EFFECTS OF UNEMPLOYMENT INSURANCE WORK-SEARCH REQUIREMENTS: THE MARYLAND EXPERIMENT

DANIEL H. KLEPINGER, TERRY R. JOHNSON, and JUTTA M. JOESCH*

This paper describes findings from a 1994 experimental evaluation of alternative work-search requirements in the Unemployment Insurance (UI) program. Requiring additional employer contacts or verification of contacts reduced UI receipt by one week and $115 per claimant. Because these additional requirements did not entail additional re-employment services, the UI spell reduction can be attributed to increased non-monetary costs for remaining on UI. A job-search workshop requirement reduced UI receipt by half a week and $75 per claimant, and additional results indicate that the effects were due to increased costs of continued UI receipt rather than to enhanced job-search productivity. These treatments did not affect employment or earnings, implying that reduced UI duration led to more intensive job search, rather than a reduction in the reservation wage. In contrast, elimination of the employer contact requirement increased UI receipt and post-UI earnings, suggesting that delayed exit from UI improved job matches.

The Unemployment Insurance (UI) program is designed to provide temporary income support to involuntarily unemployed individuals while they search for work. Although the UI program provides only temporary income support for the involuntarily unemployed, it can reduce the incentive to seek employment because UI benefits reduce the cost of being unemployed, resulting in an increase in the reservation wage and longer spells of unemployment. To partially offset the negative impact UI benefits have on job search, state UI programs typically impose work-search requirements for continued benefit receipt.

Work-search requirements increase job search in two ways. First, they can increase job search intensity by inducing claimants to make more job contacts than they would have made in the absence of requirements. In general, greater intensity of job search will result in more rapid re-employment. Second, work-search requirements may raise the non-monetary cost of continued receipt of UI benefits if claimants perceive the requirements as a burden. The increased cost of continued benefit receipt lowers the utility of UI program participation, relative to working, resulting in more

*Daniel H. Klepinger is Senior Research Economist, Terry R. Johnson is Vice President, and Jutta M. Joesch is Research Scientist, all at the Battelle Memorial Institute–Seattle.

Subject to the Department of Labor's consent, a data appendix with additional results will be made available to interested readers by the first author at Battelle Centers, 4500 Sand Point Way N.E., Suite 100, Seattle, WA 98105-3949.

Industrial and Labor Relations Review, Vol. 56, No. 1 (October 2002).
0019-7939/00/5601 $01.00

intensive job search, a reduction in the reservation wage, or both.

Although there is still considerable variation in the stringency of work-search requirements across states, over the past two decades there has been a strong trend toward reducing the stringency of work-search requirements to reduce administrative costs (Blaustein et al. 1997:31–32). Despite evidence that more intensive reemployment services are a cost-effective way to reduce claimants' duration of unemployment (Corson et al. 1984; Corson et al. 1989; Johnson and Klepinger 1994), the trend toward reducing the stringency of work-search requirements has been made with relatively little information on the efficacy of alternative work-search requirements. The only prior demonstration that has directly tested the effectiveness of alternative work-search policies is the Washington Alternative Work-Search Experiment (see Johnson and Klepinger 1994). That study provided strong evidence that a very weak work-search policy increases UI outlays.

Prior demonstrations have tended to combine work-search requirements with reemployment services in the same treatment, making it difficult to determine whether the effect of a treatment was due to increased work-search productivity or to increased cost of remaining on UI (Meyer 1995). For this same reason, prior demonstrations have been unable to quantify the costs to claimants of additional work-search requirements. Moreover, prior demonstrations have not examined whether additional work-search requirements are primarily borne by those not otherwise actively searching for work (Meyer 1995). This issue is of considerable importance because the purpose of work-search requirements is to increase job search intensity of claimants who are not seeking work with sufficient intensity, rather than burden those who are actively seeking work. Finally, no prior demonstration has examined whether informing individuals that they are participating in a study changes their behavior (the Hawthorne effect), an issue that potentially affects the validity of all prior demonstration results.

To help fill these gaps in the literature, we present findings from the Maryland UI Work-Search Demonstration, a large-scale experimental evaluation project testing the effectiveness of several alternative work-search policies. The Demonstration includes work-search treatments that were not combined with reemployment services, a treatment targeted to claimants who were not actively seeking employment, and two control groups to test whether the demonstration resulted in a Hawthorne effect.

Design of the Work-Search Experiment

The Maryland UI Work-Search Demonstration was aimed at assessing the impact of alternative work-search policies in the UI program. The demonstration was designed as a classical experiment, in which claimants were randomly assigned to one of four treatment groups, each representing a different work-search policy. In addition, two control groups were included, only one of which was informed of the demonstration, to test whether there is a significant Hawthorne effect. The experiment was implemented in six UI offices in Maryland in January 1994. Approximately 27,000 new UI claimants were randomly assigned to one of the treatment or control groups during the one-year enrollment period. Because random assignment implicitly controls for any differences between groups in observed or unobserved characteristics, differences in outcomes among the groups can be reliably attributed to the treatments.

Claims-takers informed treatment group members and members of the informed control group about the demonstration when claimants applied for UI benefits. Claimants in the treatment groups were informed of their special work-search requirements at this time. Separate claims applications describing the work-search requirements were created for each treatment group to further inform demonstration claimants of their work-search and other demonstration requirements. Within one week after the initial claim, demonstration treatment group members also received

a letter informing them of the demonstration and their work-search requirements. The work-search and reemployment services were delivered to claimants in each of the treatment groups by local Job Service (Employment Services and UI) staff.

Description of Treatments

The standard work-search policy in Maryland requires claimants to make two employer contacts per week, and to report those contacts in order to receive UI benefit payments. This policy does not include verification of employer contacts and does not provide any additional work-search services. The demonstration tested four approaches that modified various aspects of the standard work-search policy: requiring two additional employer contacts; eliminating the requirement of reporting employer contacts; providing reemployment services through a mandatory job search workshop; and verifying employer contacts.

Additional Required Employer Contacts: Treatment Group (A)

Claimants in this treatment group were instructed to make four employer contacts per week, instead of the standard two contacts. They were required to submit a form listing the four employer contacts in order to receive UI benefits. Claimants were informed that failure to make four work-search contacts in a given week could result in a loss of UI benefits for that week.

Elimination of the Requirement to Report Work-Search Contacts: Treatment Group (B)

Claimants assigned to this treatment group were told to actively search for work, but unlike under the standard policy, they were not required to report their specific employer contacts each week. To receive UI payments, however, they did need to inform the UI office by mail that they had not found employment and that they were actively looking for work.

Job Search Workshop: Treatment Group (C)

Claimants in this treatment followed the standard work-search requirement of two documented work-search contacts. In addition, they were required to attend a four-day job search workshop for a total of sixteen hours. In addition to being informed of the workshop requirement when they applied for benefits, claimants in this treatment were sent a letter following receipt of the first UI payment instructing them to report for the workshop. The letter also informed them that failure to complete the workshop could result in loss of UI benefits for that and subsequent weeks. The workshops occurred early in claimants' UI spells (over 70% of claimants who attended a workshop did so during the fifth through the seventh week after filing for benefits). The workshop consisted of three parts: (a) instruction in assessing employment options, setting realistic job goals, and identifying employment resources; (b) instruction in how to prepare resumes and job applications, and practice in telephone contacts and personal interviews; and (c) individual help in planning a job search strategy.

Verified Work-Search Contacts: Treatment Group (D)

Claimants in this treatment followed the standard work-search requirements of two documented job search contacts per week, but were told that their employer contacts would be verified. They were required to provide the names and telephone numbers of the employers they contacted, and were informed that if they failed to provide this information, or provided false information, they could lose their UI benefits for that week. Among those selected for verification, UI staff telephoned the employers listed to verify that the claimant had contacted them about employment.

Informed Control Group (E)

Claimants in this control group followed the standard work-search requirements of two documented work-search contacts.

They were told they were participating in the demonstration and that information from their UI records would be used in the study. This control group was included to examine whether knowing that they were part of a demonstration in and of itself would alter claimants' UI behavior (that is, to test for a Hawthorne effect).

Uninformed Control Group (F)

Claimants in this control group followed the standard work-search requirements of two documented work-search contacts each week. They were not told about the demonstration.

Hypotheses on Treatment Effects

Work-search requirements are intended to reduce duration on UI. To the extent that such requirements induce claimants to make more job contacts than they otherwise would, the increase in job search intensity can be expected to result in more rapid re-employment. In addition, if claimants perceive the requirements as burdensome, work-search requirements will raise the non-monetary costs of receiving UI benefits. The increased cost of continued benefits receipt lowers the utility of UI program participation, relative to working, and should lead to more intensive job search, a reduction in the reservation wage, or both. Below we summarize the hypothesized effects for the Maryland UI Work-Search Demonstration treatments.

By requiring additional employer contacts, Treatment A will lead to greater intensity of job search, a reduction in the reservation wage, or both, and it is expected to lower UI receipt. Because no additional services are provided as part of this treatment, any effect associated with it will be due to the increased employer contacts requirement, rather than more efficient job search that might arise if additional reemployment services were provided as part of the treatment. For this reason, comparing outcomes for claimants in this treatment with controls provides unambiguous estimates of the effects of the additional work-search requirement associated with this treatment. Moreover, the estimated impact of this treatment on UI receipt also provides an empirical estimate of the monetary equivalent value UI claimants place on the non-monetary costs associated with additional work-search requirements—forgone UI benefits.

Similarly, employer verification in Treatment D is also expected to increase the intensity of job search or reduce the reservation wage (or both), thus lowering UI receipt. Since the added costs associated with providing employer verification information are relatively small for those actively seeking work, the real burden associated with this treatment will be borne primarily by claimants who are not actively searching for work (that is, claimants who are at risk of losing their benefits if they are caught giving false contact information). For this reason, the impact of this treatment provides an estimate of the additional costs associated with increased work-search requirements that are borne primarily by claimants who are not actively searching for work.

In contrast, the elimination of the requirement to report work-search contacts in Treatment B is expected to result in less intensive job search or a rise in the reservation wage (or both), and greater UI receipt. The primary difference between this treatment and the exception-reporting treatment in the Washington Alternative Work-Search Experiment is that the treatment employed in that study also included automatic payment of benefits, rather than a requirement that claimants inform the UI office of their employment status.[1] Consequently, a comparison of the impact of Treatment B with that of the exception-

[1]The work-search treatment tested in the Washington Experiment was essentially an "exception-reporting" approach that involved no work-search monitoring whatsoever and included an automatic payment. That is, claimants received a benefit payment *unless* they called in to report that they had returned to work or were not eligible for benefits that week.

reporting treatment used in the Washington Alternative Work-Search Experiment will help determine whether the large impact found in the latter study is due to reduced employer contact requirements or to the automatic payment of benefits.

The workshop in Treatment C is expected to lower UI receipt. This effect may arise because the workshop increases the efficiency of work-search, or because the requirement to attend the workshop is viewed as burdensome and increases the costs of continued UI receipt, or both. If the workshop is associated with reduced UI receipt, additional analyses will be required to determine whether it is due to increased work-search ability or increased costs of UI receipt.

The only difference between Treatment E and Treatment F is that claimants in Treatment E were informed of the demonstration and claimants in Treatment F were not. Thus, comparing outcomes for these two treatments provides a direct test of the Hawthorne effect. To date, no UI demonstration has tested for the presence of that effect.

Finally, in addition to examining the effects of the treatments on UI outcomes, it is also important to consider employment and earnings effects. To the extent that the treatments affect UI receipt, they may also affect the quality of the job obtained if more stringent work-search requirements result in a reduction in the reservation wage and claimants accept less desirable jobs and lower earnings.

Site, Sample Selection, and Sample Characteristics

The objective of the site and sample selection was to ensure that the results of the demonstration could be generalized to the State of Maryland as a whole. The experiment was implemented in five sites (six UI offices) selected to provide a range of environments representative of the state, taking into account geography and local labor market conditions. 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 in the hiring practices of employers in different industries. Below, we provide additional details of the site and sample selection.

To select the sites, all local UI offices were stratified into five broad geographical areas. Demonstration budgetary and implementation constraints required the elimination of small, rural offices, and of offices with unique operational circumstances. These criteria eliminated 5 of the 26 full-service UI offices in the state. This process yielded 18 potential sites for the demonstration. Sites were then randomly selected with a site's probability of selection proportional to its size (the number of new UI claimants in the prior year). This approach yields a self-weighting sample when equal numbers of cases are selected from each site. The selected sites represent approximately 38% of the claimants in the state.

To avoid confounding the effects of old and new work-search policies, the demonstration was limited to new UI claimants who filed an initial claim for a new benefit year during 1994; individuals filing attached or partial claims were excluded. Because the objective of the demonstration was to test alternative work-search policies for claimants who would normally be required to search for work, new claimants who did not have a work-search requirement were also excluded.[2] Finally, bulk layoffs were excluded because of the unique administrative procedures involved with mass layoffs.

With the exceptions mentioned above, all monetarily eligible individuals who filed a valid new initial claim in the five sites between January 1, 1994, and December 31, 1994, were enrolled—a total of 23,758

[2]Thus, interstate claimants, claimants in the Work Share program, claimants who are required to find work through a union hiring hall, claimants on temporary layoff subject to recall by their employer, those on temporary layoff who expected recall within ten weeks, and those in approved agency-training programs were excluded.

individuals.[3] Each monetarily eligible claimant was randomly assigned to one of the four treatment groups or one of the two control groups. Random assignment was based on the last two digits of the person's Social Security Account (SSA) number. Because the last two digits of the SSA are random numbers, the use of such assignment methods ensures that the characteristics of individuals in each of the six groups are similar on average.

The assignment process was altered by a decision not to implement the treatment requiring additional employer contacts (Treatment A) in two of the sites because there were too few potential employers in these sites for claimants to be able to make four job contacts a week without contacting the same employers week after week. In addition, as shown in Table 1, a smaller proportion of claimants in sites without assignment to Treatment A were assigned to the control groups than were assigned to the control groups in the other sites.

If sites were homogeneous with respect to the type of claimants served, these deviations in treatment design across sites would not be important. However, there are site differences in the racial and income distribution of claimants. Specifically, the proportion of claimants who are black was higher in the sites where assignments were made to Treatment A than in sites without such assignments. Consequently, as shown in Table 1, the proportion of claimants who are black is higher in Treatment A and lower in Treatments B–D, relative to the control groups. Claimants in Treatment A also had higher earnings in the year prior to filing for UI benefits and higher Maximum Benefit Amounts because they were more likely, on average, to come from the Baltimore site.

Because of these treatment differences in claimant characteristics, we considered restricting the analysis sample to the sites that included assignment to Treatment A. Characteristics of the claimants in this restricted sample are displayed in the bottom panel of Table 1. The only statistically significant differences are a slightly higher proportion of claimants in Treatment C who are from site 4 and a slightly smaller proportion who are from site 2, relative to controls, and a slightly higher proportion of claimants in Treatment B who are black, again relative to controls. There are no statistically significant differences in age, gender, maximum benefit amount, or prior earnings.

These results indicate that the randomization process was successful in the three sites that included assignment to Treatment A, and that, therefore, treatment effects using this restricted sample can be estimated by mean differences. As an alternative, we also estimated treatment effects using multivariate models that included measures of claimant characteristics and site (see "Treatment Effects on UI and Employment Outcomes," below, for details). Estimates of treatment effects obtained from these models are nearly identical to differences of means calculated from the restricted sample of sites that included assignment to Treatment A, as shown in the Appendix table.[4] Furthermore, additional

[3]An additional 3,456 individuals applied for UI benefits at the five sites during the demonstration year but were ruled to be monetarily ineligible. Although random assignment ensures that the generalization of the results would not be affected by including these claimants in the analysis, we exclude them in order to focus on the effects of the treatments on new claimants eligible for benefits.

[4]Results in the Appendix table also show that estimates of net effects based on the full sample (differences of means) differ substantially both from those based on the restricted sample (differences of means) and from those based on the models that employ the full sample with controls for claimant and site differences (regression parameter estimates).

In the three instances where the results based on the restricted sample differ qualitatively from the multivariate results based on the full sample, the point estimates are quite similar. However, the standard errors are larger for the restricted (smaller) sample and the estimated effects are not statistically significant at conventional levels.

analyses (not shown) indicate that the estimated treatment effects do not vary by site, prior earnings, or race and ethnicity.[5] These results strongly suggest that the inclusion of these terms controls for race, prior earnings, and site differences that are the result of the restrictions imposed on the treatment assignment method, and that the models employing these control variables yield unbiased estimates of the treatment effects. The models that employ the full sample and include controls for claimant and site differences also produce more efficient estimates than simple differences of means calculated over the restricted sample of sites that included assignment to Treatment A. For this reason, we focus on estimates obtained from the models that use the full sample and include controls for claimant and site differences.

The figures in Table 1 also provide background information on the characteristics of demonstration participants, which is useful for understanding the population of claimants served in the demonstration. About 55% of the claimants in the sample are male and slightly over 50% are white. The claimants in the sample average approximately 35 years of age, with about 30% being 45 years of age or older. Over 95% are U.S. citizens. In terms of prior work experience, claimants enrolled in the experiment earned an average of about $16,000 during the four complete quarters prior to filing their claim. The mean weekly UI benefit amount for our sample is $169 and the average maximum benefit payable is $4,385.

Data Sources

Data for the evaluation come from Maryland State administrative data systems, and from a customized tracking system provided by the Department of Labor to monitor the demonstration activities. The primary data source contains information on UI claimants (for example, age, race, and sex), UI eligibility information (claim type, weekly benefit amount, maximum benefits payable), 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 UI outcomes (for example, total weeks paid, total benefits received), claims information was provided for each of the 52 weeks of the benefit year to develop reliable measures of spells of UI benefit receipt.

In addition, quarterly wage information was obtained from UI wage records for the four quarters prior to the quarter in which each person filed the claim and entered the experiment, and for the four quarters after the quarter in which the UI claim was filed.[6] These data were used to construct key outcome measures of employment and earnings, as well as control variables, and enabled us to determine whether claimants returned to work with their previous employer. Finally, we obtained detailed information on workshop participation, employer contact verification, and supplemental employer contacts from the demonstration tracking system.

Work-Search Requirement Compliance and Services

In interpreting the effects of the various treatments, it is important to understand

[5]F-tests indicate that the treatment effects do not vary significantly by site. The only individually statistically significant term was for Treatment B in one site for number of weeks claimants received benefits. A more informal review of the site-specific results indicates that only the results for Treatment B show appreciable differences by site, suggesting that there may have been site differences in implementation of this treatment or in claimants' expectations regarding the treatment.

[6]Although the use of UI wage records has a number of advantages for the evaluation, it must be recognized that these data only include wages in covered employment and do not include wages from other states. These coverage gaps are relatively small, however, and nearly 90% of all state wages are included in UI wage records (Baj and Trott 1991).

Table 1. Selected Claimant Characteristics by Treatment Group and Sample.

Description	*Treatment Groups*				*Control Groups*	
	A	*B*	*C*	*D*	*E*	*F*
Full Sample						
Sample						
Size	3,510	3,455	3,680	3,400	4,812	4,901
% from Site 1	32.3**	20.8**	19.3**	21.1**	23.6	24.5
% from Site 2	32.8**	21.9**	18.5**	20.9**	25.8	25.8
% from Site 3	0.0	11.8**	20.7**	12.6**	8.4	8.9
% from Site 4	34.9**	23.6**	22.0**	23.0**	26.1	25.5
% from Site 5	0.0	21.5**	19.3**	22.3**	15.8	15.1
Claimant Characteristics						
% Male	55.6	53.8	54.5	55.1	55.0	55.1
% White	41.2**	53.8**	56.2**	53.1**	50.3	50.0
% Black	54.0**	42.4**	40.5**	42.9**	45.9	45.9
Age (in years)	35.3	35.6	35.3	35.5	35.6	35.5
Earnings in Prior Year ($)[a]	17,294**	15,916	15,644	15,667	16,302	16,015
Maximum Benefit Amount ($)[a]	4,523**	4,347	4,318	4,346	4,408	4,365
Excluding Sites without Assignment to Treatment A						
Sample						
Size	3,510	2,304	2,208	2,215	3,646	3,721
% from Site 1	32.3	31.3	32.2	32.5	31.2	32.3
% from Site 2	32.8	32.9	30.8**	32.1	34.0	34.0
% from Site 4	34.9	35.4	36.7**	35.3	34.4	33.5
Claimant Characteristics						
% Male	55.6	55.9	57.2	56.9	56.1	55.9
% White	41.2	43.8*	40.1	41.6	42.4	41.5
% Black	54.0	51.0*	54.8	52.4	52.7	53.4
Age (in years)	35.3	35.7	35.4	35.8	35.5	35.6
Earnings in Prior Year ($)	17,294	17,015	16,742	16,818	17,165	16,866
Maximum Benefit Amount ($)	4,523	4,465	4,480	4,517	4,526	4,468

*Significantly different from Control Group F at the .10 level; **at the .05 level.
[a]Dollar figures are reported in fourth quarter 1995 dollars.

the extent to which claimants received employment services and adhered to the more stringent work-search directives. Approximately 50% of the claimants in the job-search workshop treatment (Treatment C) were required to attend the workshop. The remaining claimants were not expected to attend, either because they did not receive UI benefits or because they were excused from the workshop by local UI staff.[7]

Of those claimants who were told to attend the workshop, about 60% actually attended. Overall, about 30% of claimants assigned to this treatment participated in the workshop. Although this figure may seem low, nearly one-third of the claimants in this treatment group did not receive a UI payment, and many others exited UI before their workshop took place, which was usually scheduled for the fifth through the seventh week of UI receipt. After adjusting for these factors, the overall attendance rate is approximately 63%, which is comparable to attendance rates in previous demonstrations.

In the employer verification treatment

[7]Claimants were excused from the workshop if they had attended a workshop in the past couple of years, if they had received similar training from an employer or other public source, or if the workshop they were scheduled to attend was overcrowded.

(Treatment D), the study design called for verification of about 20% of claims filed, half by random selection and half by UI staff identifying cases that looked suspect. The actual verification rate for the study was about 10%, and relatively few claimants were randomly selected for verification. Overall, almost half of the claimants were selected for verification at least once. About 30% of the verification attempts confirmed that a claimant had contacted the identified employers. In less than 1% of the verification attempts were UI staff able to document that a claimant had falsely reported an employer contact. For the remaining 70% of the verification attempts, UI staff were unable to determine whether the claimant had actually contacted the identified employer.[8] Because of the relatively short duration of the study, claimants are unlikely to have been aware of the low verification rates. For this reason, estimated effects of this treatment are probably not unduly affected by the low verification rates.

In summary, the treatments were implemented successfully and claimants complied with the work-search directives. Workshop attendance was comparable to that in prior demonstrations, and compliance among claimants directed to make two additional employer contacts each week was very high. The only exception was in the employer verification treatment, as noted above.

Treatment Effects on UI and Employment Outcomes

In this section, we examine the effects of the various treatments. First, we examine whether or not the demonstration exhibits a Hawthorne effect on claimants' UI and employment decisions. We then present estimates of treatment effects on UI receipt. The section concludes with an examination of treatment effects on employment and earnings to determine whether the stringency of work-search requirements affects the quality of jobs that claimants accept when they leave the UI program.

In examining the effects of the treatments on UI benefits, we use three types of measures of benefit receipt. The first is based on the entire 52-week benefit year and includes the total dollar amount of UI benefits paid to claimants,[9] the number of weeks for which a payment was issued, and whether claimants exhausted their benefits. The other two types of measures are specific to the first two spells of UI receipt. These measures include whether a first (or second) spell of UI receipt occurs, length (that is, number of weeks) of the spell, and total UI benefits received during the spell.[10] We consider measures both of total UI receipt and of the first two spells of UI receipt to help distinguish between treatment effects that lead to temporary withdrawal from the UI rolls and effects that lead to longer-term effects. The earnings measures we employ are for earnings in covered employment during the first four complete quarters following the quarter during which a claimant filed for benefits.

[8]In most cases, an unverifiable attempt meant that no one answered the phone when UI staff called, or that employers contacted could neither confirm nor deny that the claimant applied.

[9]The measure used for total benefits includes small supplementary payments that some claimants received for children and overpayment amounts. Another measure of total benefits paid was available that eliminated both of these factors from the calculation of total benefits paid. It was not possible to remove one factor without removing both. We preferred using the measure that included supplemental payments and overpayments because overpayments are a cost to the UI system, and because this measure could be constructed on a week-by-week basis, which was needed for the spell analyses. A comparison of the two measures revealed that their means were very similar, and that estimated net effects were nearly identical. For this reason, and for comparability with the spell results, we report only the results for the measure of total benefits that includes supplemental payments and overpayments.

[10]These spells correspond to consecutive weeks of receipt of UI payments—not to spells of unemployment—as claimants can work part-time and still receive benefits.

As mentioned earlier, because of the treatment differences in claimant characteristics that resulted from the assignment method, and to improve the efficiency of the estimated effects, we estimated treatment effects using regression and logit regression models. The estimated net impact models include controls for age, sex, race/ethnicity, employment by a federal agency or the military, U.S. citizenship, earnings in each of the four quarters preceding the quarter a claimant applied for benefits, site, entry quarter, and maximum benefit amount. The results described below are based on the total sample of monetarily eligible new claimants enrolled in the demonstration. Thus, the effects can be interpreted as average effects over all eligible claimants, regardless of whether they actually adhered to the work-search requirements that were part of the treatments.

Hawthorne Effect

Decades of research have shown that people may change their behavior if they know they are participating in a research study. This effect is referred to as the Hawthorne effect. In UI demonstrations, the Hawthorne effect may arise if claimants alter their work-search behavior because they suspect that their work-search activities will be monitored more closely during a demonstration than they would have been in the absence of the demonstration. If knowledge of the demonstration does produce a Hawthorne effect, then results from prior demonstrations may not provide unbiased estimates of what would happen if a treatment were implemented state-wide. To date, no UI demonstration has tested for the existence of a Hawthorne effect. As such, this component of the study provides valuable information for interpreting results from prior demonstrations, and will assist in the cost-effective design of future demonstrations.

We test for a Hawthorne effect by comparing the estimated effects for the informed control group (Treatment E) with those for the uninformed control group (Treatment F). The results in Table 2 indicate that there is no observable Hawthorne effect on UI benefits or earnings. Nearly all of the t-values reported in Table 2 are less than unity, implying that none of the differences are close to being statistically significant.[11] The lack of any statistically significant differences in claimants' characteristics allows us to combine claimants in the two control groups into a single control group, increasing the precision of the estimates of the other treatments. All subsequent reported net impact results are based on the combined control group.

Effects on UI Benefit Receipt and Duration of UI Spell by Treatment Group

The treatment effects on UI benefits are presented in Table 3 as deviations from the combined control group. As indicated in the last column of this table, claimants in the control group received an average of $2,085 in total UI benefits during the benefit year. On average, these benefits were received for about 12 weeks of payments. About 90% of the payments were received during the first UI spell. Nearly 30% of the claimants in the control group exhausted the UI benefits available to them during the benefit year.

As expected, claimants required to make two additional employer contacts (Treatment A) received lower UI benefits, on average, than did claimants in the control group. As shown in the first column of Table 3, compared to claimants in the control group, Treatment A claimants received an average of $116 less in UI benefits dur-

[11]The only statistically significant difference between the two groups is that claimants in Treatment Group E received significantly greater overpayments (not shown) than did claimants in Treatment Group F. A reporting difference is a more likely explanation for this finding than is a difference in actual overpayments: claimants in the informed control group may have been more apt to notify the UI office of overpayments they received because they felt they were being monitored more closely.

Table 2. Tests for the Hawthorne Effect.[a]
(Standard Errors in Parentheses)

Outcome Measures	*Effects of Informed Control Group (E) Relative to Control Group (F)*	*Means of Uninformed Control Group (F)*
Full Benefit Year		
Total UI Benefits Paid ($)	−38 (40)	2,088 (31)
Number of Weeks of Benefits	−.18 (.22)	11.98 (.16)
Percent Exhausted Benefits[b]	−.03 (.05)	28.6 (.65)
First Spell		
Percent Who Received at Least One Payment[b]	−.05 (.05)	69.0 (.66)
Total UI Benefits Paid ($)	−37 (39)	1,899 (30)
Number of Weeks of Benefits	−.20 (.22)	10.9 (.16)
Second Spell		
Percent with Second Spell[b]	−.03 (.07)	15.0 (.62)
Total Benefits Paid ($)	−1 (21)	253 (14)
Number of Weeks	.02 (.12)	1.50 (.08)
First Quarter Employment		
Percent Worked[b]	.00 (.04)	55.9 (.71)
Earnings ($)	−50 (52)	1,654 (40)
Second Quarter Employment		
Percent Worked[b]	.04 (.04)	61.6 (.69)
Earnings ($)	−15 (55)	2,147 (42)
Third Quarter Employment		
Percent Worked[b]	.02 (.04)	64.1 (.69)
Earnings ($)	74 (57)	2,293 (40)
Fourth Quarter Employment		
Percent Worked[b]	.01 (.04)	62.8 (.69)
Earnings ($)	−42 (55)	2,292 (41)
Employment During Four Quarters		
Percent Worked[b]	−.02 (.05)	80.1 (.57)
Earnings ($)	−33 (179)	8,385 (139)
Returned to Work with Same Employer[b]	−.00 (.05)	17.1 (.54)

[a]Regression-based estimates include the following control variables: age, race and ethnicity, sex, citizenship, prior earnings, maximum benefit amount, federal employment status and military employment status, entry quarter, exemption status, and site.
[b]Logistic regression coefficients.
[c]Dollar figures are reported in fourth quarter 1995 dollars.

ing the benefit year, received UI benefits for 0.72 fewer weeks, and were also 2.5% less likely to exhaust their benefits. The estimated effects for this treatment are statistically significant. Because no additional services are provided as part of this treatment, the estimated effects of the treatment can be attributed to the added costs associated with making two additional employer contacts. Moreover, the forgone UI benefits can be interpreted as an estimate of the monetary equivalence value UI claimants place on having to make four rather than two employer contacts per week.

Claimants who were not required to report work-search contacts (Treatment B) received somewhat more UI benefits ($34) than did claimants in the control group, as expected, although this difference is not statistically significant at conventional levels. In addition, claimants in this treatment remained on UI for 0.36 weeks longer than controls, and were 1.5% more likely to exhaust their benefits. The latter two effects are significant at the .10 level.

The estimated effects for this treatment are considerably smaller than those found for a similar no-reporting-requirement

Table 3. Treatment Effects on UI Receipt.[a]
(Standard Errors in Parentheses)

	Treatment Groups				
Outcome Measures	*Additional Contacts (A)*	*No Reporting of Contacts (B)*	*Workshop (C)*	*Verify Contacts (D)*	*Control Group Means*
Full Benefit Year					
Total UI Benefits Paid ($)	−116 (39)**	34 (39)	−75 (38)**	−113 (39)**	2,085 (22)
Number of Weeks of Benefits	−.72 (.22)**	.36 (.22)*	−.59 (.21)**	−.86 (.22)**	11.94 (.12)
Percent Exhausted Benefits[b]	−2.51 (.84)**	1.51 (.86)*	−1.07 (.86)	−2.84 (.86)**	28.3 (.46)
First Spell					
Percent Who Received at Least One UI Payment[b]	−2.30 (.96)**	2.07 (.93)**	−1.70 (.94)*	−3.39 (.97)**	68.8 (.47)
Number of Weeks	−.83 (.21)**	.29 (.21)	−.80 (.21)**	−.86 (.21)**	10.87 (.11)
Total UI Benefits Paid ($)	−143 (38)**	14 (38)	−115 (37)**	−121 (38)**	1,894 (21)
Second Spell					
Percent with Second Spell[b]	1.10 (.92)	.16 (.84)	1.21 (.86)	.56 (.87)	15.0 (.31)
Number of Weeks	.15 (.12)	.11 (.12)	.32 (.12)**	.11 (.12)	1.05 (.04)
Total Benefits Paid ($)	38 (21)*	25 (21)	64 (21)**	24 (21)	254 (7)

*Significantly different from the control group at the .10 level; **at the .05 level.

[a]Regression-based estimates include the following control variables: age, race and ethnicity, sex, citizenship, prior earnings, maximum benefit amount, federal employment status and military employment status, entry quarter, exemption status, and site.

[b]Marginal effects (dF/dx) in percentages.

treatment in the Washington Alternative Work-Search Experiment study (Johnson and Klepinger 1994). That study found large positive effects on total benefits received ($265), weeks of UI receipt (3.3), and the percentage exhausting their benefits (12.5). The primary difference between the treatments in the two studies is that claimants in the Washington study received a check unless they informed the UI office of a change in their employment status, while claimants in this study did not receive a check unless they informed the UI office that their status had not changed. Thus, the smaller effects found in this study suggest that regular contact with the UI office greatly reduces the amount and duration of benefits received when reporting of work-search contacts is not required.

The workshop treatment (Treatment C) also had the expected negative impact on UI benefits. Overall, claimants in this treatment received $75 less in total UI benefits and received payments for 0.59 weeks less than did claimants in the control group. Although the point estimate indicates that claimants in this treatment group were slightly less likely to exhaust their benefits than were claimants in the control group, the effect is not statistically significant. The other estimated effects for this treatment are statistically significant.

As expected, claimants in the treatment that included employer contact verification (Treatment D) received fewer UI benefits on average than did claimants in the control group. As shown in the fourth column of Table 3, claimants in this treatment received $113 less in UI benefits dur-

ing the benefit year than controls did, received benefits for 0.86 fewer weeks, and were 2.8% less likely to exhaust their benefits. The estimated effects for this treatment are all statistically significant. Because the additional costs associated with supplying employer contact information are relatively trivial for claimants actively searching for work, we may conclude that the additional costs associated with verification were borne primarily by claimants who were not actively seeking work.

Turning to the spell results, the last column in Table 3 shows that nearly 70% of monetarily eligible claimants in the control group received at least one UI payment. First spells of UI lasted about 11 weeks, on average, across the entire sample, during which claimants received slightly less than $1,900 in UI payments. Claimants in the treatment requiring additional employer contacts (Treatment A) were significantly less likely (2.3%) to have received at least one payment than were claimants in the control group, and they received significantly lower UI benefits ($143) during their first spell, primarily because they received benefits for 0.83 fewer weeks. Similarly, claimants in the workshop treatment (Treatment C) were 1.7% less likely than controls (significant at the .10 level) to receive any payments, received significantly less in UI payments ($115) during their first spell, and drew benefits for 0.80 fewer weeks. Somewhat larger effects are observed for the employer verification treatment (Treatment D). Claimants in this treatment were 3.4% less likely than controls to have received any payment; they received about $121 less during their first spell, and drew benefits for 0.86 fewer weeks. In contrast, claimants in the treatment that did not require the reporting of employer contacts (Treatment B) were 2.1% more likely than controls to have initiated a UI spell.

As shown in the bottom portion of Table 3, the treatments had little impact on the likelihood of having a second spell of UI receipt during the benefit year or on UI benefits received during a second spell. The only exceptions are that claimants in the workshop treatment (Treatment C) had slightly longer second spells of UI receipt than controls and received about $65 more during their second spell. Claimants in this treatment were not, however, more likely than controls to have a second spell of UI receipt.

In general, Treatments A, C, and D had similar effects on UI receipt. The only statistically significant difference is that claimants in the workshop treatment (Treatment C) are more likely to exhaust their benefits than are claimants in the verification treatment (Treatment D). In contrast, except for the results for the second spell, results for claimants in the treatment that did not require the reporting of employer contacts (Treatment B) are significantly different from those found for claimants in Treatments A, C, and D on all measures of UI receipt.

These results indicate that work-search verification, required participation in a job search workshop, and a requirement for additional work-search contacts are effective in reducing UI spell length. Because no reemployment services are provided in the treatment requiring additional work-search contacts or in the verification treatment, the reduction in UI receipt associated with these treatments can be attributed to the additional costs to claimants associated with these more stringent work-search requirements. Moreover, because the costs of providing employer contact information are low for claimants actively looking for work, the reduction in UI receipt associated with this treatment can be attributed to the additional costs borne primarily by claimants who are not actively looking for work. In contrast, the results indicate that removing the requirement to report job search contacts increases the UI spell, but that the increase is relatively small as long as claimants are required to maintain regular contact with the UI office.[12]

[12]We also examined the extent to which UI effects differed for major claimant demographic subgroups. These results indicate that the effects of the treatments are widespread and not concentrated among specific demographic subgroups. Specifically, none

Time Pattern of UI Exit

Although the results presented above indicate that a job search workshop reduces UI receipt, they do not identify how workshops influence job search. One possible interpretation is that job search workshops provide claimants with skills that make them more employable or more efficient in their job search. An alternative interpretation is that claimants view attending a workshop as costly, and that the requirement to do so acts as a deterrent to continued receipt of UI benefits. If claimants view workshops as costly, we would expect the likelihood of exiting UI to increase immediately prior to a claimant's scheduled workshop date. In contrast, if workshops increase the efficiency of work-search, we would expect the likelihood of exiting UI to increase after the workshop has been completed.

We used hazard models for length of first spell of receipt of unemployment benefits to attempt to resolve these two competing interpretations. Because of the U-shaped hazard functions observed in these data, we employ a piece-wise exponential hazard model. This quasi-parametric approach assumes that the hazard of exiting UI is exponentially distributed within intervals, but allows the shape of the hazard function to vary non-parametrically across intervals (Breslow 1974; Laird and Oliver 1981; Trussell and Hammerslough 1983). To overcome the week-to-week noise in the data that is due to the bi-weekly UI payment schedule followed in Maryland, the hazard of exiting UI is estimated for two-week periods. Duration dependence is estimated using dummy variables for time. The hazard models include the same set of control variables used in the regression models.

We tested the time pattern of UI exit for claimants in each treatment against the time pattern of UI exit for claimants in the control group. Likelihood ratio tests indicate that the time patterns of UI exit in Treatments B and D do not differ significantly from that of controls, but that the patterns for Treatments A and C do. The point estimates (not shown) and likelihood ratio tests indicate that the effect of requiring two additional employer contacts (Group A) occurs primarily during the first bi-weekly period of the UI spell. Exit rates during the first period are 19.7% higher for group A than for the controls.[13] The time pattern for the remaining periods does not differ significantly from that of controls, and the main effect is no longer statistically significant. For the workshop treatment (Group C), the point estimates show that UI exit rates were higher than those of controls during the two bi-weekly periods when about 80% of the workshops occurred (weeks 4–7 of the UI spell). Exit rates were 32.2% and 23.8% higher during these periods. They were also higher during the seventh through the eleventh bi-weekly periods (weeks 13–14, 15–16, 17–18, and 19–20 of the UI spell), and significantly higher during the seventh, tenth, and eleventh bi-weekly periods (23.5%, 32.2%, and 54.0%, respectively).

These findings suggest that instructions to attend the workshop reduced the length of time claimants received UI because they increased the perceived costs of continued UI receipt, and that many claimants exited UI immediately prior to their scheduled workshop. This finding is consistent with

of the joint F-tests to determine whether the effects of the treatments on total UI benefits paid and the length of the first spell of UI differ by site or claimant race, age, sex, or prior earnings was statistically significant at conventional levels. In a separate analysis (not shown), we did find that the estimated effects varied significantly by the replacement ratio. Estimated effects of Treatments A, C, and D are significantly less negative, although still significantly different from zero, for claimants with higher replacement ratios. These results suggest that the effects of the costs associated with additional work-search requirements are proportional to the value of the UI benefits received relative to claimants' prior earnings.

[13]Raw exit rates for the control group for the first 14 bi-weekly periods are 36.8%, 10.6%, 8.7%, 8.4%, 7.9%, 8.1%, 6.1%, 6.5%, 5.9%, 4.8%, 4.9%, 6.5%, 8.0%, and 95.3% (benefit exhaustion).

Table 4. Treatment Effects on Employment and Earnings.[a]
(Standard Errors in Parentheses)

	Treatment Groups				
Outcome Measures	*Additional Contacts (A)*	*No Reporting of Contacts (B)*	*Workshop (C)*	*Verify Contacts (D)*	*Control Group Means*
First Quarter					
Percent Worked[b]	.97 (1.00)	1.77 (1.00)*	−.04 (.98)	1.13 (1.00)	55.9 (.50)
Earnings ($)	24 (51)	71 (50)	−14 (50)	18 (51)	1,636 (28)
Second Quarter					
Percent Worked[b]	.50 (.97)	1.02 (.97)	−.69 (.96)	.82 (.98)	62.0 (.49)
Earnings ($)	−19 (55)	86 (54)	−46 (53)	23 (55)	2,150 (29)
Third Quarter					
Percent Worked[b]	.33 (.96)	1.90 (.95)**	−1.12 (.95)	.97 (.96)	64.4 (.49)
Earnings ($)	16 (57)	98 (56)*	−79 (53)	17 (56)	2,341 (30)
Fourth Quarter					
Percent Worked[b]	.25 (.98)	1.38 (.96)	−.67 (.96)	1.06 (.97)	62.9 (.49)
Earnings ($)	34 (54)	92 (54)*	−23 (53)	67 (54)	2,280 (29)
Year 1					
Percent Worked[b]	1.12 (.75)	.78 (.76)	−.80 (.77)	1.27 (.76)	80.0 (.41)
Earnings ($)	54 (177)	347 (176)**	−163 (177)	124 (177)	8,407 (98)
Same Employer[b]	−.13 (.72)	.79 (.70)	−2.10 (.65)**	−1.44 (.67)**	17.2 (.38)

*Significantly different from the control group at the .10 level; **at the .05 level.

[a]Regression-based estimates include the following control variables: age, race and ethnicity, sex, citizenship, prior earnings, maximum benefit amount, federal and military employment statuses, entry quarter, exemption status, and site.

[b]Marginal effects (dF/dx) in percentages.

the findings from Johnson and Klepinger (1994). The results also provide support for the hypothesis that the workshop provided claimants with additional skills that made them more employable or more efficient in their job search, but that effect appears to have been delayed for several weeks following the workshop.

Effects on Employment and Earnings

The above results indicate that the treatment that did not require the reporting of employer contacts (Treatment B) extended the duration of claimants' job search, while the other treatments reduced the duration of job search. Below, we present evidence on whether the reduced/increased search time for claimants with greater/lesser work-search requirements caused them to find lower-quality/higher-quality jobs, as measured by earnings. The results are based on earnings in covered employment in the first four complete quarters following the quarter in which a person filed for benefits and entered the demonstration.

The effects of the treatments on employment and earnings are reported in Table 4. As this table indicates, slightly more than half of the claimants in the control group worked during the first full quarter after filing for UI benefits, over 60% were working by the fourth full quarter, and 80% worked at some point during the first full year after filing the UI claim. On average, these claimants earned $1,636 during the first quarter, $2,280 during the fourth quarter, and almost $8,500 during the first full year.

The treatments had relatively little impact on employment and earnings during the observed period. In particular, there is no evidence that the more rapid exit of claimants in Treatments A, C, and D oc-

curred at the cost of lower earnings. The only statistically significant effects shown in Table 4 are for claimants in the treatment that did not require the reporting of job search contacts (Treatment B). Claimants in this group were nearly 2% more likely than controls to be employed in the first and third quarters. They earned about $100 more during these quarters, and during the fourth quarter. Overall, they earned about $350 more than controls during the first full year. Given that claimants in this treatment remained on UI slightly longer than controls, these results suggest that claimants in this group found higher-paying jobs than controls. While statistically significant, these effects are not very large, and any differences in hourly wage rates are likely to be small.

It is interesting to note that claimants in the workshop treatment (Treatment C) and in the verification treatment (Treatment D) were less likely than claimants in other groups to return to work with their prior employer.[14] Thus, it appears that the greater cost of remaining on UI for claimants in these treatments reduced their length of search, but also increased their incentive to search intensively and resulted in their being less likely to return to their prior employer. Although the point estimate for the treatment that did not require reporting of job search contacts (Group B) is positive for the likelihood of returning to work with the same employer, the estimated coefficient is not statistically significant.

Conclusions

This paper describes findings from an experimental demonstration designed to evaluate alternative Unemployment Insurance work-search requirements. The main findings indicate that work-search requirements do affect the length of insured workers' unemployment spells, and that there appears to be no Hawthorne effect associated with the demonstration. More specifically, relative to the standard work-search policy, more stringent work-search requirements involving either two additional employer contacts or employer contact verification reduce UI payments by about three-quarters of a week or about $115 per claimant. In contrast to prior demonstrations, the additional work-search requirements examined in this demonstration were not combined with additional reemployment services. The shorter UI spells associated with these treatments can, therefore, be validly attributed to the increased work-search requirements. According to these results, decisions regarding time spent on UI are responsive to the costs of continued UI receipt, and indicate that an increase in the non-monetary costs of remaining on UI is associated with shorter job search. That is, the results suggest that many claimants choose to exit UI rather than incur the costs associated with the additional work-search requirements. Moreover, because the cost of providing employer verification information is relatively low for claimants who are actively seeking work, most of the costs associated with this work-search requirement are borne by those who are not or who would otherwise not have been actively seeking employment.

These results differ sharply from those of Ashenfelter et al. (1998), who reported no statistically significant effects of stricter enforcement and verification of work-search requirements. However, important differences in the data employed appear to account for the discrepancy in results. The Ashenfelter et al. study followed claimants for a very short period of time after they filed for UI benefits (about four months). Consequently, a sizable proportion of claimants in that study had right-censored spells. In contrast, the Maryland study followed claimants for a full year. In order to make the Maryland data more comparable to the Ashenfelter et al. data, we truncated the Maryland data at 14 weeks. This led to right-censoring for about 40% of the

[14]The prior employer is defined as the last employer prior to filing for UI benefits. If a claimant had more than one employer during that quarter, the employer providing the most earnings is selected.

sample, a figure comparable to that of two of the states in the Ashenfelter et al. study.[15] We then re-estimated the equation for weeks of UI receipt. Both the Ashenfelter et al. study and the Maryland demonstration contain employer verification treatments, and we compare results for this treatment.[16] The estimated impact of employer verification (Group D) based on the right-censored Maryland sample is about the same as that reported for three of the four states in the Ashenfelter et al. study. However, because the sample size of the Maryland study is much larger, the estimated impact is still statistically significant. Thus, the discrepancy between the results from the Ashenfelter et al. study and those reported here for Maryland appears to be due to right-censoring in the Ashenfelter et al. data and to the larger sample size in the Maryland study.

Despite the relatively more rapid exit from UI of claimants in treatments A, C, and D, we find no evidence that earlier exit occurs at the cost of lower earnings. This finding suggests that the shorter job search associated with more stringent work-search requirements arises from more intensive job search, rather than from a decline in the reservation wage. The insensitivity of earnings to changes in the intensity and length of job search, along with a study design that did not mix these additional work-search requirements with employment services, indicates that the amount of UI benefits that claimants are willing to forgo rather than comply with the additional work-search requirements provides a monetary equivalent estimate of the non-monetary costs associated with these additional work-search requirements. This finding suggests that job-seekers have a good idea of the type of jobs they will accept, and that the reservation wage is unaffected by additional work-search requirements. Moreover, the finding that duration of job search is positively associated with the probability of re-employment with the previous employer suggests that job-seekers are risk-averse. Apparently, job-seekers prefer working for a previous employer—perhaps because the quality of the job match is known—over working for a new, untried employer.

The results also show that reducing work-search requirements (that is, not requiring UI claimants to report their work-search contacts) is associated with an increase in UI receipt, and a slight increase in employment and earnings. Taken together, these results suggest that delayed exit from UI is associated with higher hourly wage rates, because claimants in this treatment were able to find better job matches than controls. However, because the statistically significant effects for earnings and employment are not very large, any differences in hourly wage rates are also likely to be small.

Our estimate of the increase in UI receipt associated with suspending the demand that claimants report work-search contacts is much smaller than that from the Washington Alternative Work-Search Experiment. The major difference between the no-reporting treatments in the two demonstrations is that in the Washington study payment checks were automatically sent to claimants unless claimants contacted the UI office to report a change in their status, whereas in the Maryland study claimants did not receive a payment unless they informed the UI office that their status had *not* changed. The smaller effects found in this study indicate that moral hazard is greatly reduced when claimants have regular contact with the UI office.

In addition to changing work-search requirements, the study included a mandatory job search workshop designed to enhance claimants' work-search skills and to increase their job search efficiency. Broadly consistent with findings from earlier UI demonstrations, the results presented here show the job search workshop reduces UI payments, but has no statistically signifi-

[15]The other two states lost claimants due to periods of non-observation and had much higher proportions with right-censoring.

[16]The remaining treatments in the Ashenfelter study are not directly comparable to the treatments in the Maryland demonstration.

cant effects on employment and earnings.[17] Although a job-search workshop may enhance the job search abilities of some claimants, a mandatory workshop may also increase the cost of continued UI receipt and some claimants may exit UI to avoid the workshop. Johnson and Klepinger (1994) presented findings consistent with this interpretation, and the hazard model results described here provide additional support for this hypothesis, although they also suggest that the workshop may have an impact on job search efficiency.

It is important to note, however, that job search services, including job search workshops, are now targeted to claimants determined to be in need of such services, rather than imposed on all claimants, as was done in this study.[18] For this reason, the workshop results reported here might not be directly applicable to targeted job search workshops as offered today. Corson and Decker (1995) suggested that the impact of workshops may be somewhat greater for workers targeted under new profiling requirements. Tentative support for this argument is provided by results from the recent Job Search Assistance Demonstration (JSA), which shows that claimants who pass profiling requirements tend to receive benefits for a greater number of weeks than claimants who do not pass the profiling criteria (Decker et al. 1998), and that a job-search treatment that includes a mandatory workshop has a somewhat greater impact on claimants with very high predicted probabilities of UI exhaustion than on other claimants (Decker et al. 2000).[19] These findings suggest that our estimates of the effects of a mandatory workshop may understate the impact of workshops targeted to profiled claimants.

Finally, although our findings provide strong support for the argument that work-search requirements affect the duration of UI receipt, the estimated effects reported here may understate the impact on UI outlays of increasing or decreasing work-search requirements because changes in work-search requirements are likely to affect UI participation. Increasing work-search requirements is likely to reduce UI participation because some potential claimants who would have filed for benefits under the standard work-search requirements will refrain from filing because of the additional costs associated with the increased work-search requirements. A reduction in work-search requirements will have the opposite effect on UI participation. Additional research using data that are not restricted to UI claimants is required to estimate the size of any participation effects that may be associated with an increase or decrease in UI work-search requirements.

[17]Of particular relevance are the Charleston Claimant Placement and Work Test Demonstration (Corson et al. 1984), the New Jersey Unemployment Insurance Reemployment Demonstration Project (Corson et al. 1989), and the Washington Alternative Work-Search Experiment (Johnson and Klepinger 1994). These demonstrations tested whether intensive reemployment services that involved a job search workshop were effective approaches to reducing UI spells and total UI payments. 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 a half-week. These findings are remarkably consistent with the results described above that were tested in very different settings. Similar to the experiment implemented in Maryland, the workshop in these demonstrations was scheduled to occur after claimants had drawn UI for four or five weeks. However, it should be noted that the workshop varied in duration from three hours in the Charleston demonstration to one week in the New Jersey demonstration.

[18]Recent legislation requires the implementation of worker profiling to identify dislocated workers and to provide them with intensive services. Various profiling models have been used to identify claimants likely to have long periods of UI receipt. The Department of Labor has a recommended approach for designing and implementing worker profiling based on identifying claimants likely to exhaust their benefits (U.S. Department of Labor 1994).

[19]The impact results are larger than those found in Maryland for one state in the JSA demonstration and smaller for the other state. However, because the JSA and Maryland demonstrations occurred at different times and because the JSA treatment contained other job-search requirements in addition to the workshop, the estimated effects are not fully comparable.

Appendix[a]

Mean UI Outcomes and Earnings and Differences of Means by Sample (Standard Errors in Parentheses)

Description	*Treatment Groups*				*Combined Control Groups*
	A	*B*	*C*	*D*	
			Full Sample		
Sample Size	3,510	3,455	3,680	3,400	9,713
UI Benefits Paid	2,059	2,090	1,953	1,941	2,085 (22)
Net Impact	−25 (43)	6 (44)	−131 (42)**	−144 (43)**	
Weeks of Benefits	11.37	12.25	11.18	11.03	11.94 (.12)
Net Impact	−.57 (.23)**	.31 (.23)	−.76 (.22)**	−.91 (.23)**	
% Exhausted Benefits	27.7	28.9	26.0	24.7	28.3 (.46)
Net Impact	−.6 (.91)	.6 (.88)	−2.3 (.84)**	−3.6 (.88)**	
Earnings in Year Following Filing for Benefits ($)	8,766	8,696	8,049	8,394	8,407 (98)
Net Impact	358 (195)*	291 (194)	−359 (184)**	−14 (200)	
			Excluding Sites without Assignment to Treatment A		
Sample Size	3,510	2,304	2,208	2,215	7,367
UI Benefits Paid	2,059	2,214	2,083	2,058	2,166 (26)
Net Impact	−107 (45)**	48 (52)	−82 (53)	−108 (.54)**	
Weeks of Benefits	11.4	12.6	11.5	11.3	12.12 (.13)
Net Impact	−.75 (.24)**	.48 (.27)*	−.60 (.28)**	−.87 (.28)**	
% Exhausted Benefits	27.7	32.1	29.1	27.5	30.4 (.54)
Net Impact	−2.6 (.92)**	1.7 (1.10)	−1.3 (1.13)	−2.9 (1.12)**	
Earnings in Year Following Filing for Benefits ($)	8,766	9,136	8,278	8,812	8,634 (118)
Net Impact	133 (211)	502 (245)**	−356 (244)	178 (247)	

*Significantly different from the control group at the .10 level; **at the .05 level.
[a]Unconditional means.

REFERENCES

Ashenfelter, Orley, David Ashmore, and Olivier Deschenes. 1998. "Do Unemployment Insurance Recipients Actively Seek Work? Randomized Trials in Four States." Working Paper No. 412, Princeton University.

Baj, John, and Charles E. Trott. 1991. *A Feasibility Study of the Use of Unemployment Insurance Wage-Record Data as an Evaluation Tool for JTPA.* National Commission for Employment Policy. Washington, D.C.: GPO.

Blaustein, S., C. O'Leary, and S. Wandner. 1997. "Policy Issues." In C. O'Leary and S. Wandner, eds., *Unemployment in the United States: Analysis of Policy Issues.* Kalamazoo, Mich.: W.E. Upjohn Institute for Employment Research, pp. 1–49.

Breslow, N. E. 1974. "Covariance Analysis of Censored Survival Data." *Biometrics,* Vol. 30, No. 1, pp. 89–99.

Corson, Walter, and Paul Decker. 1995. "Using the Unemployment Insurance System to Target Services to Dislocated Workers." Report prepared for the Advisory Council on Unemployment Compensation, U.S. Department of Labor under Contract No. M-4779-4-00-97-30. Princeton, N.J.: Mathematica Policy Research Inc.

Corson, Walter, Shari Dunstan, Paul Decker, and Anne Gordon. 1989. *New Jersey Unemployment Insurance Reemployment Demonstration Project.* Unemployment Insurance Occasional Paper 89-3, U.S. Department of Labor.

Corson, Walter, David Long, and Walter Nicholson. 1984. *Evaluation of the Charleston Claimant Placement and Work Test Demonstration.* Report prepared for the U.S. Department of Labor under Contract No. 20-34-82-07. Princeton, N.J.: Mathematica Policy Research Inc.

Decker, Paul, Lance Freeman, and Daniel Klepinger. 1998. "Assisting Unemployment Insurance Claimants: Effects of the Job Search Assistance Demonstration." Report submitted to the U.S. Department of Labor.

Decker, Paul, Randel Olsen, Lance Freeman, and Daniel Klepinger. 2000. "Assisting Unemployment Insurance Claimants: The Long Term Effects of the Job Search Assistance Demonstration." Report submitted to the U.S. Department of Labor.

Heckman, James J. 1976. "The Common Structure of Statistical Models of Truncation, Sample Selection, and Limited Dependent Variables and a Simple Estimator for Such Models." *Annals of Economic and Social Measurement,* Vol. 5, pp. 475–92.

____. 1979. "Sample Selection Bias as a Specification Error." *Econometrica,* Vol. 47, No. 1 (January), pp. 153-61.

Johnson, Terry R., and Daniel H. Klepinger. 1994. "Experimental Evidence on Unemployment Insurance Work-Search Policies." *Journal of Human Resources,* Vol. 29, No. 3 (Summer), pp. 695–717.

Laird, Nan, and Donald Oliver. 1981. "Covariance Analysis of Censored Survival Data Using Log-Linear techniques." *Journal of the American Statistical Association,* Vol. 76, No. 1, pp. 231–40.

Maddala, G. S. 1983. *Limited-Dependent and Qualitative Variables in Econometrics.* Cambridge: Cambridge University Press.

Meyer, Bruce D. 1995. "Lessons from the U.S. Unemployment Insurance Experiments." *Journal of Economic Literature,* Vol. 33, No. 1 (March), pp. 91–131.

Trussell, James, and Charles Hammerslough. 1983. "A Hazard-Model Analysis of the Covariates of Infant and Child Mortality in Sri Lanka." *Demography,* Vol. 20, No. 1, pp. 1–26.

U.S. Department of Labor. 1994. "The Worker Profiling and Reemployment Service System: Legislation, Implementation Process, and Research Findings." Unemployment Insurance Occasional Paper 94–4, U.S. Department of Labor.