16)	(0.17)	(0.16)	
Total benefits paid in second spell[b] ($)	-37	18	43*	$358
	(24.7)	(25.6)	(24.2)	

[a]Probit estimates.
[b]Applies only to those who had a first spell.
**Significant at .05 level.
*Significant at .1 level.

weeks of receipt of UI payments, but the impact on total UI benefits received is only significant at about the .11 level. Although the point estimate indicates that claimants in this treatment group were slightly less likely to exhaust their benefits than claimants in the control group, the effect is not statistically significant. There are very small differences in these outcomes between claimants in the control group and those in the new work search treatment (Group C), and none are statistically significant.

Turning to the spell results, the last column in Table 3 indicates that among control group members, nearly 90 percent of (monetarily and non-monetarily) eligible claimants received at least one UI payment, and, on average, the first spell of UI lasted 9.7 weeks, during which claimants received slightly over $1,400 of UI payments. Converting the probit results to percentage terms, we find that claimants in Treatment Group A were considerably more likely -- about 6 percentage points -- to have initiated a UI spell (i.e., received at least one payment) than controls. This is likely to be a direct result of the exception-reporting nature of the treatment, in that a spell of benefits began automatically unless claimants reported an exception to their eligibility. Moreover, claimants in Group A had significantly greater UI receipt (about $330) during their first spell primarily because of the greater number of weeks of UI received (2.7 weeks more than the control group). On the other hand, claimants in Group D received significantly less UI payments (about $100) during their first spell than controls and drew benefits for about .7 weeks less. Similar to the results for the full benefit year, Treatment Group C had no impact on UI receipt during the first spell.

In most cases, these results indicate that the overall impacts for Groups A and D tend to be smaller (in absolute value) for the full benefit year than for the first spell of receipt of UI. This is consistent with findings reported in the last three rows of Table 3 concerning the second spell of UI. Specifically, relative to the control group, claimants in the exception-reporting group who had a first spell were significantly less likely to have a

second spell (by about 8 percentage points) and have significantly shorter second spells (by about .4 weeks). This is in part due to their greater likelihood to exhaust benefits, but could also reflect improved outcomes of the job search process that made it less likely they would draw additional UI payments during the benefit year.

In contrast, relative to the control group, claimants in the intensive work search treatment group had second spells that were a quarter of a week longer on average and received about $45 more in UI benefits, although the impact on the second spell length is only significant at the .11 level. The greater likelihood of second spells and their increased length is consistent with claimants in Group D having their first spell interrupted by a denial of benefits for not reporting for the workshop or by not claiming for a particular week. It is also consistent with claimants in this group being relatively more likely to find a job with a different employer than they worked for in the past, or one that does not match their interests, and then returning to UI later in the benefit year for temporary income support while they search for a better job. Additional evidence on this issue is provided below.

Overall, these results indicate that Treatment A increases UI benefits and Treatment D reduces UI benefits, and that they have somewhat different short- and long-term impacts. Treatment A tends to increase total UI receipt by lengthening the first spell of UI, but this impact is partially offset by a reduction in the likelihood of having a second spell and in the length of second spells. In contrast, Treatment D tends to reduce total UI receipt by shortening the length of the first spell, which is partially offset by somewhat longer second spells of UI receipt. Treatment C appears to have little short- or long-run impact on UI receipt.

We also examined the extent to which these UI impacts differed for major claimant demographic subgroups. As shown in the Appendix, for the most part these results indicate that the effects of Treatment A in increasing total UI benefits paid and extending the

length of UI spells are widespread and not generally concentrated among specific demographic subgroups. Specifically, none of the joint F-tests to determine whether the effects of the treatments are different by claimant race, age or sex for total UI benefits paid and the length of the first spell of UI was statistically significant at conventional levels. Although the point estimates indicate that African Americans and women tend to be most responsive to Treatment D -- both groups are estimated to receive fewer benefits and have shorter first spells than whites and men -- the differences are not statistically significant. This lack of statistical significance for the treatment differences by race/ethnicity group is likely to be in part due to the very small sample sizes of minorities in the demonstration.

There is, however, some evidence that the impact of the treatments differed for men and women by marital status and the presence of children. For example, the impact of Treatment A on the length of UI spell and total benefits received is larger for women with children and for men without children. Similarly, the impact of Treatment A is larger for married women and unmarried men. The effects of Treatment Group D in reducing UI payments, length of first spell and the exhaustion rate are concentrated among women without children (and unmarried women). For example, Treatment Group D is estimated to reduce total UI payments by $182, the length of the first spell by 1.6 weeks and the exhaustion rate by 5.8 percentage points for women without children relative to comparable women in Group B, whereas there are no differences in these outcomes between women with children in Groups B and D. The joint F-tests for whether the effects of the treatments differ among these characteristics are all significant at the .05 level.

We also examined whether the impacts of the treatments differed for claimants who left their prior jobs for different reasons. As described above, the Baseline Survey collected self-reported information on whether the claimant left his/her prior job because of lack of work and, if so, what was the main reason. Although the joint F-test was not significant for

total UI payments received, it was significant for the length of the first spell. The results indicate that Treatment A extends the length of the first spell for claimants who left their prior job because of a plant closing, and that Treatment D is primarily effective in reducing the duration of first UI spell for claimants who indicated they had been permanently laid off. Additional details on the subgroup impacts are provided in the Appendix.

UI Exit and Survival Rates

The regression estimates presented above provide a useful summary of the effects of the treatments on claimants' UI experiences. These estimates, however, do not provide any information concerning the treatment impacts on the timing of UI exit. To address this issue, we estimated the weekly survival rates -- the probability that a claimant will continue to receive subsequent payments -- for each treatment group and for the control group to examine treatment effects on the timing of UI exit.

Figure 1 presents plots of the estimated survival curves for the first spell of UI for the three treatment groups and for the control group. Treatment Group A, which is represented by squares in the figure, had the highest survival rate throughout the first 30 weeks following initial claim date, indicating that claimants in this group were more likely to remain on UI at each point in time than any of the other groups. Figure 1 also shows that the survival functions for Group C and the control group are virtually indistinguishable, supporting the regression results that Treatment C has no impact on UI payment measures. The curve for Group D is somewhat below the curves for Group C and the control group, indicating that at every point claimants in Group D were less likely than controls to continue receiving UI benefits.

The shapes of the survival curves are also of interest. As can be seen in Figure 1, the survival function for Group A begins to diverge from the other groups during the first

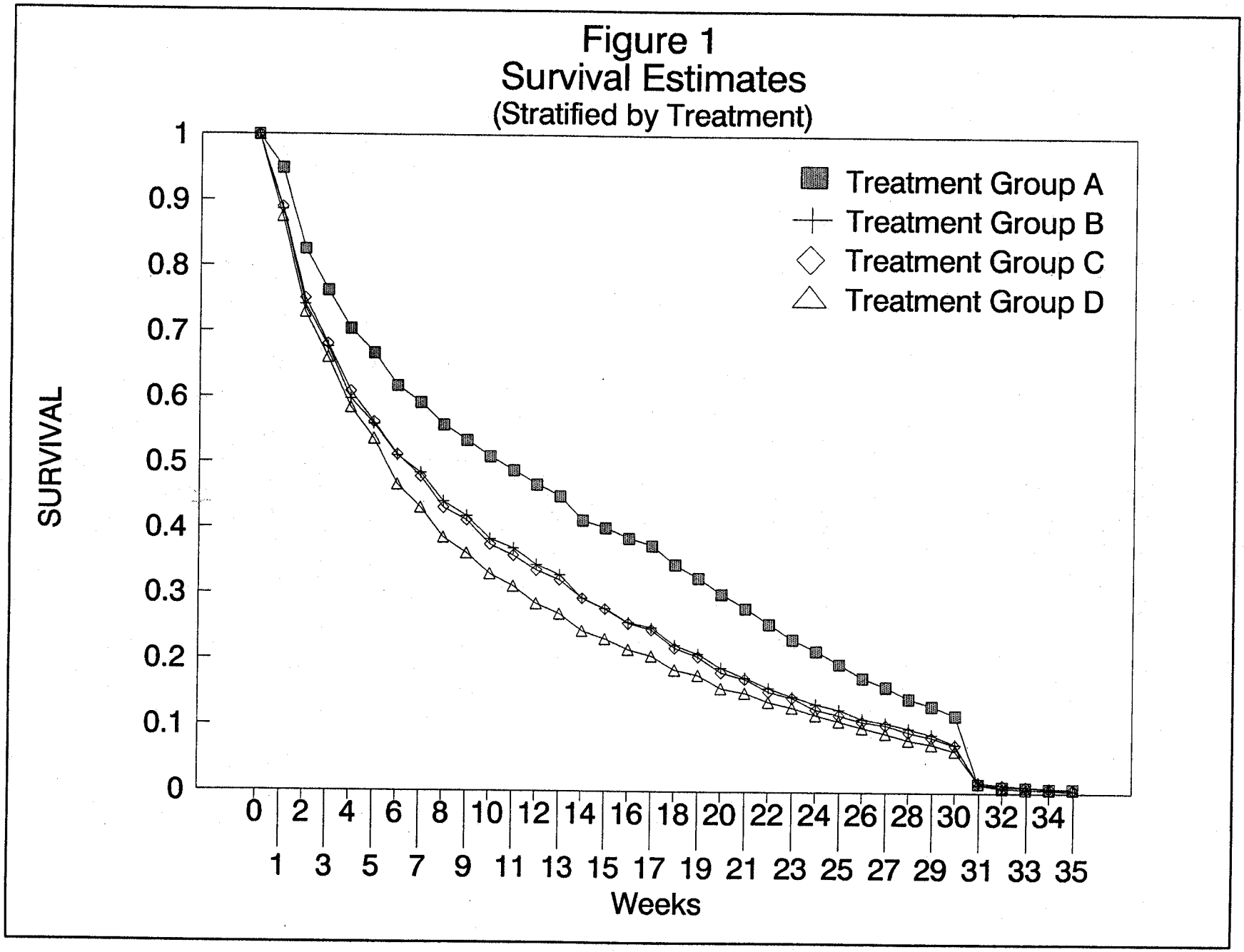
Figure 1
Survival Estimates
(Stratified by Treatment)
SURVIVAL
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
Treatment Group A
Treatment Group B
Treatment Group C
Treatment Group D
0 2 4 6 8 10 12 14 16 18 20 22 24 26 28 30 32 34
1 3 5 7 9 11 13 15 17 19 21 23 25 27 29 31 33 35
Weeks

week. This is as expected since the first spell for claimants in Group A began automatically unless claimants reported excessive earnings or an exception to eligibility. The difference between Group A and the other three groups increases over time as claimants in the other three groups begin receiving letters concerning their work search directives and eligibility review interviews. After about 18 weeks, the survival rate for Group A begins to converge with the other three groups as the maximum benefit period approaches. Note however, that the survival rate for Group A remains higher than the other groups right up to the last week of full benefits. This result is consistent with claimants in Group A being more likely to exhaust their benefits, as discussed earlier.

The survival rate for Group D begins to decline more rapidly than the survival rates for Group C and the control group at about the fourth week. This is as expected since claimants in this group are sent letters during the fourth week of the claim directing them to attend the job search workshop. The gap between Group D and the control group continues to widen until about Week 8 and then remains about the same until about Week 15. This is about the time when both groups are directed to come in for an ERI and as the maximum benefit period approaches.

To more formally test the patterns observed in Figure 1, we estimated a series of hazards models for length of first spell of receipt of unemployment benefits. The selection of an appropriate hazard model depends on the shape of the survival curve under consideration. The shape of the survival curves in Figure 1 imply that the hazard functions for all four groups are U-shaped. Because none of the usual parametric forms can adequately characterize this shape, and because hazard model results can be sensitive to misspecification of the functional form of the hazard, we estimate a piece-wise exponential model that allows the shape of the hazard function to vary over different time periods (Trussell and Hammerslough, 1983). Moreover, to overcome the week-to-week noise in the data that are in part due to the bi-weekly UI payment schedule followed in Washington

State, we redefine the analysis period as 15 two-week periods.[22] Finally, because preliminary analyses revealed that simple polynomial or spline functions did not adequately describe the shape of the hazard function, we use dummy variables for time to model the shape of the hazard function.

In the first column of Table 4 we present the estimated treatment coefficients from a model designed to test the hypothesis that all four groups have the same hazard of exiting their first UI spell. These results confirm the findings from the regression models described earlier. That is, claimants in Group A have a significantly lower log hazard of ending their UI spell than the control group and claimants in Group D have a significantly higher log hazard of leaving; as before, the rate of exiting UI for claimants in Group C does not significantly differ from those in the control group. This can be seen more clearly after exponentiating the coefficients to derive a direct measure of the rate of leaving the first UI spell. For example, the .075 coefficient for Group D corresponds to a value of 1.078, which indicates that the hazard of exiting from UI is nearly 8 percent higher for claimants in Group D relative to the control group.

For policy purposes, it is also important to understand the mechanisms through which Treatment D produces the observed higher rate of exiting from UI. One possible interpretation is that the job search workshop increases claimants' human capital, making them more employable or more efficient in their job search. An alternative interpretation is that the letter announcing the requirement to attend the workshop acts as a deterrent to continued receipt of UI benefits because it increases the costs of remaining on UI. In an attempt to resolve these two competing interpretations, we created a set of time-varying covariates for when claimants received letters instructing them to attend the workshop, for when they participated in workshops and for post-workshop periods. If claimants view

[22]In addition, we restrict the analysis to the first 29 weeks of a spell because the hazard rate becomes so large near the end of the eligible benefits period that they dominate the analysis.

Table 4

Hazards Models Estimates of Treatment Impacts on Exiting First Spell of UI Benefits (Standard Errors in Parentheses)

Variables	Basic Model	Proportional Model	Non-Proportional Model
Treatment A dummy	-.351**	-.353**	
	(.033)	(.034)	
Treatment C dummy	-.001	.001	
	(.032)	(.033)	
Treatment D dummy	.075**	.079**	
	(.030)	(.033)	
Period received workshop letter dummy		.111*	-.051
		(.067)	(.081)
Period received ERI letter dummy		.096*	.152**
		(.058)	(.067)
Periods participated in workshop dummy		-.540**	-.603**
		(.133)	(.134)
Periods following workshop participation dummy		-.090	-.073
		(.085)	(.090)
Periods following receipt of ERI dummy		-.077	-.119
		(.055)	(.085)

Note: Each of these hazards models also included the basic set of independent variables included in the models reported in Table 3. The non-proportional model included a complete set of period interactions with Treatments A and D.

**Significant at .05 level.
*Significant at .1 level.

workshops as costly, we would expect the hazard of leaving UI to increase immediately after the letter arrives informing them of these new "costs." Similarly, if the workshop helps them find work, we would expect the hazard of leaving UI to increase shortly after the workshop. We also included a similar set of ERI variables to differentiate the effects of the directive to attend the ERI from the effects of receiving the ERI service.

The results of this analysis are presented in the second column of Table 4. As expected, the variables representing the letters directing claimants to attend the job search workshop and the ERI are associated with a significantly higher hazard of leaving UI benefits. Specifically, these coefficients indicate that the hazard of exiting from the first spell of UI is about 10-12 percent higher for claimants who receive such letters. The results also indicate that during their participation in the two-day workshop and the three-week follow-up period, claimants in Group D are significantly less likely to end their UI spell. In addition, the results indicate that claimants who participated in the job search workshop or an ERI are not more likely to leave their first UI benefit spell after receiving the service than similar claimants who did not participate. In fact, the point estimates of the variables representing the effects of the post-workshop and post-ERI periods are both negative (i.e., associated with lower UI spell exit rates). Although these negative point estimates are statistically insignificant and could, in part, be the result of self-selection (i.e., that claimants who participate in the workshop or ERI services may be systematically different than those who do not), they provide no support to the interpretation that the workshop reduces UI spells through increased human capital or more effective job search abilities.

It is interesting to note that the coefficient for Treatment Group D in the second column is significant and largely unchanged from the estimate reported in the first column. If the time-varying covariates are properly measured, and if the hazard functions for the various treatment groups are proportional, then these factors should capture the effects of the treatment. To test the hypothesis that the hazard functions are non-proportional or that

the shapes of the hazard functions for Groups A and D significantly differ from the hazard function for the control group we interacted these treatment dummy variables with the time dummy variables.[23] Likelihood-ratio tests indicate that the shapes of the hazard functions for both Groups A and D differ significantly from the control group. Results of this analysis are presented in the third column of Table 4.

The results from a non-proportional model are generally similar to the results from the proportional model. For example, the point estimates for the post-workshop and post-ERI variables are negative and not statistically significant. As before, these results indicate that neither the receipt of the workshop nor ERIs have any significant effect on the hazard of exiting the first UI spell. In addition, the estimated effects of being in the workshop and receiving the ERI letter are generally unaffected by allowing for non-proportionality, except that the coefficient for the ERI letter increases in size and is significant at the .05 level. The only major change in the results concerns the coefficient for the workshop letter, which reverses sign and is no longer significant when we allow for non-proportionality. This suggests that the positive impacts for the workshop letter reported in the second column could in part be the result of incorrectly assuming proportionality.

A potential difficulty with this analysis concerns the fact that all claimants in Group D are sent the letter informing them that they must participate in the workshop at the same time (i.e., the fourth week of their claim). Consequently, the inclusion of treatment-specific time dummy variables makes it extremely difficult to separate out the effects of the letter from the time variables. To get a better understanding of this, in Figure 2 we present a graph of how the hazard functions for Groups A and D deviate from the other groups, net of the control variables. Notice that the hazard rate for Group D is nearly identical to that of the control group until the third time period (Weeks 4 and 5 of the spell), the period

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[23]Because Figure 1 and the results in Table 3 showed no tangible differences between claimants in Group C and the control group, we treated these two groups as one group for this analysis.

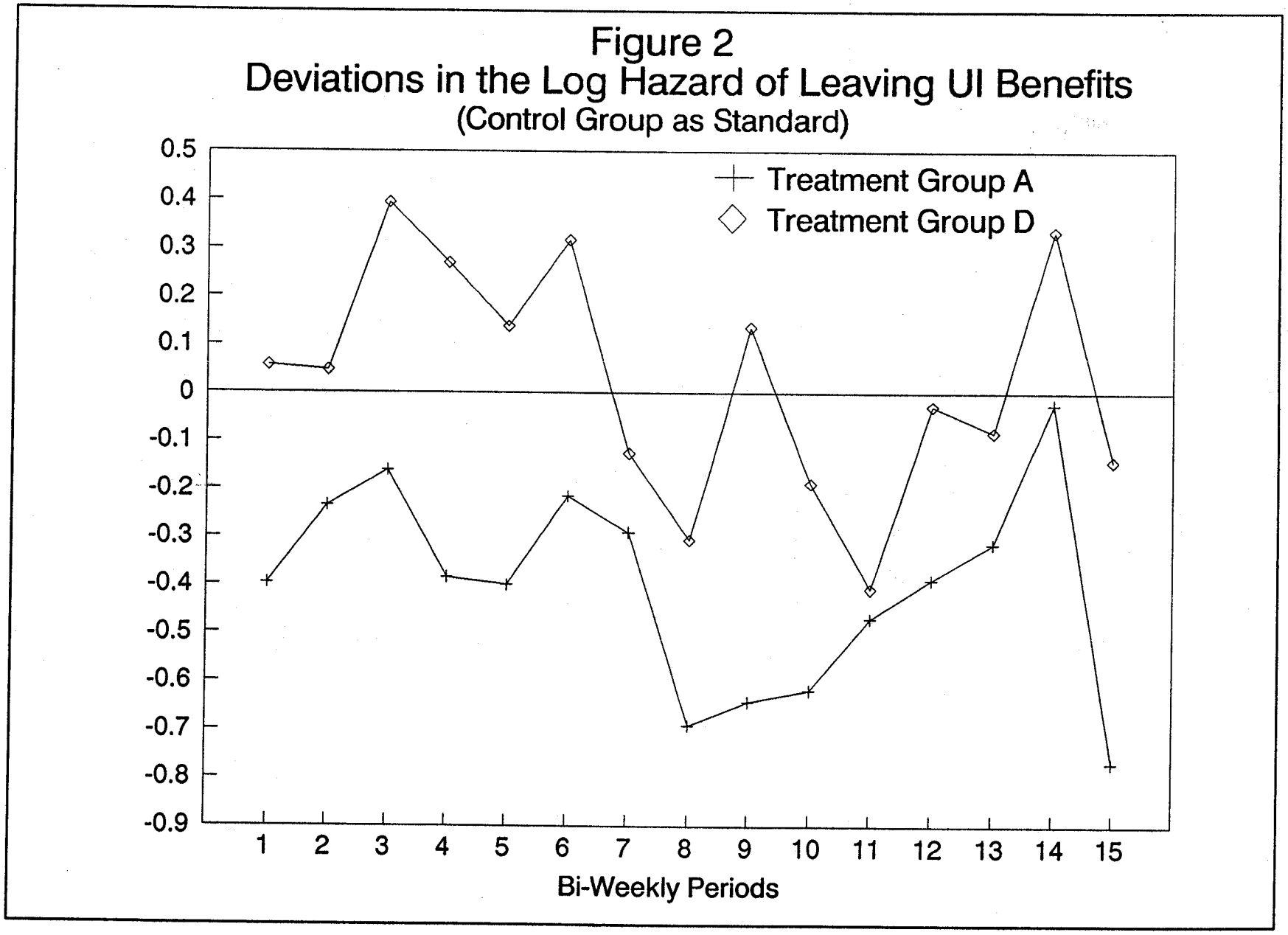
Figure 2
Deviations in the Log Hazard of Leaving UI Benefits
(Control Group as Standard)
Treatment Group A
Treatment Group D
0.5
0.4
0.3
0.2
0.1
0
-0.1
-0.2
-0.3
-0.4
-0.5
-0.6
-0.7
-0.8
-0.9
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
Bi-Weekly Periods

immediately following the mailing of the workshop letter. The hazard for Group D then begins to converge to that of the (combined) control group, except for a temporary rise in the sixth period (Weeks 10 and 11), which is about the time when claimants in Group D finished the workshop follow-up activities. Thus, although the estimated coefficient for the workshop letter is not positive when allowing for non-proportionality -- due in part to the strong overlap with the treatment group-specific time variables -- the graph in Figure 2 clearly demonstrates the increased hazard of exiting UI for claimants in Group D that occurs at about the time the workshop letter is sent.

Overall, these results indicate that the workshop and ERI treatments tend to reduce the length of time claimants receive UI benefits because they increase the perceived costs of remaining on UI. The results provide no support for the hypothesis that either the workshop or the ERI provides claimants with additional skills that make them more employable or more efficient in their job search.

Impacts on Continuing Eligibility Issues

In addition to examining treatment impacts on UI benefit payments, we also examined effects on various continuing eligibility issues. This is an important part of the evaluation as each of the treatments affected the requirements for receiving benefits and the degree to which claimant compliance was monitored. Below we provide evidence concerning the impacts of the treatments on overpayments, conditional payments, denials and appeals.

As shown in the last column of Table 5, claimants in the control group received slightly less than one-half of a week of overpayments during the benefit year, for an average of $53. The regression results reported in the first column indicate that claimants in Treatment Group A are estimated to have had about twice as many overpayments as those in the control group, and received more than twice the dollar amount. This is in large part due to claimants in Group A being late in phoning in changes in their circumstances

Table 5

Treatment Impacts on Continuing Eligibility Issues
(Standard Errors in Parentheses)

Outcome Measures	Treatment Groups			Control Group Means
	A	C	D	
Number of overpayments	0.49**	-0.02	-0.01	0.44
	(0.07)	(0.07)	(0.06)	
Overpayment amount ($)	64**	-6	-5	$53
	(9.1)	(9.3)	(8.7)	
Number of conditional payments	-0.42**	0.07	0.26**	0.90
	(0.05)	(0.05)	(0.05)	
Number of conditional payments due to work search	-0.08**	-0.01	-0.02	0.09
	(0.01)	(0.01)	(0.01)	
Number of conditional payments due to failure to report	-0.07**	0.05*	0.33**	0.25
	(0.03)	(0.03)	(0.03)	
Percent with weeks denied due to separation issues[a]	-3.9	5.0	4.5	1.8
	(8.8)	(8.8)	(8.4)	
Percent with weeks denied due to non-separation issues[a]	-25.8**	0.3	8.5	9.7
	(7.3)	(6.7)	(6.1)	
Number of appeals	-0.01*	0.01**	0.00	0.03
	(0.006)	(0.006)	(0.006)	

[a]Probit estimates.
**Significant at .05 level.
*Significant at .1 level.

(e.g., became employed) that reduced the benefits that should have been paid in a previous week, as well as the much greater likelihood of claimants in this treatment returning warrant checks.[24] It is also due to a few cases that involved fraudulent claimant behavior and resulted in many weeks and dollars of overpayments for the claimants involved. Not surprisingly, there were no significant differences in overpayments among claimants in Groups C or D and those in the control group.

Conditional payments arise when a potential issue (e.g., not available for work, failure to look for work, failure to report as directed) is identified during the process of monitoring claimant non-monetary UI eligibility. For the specific week(s) in question, the claimant is paid while the issue is being resolved. As indicated in the last column of Table 5, control group claimants received just under one conditional payment during the benefit year, and only one-tenth of these were related to work search issues. Not surprisingly, claimants in Treatment Group A had significantly fewer conditional payments (about 0.4 weeks) than the control group. In contrast, claimants in Treatment Group D are estimated to have 0.26 weeks (or 28 percent) more conditional payments than controls. This is primarily because of their large increase -- over 100 percent of the control group mean -- in conditional payments due to failure to report to the workshop as directed. Claimants in Treatment Group C had somewhat more conditional payments due to failure to report than those in the control group. This is likely to be in part due to the earlier scheduling of the ERI for the majority of Group C claimants.

In the bottom of Table 5 we provide impact results related to denials and appeals. Denials and appeals are quite rare events in this sample. For example, among control group claimants who were initially non-monetarily eligible for benefits, fewer than 2 percent were denied benefits for separation issues for any week and only about 10 percent were

[24]Agency data indicate that claimants in Treatment Group A returned roughly 10 times as many warrant checks as claimants in any other treatment group.

ever denied benefits for a week claimed due to a non-separation issue, and they filed 0.03 appeals on average. The results in the first column of this table indicate that claimants in Treatment Group A had a significantly lower likelihood of being denied benefits than controls for non-separation issues. Specifically, relative to controls, the probit results indicate that individuals in Group A are about 4.5 percentage points less likely to be denied benefits for at least one week for a non-separation issues. This is directly due to the absence of any work search monitoring for this group. Consistent with this result, claimants in Treatment Group A were slightly less likely to appeal than controls. There were no differences in the probability of being denied benefits for separation issues between claimants in Group A and the control group.[25]

The point estimates presented in the bottom of Table 5 for Treatment Groups C and D indicate that these groups had a slightly greater likelihood of being denied UI benefits for at least some week during the benefit year relative to the control group, although none of the coefficients is significant at conventional levels. Thus, despite the increased monitoring of work search efforts for these groups -- as evidenced by their greater number of conditional payments due to failure to report -- there are no differences in the likelihood of being denied benefits. This is consistent with a general policy of JSC staff giving claimants the benefit of the doubt when making a determination as to whether the reason for not reporting as directed is acceptable. Finally, the results in Table 5 indicate that claimants in

[25]In a preliminary version of this report, we reported very large positive impacts for Treatment Group A on the number of weeks denied for separation and non-separation issues. Specifically, we indicated that, relative to controls, individuals in Group A had about six times as many weeks denied for separation issues and three times as many weeks denied for non-separation issues. Discussions with State ESD staff revealed that the greater number of weeks denied for both separation and non-separation issues were primarily an artifact of the way in which the auto-warrant treatment was implemented. In particular, because people who were denied payments were aware of the denial and did not call the JSC to have them shut off their payments, these claimants continued to receive no-pay mailers until a staff person noticed the problem and shut off the auto-warrant indicator. Because these issues often involve indefinite denials, this caused the number of weeks denied to be greatly overstated. For these reasons, the denial results presented in Table 5 focus on the likelihood of experiencing a denial, and not on the number of weeks denied.

Treatment Group C had a slightly higher number of appeals that is significant at the .10 level.

The overall pattern of results for continuing eligibility issues are quite consistent with the different levels of monitoring of work search efforts among treatments. Specifically, relative to the control group, claimants in Treatment Group A -- who have less contact with the office -- tend to have more overpayments, fewer conditional payments, are less likely to be denied UI benefits for non-separation issues, and less likely to file an appeal. The reverse pattern is true for claimants in the groups with greater office contact, although the estimated impacts are not always statistically significant at conventional levels.

Impacts on Employment and Earnings

The above results indicate that Treatment A extended, and Treatment D reduced, the duration of claimants' job search. Below, we present evidence on whether the reduced search time for Group D claimants caused them to find lower quality jobs in terms of wage rates and earnings, and whether the additional search time for Group A claimants was effectively used to find better jobs. The results are based on wages and hours worked in covered employment in the first complete quarter and first complete year after filing for benefits and entering the demonstration.

The impacts of the treatments on employment and earnings are reported in Table 6. As this table indicates, two-thirds of the claimants in the control group worked during the first full quarter after filing for UI benefits. On average, these claimants worked 210 hours at about $9.50 per hour, earning about $2000 during the quarter. As indicated in Table 6, the treatments had relatively little impact on employment and earnings in the first quarter. In particular, there is no evidence that the relatively rapid re-employment of claimants in Group D occurs at the cost of lower earnings or hourly wage rates. Although the estimated impacts of Treatment D on earnings and on the hourly wage rate in the first

Table 6

Treatment Impacts on Employment and Earnings Measures
(Standard Errors in Parentheses)

Outcome Measures	Treatment Groups			Control Group Means
	A	C	D	
First Quarter:				
Percent worked[a]	-6.3	5.1	1.0	66.8
	(3.8)	(3.9)	(3.7)	
Hours worked	-2	5	7	210.4
	(6.0)	(6.1)	(5.8)	
Hourly wage rate[b] ($)	0.28**	0.13	0.01	$9.51
	(0.13)	(0.13)	(0.12)	
Total earnings ($)	-31	54	36	$2,019
	(62.7)	(63.8)	(60.2)	
Year 1:				
Percent worked[a]	-4.0	9.0*	6.8	86.1
	(4.7)	(4.9)	(4.6)	
Hours worked	-6	2	22	1,035
	(20.4)	(20.8)	(19.6)	
Hourly wage rate[b] ($)	0.12	-0.13	-0.01	$9.19
	(0.15)	(0.15)	(0.14)	
Total earnings ($)	-23	-24	292	$9,919
	(219.7)	(223.6)	(211.0)	
Other Measures:				
Percent same employer[a]	12.4**	2.1	-1.1	33.0
	(4.2)	(4.3)	(4.1)	
Percent same industry[a]	10.7**	8.3**	2.8	48.9
	(4.0)	(4.1)	(3.8)	

[a]Probit estimates.
[b]Estimates based on using a selectivity correction.
**Significant at .05 level.
*Significant at .1 level.

quarter are both positive (relative to the control group), they are not statistically significant.[26]

Our results also indicate that claimants in Group A are about 3 percentage points less likely to be working in the first quarter than claimants in the control group. However, it appears that the longer duration of UI receipt experienced by this group may have been productively used in job search as these claimants are estimated to have significantly higher hourly wage rates than controls. Specifically, we estimate that claimants in Group A earn about thirty cents more per hour than controls. Evaluated at the mean hourly wage rate of controls, this corresponds to about a 3 percent difference.

To test the hypothesis that the longer duration of UI receipt for Group A was used productively, we re-estimated this wage equation and included an instrumental variable for weeks of UI receipt.[27] Two separate equations were estimated. The first equation included an instrumented measure of total weeks on UI and the other included an instrumented measure of length of first spell. The results from both equations are qualitatively the same. Spell length is significant and its inclusion causes the coefficient for Treatment A to diminish in size by almost a factor of ten and become insignificantly different from zero. This suggests that the higher hourly wage rates experienced by claimants in Treatment Group A in the short-term are related to longer job search.

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[26]The wage rate regression was estimated over the subsample of claimants who worked in the quarter. Following a procedure outlined in Heckman (1976, 1979) to correct for sample selection, we first estimated a probit equation for working in the quarter and then constructed the inverse of Mills' ratio and included it in the wage rate regression. Identification of this model is achieved by including the following variables in the probit equation, but not in the wage equation: earnings and hours of work from two and three years ago, weekly benefit amount, maximum duration of benefits, measures of household composition, and sex and race/ethnic interactions with age. Qualitatively similar results were obtained with and without the correction factor included, and whether a linear or log-linear model was estimated.

[27]Identification of this model is achieved by including weekly benefit amount, maximum duration of benefits, and measures of household composition in the equation predicting spell length, which, as discussed earlier, are not included in the wage equation.

To understand the longer-term impacts of the treatments, we examined wages and earnings during the first full year after applying for benefits.[28] During the first full year after filing, 86 percent of the claimants in the control group had worked. On average, control group members worked slightly more than 1000 hours during the entire year, at about $9.20 per hour, resulting in annual average earnings of just under $10,000. As indicated in Table 6, the only finding that is significant at the .10 level is that claimants in Treatment Group C have a somewhat greater likelihood of working during the year than controls. Thus, there is no evidence that the relatively rapid reemployment of claimants in Group D occurs at the cost of lower earnings or other employment outcomes. In fact, the point estimates for individuals in Treatment Group D suggest that they work somewhat more hours and have greater earnings than controls, although these effects are not significant at the .10 level.[29]

The results in Table 6 also indicate that the effects of Treatment A on the hourly wage rate that were observed for the first quarter after filing the claim dissipate when examined over the complete year.[30] It is interesting to note that claimants in Treatment

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[28]We also examined data for wages and hours during the second complete year after entering the experiment. Because the results are very similar to those for the first year, we have chosen to focus on the short-term findings where we are more likely to observe any employment or wage effects.

[29]Although not reported in Table 6, we also examined whether certain groups of claimants responded differently to the treatments. Consistent with the results for UI benefits, these interaction results suggest that although there are no differences by age, race, or sex, there are some differences in impacts by treatment group by sex depending on marital status and the presence of children. Specifically, relative to controls, the effects of each treatment on the probability of working and total earnings during the year following the filing of the UI claim tend to be more positive (less negative) for married men (and men with children) than they are for unmarried men (and men without children). On the other hand, relative to controls, the effects of each treatment tend to be more negative (less positive) for married women (and women with children) than they are for unmarried women (and women without children).

[30]Similarly, there were no significant differences in the earnings or hourly wage rates of claimants across treatment groups during the second complete year after entry into the experiment.

Group A were particularly more likely to become re-employed with their prior employer than claimants in other groups. Thus, it appears that the lower cost of remaining on UI for Group A claimants extended their length of search, but also likely reduced their incentive to search intensively and resulted in them being more likely to return to their prior employer. This could, in part, account for the somewhat higher observed wage rate in the short term.

V CONCLUSIONS

In the previous sections, we have described the net impact results of the Washington Alternative Work Search Experiment. Below we briefly summarize the main findings and compare the results with those from other similar studies. We conclude by presenting some simple cost information to provide a rough indication of the cost-effectiveness of the treatments.

The impact results indicate that relative to the standard work search policy followed by many states in the past, the more intensive reemployment services offered in Treatment Group D resulted in reducing UI payments on average by about one-half of a week or about $70 per claimant. It appears that the impacts of this treatment in reducing UI spells are primarily due to raising the costs of remaining on UI, rather than through enhanced job search abilities. The finding that Group D leads to shorter UI spells is particularly strong for women without children, for white collar workers, and for claimants who reported they had been permanently laid off. There is also some indication that Group D reduces UI durations for African Americans, although the effect is not statistically significant due, in part, to the small sample sizes of minorities. There is no evidence that the relatively rapid reemployment of claimants in Group D occurs at the cost of lower earnings or hourly wage rates.

We also find that the exception-reporting treatment (Group A) resulted in a significant increase in UI outlays of approximately 3.3 weeks and $265 per claimant relative to the standard work-search approach followed in the control group. There is some evidence that claimants in Group A have higher hourly wage rates than controls in the short-term, which may be due to their being more likely to return to their previous employer. We do not find evidence of significant differences in UI or employment and

earnings outcomes between the standard work search group and the group assigned to ESD's new individualized work search policy.

Although no previous demonstrations have tested the effectiveness of an exception-reporting approach to work search, the results from Treatment Group D are quite consistent with the findings from earlier UI demonstrations. Of particular relevance are the Charleston Claimant Placement and Work Test Demonstration (Corson et.al., 1984) and the New Jersey Unemployment Insurance Reemployment Demonstration Project (Corson et.al., 1989). Both of these demonstrations tested whether intensive reemployment services that involved a job search workshop were effective approaches to reducing UI spells and total UI payments.[31] Compared to no work search assistance, these earlier demonstrations found average reductions in UI payments over the benefit year of roughly $50-100, and decreases in total number of weeks of UI paid of about one-half a week. Moreover, consistent with the evidence presented above, the earlier demonstrations identified the important role of the reporting requirement aspect of the approach to reducing UI spells and total payments, and not improved job search abilities. These findings are remarkably consistent with the results described above.

Thus, these studies taken together provide strong evidence that a work search policy that requires claimants to report to the local office for a set of intensive services early in the unemployment spell is successful in reducing the length of the initial spell and total UI benefits paid. Whether such a policy is desirable, however, depends on a comparison of the reduction in UI benefits due to the treatment -- roughly $70 per claimant -- with the additional administrative costs associated with the treatment. Based on detailed information concerning the costs of each of the activities that differed among treatment

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[31]Similar to the experiment implemented in Washington, the workshop in both of these demonstrations was scheduled to occur after claimants had drawn UI for four or five weeks. However, it should be noted that the workshop in the New Jersey demonstration lasted a full week, as compared to three hours for the Charleston demonstration.

groups (i.e., intake, continued claims processing, adjustment, work search counseling and placement), we estimate that the additional cost per claimant in Treatment Group D, relative to the control group, was $14.50.[32] This suggests that Treatment D is a very cost-effective approach to work search policy, with a net reduction in UI costs per claimant of over $50.

We also developed estimates of the net additional cost per claimant for Treatment Groups A and C. These results indicate that the cost per claimant for Treatment C was about $2.00 higher, and that Treatment A was about $0.50 higher, than for the control group.[33] Because the exception-reporting treatment greatly increases UI payments and incurs increased administrative costs, it has strong adverse consequences for the UI Trust Fund and is clearly not a desirable work search policy.

A final question concerns the cost-effectiveness of the individualized work search policy (i.e., Treatment Group C) that was implemented in other Job Service Centers in Washington State in the mid-1980s. Based on the net impact findings described in this report, there is little evidence indicating that the new work search policy leads to reduced UI benefit payments or improved wage rates or earnings. On the other hand, the increased administrative costs of Treatment Group C relative to the control group of $2.00 per claimant are reasonably modest. Moreover, the increased costs are due almost entirely to the greater number of appeals processed for Group C claimants, and because appeals are a very rare event, our estimate of increased administrative costs must be viewed with caution.

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[32]The methodology for the cost analysis is described in ESD's "A Report on the Alternative Work Search Experiment" prepared by Lloyd Williams. The specific cost estimates reported here are based on the same methodology, but use updated information concerning the frequency of specific activities during the demonstration.

[33]Despite the lack of work search services for Treatment Group A, this group incurred a higher per claimant cost than the control group because of higher costs for processing continued claims. This is primarily due to the costs of the dedicated phone line, the greater number of returned warrants, and the greater number of weeks claimed by those in Group A.

Thus, the incremental costs incurred from implementing the new work search policy appear to be quite small and an assessment of the overall effectiveness of the approach would have to take into account any non-monetary gains that accrue to claimants from a more individualized and humane approach to work search policy.

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APPENDIX A

SUPPLEMENTARY TABLES

Table A-1 (continued)

Definitions of Independent Variables Included in Basic Regression Model

Variable	Definition
Hours worked in Year -2	Total hours worked in covered employment in the 5th to the 8th complete quarters before filing for benefits
Hours worked in Year -1	Total hours worked in covered employment in the 1st to the 4th complete quarters before filing for benefits
Clerical dummy	1 if pre-UI occupation was clerical; 0 otherwise
Sales dummy	1 if pre-UI occupation was sales; 0 otherwise
Service dummy	1 if pre-UI occupation was service; 0 otherwise
Agricultural, fishery, forestry or related occupations dummy	1 if pre-UI occupation was in agricultural, fishery, forestry or related occupations; 0 otherwise
Processing dummy	1 if pre-UI occupation was in processing; 0 otherwise
Machine trades dummy	1 if pre-UI occupation was in machine trades; 0 otherwise
Benchwork dummy	1 if pre-UI occupation was in benchwork; 0 otherwise
Structural work dummy	1 if pre-UI occupation was in structural work; 0 otherwise
Miscellaneous occupations	1 if pre-UI occupation was miscellaneous; 0 otherwise
Extractive and transformative dummy	1 if pre-UI employer was in extractive or transformative sector (i.e., agriculture, mining, construction or manufacturing); 0 otherwise

Table A-1

Definitions of Independent Variables Included in Basic Regression Model

Variable	Definition
Male dummy	1 if male; 0 otherwise
Black dummy	1 if black; 0 otherwise
Other minority race dummy	1 if a non-black minority; 0 otherwise
Age 25-34 dummy	1 if age 25-34; 0 otherwise
Age 35-44 dummy	1 if age 35-44; 0 otherwise
Age 45-54 dummy	1 if age 45-54; 0 otherwise
Age 55 plus dummy	1 if age 55 or older; 0 otherwise
High school graduate dummy	1 if highest grade completed is 12; 0 otherwise
Some college dummy	1 if highest grade completed is 13-15; 0 otherwise
College graduate dummy	1 if highest grade completed is 16 or more; 0 otherwise
Veteran dummy	1 if veteran; 0 otherwise
Earnings in Year -3	Total earnings in covered employment in the 9th to the 12th complete quarters before filing for benefits
Earnings in Year -2	Total earnings in covered employment in the 5th to the 8th complete quarters before filing for benefits
Earnings in Year -1	Total earnings in covered employment in the 1st to the 4th complete quarters before filing for benefits
Hours worked in Year -3	Total hours worked in covered employment in the 9th to the 12th complete quarters before filing for benefits

Table A-1 (continued)

Definitions of Independent Variables Included in Basic Regression Model

Variable	Definition
Distributive dummy	1 if pre-UI employer was in distributive sector (e.g., transportation, communications, utilities and wholesale trade); 0 otherwise
Non-profit dummy	1 if pre-UI employer was in non-profit sector; 0 otherwise
Producer services dummy	1 if pre-UI employer was in producer services sector; 0 otherwise
Consumer services dummy	1 if pre-UI employer was in consumer services sector; 0 otherwise
Government dummy	1 if pre-UI employer was in the government or public administration sector; 0 otherwise
Weekly benefit amount	Maximum weekly UI payment claimant could receive
Maximum number of weeks of UI benefits payable	Maximum UI benefits payable divided by the weekly benefit amount
Federal employee claim dummy	1 if UCFE claim; 0 otherwise
Military claim dummy	1 if UCX claim; 0 otherwise
Combined wage claim dummy	1 if claim involved wages from more than one state; 0 otherwise
Full-referral union dummy	1 if a member of a full-referral union; 0 otherwise
Qualified-referral union dummy	1 if a member of a qualified-referral union; 0 otherwise

Table A-1 (continued)

Definitions of Independent Variables Included in Basic Regression Model

Variable	Definition
Standby dummy	1 if on standby (i.e., temporary layoff and expect to be recalled); 0 otherwise
Unemployment rate	Unemployment rate in Pierce County three months after claim filed (i.e., at about mean duration of spell)
Summer 1986 dummy	1 if claim filed during the summer of 1986; 0 otherwise
Fall 1986 dummy	1 if claim filed during the fall of 1986; 0 otherwise
Winter 1987 dummy	1 if claim filed during the winter of 1987; 0 otherwise
Spring 1987 dummy	1 if claim filed during the spring of 1987; 0 otherwise
Treatment A dummy	1 if member of Treatment Group A; 0 otherwise
Treatment C dummy	1 if member of Treatment Group C; 0 otherwise
Treatment D dummy	1 if member of Treatment Group D; 0 otherwise

Table A-2

Means and Standard Deviations of Independent Variables Included in the Basic Regression Model

Variable	Mean	Standard Deviation
Male dummy	0.701	0.458
Black dummy	0.098	0.297
Other minority race dummy	0.072	0.258
Age 25-34 dummy	0.386	0.487
Age 35-44 dummy	0.234	0.423
Age 45-54 dummy	0.113	0.317
Age 55 plus dummy	0.061	0.240
High school graduate dummy	0.536	0.499
Some college dummy	0.244	0.430
College graduate dummy	0.086	0.281
Veteran dummy	0.238	0.426
Earnings in Year -3 ($)	10,147	10,743
Earnings in Year -2 ($)	11,769	10,302
Earnings in Year -1 ($)	14,511	9,232
Hours worked in Year -3	992	807
Hours worked in Year -2	1,202	730
Hours worked in Year -1	1,540	572
Clerical dummy	0.135	0.341
Sales dummy	0.055	0.227
Service dummy	0.116	0.320
Agriculture, fishery, forestry or related occupations dummy	0.020	0.141
Processing dummy	0.033	0.179
Machine trades dummy	0.081	0.273
Benchwork dummy	0.045	0.208
Structural work dummy	0.261	0.439

Table A-2 (continued)

Means and Standard Deviations of Independent Variables Included in the Basic Regression Model

Variable	Mean	Standard Deviation
Miscellaneous occupations dummy	0.130	0.337
Extractive and transformative dummy	0.430	0.493
Distributive dummy	0.104	0.304
Non-profit dummy	0.051	0.218
Producer services dummy	0.105	0.305
Consumer services dummy	0.058	0.233
Government dummy	0.068	0.250
Weekly benefit amount ($)	146	50.5
Maximum number of weeks of UI benefits payable	25.9	5.11
Federal employee claim dummy	0.019	0.135
Military claim dummy	0.039	0.194
Combined wage claim dummy	0.051	0.220
Full-referral union dummy	0.191	0.393
Qualified-referral union dummy	0.012	0.111
Standby dummy	0.087	0.282
Unemployment rate	8.4	0.71
Summer 1986 dummy	0.176	0.381
Fall 1986 dummy	0.224	0.417
Winter 1987 dummy	0.249	0.433
Spring 1987 dummy	0.131	0.338
Treatment A dummy	0.233	0.423
Treatment C dummy	0.204	0.403
Treatment D dummy	0.265	0.441

Table A-3

Regression Estimates for Key UI Outcome Measures
(Standard Errors in Parentheses)

Variable	Total UI Benefits	Length of First UI Spell	Likelihood of Exhaustion[a]
Male dummy	62.4	0.331	-0.047
	(43.4)	(0.269)	(0.039)
Black dummy	104.8*	0.576*	-0.133**
	(54.5)	(0.338)	(0.048)
Other minority race dummy	111.0*	0.208	-0.104*
	(62.3)	(0.386)	(0.055)
Age 25-34 dummy	139.8**	1.262**	-0.213**
	(45.1)	(0.279)	(0.041)
Age 35-44 dummy	337.4**	2.582**	-0.337**
	(52.0)	(0.322)	(0.047)
Age 45-54 dummy	465.4**	2.854**	-0.453**
	(63.2)	(0.391)	(0.057)
Age 55 plus dummy	461.7**	3.698**	-0.498**
	(76.9)	(0.476)	(0.068)
High school graduate dummy	-58.0	-0.484	0.070
	(48.9)	(0.303)	(0.043)
Some college dummy	-142.1**	-0.987**	0.146**
	(55.6)	(0.344)	(0.050)

Table A-3 (continued)

Regression Estimates for Key UI Outcome Measures
(Standard Errors in Parentheses)

Variable	Total UI Benefits	Length of First UI Spell	Likelihood of Exhaustion[a]
College graduate dummy	-239.8**	-1.588**	0.296**
	(75.9)	(0.470)	(0.068)
Veteran dummy	-99.3**	-0.304	0.079**
	(39.9)	(0.247)	(0.036)
Earnings in Year -3 (in $1,000)	-10.5**	-0.059**	0.012**
	(4.5)	(0.028)	(0.004)
Earnings in Year -2 (in $1,000)	0.6	-0.051	-0.004
	(5.2)	(0.032)	(0.005)
Earnings in Year -1 (in $1,000)	-12.0**	-0.074**	0.011**
	(4.2)	(0.026)	(0.004)
Hours worked in Year -3 (in 100s)	1.8	0.039	-0.005
	(4.8)	(0.030)	(0.004)
Hours worked in Year -2 (in 100s)	-10.0*	0.009	0.006
	(5.5)	(0.034)	(0.005)
Hours worked in Year -1 (in 100s)	-22.5**	-0.032	0.006
	(4.8)	(0.030)	(0.004)
Clerical dummy	16.0	0.303	-0.135**
	(67.9)	(0.421)	(0.060)

Table A-3 (continued)

Regression Estimates for Key UI Outcome Measures
(Standard Errors in Parentheses)

Variable	Total UI Benefits	Length of First UI Spell	Likelihood of Exhaustion[a]
Sales dummy	46.2	0.108	-0.140*
	(84.2)	(0.522)	(0.074)
Service dummy	-183.2**	-1.089**	-0.022
	(72.1)	(0.446)	(0.064)
Agricultural, fishery, forestry or related occupations dummy	-29.3	-0.788	-0.014
	(124.8)	(0.773)	(0.111)
Processing dummy	-432.7	-1.696**	0.116
	(104.7)	(0.648)	(0.096)
Machine trades dummy	-305.2**	-1.397**	0.113
	(78.9)	(0.488)	(0.072)
Benchwork dummy	-230.5**	-1.163**	-0.016
	(93.4)	(0.579)	(0.085)
Structural work dummy	-97.8	-1.653**	0.039
	(69.2)	(0.429)	(0.062)
Miscellaneous occupations dummy	-268.3**	-1.769**	0.077
	(71.5)	(0.443)	(0.064)
Extractive and transformative dummy	72.3	-0.132	-0.113**
	(52.7)	(0.326)	(0.048)

Table A-3 (continued)

Regression Estimates for Key UI Outcome Measures
(Standard Errors in Parentheses)

Variable	Total UI Benefits	Length of First UI Spell	Likelihood of Exhaustion[a]
Distributive dummy	71.6	0.002	-0.058
	(64.4)	(0.399)	(0.058)
Non-profit dummy	-28.5	-0.014	0.000
	(81.4)	(0.504)	(0.073)
Producer services dummy	149.4**	0.873**	-0.131**
	(62.2)	(0.385)	(0.055)
Consumer services dummy	-20.5	-0.116	0.072
	(75.7)	(0.469)	(0.070)
Government dummy	171.4*	1.585**	-0.259**
	(99.7)	(0.617)	(0.086)
Weekly benefit amount	19.3**	0.038**	-0.004**
	(0.5)	(0.003)	(0.0005)
Maximum number of weeks of UI benefits payable	47.0**	0.175**	0.036**
	(5.0)	(0.031)	(0.004)
Federal employee claim dummy	-11.3	0.041	-0.021
	(127.6)	(0.790)	(0.109)
Military claim dummy	-478.7**	-3.288**	0.108
	(143.8)	(0.891)	(0.124)

Table A-3 (continued)

Regression Estimates for Key UI Outcome Measures
(Standard Errors in Parentheses)

Variable	Total UI Benefits	Length of First UI Spell	Likelihood of Exhaustion[a]
Combined wage claim dummy	-44.9	-0.333	0.051
	(73.1)	(0.453)	(0.065)
Full-referral union dummy	-90.2*	-2.390**	0.204**
	(51.5)	(0.319)	(0.047)
Qualified-referral union dummy	-13.2	-0.042	0.049
	(143.0)	(0.886)	(0.128)
Standby dummy	-809.4**	-5.197**	0.640**
	(59.9)	(0.371)	(0.064)
Unemployment rate	-273.8**	-1.283**	0.135**
	(53.0)	(0.328)	(0.047)
Summer 1986 dummy	547.3**	2.616**	-0.327**
	(60.1)	(0.372)	(0.053)
Fall 1986 dummy	689.4**	3.905**	-0.350**
	(96.6)	(0.598)	(0.086)
Winter 1987 dummy	365.1**	2.336**	-0.115
	(92.0)	(0.570)	(0.082)
Spring 1987 dummy	-25.0	0.065	0.086
	(60.5)	(0.375)	(0.056)

Table A-3 (continued)

Regression Estimates for Key UI Outcome Measures
(Standard Errors in Parentheses)

Variable	Total UI Benefits	Length of First UI Spell	Likelihood of Exhaustion[a]
Treatment A dummy	264.6**	2.649**	-0.379**
	(44.2)	(0.273)	(0.039)
Treatment C dummy	4.8	0.082	-0.031
	(45.0)	(0.279)	(0.041)
Treatment D dummy	-67.6	-0.659**	0.020
	(42.5)	(0.263)	(0.039)
Constant	681.3	11.372**	-.789**
	(422.9)	(2.619)	(0.378)
R-Squared	.31	.11	--

[a]Probit estimates.
**Significant at .05 level.
*Significant at .10 level.

Table A-4

Treatment Effects for Various Subgroups

Treatment Group and Characteristic	Total UI Benefits	Length of First Spell
Race/Ethnicity		
Treatment A:		
White	269**	2.46**
Black	328**	3.86**
Other	106	3.04**
Treatment C:		
White	-3	0.08
Black	-89	-0.72
Other	208	1.08
Treatment D:		
White	-50	-0.67**
Black	-295**	-1.44*
Other	26	0.41
F-Test	1.71	1.52
Sex		
Treatment A:		
Male	295**	2.55**
Female	191**	2.90**
Treatment C:		
Male	-16	-0.04
Female	55	0.37
Treatment D:		
Male	-50	-0.44
Female	-109	-1.18**
F-Test	1.24	1.64

Table A-4 (continued)

Treatment Effects for Various Subgroups

Treatment Group and Characteristic	Total UI Benefits	Length of First Spell
Age		
Treatment A:		
Less than 24 years old	224**	3.33**
25-34	263**	2.20**
35-44	355**	2.44**
45-54	324**	2.97**
55 years old or older	-17	3.30**
Treatment C:		
Less than 24 years old	15	0.19
25-34	4	-0.43
35-44	47	0.30
45-54	-61	0.88
55 years old or older	-64	0.51
Treatment D:		
Less than 24 years old	-45	-0.41
25-34	-40	-0.84**
35-44	-72	-0.72
45-54	-193	-0.61
55 years old or older	-77	-0.35
F-Test	0.66	0.46
Marital Status and Sex[#]		
Treatment A:		
Married men	305**	1.91**
Unmarried men	350**	3.55**
Married women	369**	4.53**
Unmarried women	64	2.30**

Table A-4 (continued)

Treatment Effects for Various Subgroups

Treatment Group and Characteristic	Total UI Benefits	Length of First Spell
Treatment C:		
Married men	-118	-0.41
Unmarried men	-20	-0.32
Married women	283**	1.82**
Unmarried women	-63	-0.18
Treatment D:		
Married men	-55	-0.48
Unmarried men	-24	-0.86
Married women	6	0.09
Unmarried women	-187*	-2.31**
F-Test	2.25**	3.74**
Sex and Presence of Children		
Treatment A:		
Men with children	218**	1.32*
Men without children	373**	3.31**
Women with children	455**	3.44**
Women without children	109	3.10**
Treatment C:		
Men with children	-177	-0.99
Men without children	-27	-0.14
Women with children	544**	2.55**
Women without children	-6	0.32
Treatment D:		
Men with children	-122	-1.01
Men without children	-16	-0.58
Women with children	208	-0.08
Women without children	-182**	-1.60**
F-Test	4.28**	4.66**

Table A-4 (continued)

Treatment Effects for Various Subgroups

Treatment Group and Characteristic	Total UI Benefits	Length of First Spell
Occupation Group		
Treatment A:		
Blue collar	282**	2.44**
White collar	241**	2.97**
Treatment C:		
Blue collar	-33	0.08
White collar	56	0.09
Treatment D:		
Blue collar	-36	-0.15
White collar	-107	-1.31**
F-Test	1.07	3.47**
Reason for Leaving Prior Job#		
Treatment A:		
Plant closed	643**	5.62**
Job completed	153	1.56*
Seasonal layoff	243	2.75**
Temporary layoff and expect to return within 30 days	-23	-0.35
Temporary layoff and expect to return after 30 days	167	2.50*
Temporary layoff with no known recall date	250*	2.56**
Permanent layoff	312**	3.47**
Not due to lack of work	432**	4.18**

Table A-4 (continued)

Treatment Effects for Various Subgroups

Treatment Group and Characteristic	Total UI Benefits	Length of First Spell
Treatment C:		
Plant closed	-37	-0.47
Job completed	94	0.32
Seasonal layoff	-90	-0.26
Temporary layoff and expect to return within 30 days	-150	-0.54
Temporary layoff and expect to return after 30 days	-202	-0.18
Temporary layoff with no known recall date	-113	-0.75
Permanent layoff	-113	-0.09
Not due to lack of work	133	0.52
Treatment D:		
Plant closed	-58	-0.38
Job completed	-151	-0.39
Seasonal layoff	4	0.43
Temporary layoff and expect to return within 30 days	130	0.23
Temporary layoff and expect to return after 30 days	-236	-1.40
Temporary layoff with no known recall date	-69	-0.72
Permanent layoff	-299**	-2.70**
Not due to lack of work	50	-0.94*
F-Test	1.37	2.51**

**Significant at .05 level.
*Significant at .10 level.
#Indicates results based on subset of claimants who responded to the Baseline Survey.

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> NOTE: A public use data tape also is available from the Bureau of the Census. To obtain the tape contact Customer Services, Bureau of the Census, Washington, D.C. 20233 or telephone 301-763-4100; when requesting the public use tape cite: Current Population Survey, Unemployment Compensation Benefits: May, August and November 1989 and February 1990 (machine readable data file) conducted by the Bureau of the Census for the Employment and Training Administration, U.S. Department of Labor, Washington: Bureau of the Census (producer and distributor), 1990.

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