The Effects of Eliminating the Work Search Requirement on Job Match Quality and Other Long-Term Employment Outcomes

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Abstract

We exploit data from the 1986–87 Washington Alternative Work Search experiment (merged with nine years of follow-up administrative wage records) to estimate the causal effects of eliminating the unemployment insurance (UI) work search requirement (WSR) on duration of nonemployment, tenure with first post-claim employer, number of post-claim employers, long-term earnings, employment, and hours worked. For UI claimants as a whole, we find that eliminating the WSR had little influence, either positive or negative, on long-term post-claim outcomes. In contrast, for permanent job losers, we find strong evidence that eliminating the WSR had a negative effect on employment outcomes, resulting in a longer time to reemployment, lower earnings, and a shorter duration of tenure with first post-claim employer. For claimants who were not permanent job losers, eliminating the WSR resulted in more UI benefit payments and longer unemployment durations, but made no difference for their employment outcomes. We conclude that, in addition to reducing moral hazard associated with UI, the WSR is an important policy for improving the long-term employment outcomes of permanent job losers.

JEL classification: C21, C93, I38, J18, J24, J38, J64, J65, J68

Keywords: Unemployment insurance, random-assignment experiment, work search requirement, reemployment policy, long-term evaluation of public policy, administrative data

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List of Abbreviations

ER—Exception Reporting

JSA—Job Search Assistance

NWS—New Work Search

UI—Unemployment Insurance

WAWS—Washington Alternative Work Search experiment

WSR—Work Search Requirement

1 Introduction

The work search requirement (WSR) for unemployment insurance (UI) recipients has been a central part of UI in the United States since the system began in the 1930s. Typically, to be eligible for UI benefits, a claimant initially needs an adequate work history and must have lost her job through lack of work and no fault of her own. In addition, to remain eligible, the worker must be "able, available, and searching" for work—that is, must satisfy the *work search requirement*, or WSR.

Although the WSR aims to reduce the moral hazard associated with UI—that is, to counter the incentive to reduce job search effort and take longer to become reemployed—it may also pressure workers into accepting a relatively poor job match, leading to an unstable pattern of employment and lower long-term earnings.[1] Hence, eliminating the WSR could allow the claimants to search for a better job match and lead to improved employment outcomes—the *improved job match hypothesis*. Alternatively, eliminating the requirement could prolong duration of unemployment, making the claimant less attractive to employers and hence worsen employment outcomes—the *negative duration dependence hypothesis*.[2] Finally, eliminating the work search requirement could impose greater costs to the UI system, without any effect on employment outcomes—the *moral hazard hypothesis*.

Understanding the effects of the WSR on employment outcomes is of ongoing importance because in recent years most states have relaxed enforcement of the requirement by shifting toward taking claims over the phone or on-line (see O'Leary [2006] and Ebenstein and

[1] A UI claimant does not need to accept the first available job offer, but he or she is required to accept a job offer that satisfies the "suitable work" condition. In practice, claimants do not need to accept work that is not in line with their training and experience. The work search requirement could nevertheless pressure a claimant to accept a less attractive job offer that meets the suitable work condition instead of holding out for a better offer.

[2] See, e.g., Notowidigdo, Kroft, and Lange (2013) for recent evidence of scarring effects of long spells of unemployment.

Stange [2010]). Because telephone and on-line claiming in effect reduces the frequency of in-person contact between a claimant and the state workforce agency, it is important to know whether a more "hands-off" approach to the WSR has any beneficial effect on post-unemployment job match quality.

The aim of this paper is to examine the effects of eliminating the WSR on post-unemployment job match quality, proxied by employment tenure, and other long-term employment outcomes, such as duration of nonemployment, the number of post-claim employers, earnings, hours worked, and employment of UI claimants. To do this, we add nine years of quarterly follow-up wage records to the original data from the Washington Alternative Work Search (WAWS) experiment (Johnson and Klepinger 1991, 1994). In the WAWS experiment, all eligible UI claimants at the Tacoma Employment Service Center between July 1986 and August 1987 were randomly assigned to a control group, which imposed a standard WSR, or to an exception reporting (ER) treatment group, which effectively eliminated the WSR.

Claimants in the control group were told to contact at least three employers per week and be prepared to give evidence that they had done so in an *eligibility review interview*, usually conducted 13–15 weeks after the initial claim. Claimants in the ER treatment group were told (at the time of their initial claim) to actively seek work, but were also told that they would not be called in for an eligibility review interview, and that weekly UI benefits would be mailed unless they called the Tacoma Employment Service Center to report that they had stopped looking for work or had taken a job. As such, ER amounted to an "honor system" with no WSR (Johnson and Klepinger 1991, pp. 3–9).

When studying the short-term effects of ER, Johnson and Klepinger (1991, 1994) find that eliminating the WSR substantially increased benefits received, the duration of benefit

receipt, and the probability of exhausting benefits, but without affecting earnings or hours worked during the claim quarter or the benefit year. This combination of increased benefit receipt without any changes in earnings or hours suggests that ER led to increased abuse of the UI system. At the same time, however, ER also increased the probability that a worker returned to a former employer. Although this increased likelihood of return to a past employer suggests that relaxing the WSR may have been beneficial to at least some of the claimants (in that they reestablished a previous job match), Johnson and Klepinger find no evidence of improved short-term post-unemployment outcomes. On balance, then, Johnson and Klepinger's findings suggest that eliminating the WSR led to increased abuse of the UI system by claimants but did not lead to better employment outcomes.

Other studies of the WSR arrive at quite different conclusions from the WAWS experiment. For example, the evaluation of the 1994 Maryland UI Work-Search Demonstration (Klepinger, Johnson, and Joesch 2002) concluded that although a *relaxed* enforcement of WSR prolonged the duration of UI receipt, it also increased the probability of subsequent employment and led to higher earnings in the quarters following the experiment.[3] Poe-Yamagata et al. (2011) find that an increased emphasis on WSR under the 2005 Reemployment and Eligibility Assessment initiative decreased the duration of UI receipt and had a positive impact on reemployment probability in the short-run. Finally, Ashenfelter, Ashmore, and Deschênes (2005) find that reducing the enforcement of the WSR did not lead to increased abuse of the UI system by the claimants. Hence, the issue of whether a relaxed WSR leads to more abuse or has the

[3] The treatment resembling the WAWS' ER treatment in the Maryland experiment only relaxed some aspects of WSR. This treatment did not include automatic payments to the claimants. Instead, the claimants needed to inform the UI office on a weekly basis that they had not found work and were actively searching. This treatment group was, however, not required to report their employer contacts. In effect, the Maryland treatment relaxed some features of the WSR, but did not eliminate it all together.

positive effect of helping claimants obtain more stable and better paying post-unemployment jobs remains a matter of debate.

Studying the long-term effects of eliminating the WSR is related to the more general issue of how design of UI—e.g., the generosity and duration of benefits—affects earnings and employment. Thanks to the availability of high-quality microdata, this literature has expanded in the recent decades. Addison and Blackburn (2000) and Tatsiramos and van Ours (2014) review the literature on the relationship between UI benefit generosity and post-unemployment earnings. Both literature surveys conclude that the evidence has been mixed. For example, Ehrenberg and Oaxaca (1976), Burgess and Kingston (1976), McCall and Chi (2008), Caliendo, Tatsiramos, and Uhlendorff (2012), and Nekoei and Weber (2013) find a positive association between a more generous UI system and reemployment earnings, whereas Addison and Portugal (1989), Gregory and Jukes (2001), and Schmider, von Wachter, and Bender (2012) find a negative association. Finally, some research has not found any convincing relationship between reemployment earnings and either UI benefit generosity (Classen 1977; Belzil 2001) or longer potential duration of UI benefits (Lalive 2007; Card, Chetty, and Weber 2007).

A subset of this literature studies whether the design of UI has an impact on post-unemployment job match quality, measured by job or employment tenure. The conclusions have varied. Belzil (2001), Card, Chetty, and Weber (2007), and van Ours and Vodopivec (2008) find little or no relationship between UI generosity and subsequent tenure, whereas Centeno (2004), Centeno and Novo (2009), and Tatsiramos (2009) conclude that a more generous UI leads to a longer post-unemployment tenure.

Consequently, whether there is a link between various aspects of the UI system and post-unemployment job-market outcomes remains unclear. The controversy is due, in part, to the lack

of long-term post-unemployment data that can be matched to the kind of exogenous variation necessary to identify a causal effect. Because the WAWS experiment randomly assigned a group of new UI claimants to a treatment that effectively eliminated the WSR, in this paper we are able to study the causal effect of eliminating the WSR on long-run outcomes. By using nine years of post-experimental quarterly earnings records, merged to data from a random-assignment experiment, we are able to address two main questions: "How does elimination of the WSR affect the post-claim job match quality and long-term employment outcomes?" and "Does the effect vary by different groups of claimants?"

We address these questions by estimating regression models comparing the long-term outcomes of claimants assigned to the ER and control groups. We measure job match quality as the duration of tenure with the first post-claim employer and we measure other long-term employment outcomes along several dimensions: the duration of nonemployment, the number of post-unemployment employers, long-term earnings (and the volatility of those earnings), annual probability of employment, and hours worked in the nine years following the experiment.

Because it seems likely the WSR may have different effects on different groups of claimants, we estimate separate long-term effects for claimants who suffered permanent job loss, were temporarily laid off, quit for good cause, and were temporary or seasonal workers. We also examine how relaxing the WSR might affect long-term unemployed claimants; we do this by estimating the effects of ER separately for claimants with high and low probabilities of exhausting their UI benefits.

The paper has the following main findings. Although, for UI claimants as a whole, we find that the long-term employment outcomes of ER claimants were no different from outcomes of the comparison group, we find significant differences among various subgroups.

For permanent job losers, eliminating the WSR resulted in clearly worse employment outcomes: greater earnings losses in the year following job loss, a longer spell of nonemployment, and shorter tenure with the first post-claim employer. In contrast, eliminating the WSR had no impact on employment outcomes for workers who were not permanent job losers—those on a temporary layoff, quits, and contract or seasonal workers. That these claimants claimed more benefits for a longer period of time, but had employment prospects no different than workers in the control group, is consistent with the interpretation that they continued claiming benefits even after becoming reemployed.

The results for claimants who were not permanent job losers imply that the WSR plays an important role in mitigating claimant moral hazard: without the WSR, these claimants would draw more UI benefits, but would not ultimately have improved employment outcomes. The results also show that the WSR is an important policy for improving the welfare of permanent job losers, who in absence of the WSR would have worse employment outcomes. As permanent layoffs as a share of all layoffs have increased in the past 20 years (O'Leary, 2007), the findings of this paper are relevant to policymakers concerned with the current reemployment prospects of permanent job losers.

The rest of the paper is organized as follows. Section 2 briefly describes the design of the Washington experiment, describes the intention-to-treat effects, and includes a discussion of the effect of eliminating the WSR on returning to a former employer. Section 3 describes the methods for estimating the long-term effects and differences in long-term effects for various subgroups. Section 4 presents the results, and section 5 summarizes the findings and concludes. To keep the main discussion as direct as possible, we relegate a detailed description of the data

and details of how we created a long-term panel, as well as sample definitions, to a Data Appendix.

2 Exception Reporting and the Washington Alternative Work Search Experiment

The main purpose of the WAWS experiment was to test alternative means of reducing the duration of UI receipt and unemployment duration. To be eligible for UI in Washington, a claimant must have worked at least 680 hours in roughly the year before claiming UI, must have been laid off for lack of work and through no fault of her own, and must be "able, available, and searching" for work. This last criterion for UI eligibility is the *work search requirement* (WSR). In order to fulfill the WSR in Washington, the Employment Security Department personnel tell the claimants to contact at least three employers per week and to be prepared to give evidence that they have done so in an eligibility review interview, which may be conducted 13–15 weeks after the claimant files for benefits. For an eligibility review interview, a claimant reports to the public Employment Service for a one-hour group "interview" (or lecture) followed by (in some cases) a 15-minute individual interview during which employer contacts are checked.

The WAWS experiment tested the effects of eliminating this WSR by randomly assigning new UI claimants to a control group (subject to the standard WSR) and an ER treatment group. The latter were told (at the time of their initial claim) to actively seek work, but also that they would not be called in for an eligibility review interview (so they did not need to keep a record of job search contacts), and that weekly UI benefits would be mailed unless they called the Tacoma Employment Service Center to report they had stopped looking for work or had taken a job. In effect, ER amounted to an honor system with no WSR (Johnson and

Klepinger 1991, pp. 3–9). Random assignment occurred between July 1986 and August 1987 at the Tacoma Employment Service Center, based on the last digit of each claimant's Social Security number (see the Data Appendix for details).

2.1 Sample definition

Because the follow-up administrative wage records available to us begin in the first quarter of 1987, we do not have data on earnings, hours, and employer information for the first post-claim quarter for those who claimed in the third quarter of 1986 (that is, July, August, and September). Because of this data limitation, the sample we use is smaller than the sample studied by Johnson and Klepinger (1991, 1994); the Data Appendix provides details on how we define our analysis sample.

The experiment also tested a policy alternative called a "new work search" (NWS) policy, similar to the standard WSR except that selected claimants were called for an eligibility review interview earlier than usual (in week 6 after the claim rather than week 13–15 and at discretion of the UI office) and received a detailed job development plan (see Johnson and Klepinger [1991, p. 4]).[4] Since there is considerable variation between the states in the implementation of the eligibility review interview (see O'Leary [2006]), the NWS policy treatment could conceivably be a "standard" WSR in another state. As we document in Tables 3 below and in Table A1 in the Results Appendix, we argue that because the NWS policy differed little from the standard WSR in Washington at the time and because there is no evidence that

[4] The WAWS experiment also included an "intensive services" treatment, in which claimants were assigned to job search assistance (see Johnson and Klepinger [1991]). We study the long-term effects of this treatment in Cebi, Lachowska, and Woodbury (2014).

NWS policy affected outcomes, we can treat the NWS policy group as an alternative control group, and hence increase the sample size by pooling the NWS policy group with the controls.[5]

Table 1 offers a profile of how the different treatments worked in practice by showing proportions of the control, ER, and NWS policy groups that were called for an eligibility review interview and received various employment services. Two points are worth noting. First, almost none of the ER claimants were subjected to an eligibility review interview, consistent with the design of the treatment. ER claimants were also less likely to receive employment services, especially those requiring some initiative on the part of the claimant, such as assistance with a job development plan. The main services provided to ER claimants were job referral and placement, which are typically initiated by the Employment Service.

Second, Table 1 shows that when compared to the control group, the NWS policy group was more likely to receive an eligibility review interview and a job development plan, both likely due to the earlier scheduling of an eligibility review interview and the additional emphasis placed on a job development plan for claimants assigned to this group (see Johnson and Klepinger [1991, pp. 3–9]). Otherwise, the claimants assigned to the control and NWS groups received a similar mix of employment services (that is, job consultation, receipt of or referral to training, testing, support services, contacting an employer on the claimant's behalf, or any other contact with the Employment Service), suggesting that this treatment was effectively very similar to the standard WSR experienced by the controls.[6]

[5] In Table A1 in the Results Appendix, we show that there is no statistically significant difference in any of the short-term outcomes between the control and the NWS policy groups. In Tables A2–A9 in the Results Appendix, we show that our conclusions regarding the effect of ER on job-match quality and other long-term outcomes are unchanged if we limit the estimation sample to only include the ER and control groups (N = 3,145). Together, these findings strengthen our rationale for pooling the NWS policy group together with the control group.

[6] The differences between NWS policy group and the controls in the receipt of these six employment services were not statistically significant.

Since neither we nor Johnson and Klepinger (1991, 1994) find evidence that the NWS policy had a differential impact on outcomes when compared to the control group, we pool the control group together with the NWS policy group as a way to increase the size of our analysis sample. We refer to this larger, pooled control group as the *comparison group*.

2.2 Descriptive statistics

Table 2 displays various mean characteristics of the control, ER, and NWS policy groups, and the differences among them. The characteristics can be classified as

- demographic — sex, race, age, schooling, veteran status, marital and household status
- pre-claim — earnings and hours in the three prior years; industry and occupation before the claim; whether the individual had a prior UI claim
- claim-related — reason for job loss, whether the claimant had a recall date or was placed through a union hiring hall, UI benefits and claim type, and reservation wage

In general, the randomization protocol appears to have been successful, although there is evidence of nonrandomness between the controls and ER groups for some observables, for example, the distribution of age, schooling, industry, and reason for job loss across the groups. Also, relatively few ER group claimants were on standby or in a union that referred claimants to jobs. Johnson and Klepinger (1991, 1994) suggest that this difference is a matter of reporting rather than actual status: because claimants in the ER group did not need to submit continued claims for UI, the UI staff had no incentive to record the standby or union status of claimants in this group. A baseline survey completed by claimants (reported in Johnson and Klepinger [1994,

p. 704] but not available to us) supports the claim and shows no difference between the groups in the proportion on standby or placed by a union. Nonetheless, the measurable differences between the control and ER groups offer a rationale for regression-adjustment in comparing the groups.

Because the difference between control and ER groups could be due, in part, to using a smaller sample than Johnson and Klepinger (1991, 1994), in Table 2 we make additional comparisons of mean characteristics of the control group with the NWS policy group and of the ER group with a pooled sample of the control group and the NWS group (i.e., the comparison group). We note two things. First, randomization into the control and NWS groups appears to have been successful. Second, for only 3 characteristics out of 60 shown are the differences between claimants assigned to ER and the pooled control and NWS group with a p-value < 0.05.

2.3 Replication of Johnson and Klepinger's main results

Table 3 replicates the estimated effects of the ER treatment on various short-term outcomes considered by Johnson and Klepinger (1994). We group the outcome variables into two categories: 1) variables pertaining to UI receipt (total UI benefits paid, weeks of UI payments, and proportion that exhausted UI benefits); and 2) variables pertaining to short-term post-claim employment outcomes (proportion employed, hours worked, earnings, and proportion who returned to previous employer or industry). Each cell in the third and fourth columns from the left is a point estimate and a standard error from a separate regression. We will follow this convention throughout the paper.

Like Johnson and Klepinger (1994), we find that, on average, claimants in the ER group received more UI benefits (an additional $445 in Table 3), received benefits for an additional 3 weeks, and were more likely to exhaust their benefits (by about 11 percentage points) compared

with the comparison group. Also like Johnson and Klepinger, we find no statistically significant difference between the ER and comparison groups in hours worked or earnings in year 0 (the benefit year) or year 1 (the subsequent year).

On one hand, these findings suggest that eliminating the WSR may have led to abuse of the system by the claimants—the ER group received more UI benefits than the comparison group, but their earnings and work hours did not fall relative to the comparison group. It would seem that claimants in the ER group may have returned to work without informing the UI agency, and hence continued to receive benefits to which they were not entitled.

On the other hand, the estimates in Table 3 suggest that the ER group had a marginally lower probability of employment in the first post-claim quarter and in the year of the experiment. That the total earnings and hours of ER claimants in year 0 and 1 did not fall in spite of this lower probability of reemployment suggests that the ER claimants who did become reemployed could have worked at higher wages than the comparison group. This interpretation is consistent with the findings in Johnson and Klepinger (1994), who impute hourly wages using a Heckman selection-correction model and find that hourly wages increased for ER claimants (we do not attempt to impute hourly wages).[7] This potential hourly wage gain for ER claimants who were reemployed suggests they may have found better job matches. This interpretation is also consistent with the finding that ER claimants had almost a 3 percentage-point higher likelihood of returning to a former employer than the comparison group.

[7] Johnson and Klepinger (1994) find higher imputed hourly wages for ER claimants, but unlike us, they do not find a statistically significant decrease in the probability of reemployment. Our finding appears to be in part due to pooling together the NWS policy group and the control group. When comparing the claimants assigned to ER and the control group, the decrease in the probability of employment during the first year is negative, but not statistically different from zero. In Table A1, we show that the NWS policy group had a higher probability of reemployment in the first post-claim quarter (by about 0.5 percentage points) and in the year of the experiment (by about 0.8 percentage points) than the controls, but this gain is not statistically significant. Pooling the NWS policy group and the control group increases the average reemployment probability sufficiently to explain the statistically negative effect in Table 3.

Together, these findings suggest that eliminating the WSR may have improved the employment prospects of some claimants by allowing them more time to establish (or reestablish) a successful job match and earn higher wages. In section 4, we address this issue further by studying whether ER resulted in any long-term job match quality gains, and if so, for what type of claimant.

2.4 Post-claim employment outcomes

The administrative wage records allow us to follow each claimant's post-experiment employment, earnings, and hours for nine years. Because administrative wage records also include quarterly information about each claimant's employer account number (EAN), we construct post-claim employment outcomes not considered by Johnson and Klepinger (1991, 1994). First, for each claimant, we compute the number of unique employers (identified by EANs) we observe from the first quarter after the initial claim to the last follow-up quarter in which we can observe every claimant. We refer to this variable as *number of post-claim employers*. Figure 1 presents a histogram of the number of employers for the comparison group and the ER group. Table 5 shows the mean, median, and the standard deviation of this variable.

Second, we construct the variable *quarters of nonemployment* by computing the number of consecutive post-claim quarters in which a claimant is observed without covered earnings. This variable allows us to examine whether ER resulted in an increase in the time to reemployment beyond what we can infer from UI claims records that can only measure duration of *insured* unemployment. Figure 2 shows the distribution of this variable.

Third, we measure the volatility of post-claim earnings by standard deviation of earnings from year 0 to year 9. We refer to this variable as *standard deviation of post-claim earnings*.

Finally, we construct a proxy for post-claim job match quality. For each claimant, we compute the number of quarters in which a claimant is observed with earnings from the first post-claim employer. This variable ranges from 0, if no EAN is observed, to 40, if the claimant is with the same EAN throughout our window of observation. We refer to this employment tenure variable as *quarters with first post-claim employer*. Figure 3 shows the distribution of this variable.

3 Methods

In order to estimate the effect of ER on post-unemployment outcomes, we merge the WAWS experimental data on each claimant (derived from UI claims records, administrative wage records, and Employment Service records) with quarterly administrative records on the claimant's employment, earnings, and hours worked in the 40 quarters following the claim quarter (and the enrollment in the experiment).

The effect of assignment to the ER treatment group on outcomes can be obtained by pooling the comparison group (consisting of the control and NWS policy groups) and ER group and estimating linear models of the following form:

$$y_i = \alpha + \beta ER_i + X_i\gamma + u_i, \quad (1)$$

where y_i is an outcome for individual i in any of the years following enrollment in the experiment; ER_i is an indicator for assignment to the ER group (that is, the group not subject to the WSR); X_i includes all of the variables listed in Table 2, as well as the unemployment rate in the county where the claim was filed and indicators for the quarter the individual claimed benefits; and u_i denotes i's unobservable traits.

The identifying assumption is that assignment to treatment indicator is independent of any individual characteristics, including those unobserved by the researcher: $E(u|ER) = 0$. As

Johnson and Klepinger (1994) note, because the random assignment to control and ER treatment groups appears to have succeeded, this assumption is reasonable. In this case, the ordinary least squares (OLS) estimator of β is a consistent estimator of the intention-to-treat effect on outcome y. Including the demographic variables (X) reduces sampling error and controls for observable differences between treatment and control groups that may arise even under random assignment.

The outcomes (y) include the claimant's post-experiment employment, earnings, hours, quarters with first post-claim employer, number of post-claim employers, quarters of nonemployment, and standard deviation of earnings. By estimating a model for each of the nine years following enrollment in the experiment, we can trace out the path of long-term effect of assignment to the ER group on hours, earnings, and probability of employment. For the remaining outcomes—number of post-claim employers, quarters of nonemployment, standard deviation of earnings, and quarters with first post-claim employer—we also estimate linear models. Since the first three outcomes listed above are count variables, we have also estimated Poisson maximum-likelihood models. Our findings remain qualitatively unchanged.

Taken together, all these outcomes capture different, but not necessarily independent dimensions of the effect of assignment to ER. If, according to the improved job match hypothesis, eliminating the WSR prolonged the duration of unemployment, but had a beneficial effect on post-claim outcomes, we would expect the estimate of β to have a positive effect on post-claim hours, earnings, employment, and the number of quarters with the first post-claim employer. On the contrary, if eliminating the WSR only prolongs the unemployment spell, then, according to the negative duration dependence hypothesis, we would expect the estimate of β to have a negative effect on post-claim hours, earnings, employment, and the number of quarters with the first post-claim employer.

The effect of ER on the remaining outcomes—quarters of nonemployment, number of post-claim employers, and standard deviation of earnings—is more ambiguous and ought to be considered jointly with the estimated effect on other outcomes. For example, if ER did not have any effect on the level of post-claim earnings but at the same time had a negative effect on the volatility of post-claim earnings, it could be argued that ER had a beneficial effect, since, on average, claimants assigned to ER are earning just as much but experience less variability. Analogously, a longer duration of nonemployment and fewer post-claim employers should be interpreted jointly with the effect on post-claim earnings of ER claimants, since it is difficult to interpret the effect of ER on these outcomes in isolation.

3.1 Effect of ER by reason for job loss

In order to study whether the effects of eliminating the WSR are different for claimants on permanently laid off than for claimants who lost their jobs for other reasons, we estimate separate models by five mutually exclusive reasons for job loss: 1) quit for reasons satisfying the standard for "good cause," 2) lost job permanently, 3) temporary layoff, 4) contract ended/seasonal layoff, and 5) lost job for reasons unknown. The Data Appendix explains in detail how we created these indicators.

We estimate Equation (1) for each of the five reasons for job loss, where each model compares outcomes for claimants assigned to ER who lost their jobs due to a given reason to claimants in the control group who lost their job for the same reason. Since reason for job loss is pre-determined with respect to treatment assignment, the coefficient on the ER indicator yields an intention-to-treat effect of eliminating the WSR for a given reason-for-job-loss category of claimants.

3.2 Effect of ER by likelihood of benefit exhaustion

In order to study whether the long-term unemployed benefit from the elimination of the WSR, we study claimants with a high and low probability of exhausting benefits separately. In practice, we construct an ex ante probability of benefit exhaustion. First, using a probit, we estimate a likelihood of benefit exhaustion over the comparison group sample. To estimate the probit, we include all of the variables in Table 2, plus the unemployment rate in the county and month the claim was filed and quarter of claim in the conditioning set. Second, we assign the predicted likelihood values to all the claimants in the analysis sample.[8] We define a claimant as "high probability" if the claimant's ex ante probability of exhausting benefits is higher than the comparison group average, which equals 26.4 percent.[9] We define a claimant as "low probability" if the claimant has an ex ante probability that is lower than the comparison group average.

3.3 Threats to validity

Since WAWS is a random-assignment experiment, it has high internal validity. However, external validity might be compromised if the inferences and conclusions cannot be generalized from the population and setting in which they are studied to other populations and settings. We believe that external validity of the study is reasonably high, as the state of Washington is not an outlier with respect to the characteristics of its population. As Johnson and Klepinger (1994) note, the UI practices implemented in the state of Washington at the time of the demonstration

[8] This bears similarities to estimating a worker profiling score; see Berger et al. (1997) and Berger et al. (2000).
[9] In order to increase the number of observations and avoid colinearity problems, we estimate the likelihood model using the pooled NWS policy group and control group for all the quarters of the experiment. The mean of value of exhausted benefits is 26.4 percent in this sample, which is slightly higher than the mean value in Table 3, where it is 23.1 percent. Table A10 in the Results Appendix shows the estimated coefficients for the model predicting benefit exhaustion.

(that is, the standard WSR that the claimants assigned to the control group were subject to) did not deviate from the approach used in most other states at that time. It is also worthwhile to note that the average unemployment rate in Tacoma, the location of WAWS experiment, was at the time about 7.9 percent. Therefore, the estimated effects pertain to relatively slack labor market conditions, a setting that makes our findings of current interest.

Another concern regarding external validity is whether compliance with the experimental protocol is specific to a given demonstration. In the case of ER, the issue of noncompliance (opting out of treatment) is not really a concern because the ER treatment is in the form of information and instructions supplied to claimants when they file for benefits. That is, the treatment does not include a follow up, and hence the possibility of noncompliance as would be the case with a training program or job search assistance.

A potential threat to external validity is whether turning a temporary and local experimental program into a permanent and widespread policy might change the economic environment in such a way that the conclusions from the smaller-scale experiment cannot be generalized. For example, in the permanent absence of the WSR, more workers might be induced to enter the UI system, thus changing the composition of the pool of claimants from that studied in the original WAWS demonstration. This would reduce the external validity of the experiment.

Finally, we discuss attrition from our long-term panel and the reliability of our follow-up outcome measures in the Data Appendix.

4 Results

4.1 Baseline results

Table 4 reports the estimated long-term effect of assignment to ER on the probability of employment, hours worked, and total earnings in each of the nine years following enrollment in the WAWS experiment. In order for the treatment effects to be interpreted as deviations from the comparison group mean, we present the mean and standard deviation of the comparison group to the left of the β estimate. Except for the 2 percentage point lower probability of employment in the year of the experiment, ER did not have a statistically significant effect on employment in the other post-experimental years, nor did it have an effect on hours worked or earnings.

Table 5 reports the estimated effect of assignment to ER on the other post-claim long-term employment outcomes: the number of post-claim employers, quarters of nonemployment, the standard deviation of subsequent earnings, and our proxy for job match quality—quarters with the first post-claim employer. As in the previous table, we present the mean and standard deviation of the comparison group to the left of each estimated coefficient. Since Figures 1–3 imply that some of these variables have a long right-tail, we also present the comparison-group median.

On average, a claimant in the comparison group spent about two years with the first post-claim employer, but the median tenure equals only three quarters. Rounding down the mean, we see that the mean and the median number of post-claim employers in the 40 quarters following the experiment equals four. The median number of quarters of nonemployment equals one quarter, while the mean equals about 3.6 quarters.

Turning to the β coefficient, we see that the point estimates in Table 5 suggest that ER prolonged the duration of nonemployment but also increased tenure with first employer, reduced the number of post-claim employers, and reduced the volatility of earnings. However, all the point estimates in Table 5 are small, and no point estimate is statistically different from zero. In

sum, the results in Tables 4 and 5 suggest that for UI claimants as a whole, eliminating the WSR did not have a statistically significant effect on any employment outcome in the nine years following the experiment.

4.2 The effect of ER by reason for job loss

In order to see if the effect of ER on outcomes differs depending on reason for job loss, in Table 6, each row presents the estimated effect of ER on a selected outcome, by reason for job loss. Table 7 complements Table 6 by presenting the mean and standard deviation of each outcome for each reason-for-job-loss category for the claimants in the comparison group.

Turning to the effect of ER on UI receipt outcomes (total UI benefits paid, weeks of UI payments, and whether a claimant exhausted benefits), we see that the estimates in Table 6 are numerically similar to the estimates from Table 3.[10] For every reason for job loss category, the ER claimants received between about $410 and $510 more in total UI benefits, for about 3–4 weeks longer, and were about 10 percentage points more likely to exhaust benefits than claimants in the comparison group.

Caution must be exercised when comparing the results *across* the groups in Table 6, as the comparison group baseline average is different depending on reason for job loss; see the means of outcomes of the comparison group in Table 7.[11] Taking these differences into account, it turns out that, relative to the comparison group average, the increase in total UI benefits paid and weeks of UI payments is similar across the reason for job loss categories; however, the

[10] To save space, we present only benefit year outcomes and not both benefit year and first spell outcomes, as in Table 3.

[11] For example, claimants who are unemployed due to a permanent job loss are more likely to be female, white, college educated, and work more in finances and services compared to the entire sample of UI claimants in WAWS. They are also likely to have had a prior UI claim. Claimants temporarily laid off are on the other hand more likely to be male, younger, less likely to have a college degree, but more likely to work in construction or manufacturing. They are also less likely to have had a prior UI claim. The underlying descriptive statistics are available from the authors.

likelihood of benefit exhaustion for ER claimants on temporary layoff is strikingly 79 percent higher (that is, 0.108/0.136). The likelihood of exhausting benefits is 40–52 percent higher for ER claimants who became unemployed for other reasons.

Turning to the year 0 employment outcomes (in Table 6), we see that ER claimants who were permanently laid off had lower chances of employment, worked fewer hours, and had lower total earnings compared to comparison claimants laid off permanently. This decrease is, however, only transitory: by year 1, the outcomes for the ER claimants were statistically undistinguishable from the comparison group.[12] The temporary negative effect on employment outcomes during the year of the experiment is consistent with the ER claimants taking almost 1.5 quarters longer to find employment than the baseline average of 4.2 quarters or (see Table 7). It appears that the longer duration of insured unemployment resulted in a longer duration of nonemployment. In Table 6, we also see that ER claimants who were permanently laid off had a shorter tenure with their first post-claim employer by about 1.65 quarters. This suggests that the first job match of permanently laid off claimants assigned to ER was less successful than the first job match of permanently laid off claimants in the comparison group.

The effect of ER claimants on temporary layoff is very different. We see that the only statistically significant employment outcome effect is a decrease in the number of post-claim employers. We also see that ER claimants on temporary layoff had a 4.4 percentage point higher probability of returning to a previous employer, but this effect is not statistically significant (t-value is 1.42). Overall, the marginally improved probability of returning to a former employer and the reduction in job changing following ER did not lead to long-term gains in earnings or employment.

[12] Also in later years the employment outcome differences are not statistically different from zero; we do not show this in Table 6 to conserve space.

Interestingly, the largest group, the claimants who lost their jobs for reasons unknown to us, had higher total earnings in year 1, experienced a shorter duration on nonemployment by almost a quarter, and were more likely to return to their pre-claim employers by 4.7 percentage points. This is intriguing, as this is the only group in Table 6 for which there is a statistically significant effect on return to same employer. Initially, we expected that the increase in the probability of return to same employer reported in Table 3 would be explained by a higher probability of return by claimants placed on recall. However, as Table 6 shows, this effect is driven by the group whose reasons for unemployment are unknown to us.

Other than a higher probability of return to former industry for ER claimants who were seasonal or contract workers, for the claimants in the remaining category, claimants who quit, ER did not have a statistically different effect on any employment outcomes.

4.3 The effect of ER by likelihood of benefit exhaustion

In Table 8, we show the long-term effects of assignment to ER on the probability of employment, hours worked, and earnings during the nine years following enrollment in the experiment for claimants likely to exhaust their benefits, i.e., claimants whose predicted likelihood is higher than the comparison-group average. We see that in year 0, ER claimants had a 4.4 percentage point lower likelihood of reemployment than claimants in the comparison group. We also see a negative effect on employment in year 3 but not in the years before and after, which may question how much stock we can put on this finding. There is no statistically significant effect on any of the other employment outcomes, hours worked and earnings.

In Table 9, we present the results for claimants statistically unlikely to exhaust benefits. We see that the outcomes for these ER claimants were not statistically different from the outcomes of claimants assigned to the comparison group.

Finally, in Table 10 we show the effect of ER for claimants with both high and low probability of exhausting benefits on the remaining employment outcomes: number of post-claim employers, quarters of nonemployment, standard deviation of earnings, quarters with the first post-claim employer, and the likelihood to return to a former employer and former industry. Except for an increase in the probability to return to a former employer for ER claimants unlikely to exhaust benefits, in no remaining case is the effect of ER statistically different from zero.

5 Discussion and Summary

A longstanding concern about strict enforcement of the UI work search requirement (WSR) is that it may pressure unemployed job seekers to accept a job "too soon," reducing job match quality and long-term earnings. In addition to being undesirable for workers this could be detrimental to employers, many of whom value long-term relationships and are willing to pay higher wages to encourage long tenure; see Farber (1999).

The Washington Alternative Work Search experiment tested the effects of eliminating the WSR by randomly assigning new UI claimants to a control group and to an "exception reporting" (ER) honor system in which claimants were told to search actively for reemployment but were also told their benefits would be sent to them unless they told the UI agency that they had found a job or had stopped looking for work. By appending nine years of administrative wage records to the original data from the experiment, we are able to examine the long-term

effects of ER—that is, the effects on employment tenure, number of post-claim employers, employment, hours, and earnings.

In the short term, ER increased the duration of UI benefit receipt, benefits received, and the probability of exhausting benefits. Although it also increased the probability that a worker would return to a former employer, which could be a positive outcome, in the long-term, (that is, in the nine years following the experiment), ER had no effect on earnings, hours worked, or other employment outcomes. We also find no evidence of a statistically significant effect of ER on time to reemployment, post-claim employment tenure, number of post-claim employers, or volatility of earnings. Overall, then, ER increased claimant moral hazard and the costs to the UI system without observable gains for workers.

We also study the effects of ER by reason for unemployment, and find differences among different groups of claimants. First, eliminating the WSR was harmful in the short run for claimants who lost their job as a result of a permanent layoff, consistent with negative duration dependence. During the year of the experiment, these claimants experienced lower probability of reemployment, worked fewer hours, and had lower earnings. Moreover, in the long term, these claimants were reemployed about 1.4 quarters later then the comparison group and experienced shorter job tenure with their first post-claim employer by 1.65 quarters. Both of these effects are economically large and imply strongly that the WSR is a policy that benefits UI claimants who were permanently laid off.

Second, it appears that eliminating the WSR led to more abuse of the UI system by all groups of claimants who were not permanent job losers—claimants who quit, claimants on temporary layoff, or claimants who were contract or seasonal workers. For these claimants, ER led to more benefit payments, a longer spell of insured unemployment, and a higher likelihood of

exhausting benefits. However, the probability of reemployment, the number of hours worked, and earnings for these claimants were no different from those assigned to the comparison group (who were subject to the WSR). This implies that eliminating the WSR led to increased claimant moral hazard—UI benefits drawn were greater, but for the ER claimants who were not permanent job losers, the employment outcomes were no different than for claimants subjected to the standard WSR.

Overall, eliminating the WSR was costly to the UI system without convincingly improving employment outcomes for any claimant category considered. The clear conclusion for policy is that the WSR is an important tool for improving outcomes of permanent job losers and for reducing moral hazard associated with UI for other UI claimants.

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Results

Table 1
Eligibility Review Interviews and Employment Services Received by Control, Exception Reporting, and New Work Search Groups

Service[a]	(1) Control	(2) Exception Reporting	(3) New Work Search	(4) Difference between (1) and (2)*	(5) Difference between (1) and (3)*
Eligibility review interview	0.250	0.004	0.322	0.000	0.000
Employment services					
job referral/placement	0.185	0.155	0.160	0.027	0.102
job development plan	0.114	0.007	0.182	0.000	0.000
other employment service[b]	0.107	0.062	0.116	0.000	0.466
Sample size	1,539	1,606	1,073		

Source: Author's tabulations of the Washington Alternative Work Search experimental data, from UI claims records, administrative wage records, and Employment Service records. See the Data Appendix for details.

Notes: Universe consists of exception reporting, control and new work search groups during fall 1986, winter 1987, and spring 1987.

* *p* -value for test of difference of means.

a. A claimant may receive more than one category of services.

b. Job consultation, receipt of or referral to training, testing, support services, job development (contacting an employer on the claimant's behalf), or any other contact with the Employment Service.

Table 2
Characteristics of Control, New Work Search, and Exception Reporting Groups

Covariate	Control	Exception Reporting	Difference[1] (p -value)	Control	New Work Search	Difference[1] (p -value)	New Work Search and Control	Exception Reporting	Difference[1] (p -value)
Male	0.718	0.717	0.935	0.718	0.713	0.779	0.716	0.717	0.958
Race									
white	0.819	0.828	0.488	0.819	0.829	0.479	0.823	0.828	0.677
black	0.097	0.099	0.885	0.097	0.087	0.350	0.093	0.099	0.521
other	0.084	0.073	0.252	0.084	0.084	0.996	0.084	0.073	0.201
Age									
≤ 24	0.218	0.210	0.592	0.218	0.192	0.111	0.207	0.210	0.833
25-34	0.389	0.404	0.394	0.389	0.391	0.909	0.390	0.404	0.367
35-44	0.240	0.207	**0.029**	0.240	0.222	0.285	0.232	0.207	0.058
45-54	0.103	0.111	0.459	0.103	0.129	0.040	0.113	0.111	0.804
≥ 54	0.051	0.068	**0.042**	0.051	0.066	0.093	0.057	0.068	0.154
Schooling									
less than high school	0.159	0.123	**0.004**	0.159	0.148	0.471	0.154	0.123	**0.004**
high school	0.537	0.566	0.099	0.537	0.542	0.774	0.539	0.566	0.088
some college	0.225	0.240	0.303	0.225	0.242	0.298	0.232	0.240	0.535
college graduate	0.080	0.071	0.343	0.080	0.067	0.220	0.075	0.071	0.657
Veteran	0.196	0.190	0.654	0.196	0.215	0.235	0.204	0.190	0.264
Marital status/gender									
married male	0.270	0.264	0.721	0.270	0.242	0.116	0.258	0.264	0.688
married female	0.099	0.094	0.609	0.099	0.096	0.772	0.098	0.094	0.670
Household status									
no dependents	0.309	0.329	0.212	0.309	0.322	0.485	0.314	0.329	0.296
1 dependent	0.155	0.148	0.546	0.155	0.169	0.360	0.161	0.148	0.250
2 or more dependents	0.236	0.229	0.626	0.236	0.207	0.081	0.224	0.229	0.731
homeowner	0.286	0.285	0.934	0.286	0.253	0.067	0.273	0.285	0.399
Pre-claim earnings ($)									
1 year before	13,841	13,559	0.436	13,841	13,531	0.447	13,713	13,559	0.632
2 years before	11,900	11,571	0.417	11,900	11,639	0.563	11,793	11,571	0.538
3 years before	10,744	10,737	0.988	10,744	10,801	0.904	10,767	10,737	0.936
Pre-claim hours									

Covariate	Control	Exception Reporting	Difference[1] (p -value)	Control	New Work Search	Difference[1] (p -value)	New Work Search and Control	Exception Reporting	Difference[1] (p -value)
1 year before	1334	1313	0.376	1334	1286	0.073	1314	1313	0.938
2 years before	1101	1064	0.178	1101	1076	0.414	1091	1064	0.271
3 years before	946	931	0.599	946	964	0.583	954	931	0.382
Occupation									
professional	0.105	0.102	0.772	0.105	0.106	0.936	0.106	0.102	0.714
clerical	0.122	0.133	0.379	0.122	0.116	0.661	0.120	0.133	0.222
sales	0.058	0.059	0.933	0.058	0.050	0.407	0.055	0.059	0.605
service	0.101	0.101	0.988	0.101	0.123	0.073	0.110	0.101	0.357
agric., fishery, forestry	0.026	0.028	0.726	0.026	0.021	0.455	0.024	0.028	0.436
processing	0.038	0.033	0.420	0.038	0.035	0.698	0.037	0.033	0.481
machine trades	0.086	0.090	0.700	0.086	0.107	0.066	0.095	0.090	0.594
benchwork	0.046	0.048	0.811	0.046	0.049	0.700	0.047	0.048	0.944
structural work	0.266	0.265	0.910	0.266	0.274	0.667	0.270	0.265	0.728
miscellaneous	0.151	0.143	0.486	0.151	0.116	**0.011**	0.137	0.143	0.614
Industry									
agriculture	0.025	0.025	0.969	0.025	0.021	0.588	0.023	0.025	0.749
mining	0.001	0.001	0.539	0.001	0.001	0.785	0.001	0.001	0.590
construction	0.205	0.196	0.520	0.205	0.190	0.338	0.199	0.196	0.816
manufacturing	0.237	0.232	0.778	0.237	0.263	0.125	0.247	0.232	0.267
transportation, utilities	0.038	0.054	**0.028**	0.038	0.034	0.577	0.036	0.054	**0.005**
wholesale trade	0.070	0.060	0.237	0.070	0.048	**0.023**	0.061	0.060	0.845
retail trade	0.159	0.158	0.938	0.159	0.158	0.994	0.158	0.158	0.934
finance, ins., real estate	0.028	0.031	0.597	0.028	0.031	0.674	0.029	0.031	0.706
services	0.174	0.172	0.866	0.174	0.172	0.909	0.173	0.172	0.896
government	0.045	0.057	0.135	0.045	0.054	0.318	0.049	0.057	0.240
unclassified	0.018	0.014	0.390	0.018	0.027	0.129	0.022	0.014	0.083
Prior UI claim									
none	0.804	0.804	0.969	0.804	0.791	0.408	0.799	0.804	0.701
duration ≤ 15 weeks	0.104	0.100	0.688	0.104	0.106	0.852	0.105	0.100	0.584
duration > 15 weeks	0.092	0.097	0.638	0.092	0.103	0.353	0.096	0.097	0.964
Reason for job loss									
permanent layoff	0.172	0.153	0.149	0.172	0.157	0.291	0.166	0.153	0.280
temporary layoff with recall date	0.231	0.265	**0.027**	0.231	0.253	0.179	0.240	0.265	0.073
contract/seasonal	0.155	0.154	0.908	0.155	0.156	0.981	0.155	0.154	0.886

Covariate	Control	Exception Reporting	Difference[1] (p -value)	Control	New Work Search	Difference[1] (p -value)	New Work Search and Control	Exception Reporting	Difference[1] (p -value)
quit	0.172	0.000	0.697	0.172	0.167	0.719	0.170	0.000	0.533
Employer-attached/placed by union[2]	0.355	0.286	**0.000**	0.355	0.371	0.418	0.362	0.286	**0.000**
UI benefits/claim type									
weekly amount ($)	146	145	0.564	146	145	0.640	146	145	0.686
maximum amount ($)	3,868	3,830	0.529	3,868	3,849	0.776	3,860	3,830	0.576
potential duration	26.0	25.9	0.887	26.0	26.0	0.875	26.0	25.9	0.810
replacement rate (percent)[3]	61.7	61.6	0.841	61.7	61.4	0.733	61.6	61.6	0.961
combined wage claim[4]	0.049	0.044	0.492	0.049	0.045	0.635	0.047	0.044	0.597
ex-service member claim	0.034	0.035	0.868	0.034	0.034	0.923	0.034	0.035	0.890
federal employee claim	0.009	0.018	**0.031**	0.009	0.014	0.241	0.011	0.018	0.060
Reservation wage (hourly)									
≤ $5.00	0.190	0.181	0.479	0.190	0.175	0.325	0.184	0.181	0.770
$5.01–$7.00	0.151	0.164	0.294	0.151	0.142	0.519	0.147	0.164	0.129
$7.01–$10.00	0.138	0.161	0.065	0.138	0.157	0.180	0.145	0.161	0.165
$10.01–$20.00	0.143	0.130	0.272	0.143	0.138	0.717	0.141	0.130	0.296
> $20.00	0.110	0.106	0.719	0.110	0.117	0.581	0.113	0.106	0.491
Sample size	1,539	1,606		1,539	1,073		2,612	1,606	

Notes: Universe consists of exception reporting, control and new work search groups during fall 1986, winter 1987, and spring 1987.

Source: Author's tabulations of the Washington Alternative Work Search experimental data.

1. Bold denotes p -values for the test of mean differences between groups < .05.

2. Claimants were not required to search for work if they were on layoff with a set recall date or if they were placed through a union hiring hall.

3. The replacement rate is the weekly benefit amount as a percentage of average weekly earnings before the UI claim.

4. Combined wage claims use earnings from more than one state to calculate base period earnings.

Table 3

Effect of Exception Reporting on Selected Outcomes as Deviations from the Comparison Group (Control and New Work Search Group pooled)

	Comparison (Control and New Work Search)		Exception Reporting	
Outcome	Mean (Std.Dev.)		Coefficient (Std. error)	
UI Receipt Outcomes				
Benefit year				
Total UI benefits paid ($)	1,956	(1728)	451***	(47)
Weeks of UI payments	14.18	(10.5)	3.26***	(0.31)
Exhausted benefits (proportion)	0.231	(0.422)	0.114***	(0.014)
First spell				
Total benefits in first UI spell ($)	1,638	(1636)	445***	(48)
Weeks of first UI spell	13.58	(10.58)	3.43***	(0.33)
Employment Outcomes				
First quarter outcomes[a]				
Employed (proportion)	0.695	(0.46)	-0.032**	(0.014)
Hours worked	201.3	(210)	-7.3	(6.1)
Total Earnings ($)	2,285	(2676)	-84	(71)
Year 0 outcomes[a]				
Employed (proportion)	0.89	(0.313)	-0.020*	(0.010)
Hours worked	1016	(746)	-29.0	(21.7)
Total Earnings ($)	11,617	(10143)	-277	(253)
Year 1 outcomes[a]				
Employed (proportion)	0.843	(0.364)	-0.005	(0.011)
Hours worked	1134	(838)	0.9	(25.2)
Total Earnings ($)	13,122	(11210)	321	(305)
Other outcomes				
Returned to same employer (proportion)	0.322	(0.467)	0.029**	(0.013)
Returned to same industry (proportion)	0.441	(0.497)	0.019	(0.014)
Sample size			4,218	

Notes: Universe consists of exception reporting, control, and new work search groups during fall 1986, winter 1987, and spring 1987. Robust standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Regression-adjusted differences control for all variables displayed in Table 2 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed.

a. First quarter is the quarter following the claim quarter. Year 0 is defined as the sum of the first, second, third, and fourth quarter after the claim quarter. Year 1 is defined as the sum of the fifth, sixth, seventh, and eighth quarter after the claim quarter. Earnings are expressed in 1988:4 prices.

Table 4

Effect of Exception Reporting on Long-Term Employment, Hours, and Earnings as Deviations from the Comparison Group

Outcome	Proportion employed				Hours worked				Total Earnings ($)			
	Comparison group		Exception Reporting		Comparison group		Exception Reporting		Comparison group		Exception Reporting	
	Mean (Std. dev.)		Coeff.	(Std. error)	Mean (Std. dev.)		Coeff.	(Std. error)	Mean (Std. dev.)		Coeff.	(Std. error)
Year relative to claim												
Year 0	0.890	(0.313)	-0.020*	(0.010)	1,016	(746)	-29.0	(21.7)	11,617	(10,143)	-277	(253)
Year 1	0.843	(0.364)	-0.005	(0.011)	1,134	(838)	0.9	(25.2)	13,122	(11,210)	321	(305)
Year 2	0.810	(0.393)	-0.008	(0.012)	1,133	(867)	-13.2	(25.9)	13,828	(12,014)	-180	(329)
Year 3	0.776	(0.417)	-0.015	(0.013)	1,104	(883)	7.0	(26.8)	13,615	(12,204)	-114	(350)
Year 4	0.741	(0.438)	-0.011	(0.014)	1,038	(886)	4.6	(27.0)	13,199	(12,714)	-96	(364)
Year 5	0.713	(0.452)	-0.007	(0.014)	992	(897)	29.3	(27.5)	12,728	(12,752)	349	(368)
Year 6	0.681	(0.466)	0.005	(0.014)	942	(912)	27.0	(27.7)	12,001	(12,925)	304	(368)
Year 7	0.659	(0.474)	0.003	(0.014)	932	(922)	9.8	(28.0)	11,621	(12,674)	445	(374)
Year 8	0.637	(0.481)	-0.003	(0.015)	891	(915)	6.7	(28.1)	11,442	(12,918)	415	(387)
Year 9	0.624	(0.484)	-0.001	(0.015)	913	(943)	1.7	(28.6)	11,780	(13,532)	280	(401)
Sample size			4,218				4,218				4,218	

Notes: See annotations to Table 3.

Table 5
Effect of Exception Reporting on Selected Long-Term Employment Outcomes as Deviations from the Comparison Group

	Comparison group			Exception Reporting	
	Mean	Median	(Std. dev.)	Coeff.	(Std. error)
Outcome					
Number of post-claim employers	4.68	4	(3.78)	-0.13	(0.11)
Quarters of nonemployment	3.63	1	(8.34)	0.12	(0.25)
Std. deviation of post-claim earnings ($)	5,850	5,253	(3,965)	-70	(115)
Quarters with first post-claim employer	8.05	3	(10.85)	0.23	(0.33)
Sample size				4,218	

Notes: See annotations to Table 3. Number of post-claim employers is defined as the number of unique employers we observe from the first quarter after the claim until the last quarter. Quarters of nonemployment is defined as the number of consecutive post-claim quarters without covered earnings. Std. deviation of earnings is defined as the standard deviation of earnings in the nine years following the claim.

Table 6

Effects of Exception Reporting on Selected Outcomes by Reason for Job Loss as Deviations from the Comparison Group

Outcome	Reason for Job Loss				
	Quit	Permanent job loss	Temporary layoff	Contract/Seasonal layoff	Unknown
UI Receipt Outcomes					
Benefit year outcomes					
Total UI benefits paid ($)	456***	491***	416***	518***	485***
	(117)	(118)	(102)	(120)	(93)
Weeks of UI payments	3.99***	3.64***	2.93***	3.33***	3.18***
	(0.81)	(0.82)	(0.65)	(0.77)	(0.61)
Exhausted benefits (proportion)	0.117***	0.157***	0.108***	0.096***	0.123***
	(0.038)	(0.039)	(0.025)	(0.036)	(0.027)
Employment Outcomes					
Year 0 outcomes					
Employed (proportion)	-0.048	-0.072**	-0.011	-0.006	0.004
	(0.030)	(0.028)	(0.016)	(0.022)	(0.021)
Hours worked	-56.9	-105.2*	14.5	-34.5	-25.6
	(60.0)	(58.9)	(41.3)	(53.0)	(42.4)
Total Earnings ($)	-450	-1,701**	-419	-27	408
	(583)	(663)	(526)	(709)	(477)
Year 1 outcomes					
Employed (proportion)	-0.036	-0.050	0.004	0.007	0.018
	(0.031)	(0.031)	(0.020)	(0.028)	(0.023)
Hours worked	-45.4	-36.8	56.5	-11.2	16.7
	(65.0)	(65.4)	(51.1)	(63.5)	(50.4)
Total Earnings ($)	-499	-673	46	1,066	1,264**
	(699)	(778)	(626)	(822)	(595)
Other post-claim outcomes					
Number of post-claim employers	-0.14	0.08	-0.40*	0.18	-0.23
	(0.27)	(0.30)	(0.21)	(0.36)	(0.21)
Quarters of nonemployment	0.55	1.44**	0.29	0.22	-0.99*
	(0.76)	(0.71)	(0.39)	(0.59)	(0.51)
Std. deviation earnings ($)	-248	-7	-125	-351	327
	(276)	(293)	(235)	(311)	(229)
Quarters with first post-claim employer	-0.83	-1.65**	1.21	0.49	0.88
	(0.70)	(0.77)	(0.75)	(0.91)	(0.68)
Returned to same employer (proportion)	0.001	-0.017	0.044	0.058	0.047*
	(0.028)	(0.027)	(0.031)	(0.039)	(0.027)
Returned to same industry (proportion)	-0.027	-0.044	0.045	0.070*	0.038
	(0.034)	(0.035)	(0.030)	(0.041)	(0.028)
Sample size	729	679	1,052	653	1,105

Notes: See annotations to Table 3.

Table 7
Mean and Standard Deviation (in parentheses) of Outcomes of the Comparison Group by Reason for Job Loss

Outcome	Reason for Job Loss				
	Quit	Permanent job loss	Temporary layoff	Contract/Seasonal layoff	Unknown
UI Receipt Outcomes					
Benefit year outcomes					
Total UI benefits paid ($)	1,857	2,322	1,685	2,244	1,867
	(1,763)	(1,813)	(1,580)	(1,751)	(1,710)
Weeks of UI payments	14.2	16.9	12.1	15.4	13.7
	(11.2)	(11.2)	(11.2)	(11.2)	(11.2)
Exhausted benefits (proportion)	0.273	0.316	0.136	0.236	0.235
	(0.446)	(0.466)	(0.343)	(0.425)	(0.424)
Employment Outcomes					
Year 0 outcomes					
Employed (proportion)	0.849	0.868	0.944	0.924	0.862
	(0.358)	(0.338)	(0.23)	(0.266)	(0.345)
Hours worked	860	920	1,228	978	1,005
	(779)	(738)	(710)	(690)	(751)
Total Earnings ($)	8,083	9,651	15,300	13,437	10,721
	(8,514)	(9,586)	(10,954)	(10,698)	(9,071)
Year 1 outcomes					
Employed (proportion)	0.806	0.829	0.885	0.874	0.819
	(0.396)	(0.377)	(0.319)	(0.332)	(0.385)
Hours worked	1,005	1,117	1,293	1,099	1,103
	(848)	(819)	(847)	(764)	(857)
Total Earnings ($)	10,085	11,715	16,185	14,949	12,118
	(9,694)	(10,482)	(12,288)	(11,638)	(10,462)
Other post-claim outcomes					
Number of post-claim employers	4.59	4.52	4.51	5.62	4.43
	(3.5)	(3.69)	(3.5)	(4.4)	(3.77)
Quarters of nonemployment	4.60	4.20	2.21	2.85	4.38
	(9.42)	(9.15)	(5.49)	(6.99)	(9.63)
Std. deviation earnings ($)	5,229	5,640	6,212	6,668	5,576
	(3,754)	(4,153)	(3,842)	(4,161)	(3,873)
Quarters with first post-claim employer	6.39	7.40	9.31	8.13	8.32
	(9.33)	(10.7)	(11.52)	(10.77)	(11.12)
Returned to same employer (proportion)	0.151	0.127	0.52	0.347	0.359
	(0.358)	(0.333)	(0.5)	(0.477)	(0.48)
Returned to same industry (proportion)	0.286	0.273	0.616	0.48	0.463
	(0.452)	(0.446)	(0.487)	(0.5)	(0.499)
Sample size	729	679	1,052	653	1,105

Notes: Universe consists of control and new work search groups during fall 1986, winter 1987, and spring 1987.

Table 8

Effect of Exception Reporting on Long-Term Employment, Hours, and Earnings for Claimants with a Higher than Average Predicted Likelihood of Benefit Exhaustion

Outcome	Proportion employed				Hours worked				Total Earnings ($)			
	Comparison group		Exception Reporting		Comparison group		Exception Reporting		Comparison group		Exception Reporting	
	Mean (Std.dev.)		Coeff.	(Std. error)	Mean (Std.dev.)		Coeff.	(Std. error)	Mean (Std.dev.)		Coeff.	(Std. error)
Year relative to claim												
Year 0	0.834	(0.372)	-0.044**	(0.020)	805	(744)	-45.1	(37.7)	8838	(8807)	-452	(453)
Year 1	0.794	(0.405)	-0.013	(0.021)	964	(832)	6.3	(43.6)	10688	(10086)	314	(555)
Year 2	0.759	(0.428)	-0.020	(0.023)	956	(851)	5.1	(44.4)	11278	(11189)	-199	(588)
Year 3	0.729	(0.445)	-0.054**	(0.024)	942	(864)	18.7	(46.0)	11072	(11543)	29	(609)
Year 4	0.673	(0.469)	-0.022	(0.025)	869	(875)	36.7	(46.5)	10480	(11972)	184	(630)
Year 5	0.627	(0.484)	-0.013	(0.025)	818	(892)	45.0	(46.8)	9882	(11626)	720	(629)
Year 6	0.608	(0.489)	-0.013	(0.026)	772	(884)	44.0	(46.3)	9092	(11523)	861	(615)
Year 7	0.578	(0.494)	-0.006	(0.026)	755	(902)	37.6	(47.1)	8689	(11137)	884	(610)
Year 8	0.545	(0.498)	0.006	(0.026)	720	(885)	20.0	(46.6)	8549	(11353)	817	(627)
Year 9	0.539	(0.499)	0.004	(0.026)	757	(919)	-3.7	(48.1)	8826	(11712)	844	(665)
Sample size			1,543				1,543				1,543	

Notes: See annotations to Table 3.

The comparison group consist of claimants in the control and new work search groups with a higher than average probability of exhausting benefits. The average probability of exhausting benefits equals 0.264.

Table 9

Effect of Exception Reporting on Long-Term Employment, Hours, and Earnings for Claimants with a Lower than Average Predicted Likelihood of Benefit Exhaustion

Outcome	Proportion employed				Hours worked				Total Earnings ($)			
	Comparison group		Exception Reporting		Comparison group		Exception Reporting		Comparison group		Exception Reporting	
	Mean (Std.dev.)		Coeff.	(Std. error)	Mean (Std.dev.)		Coeff.	(Std. error)	Mean (Std.dev.)		Coeff.	(Std. error)
Year relative to claim												
Year 0	0.921	(0.27)	-0.007	(0.011)	1132	(721)	-27.0	(29.1)	13153	(10502)	-449	(416)
Year 1	0.87	(0.336)	-0.004	(0.014)	1228	(827)	-8.9	(33.4)	14467	(11571)	144	(472)
Year 2	0.838	(0.369)	-0.005	(0.015)	1231	(860)	-28.2	(34.3)	15237	(12224)	-288	(488)
Year 3	0.803	(0.398)	0.006	(0.016)	1194	(881)	0.5	(34.9)	15022	(12335)	-225	(498)
Year 4	0.779	(0.415)	-0.006	(0.017)	1131	(879)	-14.5	(34.7)	14702	(12865)	-292	(507)
Year 5	0.761	(0.427)	-0.006	(0.017)	1089	(885)	16.5	(35.7)	14302	(13074)	48	(516)
Year 6	0.722	(0.448)	0.012	(0.018)	1035	(914)	12.2	(36.3)	13609	(13372)	-67	(515)
Year 7	0.704	(0.457)	0.003	(0.018)	1030	(918)	-8.2	(36.6)	13243	(13175)	200	(530)
Year 8	0.687	(0.464)	-0.013	(0.019)	985	(918)	-5.5	(37.0)	13042	(13445)	181	(550)
Year 9	0.671	(0.47)	-0.009	(0.019)	1000	(945)	-2.0	(37.5)	13413	(14182)	-37	(562)
Sample size			2,675				2,675				2,675	

Notes: Same as above.

The comparison group consist of claimants in the control and new work search groups with a lower than average probability of exhausting benefits. The average probability of exhausting benefits equals 0.264.

Table 10

Effect of Exception Reporting on Long-Term Employment Outcomes by Varying Likelihood of Exhausting Benefits

Sample	Probability of exhausting benefits higher than average[a]				Probability of exhausting benefits lower than average[b]			
	Comparison group		Exception Reporting		Comparison group		Exception Reporting	
	Mean	(Std. dev.)	Coeff.	(Std. error)	Mean	(Std. dev.)	Coeff.	(Std. error)
Outcome								
Number of post-claim employers	4.67	(4.00)	-0.31	(0.20)	4.68	(3.64)	-0.09	(0.15)
Quarters of nonemployment	5.17	(10.46)	0.05	(0.52)	2.78	(6.75)	0.18	(0.28)
Std. deviation of post-claim earnings ($)	5,491	(4,139)	-70	(209)	6,049	(3,853)	-26	(155)
Quarters with first post-claim employer	6.82	(9.76)	-0.23	(0.50)	8.72	(11.35)	0.3	(0.46)
Returned to same employer (proportion)	0.215	(0.411)	0.001	(0.021)	0.381	(0.486)	0.038*	(0.020)
Returned to same industry (proportion)	0.328	(0.47)	-0.009	(0.024)	0.503	(0.5)	0.024	(0.020)
Sample size			1,543				2,675	

Notes: See annotations to Table 3.

a. The comparison group consist of claimants in the control and new work search groups with a higher than average probability of exhausting benefits. The average probability of exhausting benefits equals 0.264.

b.The comparison group consist of claimants in the control and new work search groups with a lower than average probability of exhausting benefits. The average probability of exhausting benefits equals 0.264.

Figure 1: Distribution of the number of post-claim employers

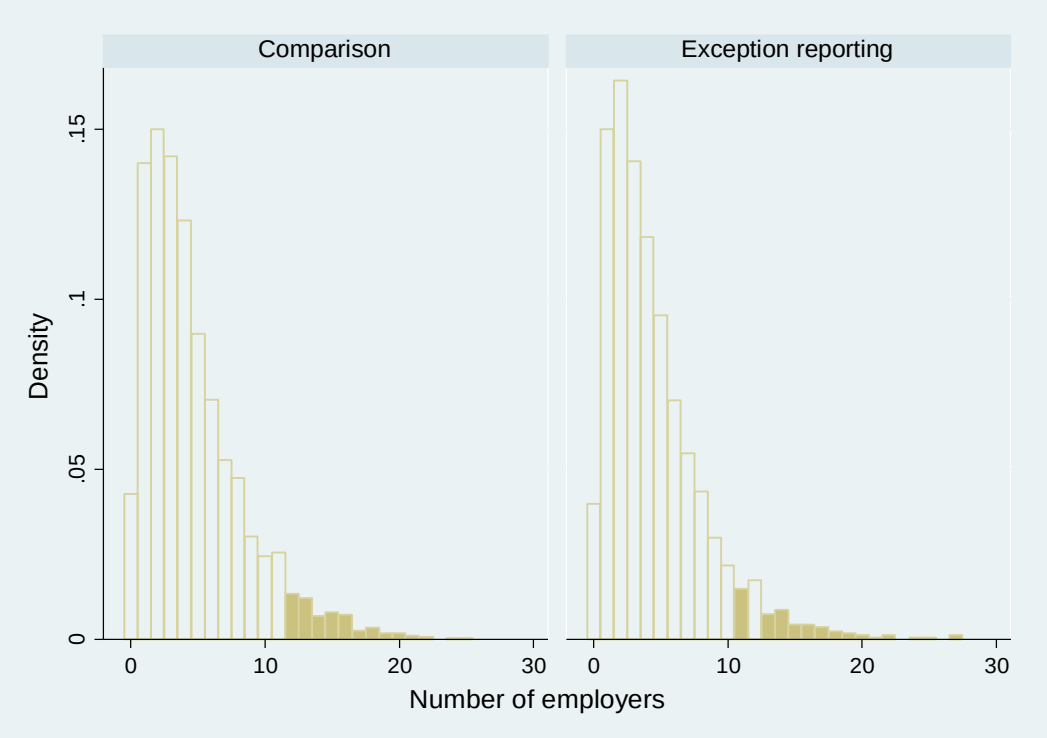


Figure 2: Distribution of quarters of nonemployment

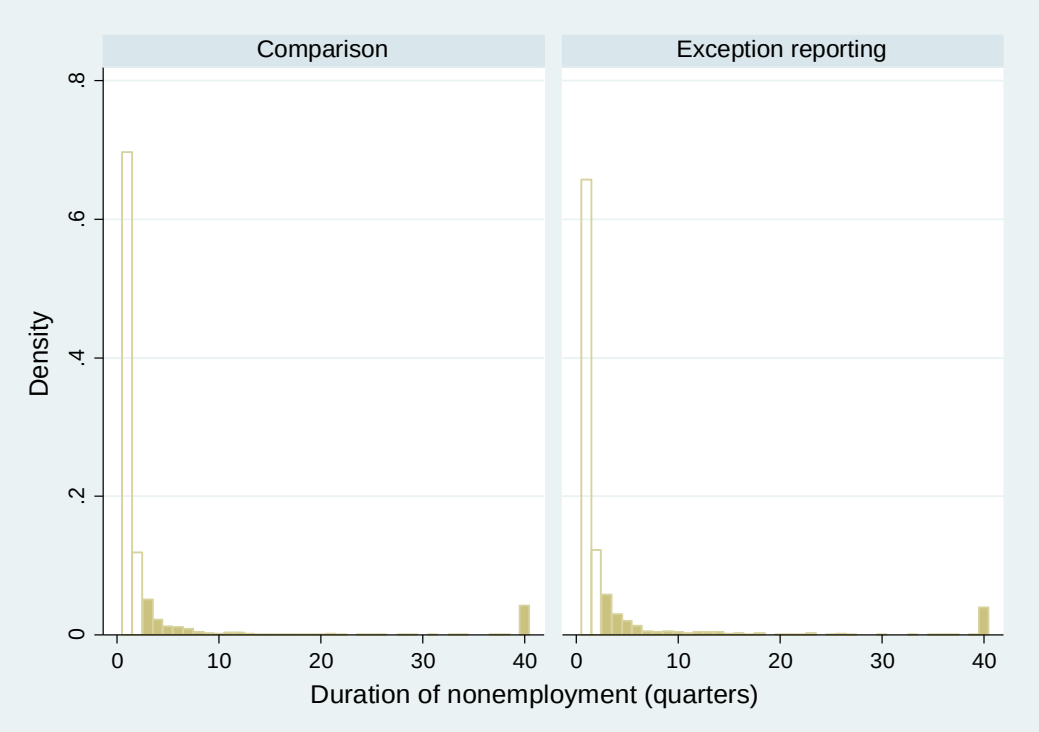
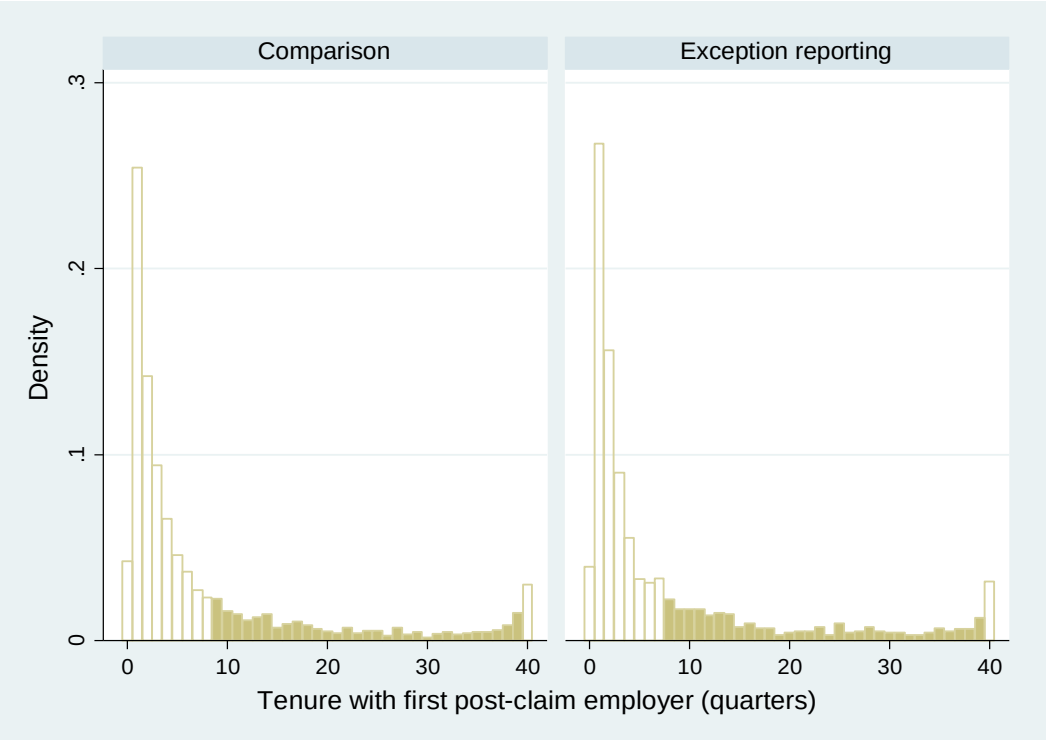


Figure 3: Distribution of the number of quarters with first post-claim employer



Results Appendix

Table A1

Effect of New Work Search on Selected Outcomes as Deviations from the Control Group

Outcome	Control group Mean (Std. dev.)		New Work Search Coefficient (Std.error)	
UI Receipt Outcomes				
Benefit year				
Total UI benefits paid ($)	$1,978	(1731)	-50	(60)
Weeks of UI payments	14.13	(10.40)	0.07	(0.40)
Exhausted benefits (proportion)	0.227	(0.419)	0.007	(0.016)
First spell				
Total benefits in first UI spell ($)	$1,654	(1642)	-34	(59)
Weeks of first UI spell	13.48	(10.46)	0.15	(0.41)
Employment Outcomes				
First quarter outcomes[a]				
Employed (proportion)	0.692	(0.462)	0.005	(0.018)
Hours worked	199.8	(212)	1.2	(7.9)
Total Earnings ($)	$2,285	(2,786)	-16	(88)
Year 0 outcomes[a]				
Employed (proportion)	0.888	(0.316)	0.008	(0.012)
Hours worked	1014	(751)	5.3	(27.3)
Total Earnings ($)	$11,701	(10,400)	-135	(314)
Year 1 outcomes[a]				
Employed (proportion)	0.847	(0.360)	-0.005	(0.014)
Hours worked	1138	(823)	-7.7	(31.7)
Total Earnings ($)	$13,304	(11,315)	-326	(373)
Other outcomes				
Returned to same employer (proportion)	0.316	(0.465)	0.008	(0.017)
Returned to same industry (proportion)	0.428	(0.495)	0.021	(0.018)
Sample size			2,612	

Notes: Universe consists of new work search and control groups during fall 1986, winter 1987, and spring 1987. Robust standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Regression-adjusted differences control for all variables displayed in Table 2 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed.

[a] First quarter is the quarter following the claim quarter. Year 0 is defined as the sum of the first, second, third, and fourth quarter after the claim quarter. Year 1 is defined as the sum of the fifth, sixth, seventh, and eighth quarter after the claim quarter. Earnings are expressed in 1988:4 prices.

Table A2

Effect of Exception Reporting on Selected Outcomes as Deviations from the Control Group

Outcome	Control group Mean (Std. dev.)		Exception Reporting Coefficient (Std.error)	
UI Receipt Outcomes				
Benefit year				
Total UI benefits paid ($)	$1,978	(1731)	429***	(54)
Weeks of UI payments	14.13	(10.40)	3.27***	(0.35)
Exhausted benefits (proportion)	0.227	(0.419)	0.116***	(0.015)
First spell				
Total benefits in first UI spell ($)	$1,654	(1642)	424***	(54)
Weeks of first UI spell	13.48	(10.46)	3.47***	(0.37)
Employment Outcomes				
First quarter outcomes[a]				
Employed (proportion)	0.692	(0.462)	-0.027*	(0.016)
Hours worked	199.8	(212)	-6.0	(7.0)
Total Earnings ($)	$2,285	(2,786)	-84	(82)
Year 0 outcomes[a]				
Employed (proportion)	0.888	(0.316)	-0.015	(0.011)
Hours worked	1014	(751)	-23.1	(24.7)
Total Earnings ($)	$11,701	(10,400)	-315	(290)
Year 1 outcomes[a]				
Employed (proportion)	0.847	(0.360)	-0.006	(0.013)
Hours worked	1138	(823)	-0.7	(28.4)
Total Earnings ($)	$13,304	(11,315)	186	(342)
Other outcomes				
Returned to same employer (proportion)	0.316	(0.465)	0.035**	(0.015)
Returned to same industry (proportion)	0.428	(0.495)	0.031*	(0.016)
Sample size			3,145	

Notes: Universe consists of exception reporting and control groups during fall 1986, winter 1987, and spring 1987. Robust standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Regression-adjusted differences control for all variables displayed in Table 2 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed.

[a] First quarter is the quarter following the claim quarter. Year 0 is defined as the sum of the first, second, third, and fourth quarter after the claim quarter. Year 1 is defined as the sum of the fifth, sixth, seventh, and eighth quarter after the claim quarter. Earnings are expressed in 1988:4 prices.

Table A3

Effect of Exception Reporting on Long-Term Employment, Hours, and Earnings as Deviations from the Control Group

Outcome	Proportion employed				Hours worked				Total Earnings ($)			
	Control group		Exception Reporting		Control group		Exception Reporting		Control group		Exception Reporting	
	Mean (Std. dev.)		Coeff.	(Std. error)	Mean (Std. dev.)		Coeff.	(Std. error)	Mean (Std. dev.)		Coeff.	(Std. error)
Year relative to claim												
Year 0	0.888	(0.316)	-0.015	(0.011)	1,014	(751)	-23.1	(24.7)	11,701	(10,400)	-315	(290)
Year 1	0.847	(0.36)	-0.006	(0.013)	1,138	(823)	-0.7	(28.4)	13,304	(11,315)	186	(342)
Year 2	0.817	(0.386)	-0.013	(0.014)	1,140	(858)	-19.8	(29.0)	14,154	(12,266)	-488	(372)
Year 3	0.781	(0.414)	-0.017	(0.015)	1,100	(881)	17.2	(30.1)	13,778	(12,416)	-209	(396)
Year 4	0.749	(0.434)	-0.014	(0.015)	1,044	(889)	8.9	(30.4)	13,503	(12,977)	-284	(413)
Year 5	0.728	(0.445)	-0.016	(0.015)	1,002	(893)	31.2	(30.7)	12,993	(12,832)	207	(414)
Year 6	0.686	(0.464)	0.006	(0.016)	953	(920)	22.3	(31.3)	12,204	(12,990)	196	(418)
Year 7	0.667	(0.471)	0	(0.016)	960	(925)	-7.5	(31.5)	12,075	(12,941)	136	(424)
Year 8	0.643	(0.479)	-0.005	(0.016)	904	(916)	5.2	(31.6)	11,800	(13,127)	220	(438)
Year 9	0.63	(0.483)	0	(0.017)	923	(943)	4.9	(32.3)	12,115	(13,817)	127	(456)
Sample size			3,145				3,145				3,145	

Notes: See annotations to Table A2.

Table A4

Effect of Exception Reporting on Selected Long-Term Employment Outcomes as Deviations from the Control Group

	Control group			Exception Reporting	
	Mean	Median	(Std. dev.)	Coeff.	(Std. error)
Outcome					
Number of post-claim employers	4.73	4	(3.79)	-0.14	(0.13)
Quarters of nonemployment	3.60	1	(8.24)	0.08	(0.28)
Std. deviation of post-claim earnings ($)	6,026	5,407	(4,074)	-214*	(130)
Quarters with first post-claim employer	7.79	3	(10.6)	0.40	(0.37)
Sample size				3,145	

Notes: See annotations to Table A2. Number of post-claim employers is defined as the number of unique employers we observe from the first quarter after the claim until the last quarter. Quarters of nonemployment is defined as the number of consecutive post-claim quarters without covered earnings. Std. deviation of earnings is defined as the standard deviation of earnings in the nine years following the claim.

Table A5

Effects of Exception Reporting on Selected Outcomes by Reason for Job Loss as Deviations from the Control Group

Outcome	Reason for Job Loss				
	Quit	Permanent job loss	Temporary layoff	Contract/ Seasonal layoff	Unknown
UI Receipt Outcomes					
Benefit year outcomes					
Total UI benefits paid ($)	541***	444***	307***	472***	510***
	(133)	(134)	(116)	(142)	(104)
Weeks of UI payments	4.66***	3.46***	2.14***	3.35***	3.38***
	(0.92)	(0.92)	(0.74)	(0.91)	(0.68)
Exhausted benefits (proportion)	0.142***	0.156***	0.086***	0.088**	0.134***
	(0.042)	(0.044)	(0.028)	(0.042)	(0.030)
Employment Outcomes					
Year 0 outcomes					
Employed (proportion)	-0.036	-0.058*	-0.002	-0.006	0.004
	(0.034)	(0.031)	(0.018)	(0.024)	(0.024)
Hours worked	-91.7	-94.0	58.0	-9.6	-14.7
	(70.1)	(68.0)	(46.9)	(58.6)	(48.7)
Total Earnings ($)	-597	-1,631**	-17	91	123
	(673)	(758)	(591)	(875)	(545)
Year 1 outcomes					
Employed (proportion)	-0.028	-0.059*	0.016	0.001	0.005
	(0.037)	(0.034)	(0.024)	(0.032)	(0.025)
Hours worked	-36.6	-36.3	61.3	-29.9	7.1
	(73.8)	(72.4)	(58.9)	(70.7)	(57.7)
Total Earnings ($)	-569	-655	271	635	935
	(827)	(872)	(706)	(937)	(675)
Other post-claim outcomes					
Number of post-claim employers	-0.07	0.07	-0.32	-0.19	-0.15
	(0.30)	(0.34)	(0.25)	(0.42)	(0.24)
Quarters of nonemployment	0.11	1.04	0.36	0.32	-0.69
	(0.89)	(0.78)	(0.39)	(0.61)	(0.58)
Std. deviation earnings ($)	-217	-135	-13	-644*	53
	(333)	(320)	(268)	(353)	(254)
Quarters with first post-claim employer	-0.09	-1.51*	1.33	0.58	0.99
	(0.74)	(0.87)	(0.87)	(1.00)	(0.79)
Returned to same employer (proportion)	0.021	-0.013	0.056	0.050	0.062**
	(0.033)	(0.030)	(0.036)	(0.045)	(0.032)
Returned to same industry (proportion)	0.017	-0.013	0.058*	0.081*	0.035
	(0.038)	(0.039)	(0.035)	(0.048)	(0.032)
Sample size	550	511	780	486	818

Notes: See annotations to Table A2.

Table A6

Mean and Standard Deviation (in parentheses) of Outcomes of the Control Group by Reason for Job Loss

Outcome	Reason for Job Loss				
	Quit	Permanent job loss	Temporary layoff	Contract/ Seasonal layoff	Unknown
UI Receipt Outcomes					
Benefit year outcomes					
Total UI benefits paid ($)	1,794	2,376	1,767	2,283	1,846
	(1,734)	(1,822)	(1,592)	(1,853)	(1,649)
Weeks of UI payments	13.4	16.9	12.6	15.3	13.5
	(10.8)	(10.7)	(9.7)	(10.)	(10.4)
Exhausted benefits (proportion)	0.253	0.313	0.149	0.226	0.224
	(0.435)	(0.465)	(0.357)	(0.419)	(0.417)
Employment Outcomes					
Year 0 outcomes					
Employed (proportion)	0.834	0.868	0.941	0.925	0.867
	(0.373)	(0.339)	(0.236)	(0.264)	(0.339)
Hours worked	889	923	1,211	969	1,008
	(826)	(743)	(706)	(685)	(749)
Total Earnings ($)	8,180	9,846	15,223	13,571	11,043
	(8,783)	(9,779)	(11,568)	(10,993)	(9,192)
Year 1 outcomes					
Employed (proportion)	0.796	0.845	0.882	0.874	0.834
	(0.404)	(0.362)	(0.323)	(0.332)	(0.373)
Hours worked	979	1,120	1,309	1,115	1,118
	(835)	(793)	(834)	(752)	(842)
Total Earnings ($)	10,032	11,924	16,215	15,567	12,480
	(9,757)	(10,566)	(12,654)	(11,881)	(10,329)
Other post-claim outcomes					
Number of post-claim employers	4.63	4.63	4.50	5.93	4.36
	(3.57)	(3.6)	(3.53)	(4.46)	(3.74)
Quarters of nonemployment	5.07	4.19	2.05	2.78	4.07
	(10.24)	(9.09)	(4.69)	(6.75)	(9.12)
Std. deviation earnings ($)	5,217	5,985	6,228	7,067	5,794
	(3,920)	(4,451)	(3,861)	(4,278)	(3,849)
Quarters with first post-claim employer	5.84	7.06	9.30	7.77	8.24
	(9.09)	(10.24)	(11.62)	(10.16)	(10.89)
Returned to same employer (proportion)	0.136	0.125	0.51	0.347	0.369
	(0.343)	(0.331)	(0.501)	(0.477)	(0.483)
Returned to same industry (proportion)	0.26	0.242	0.606	0.456	0.487
	(0.44)	(0.429)	(0.489)	(0.499)	(0.5)
Sample size	550	511	780	486	818

Notes: Universe consists of the control group during fall 1986, winter 1987, and spring 1987.

Table A7

Effects of Exception Reporting as Deviations from the Control Group on Long-Term Employment, Hours, and Earnings for Claimants with a Higher than Average Predicted Likelihood of Benefit Exhaustion

Outcome	Proportion employed				Hours worked				Total Earnings ($)			
	Comparison group		Exception Reporting		Comparison group		Exception Reporting		Comparison group		Exception Reporting	
	Mean (Std.dev.)		Coeff.	(Std. error)	Mean (Std.dev.)		Coeff.	(Std. error)	Mean (Std.dev.)		Coeff.	(Std. error)
Year relative to claim												
Year 0	0.828	(0.378)	-0.038	(0.023)	784.9	(754)	-25.9	(43.1)	8723	(8684)	-344	(509)
Year 1	0.788	(0.409)	-0.008	(0.024)	958	(830)	12.7	(49.0)	10636	(10077)	369	(619)
Year 2	0.758	(0.429)	-0.019	(0.025)	945.2	(837)	16.2	(49.6)	11432	(11487)	-355	(671)
Year 3	0.722	(0.449)	-0.046*	(0.027)	914.1	(876)	46.8	(52.0)	11110	(12146)	-23	(704)
Year 4	0.68	(0.467)	-0.030	(0.028)	855.6	(875)	49.5	(52.2)	10524	(11958)	133	(709)
Year 5	0.642	(0.48)	-0.028	(0.028)	825.5	(894)	36.1	(52.7)	10058	(11560)	531	(702)
Year 6	0.599	(0.491)	-0.004	(0.029)	763.5	(898)	51	(52.6)	8903	(11460)	1,032	(688)
Year 7	0.577	(0.495)	-0.004	(0.029)	759.7	(904)	32.8	(53.1)	8706	(11176)	857	(682)
Year 8	0.537	(0.499)	0.013	(0.029)	701.5	(878)	38	(52.2)	8421	(11168)	929	(694)
Year 9	0.537	(0.499)	0.006	(0.029)	766.2	(929)	-13.2	(54.4)	8853	(11680)	820	(736)
Sample size			1,166				1,166				1,166	

Notes: See annotations to Table A2.

The control group consists of claimants in the control and new work search groups with a higher than average probability of exhausting benefits. The average probability of exhausting benefits equals 0.264.

Table A8

Effecst of Exception Reporting as Deviations from the Control Group on Long-Term Employment, Hours, and Earnings for Claimants with a Lower than Average Predicted Likelihood of Benefit Exhaustion

Outcome	Proportion employed				Hours worked				Total Earnings ($)			
	Comparison group		Exception Reporting		Comparison group		Exception Reporting		Comparison group		Exception Reporting	
	Mean (Std.dev.)		Coeff.	(Std. error)	Mean (Std.dev.)		Coeff.	(Std. error)	Mean (Std.dev.)		Coeff.	(Std. error)
Year relative to claim												
Year 0	0.921	(0.27)	-0.006	(0.012)	1142	(718)	-34.8	(32.6)	13371	(10902)	-636	(478)
Year 1	0.879	(0.326)	-0.013	(0.015)	1239	(802)	-17.5	(36.8)	14800	(11695)	-149	(530)
Year 2	0.851	(0.356)	-0.017	(0.016)	1249	(851)	-43.9	(38.3)	15681	(12430)	-692	(553)
Year 3	0.814	(0.389)	-0.005	(0.018)	1204	(868)	-5.7	(38.9)	15275	(12319)	-422	(556)
Year 4	0.787	(0.41)	-0.013	(0.019)	1149	(879)	-29.2	(39.0)	15173	(13230)	-712	(578)
Year 5	0.776	(0.417)	-0.02	(0.019)	1101	(877)	8	(39.8)	14639	(13217)	-225	(583)
Year 6	0.734	(0.442)	0.001	(0.020)	1059	(916)	-8	(40.9)	14055	(13428)	-463	(584)
Year 7	0.718	(0.45)	-0.01	(0.020)	1073	(919)	-47.9	(41.1)	13964	(13474)	-477	(600)
Year 8	0.703	(0.457)	-0.027	(0.021)	1018	(917)	-34.8	(41.5)	13695	(13754)	-426	(621)
Year 9	0.682	(0.466)	-0.018	(0.021)	1010	(940)	-9.7	(42.1)	13944	(14572)	-532	(642)
Sample size			1,979				1,979				1,979	

Notes: Same as above.

The control group consists of claimants in the control and new work search groups with a lower than average probability of exhausting benefits. The average probability of exhausting benefits equals 0.264.

Table A9
Effects of Exception Reporting as Deviations from the Control Group on Long-Term Employment Outcomes by Varying Likelihood of Exhausting Benefits

Sample	Probability of exhausting benefits higher than average[a]				Probability of exhausting benefits lower than average[b]			
	Control group		Exception Reporting		Control group		Exception Reporting	
	Mean	(Std. dev.)	Coeff.	(Std. error)	Mean	(Std. dev.)	Coeff.	(Std. error)
Outcome								
Number of post-claim employers	4.81	(4.03)	-0.44*	(0.23)	4.69	(3.66)	-0.1	(0.17)
Quarters of nonemployment	5.20	(10.45)	0.01	(0.59)	2.70	(6.52)	0.24	(0.30)
Std. deviation of post-claim earnings ($)	5,676	(4,317)	-254	(243)	6,221	(3,920)	-188	(175)
Quarters with first post-claim employer	6.32	(9.3)	0.25	(0.55)	8.62	(11.19)	0.43	(0.51)
Returned to same employer (proportion)	0.204	(0.404)	0.012	(0.024)	0.378	(0.485)	0.043*	(0.022)
Returned to same industry (proportion)	0.298	(0.458)	0.02	(0.027)	0.501	(0.5)	0.028	(0.022)
Sample size			1,166				1,979	

Notes: See annotations to Table A2.

a. The comparison group consist of claimants in the control group with a higher than average probability of exhausting benefits. The average probability of exhausting benefits equals 0.264.

b.The comparison group consist of claimants in the control group with a lower than average probability of exhausting benefits. The average probability of exhausting benefits equals 0.264.

Table A10

Estimated probability of exhausting benefits (probit coefficients). Dependent variable: exhausted benefits

Covariates	Probability of exhausting benefits
Mean of dependent variable	0.264
Male	0.087
	(0.064)
Race/Etnicity	
Black non-Hispanic	0.134*
	(0.070)
Hispanic/Am.Indian/Asian/other	0.137*
	(0.076)
Age	
25-34	0.184***
	(0.061)
35-44	0.356***
	(0.069)
45-54	0.545***
	(0.083)
≥ 54	0.620***
	(0.101)
Schooling	
high school	-0.101
	(0.062)
some college	-0.156**
	(0.071)
college graduate	-0.380***
	(0.098)
Pre-claim earnings ($)	
Earnings 1 year before claim	0.000
	(0.000)
Earnings 2 years before claim	0.000
	(0.000)
Earnings 3 years before claim	-0.000*
	(0.000)
Pre-claim hours	
Hours 1 year before claim	-0.000**
	(0.000)
Hours 2 years before claim	-0.000
	(0.000)
Hours 3 years before claim	0.000
	(0.000)
Occupation dummies?	Yes

Covariates	Probability of exhausting benefits
Industry dummies?	Yes
Reason for job loss	
permanent layoff	0.189***
	(0.064)
temporary layoff with recall date	-0.238***
	(0.068)
contract/seasonal	0.030
	(0.072)
Employer-attached/placed by union	-0.348***
	(0.056)
Prior UI claim	
duration ≤ 15 weeks	-0.418***
	(0.086)
duration > 15 weeks	0.155**
	(0.075)
UI benefits/claim type	
weekly amount ($)	0.008***
	(0.002)
maximum amount ($)	-0.000*
	(0.000)
potential duration	-0.011
	(0.014)
replacement rate (percent)	0.002
	(0.002)
combined wage claim	-0.098
	(0.097)
ex-service member claim	-0.053
	(0.180)
federal employee claim	0.353**
	(0.164)
Veteran	-0.093*
	(0.053)
Reservation wage (hourly)	
≤ $5.00	0.030
	(0.076)
$5.01–$7.00	0.180**
	(0.078)
$7.01–$10.00	0.114
	(0.078)
$10.01–$20.00	0.040
	(0.091)
> $20.00	0.184**
	(0.088)
Marital status/gender	
married male	-0.234***
	(0.067)

Covariates	Probability of exhausting benefits
married female	0.177**
	(0.085)
Household status	
1 dependent	-0.033
	(0.064)
2 or more dependents	0.002
	(0.063)
homeowner	-0.059
	(0.061)
unemployment rate in the area	-0.143**
	(0.062)
Quarter of claim	
Summer 1986	0.333***
	(0.076)
Fall 1986	0.173**
	(0.087)
Winter 1987	0.192
	(0.152)
Spring 1987	0.061
	(0.092)
Constant	-0.079
	(0.599)
Sample size	4,811

Notes: Standard errors are in parentheses (*** p<0.01, ** p<0.05, * p<0.1).

Data Appendix

This appendix describes the original data from the Washington Alternative Work Search (WAWS) experiment (Johnson and Klepinger 1991, 1994) and how we constructed the long-term panel used to examine the long-term effects of the WAWS treatments on labor market outcomes (sections 1 and 2). We also provide a discussion of the extent and severity of attrition from the long-term sample (section 3).

1. Description of the Experimental Data

The data we use to replicate and extend the short-term findings of the Washington experiment were provided to the W.E. Upjohn Institute by the Washington State Employment Security Department (ESD) in June 1989. According to ESD, they are the same data provided to Terry Johnson and Daniel Klepinger, who performed the evaluation at Battelle Memorial Institute for the U.S. Department of Labor (Johnson and Klepinger 1991, 1994); however, they do not include responses to the survey referred to by Johnson and Klepinger (1991). We refer to these as "the experimental data."

The experimental data combine data from three sources:

- UI claims records — the records maintained by the ESD to track each worker's benefit eligibility and the timing and amount of benefits paid in each week of the benefit year
- Administrative wage records — the quarterly records provided by each covered employer in the state to the ESD to determine the employer's UI payroll tax liability and to track the wages and hours of each employee (which in turn are used to determine UI eligibility and the weekly benefit amount)

- Employment Service records — the records maintained by the state Employment Service on each job seeker, which include a range of personal characteristics of claimants that are inessential to running the UI system but useful in referring and placing workers in jobs

For example, data on UI benefits, including weeks of benefits paid and whether a worker exhausted benefits, are from UI claims records; quarterly earnings, hours, and data on industry of employment and whether a worker returned to the pre-UI employer are from administrative wage records; and individual characteristics (including occupation) and whether a worker was on standby are from the Employment Service records.

Details of the assignment protocol

The experimental data do not include an explicit treatment indicator, so we created one using the last digit of each claimant's pseudo-Social Security number and the date of his or her enrollment in the experiment. (The last number of the pseudo-SSN was not altered from the last number of the true SSN.) Initially, the Tacoma UI office assigned claimants with SSN ending in 0, 1, or 2 to the Exception Reporting group; 3, 4, or 5 to the control group; 6 or 7 to the New Work Search (NWS) policy group; and 8 or 9 to the Job Search Assistance (JSA) group (Johnson and Klepinger refer to this as the Intensive Services group). However, starting with the week of May 10, 1987, ESD stopped enrolling claimants in the Exception Reporting (ER) treatment because it was clear that claimants assigned to this group were claiming more benefits and experiencing longer spells of insured unemployment than the control group. Thereafter, the Tacoma office assigned claimants with SSN ending in 0, 1, or 2 to JSA. Accordingly, we assigned claimants with SSN = 0, 1, or 2 and benefit year starting in week 27 of 1986 through week 18 of 1987 (inclusive) to the ER group, and claimants with SSN = 0, 1, or 2 and benefit

year starting in week 19 of 1987 through week 35 of 1987 (inclusive) to the JSA treatment group (as well as all claimants with SSN = 8 or 9).

Because the assignment procedure changed in May 1987, the number of claimants in each of the treatment groups does not reflect the 30-30-20-20 proportions that would be suggested by the original assignment protocol. Rather, 30 percent of all assigned claimants are controls, 23 percent (rather than 30 percent) are in the ER treatment, 21 percent are in the New Work Search Policy treatment, and 27 percent (rather than 20 percent) are in the JSA treatment.[1]

Replicating Johnson and Klepinger's sample

Claimants who failed the separation requirement for UI need to be dropped from the sample, but the experimental data do not include a binary indicator of whether the claimant satisfied that requirement. The data do include two "separation issue" variables, one for the total number of separation issues raised during the benefit year, the other a set of weekly variables indicating whether a separation issue was raised in that week. The two are inconsistent, and our solution was to drop all workers with at least one separation issue during the benefit year and zero benefits paid, on the assumption that they were ineligible due to a separation issue.

For our project studying the long-term effects of assignment to intensive services (JSA), we also dropped four claimants who were assigned to the control group but who received intensive services. Presumably, these claimants happened to be at the Tacoma Employment Service Center when an intensive services workshop was conducted and were either directed to attend (mistakenly) or attended voluntarily. This "cross-over" of four claimants from the control group to the JSA treatment group represents an assignment error rate of about 0.15 percent (2,597 controls received at least one week of benefits and did not attend the intensive services

1. These proportions are with respect to the full sample of 9,607, which is the number of benefit years in the experimental data before dropping any claimants who never received a benefit check.

workshop). We do not drop these claimants from the study of the effects of ER on long-term outcomes.

The sample we use to study the effects of ER on long-term outcomes is smaller than the sample used by Johnson and Klepinger (1991, 1994). This is for two reasons. First, note that the ER treatment consisted of four experimental cohorts, with the first cohort assigned in week 27 of 1986:III and the last cohort assigned in week 18 of 1987:II. In order to make sure that we observe the ER, control, and new work search policy group over the same time period, we only keep claimants who claimed between week 27 of 1986 (inclusive) and week 18 of 1987 (inclusive). Second, because the follow-up administrative wage records available to us begin in 1987:I, for the first experimental cohort we do not have follow-up records for the first post-claim quarter (denoted Q(t+1)), 1986:IV; see the table below.

Structure of the long-term panel

Experimental cohort	Claim Quarter, Q(t)	Q(t+1)	Q(t+2)	Q(t+3)		Q(t+42)
1	1986:Q3	N/A	1987:Q1	1987:Q2	…	1997:Q1
2	1986:Q4	1987:Q1	1987:Q2	1987:Q3	…	1997:Q2
3	1987:Q1	1987:Q2	1987:Q3	1987:Q4	…	1997:Q3
4	1987:Q2	1987:Q3	1987:Q4	1988:Q1	…	1997:Q4

Because we are missing administrative records for the first post-claim quarter for experimental cohort 1 (see the table), we *exclude* all the claimants from this cohort. Hence, our final analysis sample consists of claimants assigned to ER, control, and NWS group during who claimed between 1986:IV (specifically, claimants who claimed beginning with week 40 of 1986) and 1987:II (specifically, claimants who claimed in week 18 of 1987).

Because the administrative wage records all end in 1997:IV and the last quarter the ER treatment was conducted in 1987:II, we have at least 42 quarters of post-claim quarter observations; see the table above. In order to have nine complete years of follow-up administrative wage records for all claimants in the analysis sample, we decided to study long-term outcomes for 40 quarters after the claim quarter.

Although the sample we use is not identical to that used by Johnson and Klepinger (1991, 1994), both our short-term treatment effects and theirs are essentially similar, as can be seen by comparing Tables 2 and 3 in our ER paper with Tables 1, 2, and 3 in Johnson and Klepinger (1994).

Claimants with two benefit years

Each observation in the experimental data represents a UI claimant's "benefit year"—that is, the year following the initial UI claim, during which the claimant could receive UI benefits. For 9,207 initial UI claimants, one observation (that is, one benefit year) appears in the experimental data; however, for 200 claimants, two observations appear. These latter claimants each started a benefit year in the early months of the WAWS experiment (in July or August 1986). This benefit year lapsed a year later, and these 200 claimants established a new benefit year in July or August 1997, while the Washington experiment was still in progress. As a result, they were again assigned to a treatment or to the control group and are included in our sample.

Benefit duration

Another key variable we constructed is the *duration of the first UI claim spell*. The experimental data include a variable for the amount of UI benefit paid in each of the 52 weeks of the benefit year, so we followed each claimant's benefit payments from the time of the initial claim until the series lapsed for at least four weeks. This follows the procedure used by

Spiegelman, O'Leary and Kline (1992, 1995) and Corson, Decker, Dunstan, and Kerachsky (1992) in a similar setting, although appears to differ from Johnson and Klepinger (1991, 1994), who ended the first spell with a lapse of one week. Inspection of the data shows that many claimants with a lapse of three or fewer weeks had a subsequent claim series of at least another four weeks, which suggested a single spell of insured unemployment briefly interrupted by time out of the labor force, rather than reemployment. We have checked the sensitivity of the findings and hazards we report to different definitions of a first spell (a one-week lapse, two-weeks, and so on) and obtain similar findings in each case.

We counted claimants who were eligible for benefits, but who never received a payment, as having a one-week UI claim spell (corresponding to the waiting week). There are 726 such claimants. Another 1,830 claimants did not receive benefits within two weeks of their initial claim, but did receive benefits later in the benefit year. For these, we started the first UI spell in the week before the first payment (to account for the waiting week) and again followed the benefit payments until the series lapsed for at least four weeks.

For 7,051 claimants (73.4 percent of those in the experiment), the first spell of UI accounts for all benefits paid in the benefit year. The rest received additional benefits later in the benefit year. We refer to the total number of weeks in which a claimant received benefits as the "compressed UI spell." Although useful as a measure of the total effect of a treatment, a compressed spell does not correspond to a true duration because it combines spells of benefit receipt separated by at least four weeks. Accordingly, the hazard functions we display (and our interpretation of the treatment effects on search behavior) focus on the duration of the first claim spell.

Reason for job loss

Reason for job loss is another key claim characteristic. To construct this variable we create five mutually exclusive categories by using "unemployed due to lack of work" and "reason for lack of work" indicators from the Employment Service records. We define "unemployed due to permanent job loss" if a claimant was either laid off permanently because of a plant or company closure; "unemployed due to temporary layoff" if a claimant was laid off temporarily either with a known recall date or without a recall date; "unemployed due to contract completion or seasonal layoff" if a claimant was laid off because of job or contract completion or because of a seasonal layoff; "unemployment not due to job loss," which we call "quit" for short, if the reason for lack of work was missing and the claimant was unemployed not due to lack of work. Note that, typically, if a worker voluntarily quit his or her job, he or she is not eligible for UI benefits. Such a worker might still receive benefits, if the reason for the quit meets the "standard for good cause," such as showing that the claimants left due to a hostile work environment (e.g., because of discrimination or sexual harassment). Finally, we define "reason for unemployment unknown" if there is no information regarding if the claimant was unemployed due to lack of work or if the reason for lack of work is missing. This last group is the largest of the groups.

2. Constructing a Long-Term Panel

To construct a long-term panel, we appended additional administrative wage records to the experimental data described above. In Washington, wage records include the following for each worker in each quarter:

- a worker identifier (pseudo-Social Security number)
- the year and quarter

- the pseudo-employer account number and earnings received from that employer in that quarter (for each employer from whom the worker received earnings), and
- hours worked in the quarter (again, for each employer)

Coverage of the UI system is nearly universal (self-employed workers are the only significant group of "above-ground" workers who are not covered), and any UI-covered worker who has earnings in a given quarter from an employer in the state appears in the wage records. As a result, wage records can be used to construct an earnings history of most workers who were in the WAWS experiment. For this study, we had available the population of Washington administrative wage records for quarters 1987:I through 1997:IV inclusive, so we can observe up to nine years of earnings following the Washington experiment by matching workers in the experiment with their wage records.

How reliable are earnings histories constructed from wage records likely to be? Because wage records are central to financing and administering UI, most states randomly audit employer wage reports. Analyses of these audits by Blakemore et al. (1996) and Burgess, Blakemore, and Low (1998) suggest that small employers and employers with high turnover tend to underreport their workers' earnings, raising questions about the value of wage records for research. However, validation studies comparing wage records (whose source is the employer) with survey data (whose source is the worker) suggest that the reliability of wage records is similar to that of surveys. Kornfeld and Bloom (1999) performed a landmark study comparing UI wage records with survey data in a 12-state sample of over 12,000 low-wage workers who participated in the National JTPA Study. They concluded that, except for young males with past arrests, "UI wage records provide a valid alternative to surveys" for the purpose of evaluating employment and earnings outcomes of training programs (Kornfeld and Bloom 1999, p. 171). Wallace and

Haveman's (2007) validation study focused on welfare recipients in Wisconsin and found that, despite discrepancies, wage records and survey data gave similar results on employment and earnings outcomes. Wallace and Haveman conclude that, given their availability, low cost, and similarity across states, UI wage records are preferable to surveys for monitoring labor market outcomes of low-wage workers.

Table 1 shows the results of matching workers enrolled in the Washington experiment with the Washington wage records. The table shows the number of matches for the all claimants in the experiment (the "Total" column) and each treatment group (including controls) by year.[2] A "match" occurs when a claimant is observed with earnings in the Washington administrative wage records in a given year. The table also shows "match rates" (in parentheses) defined as the proportion of claimants initially enrolled in the experiment (or each treatment) observed with positive earnings (or "matched") in a given year. In the table, "year 0" refers to the claim year, defined as the quarter in which the initial UI claim was filed and the three following quarters.[3]

The Washington experiment enrolled new claimants between July 1986 and August 1987, so it should be possible to match most of the enrolled claimants with their 1987 wage records. (Claimants would not have a 1987 match if they claimed benefits in 1986 or early 1987 and never found reemployment, withdrew from the labor force, or for a few other reasons — see the following section.) As Table 1 shows, 86.5 percent of claimants enrolled in the experiment could be matched with wage records at some time during the claim year. This match rate falls unevenly to 62.5 percent in year 9, or at an average of just under 3 percentage points per year. Specifically,

2. The "Total" column gives the sum of the full control, Exception Reporting, New Work Search, and JSA groups. The full control group is used with the Exception Reporting and New Work Search treatments. The restricted control group, which drops claimants who never received a benefit, is used with the JSA treatment because only JSA assignees who received a first benefit payment received a JSA call-in notice.
3. This is the definition we use in all long-term analyses. It differs from the definition we use in the short-term analyses, where "claim year" refers to the quarter in which the initial UI claim was filed and the four following quarters.

the match rate falls by about 4 percentage points between years 0 and 1, then by 3 percentage points per year until year 5, after which it falls somewhat more slowly.

3. Attrition from the Long-term Sample

The match rates shown in Table 1 decline over time for two reasons. First, workers may remain unemployed or drop out of the labor force after claiming UI, so they will have no earnings and none will appear in the wage records. Second, workers could become self-employed, leave the formal labor force for the underground economy, or leave Washington State and find employment elsewhere.[4] In these latter cases, a worker will have earnings, but those earnings will not be recorded in the UI wage records of Washington State. (Self-employed workers are not covered by UI, earnings in the underground economy are not reported, and out-of-state earnings will be picked up in the wage records of another state.)

In the first case (continued unemployment or departure from the labor force) wage records give a correct picture of the individual's labor market status. In the second (movement to self-employment, the underground economy, or out of Washington), we have a form of sample attrition. There is no way of distinguishing between the two cases — if an individual has covered earnings in Washington, they appear (or should) in administrative wage records; otherwise, we observe a missing value for the individual in a given quarter. (In wage records, there is no difference between zero earnings and missing earnings.)

Attrition of participants from a long-term panel poses a threat to the validity of an experimental study if the subjects who leave the sample differ systematically and in unobserved ways from those who remain (see for example, the discussion and references in Murnane and

4. The WAWS experimental design attempted to lessen the problem of losing workers who move to another state by excluding interstate claims; however, this by no means eliminates the possibility.

Willett 2011). Sample attrition is usually considered as a problem of nonresponse: experiments conducted with household surveys depend on both a survey center's ability to find participants and on participants' cooperation over a long period of time (Hausman and Wise 1979, McFadden 1985). Administrative data have an advantage over survey data because they are potentially less vulnerable to attrition: anyone who receives covered earnings in given state in a given quarter should be observed in UI administrative wage records, regardless of whether that individual can be found or is willing to cooperate.[5]

Nevertheless, earnings histories constructed from UI administrative data could be subject to attrition that would bias experimental estimators. In the case of the WAWS experiment, if a treatment increased the long-term probability of becoming self-employed, moving to the underground economy, or taking a job in another state, then sample attrition from the treatment group would be greater than from the control group, the earnings of those assigned to the treatment would be understated in Washington State administrative wage records, and the estimated treatment effects estimator would be downward-biased.

To investigate the extent to which sample attrition could be a threat to the validity of the estimators, we perform a difference-in-differences analysis of the characteristics of successive groups of claimants who are observed with earnings. We first calculate the claim year (year 0) differences in average characteristics between a given treatment group (with earnings) and the control group (also with earnings), then do the same for each subsequent year. We then calculate the differences in these differences between the claim year and each subsequent year (along with the standard error of each difference in differences).

5. Also, with survey data, attrition is typically an absorbing state — once a subject leaves the sample, he or she does not return. This is not the case with administrative data, where a subject may have no earnings for one or several quarters but then return to work and appear again in the administrative records.

Specifically, for a given characteristic *x*, the difference in differences (or *change* in the differences) between the treatment group (*T*) and the control group (*C*), between year *t* (a subsequent year) and year 0 (the claim year), can be written:

$$(\overline{x}_{t,T} - \overline{x}_{t,C}) - (\overline{x}_{0,T} - \overline{x}_{0,C}) \quad (1)$$

If the differences in differences are insubstantial for most characteristics, then we would infer that the observable characteristics of claimants who are leaving the treatment and control samples over time (that is, no longer observed with earnings) are similar. It does not necessarily follow that the *unobservable* characteristics of claimants leaving the two groups over time are also similar; however, given that we find the selection on observables assumption to be reasonable based on pre-treatment outcome tests, it seems plausible that unobservables and observables are correlated in these samples. If so, then finding that the observable characteristics of claimants who leave the treatment and controls groups over time are similar would suggest that selective attrition is not a threat to the validity of the estimators we use.

Tables 2, 3, and 4 display the findings of such a difference-in-differences analysis. Consider first Table 2, which pertains to attrition from the JSA treatment. Columns 1 and 2 show sample means for the control and JSA groups in the claim year (year 0), and column 3 shows the difference for each characteristic in the claim year. Columns 4, 5, and 6 show the difference (or change) between this claim-year difference and the difference in years 3, 6, and 9. (We have also computed differences in differences for years 1, 2, 4, 5, 7, and 8, but for the sake of economy we do not report them. As discussed below, and their implications are the same as those for the years shown.)

For example, the claim-year difference in the proportion of the control and JSA groups who were male was about 2 percentage points (0.0192, *p*-value = 0.17) (see column 1, Table 2).

By year 3, this difference had increased by 0.0001 (that is, to 0.0193, *p*-value = 1.00), by year 6 it had increased by 0.0023 (*p*-value = 0.91), and by year 9 it had fallen by 0.0037 (*p*-value = 0.86). None of these differences in differences is significant, statistically or in practical terms (the absolute values of the standard errors of these differences in differences are at least 5 times the point estimates).

For all but five variables shown in Table 2, the claim-year difference between the control and JSA groups had a *p*-value greater than 0.10; that is, for all but five characteristics, there was no initial difference between the control and JSA groups. Further, in all these cases, there was no subsequent change over time in the difference between the control and JSA groups — that is, the differences in differences over the following 9 years all have *p*-values greater than 0.10.

For five variables, the claim-year difference between the control and JSA groups has a *p*-value of 0.10 or less: claimants from households with 1 dependent, who formerly worked in wholesale trade, who formerly worked in government, who were ex-service members, and whose reservation wage was between $10 and $20. For each of these characteristics, the initial difference between the control and JSA groups persists throughout the 9-year follow-up period — the estimated differences in differences all have *p*-values greater than 0.10.

Overall, the inference is that, if there was no initial difference between the control and JSA groups in a given characteristic, none appeared over time. And if there was initially a difference in a characteristic between the control and JSA groups, that difference did not change over time. Assignment to treatment appears to have had no impact on the characteristics of the JSA claimants compared with those of the controls.

Consider now Table 3 (comparing controls with Exception Reporting claimants) and Table 4 (comparing controls with New Work Search claimants). Columns 3, 4, and 5 of those

tables show only two differences in differences with a *p*-value of 0.10 or less: for both the Exception Reporting and New Work Search groups, the year 9 differences in differences for the proportion of females aged 45–54 have *p*-values between 0.08 and 0.09. That is, by year 9, the difference between the control and Exception Reporting groups in the proportion of females aged 45–54 had increased by 1.4 percentage point (*p*-value = 0.09); a similar finding holds for women aged 45–54 in the control and New Work Search groups (an increase of 1.6 percentage points by year 9, *p*-value = 0.08).

As mentioned above, in addition to the differences in differences for years 3, 6, and 9 shown in Tables 2, 3, and 4, we have computed corresponding differences in differences for years 1, 2, 4, 5, 7, and 8. None of these estimates has a *p*-value of 0.10 or less.

We conclude that the observable characteristics of claimants who left the control group over time and those who left each of the treatment groups over time are essentially the same. Again, although this does not dispose of the possibility that those who left the treatment and control groups differed in unobservable ways, our finding that selection on observables is a reasonable assumption suggests that sample attrition probably does not threaten the validity of the estimators we use. Given that we are unable to detect any changes over time in the observable characteristics of controls compared with each of the treatment groups, it seems unlikely that sample attrition of any consequence is taking place.

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Table 1

Matches[1] and match rates[2] (in parentheses) between population wage records and claimants enrolled in the Washington Alternative Work Search experiment, by year and treatment group

	Total[3]	Full control group[4]	Exception Reporting	New Work Search	Restricted control group[4]	JSA
Year 0	8,092	2,447	1,906	1,728	2,227	2,011
	(0.865)	(0.859)	(0.848)	(0.880)	(0.858)	(0.878)
Year 1	7,720	2,350	1,849	1,621	2,145	1,900
	(0.826)	(0.825)	(0.823)	(0.826)	(0.826)	(0.829)
Year 2	7,438	2,266	1,769	1,552	2,062	1,851
	(0.796)	(0.796)	(0.787)	(0.791)	(0.794)	(0.808)
Year 3	7,183	2,195	1,692	1,498	1,999	1,798
	(0.768)	(0.771)	(0.753)	(0.763)	(0.770)	(0.785)
Year 4	6,888	2,123	1,625	1,417	1,927	1,723
	(0.737)	(0.745)	(0.723)	(0.722)	(0.742)	(0.752)
Year 5	6,590	2,028	1,570	1,353	1,835	1,639
	(0.705)	(0.712)	(0.698)	(0.689)	(0.707)	(0.715)
Year 6	6,357	1,930	1,522	1,320	1,749	1,585
	(0.680)	(0.678)	(0.677)	(0.672)	(0.673)	(0.692)
Year 7	6,146	1,873	1,458	1,270	1,701	1,545
	(0.657)	(0.658)	(0.649)	(0.647)	(0.655)	(0.674)
Year 8	5,942	1,825	1,413	1,219	1,658	1,485
	(0.636)	(0.641)	(0.629)	(0.621)	(0.638)	(0.648)
Year 9	5,848	1,785	1,388	1,201	1,629	1,474
	(0.625)	(0.627)	(0.617)	(0.612)	(0.627)	(0.643)
Number enrolled	9,350	2,848	2,248	1,963	2,597	2,291

Source: Tabulated from the Washington Alternative Work Search experimental data base and population wage records from the Washington State Department of Employment Security.

1. A "match" occurs for a claimant in a given year if positive earnings were found in at least one quarter of the year for the claimant.

2. The match rate is the proportion of claimants initially enrolled in the experiment (or in a treatment) who were observed with earnings in a given year.

3. Sum of the full control, Exception Reporting, New Work Search, and JSA groups.

4. The full control group is used with the Exception Reporting and New Work Search treatments. The restricted control group, which drops claimants who never received a benefit, is used with the JSA treatment because only JSA assignees who received a first benefit payment received a JSA call-in notice.

Table 2

Characteristics of controls and JSA claimants with earnings in claim year, and differences in differences between controls and JSA claimants (subsequent years against claim year)
(standard errors in parentheses)

	Claim year			Difference in differences between control and JSA groups (year t – claim year)		
Covariate	Control mean (1)	JSA mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
Male	0.6974	0.7166	0.0192	0.0001	0.0023	-0.0037
	(0.0097)	(0.0101)	(0.0140)	(0.0203)	(0.0211)	(0.0216)
Age (years)						
≤ 24	0.2021	0.1949	-0.0071	0.0015	-0.0011	0.0071
	(0.0085)	(0.0088)	(0.0123)	(0.0179)	(0.0187)	(0.0192)
25-34	0.3767	0.3894	0.0126	-0.0057	-0.0121	-0.0263
	(0.0103)	(0.0109)	(0.0150)	(0.0218)	(0.0226)	(0.0231)
35-44	0.2506	0.2496	-0.0009	0.0100	0.0150	0.0207
	(0.0092)	(0.0097)	(0.0133)	(0.0195)	(0.0202)	(0.0208)
45-54	0.1159	0.1094	-0.0065	-0.0040	0.0050	0.0062
	(0.0068)	(0.0070)	(0.0097)	(0.0142)	(0.0145)	(0.0143)
≥ 54	0.0548	0.0567	0.0019	-0.0019	-0.0068	-0.0078
	(0.0048)	(0.0052)	(0.0071)	(0.0098)	(0.0088)	(0.0086)
Gender-age interactions						
male ≤ 24	0.1352	0.1348	-0.0004	0.0044	0.0008	0.0042
	(0.0072)	(0.0076)	(0.0105)	(0.0154)	(0.0161)	(0.0165)
male 25-34	0.2762	0.2904	0.0142	-0.0064	-0.0095	-0.0216
	(0.0095)	(0.0101)	(0.0139)	(0.0202)	(0.0211)	(0.0215)
male 35-44	0.1652	0.1735	0.0083	0.0039	0.0112	0.0146
	(0.0079)	(0.0084)	(0.0115)	(0.0168)	(0.0175)	(0.0180)
male 45-54	0.0790	0.0771	-0.0020	-0.0053	0.0002	0.0008
	(0.0057)	(0.0059)	(0.0083)	(0.0121)	(0.0122)	(0.0121)
male ≥ 54	0.0418	0.0408	-0.0010	0.0034	-0.0004	-0.0017
	(0.0042)	(0.0044)	(0.0061)	(0.0084)	(0.0075)	(0.0073)
female ≤ 24	0.0669	0.0602	-0.0067	-0.0029	-0.0019	0.0030
	(0.0053)	(0.0053)	(0.0075)	(0.0109)	(0.0113)	(0.0116)
female 25-34	0.1006	0.0990	-0.0016	0.0007	-0.0026	-0.0046
	(0.0064)	(0.0067)	(0.0092)	(0.0133)	(0.0140)	(0.0144)
female 35-44	0.0853	0.0761	-0.0092	0.0061	0.0038	0.0061
	(0.0059)	(0.0059)	(0.0084)	(0.0123)	(0.0128)	(0.0133)
female 45-54	0.0368	0.0323	-0.0045	0.0013	0.0048	0.0054
	(0.0040)	(0.0039)	(0.0056)	(0.0082)	(0.0084)	(0.0082)
female ≥ 54	0.0130	0.0159	0.0029	-0.0053	-0.0064	-0.0061
	(0.0024)	(0.0028)	(0.0037)	(0.0051)	(0.0047)	(0.0047)
Ethnicity						
white	0.8244	0.8424	0.0179	-0.0105	-0.0036	0.0026
	(0.0081)	(0.0081)	(0.0114)	(0.0165)	(0.0171)	(0.0173)
black	0.0997	0.0880	-0.0117	0.0065	0.0020	0.0065
	(0.0063)	(0.0063)	(0.0090)	(0.0128)	(0.0132)	(0.0134)
other	0.0759	0.0696	-0.0063	0.0040	0.0015	-0.0091

	Claim year			Difference in differences between control and JSA groups (year t – claim year)		
Covariate	Control mean (1)	JSA mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
	(0.0056)	(0.0057)	(0.0080)	(0.0116)	(0.0121)	(0.0122)
Schooling (years)						
less than high school	0.1383	0.1273	-0.0110	0.0004	-0.0016	-0.0043
	(0.0073)	(0.0074)	(0.0104)	(0.0151)	(0.0154)	(0.0156)
high school	0.5384	0.5525	0.0141	-0.0004	0.0039	-0.0080
	(0.0106)	(0.0111)	(0.0153)	(0.0223)	(0.0231)	(0.0235)
some college	0.2434	0.2387	-0.0047	0.0018	-0.0011	0.0117
	(0.0091)	(0.0095)	(0.0132)	(0.0191)	(0.0199)	(0.0203)
college graduate	0.0799	0.0816	0.0016	-0.0017	-0.0012	0.0006
	(0.0057)	(0.0061)	(0.0084)	(0.0123)	(0.0128)	(0.0131)
Marital status/gender						
married male	0.2690	0.2636	-0.0054	-0.0084	-0.0138	-0.0121
	(0.0094)	(0.0098)	(0.0136)	(0.0198)	(0.0203)	(0.0208)
married female	0.0961	0.0865	-0.0096	0.0072	0.0107	0.0113
	(0.0062)	(0.0063)	(0.0089)	(0.0129)	(0.0133)	(0.0135)
Household status						
no dependents	0.3044	0.3068	0.0024	0.0045	0.0078	0.0170
	(0.0098)	(0.0103)	(0.0142)	(0.0206)	(0.0212)	(0.0216)
1 dependent	0.1504	0.1318	*-0.0187*	-0.0005	-0.0004	0.0055
	(0.0076)	(0.0075)	(0.0107)	(0.0156)	(0.0161)	(0.0166)
2 or more dependents	0.2393	0.2392	-0.0002	-0.0028	0.0025	-0.0098
	(0.0090)	(0.0095)	(0.0131)	(0.0191)	(0.0199)	(0.0203)
homeowner	0.2928	0.2740	-0.0188	0.0010	0.0019	0.0060
	(0.0096)	(0.0099)	(0.0139)	(0.0201)	(0.0208)	(0.0212)
Veteran	0.2411	0.2307	-0.0104	-0.0075	-0.0019	-0.0131
	(0.0091)	(0.0094)	(0.0131)	(0.0190)	(0.0196)	(0.0200)
Union/standby[a]	0.3992	0.4013	0.0021	0.0025	0.0015	-0.0109
	(0.0104)	(0.0109)	(0.0151)	(0.0219)	(0.0227)	(0.0231)
Occupation						
professional	0.0754	0.0885	0.0131	-0.0020	-0.0076	0.0020
	(0.0056)	(0.0063)	(0.0085)	(0.0124)	(0.0127)	(0.0129)
clerical	0.1392	0.1283	-0.0109	0.0008	-0.0016	0.0078
	(0.0073)	(0.0075)	(0.0105)	(0.0152)	(0.0158)	(0.0164)
sales	0.0548	0.0517	-0.0031	0.0025	0.0048	-0.0001
	(0.0048)	(0.0049)	(0.0069)	(0.0101)	(0.0105)	(0.0106)
service	0.1006	0.1109	0.0103	0.0071	0.0129	0.0126
	(0.0064)	(0.0070)	(0.0095)	(0.0138)	(0.0143)	(0.0144)
agric., fishery, forestry	0.0278	0.0239	-0.0040	0.0013	-0.0001	-0.0002
	(0.0035)	(0.0034)	(0.0049)	(0.0070)	(0.0073)	(0.0073)
processing	0.0382	0.0338	-0.0044	-0.0047	-0.0039	-0.0054
	(0.0041)	(0.0040)	(0.0057)	(0.0085)	(0.0088)	(0.0089)
machine trades	0.0786	0.0746	-0.0040	-0.0015	-0.0005	-0.0108
	(0.0057)	(0.0059)	(0.0082)	(0.0119)	(0.0123)	(0.0126)
benchwork	0.0557	0.0582	0.0025	-0.0004	-0.0014	-0.0043
	(0.0049)	(0.0052)	(0.0071)	(0.0103)	(0.0106)	(0.0108)

Table 2

	Claim year			Difference in differences between control and JSA groups (year t – claim year)		
Covariate	Control mean (1)	JSA mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
structural work	0.2717	0.2740	0.0023	0.0019	0.0072	0.0023
	(0.0094)	(0.0099)	(0.0137)	(0.0200)	(0.0206)	(0.0211)
miscellaneous	0.1513	0.1437	-0.0076	-0.0045	-0.0069	-0.0005
	(0.0076)	(0.0078)	(0.0109)	(0.0158)	(0.0165)	(0.0169)
Industry						
agriculture	0.0211	0.0224	0.0013	-0.0011	-0.0017	-0.0031
	(0.0030)	(0.0033)	(0.0045)	(0.0065)	(0.0066)	(0.0067)
mining	0.0022	0.0005	-0.0017	-0.0003	-0.0005	-0.0007
	(0.0010)	(0.0005)	(0.0011)	(0.0015)	(0.0016)	(0.0017)
construction	0.2142	0.2014	-0.0128	-0.0088	-0.0021	-0.0128
	(0.0087)	(0.0089)	(0.0125)	(0.0181)	(0.0187)	(0.0191)
manufacturing	0.2335	0.2302	-0.0033	-0.0062	-0.0138	-0.0066
	(0.0090)	(0.0094)	(0.0130)	(0.0189)	(0.0195)	(0.0199)
transportation, utilities	0.0476	0.0532	0.0056	-0.0006	0.0013	-0.0009
	(0.0045)	(0.0050)	(0.0067)	(0.0099)	(0.0102)	(0.0106)
wholesale trade	0.0606	0.0487	*-0.0119*	0.0026	0.0001	0.0031
	(0.0051)	(0.0048)	(0.0070)	(0.0103)	(0.0109)	(0.0110)
retail trade	0.1576	0.1492	-0.0084	0.0027	0.0007	0.0050
	(0.0077)	(0.0079)	(0.0111)	(0.0162)	(0.0167)	(0.0170)
finance, ins., real estate	0.0269	0.0323	0.0054	-0.0004	0.0005	0.0034
	(0.0034)	(0.0039)	(0.0052)	(0.0075)	(0.0079)	(0.0080)
services	0.1720	0.1830	0.0110	0.0074	0.0100	0.0072
	(0.0080)	(0.0086)	(0.0118)	(0.0171)	(0.0177)	(0.0181)
government	0.0471	0.0607	*0.0135*	0.0021	0.0057	0.0022
	(0.0045)	(0.0053)	(0.0070)	(0.0101)	(0.0105)	(0.0108)
unclassified	0.0171	0.0184	0.0013	0.0024	-0.0001	0.0032
	(0.0027)	(0.0030)	(0.0041)	(0.0061)	(0.0062)	(0.0064)
Reason for job loss						
permanent layoff	0.1652	0.1507	-0.0146	0.0003	0.0072	0.0071
	(0.0079)	(0.0080)	(0.0112)	(0.0163)	(0.0169)	(0.0172)
temporary layoff	0.2236	0.2372	0.0136	0.0046	0.0081	-0.0039
	(0.0088)	(0.0095)	(0.0130)	(0.0188)	(0.0195)	(0.0199)
contract/seasonal	0.1702	0.1546	-0.0155	-0.0113	-0.0154	-0.0028
	(0.0080)	(0.0081)	(0.0113)	(0.0164)	(0.0169)	(0.0173)
quit	0.1608	0.1726	0.0118	0.0095	0.0105	0.0131
	(0.0078)	(0.0084)	(0.0115)	(0.0168)	(0.0174)	(0.0178)
missing	0.2802	0.2849	0.0047	-0.0031	-0.0104	-0.0136
	(0.0095)	(0.0101)	(0.0139)	(0.0202)	(0.0210)	(0.0214)
Prior claim						
none	0.7912	0.7892	-0.0020	0.0053	0.0004	0.0128
	(0.0086)	(0.0091)	(0.0125)	(0.0182)	(0.0188)	(0.0191)
duration ≤ 15 weeks	0.1078	0.0970	-0.0108	-0.0016	0.0031	0.0009
	(0.0066)	(0.0066)	(0.0093)	(0.0135)	(0.0141)	(0.0143)
duration > 15 weeks	0.1010	0.1139	0.0128	-0.0037	-0.0035	-0.0137
	(0.0064)	(0.0071)	(0.0095)	(0.0139)	(0.0141)	(0.0145)

Table 2

Covariate	Claim year: Control mean (1)	Claim year: JSA mean (2)	Claim year: Difference (treatment – control) (3)	Difference in differences between control and JSA groups (year *t* – claim year): Year 3 (4)	Year 6 (5)	Year 9 (6)
UI benefits/claim type						
weekly amount ($)	148.77	148.46	-0.31	-1.17	-1.74	-0.13
	(1.05)	(1.10)	(1.52)	(2.20)	(2.28)	(2.31)
maximum amount ($)	3,956.8	3,920.7	-36.1	-58.5	-74.1	-1.9
	(35.9)	(37.4)	(51.8)	(75.2)	(77.9)	(79.3)
potential benefit duration (weeks)	26.172	26.059	-0.113	-0.175	-0.227	-0.001
	(0.102)	(0.112)	(0.151)	(0.219)	(0.227)	(0.232)
replacement rate (percent)[b]	61.052	61.425	0.373	-0.129	0.586	-0.377
	(0.459)	(0.486)	(0.668)	(0.970)	(1.006)	(1.024)
combined wage claim[c]	0.0431	0.0502	0.0071	-0.0031	-0.0042	0.0012
	(0.0043)	(0.0049)	(0.0065)	(0.0093)	(0.0095)	(0.0099)
ex-service member claim	0.0265	0.0368	*0.0103*	0.0033	0.0058	0.0017
	(0.0034)	(0.0042)	(0.0054)	(0.0077)	(0.0080)	(0.0082)
federal employee claim	0.0184	0.0139	-0.0045	0.0014	0.0041	0.0059
	(0.0028)	(0.0026)	(0.0039)	(0.0056)	(0.0057)	(0.0058)
Reservation wage (hourly)						
≤ $5.00	0.1715	0.1626	-0.0089	0.0076	0.0071	0.0072
	(0.0080)	(0.0082)	(0.0115)	(0.0166)	(0.0171)	(0.0174)
$5.01–$7.00	0.1392	0.1546	0.0154	-0.0032	0.0027	0.0112
	(0.0073)	(0.0081)	(0.0109)	(0.0159)	(0.0165)	(0.0170)
$7.01–$10.00	0.1581	0.1621	0.0040	-0.0085	-0.0044	-0.0075
	(0.0077)	(0.0082)	(0.0113)	(0.0165)	(0.0172)	(0.0175)
$10.01–$20.00	0.1464	0.1283	*-0.0181*	-0.0029	-0.0116	-0.0122
	(0.0075)	(0.0075)	(0.0106)	(0.0153)	(0.0158)	(0.0162)
> $20.00	0.0925	0.0816	-0.0109	0.0043	0.0072	0.0068
	(0.0061)	(0.0061)	(0.0087)	(0.0126)	(0.0129)	(0.0131)
Pre-claim earnings ($)						
3 years before	10,870	10,610	-260	61	143	322
	(244)	(261)	(358)	(518)	(537)	(554)
2 years before	12,097	11,951	-146	-199	-151	1
	(238)	(248)	(344)	(500)	(518)	(528)
1 year before	14,241	14,095	-146	-155	-212	242
	(208)	(222)	(304)	(444)	(460)	(469)
Pre-claim hours						
3 years before	950.1	920.4	-29.7	8.6	3.9	14.4
	(16.9)	(17.4)	(24.3)	(35.3)	(36.5)	(37.2)
2 years before	1,101.9	1,089.2	-12.7	-5.7	-8.3	9.9
	(15.9)	(16.6)	(23.0)	(33.5)	(34.7)	(35.4)
1 year before	1,345.0	1,322.0	-22.9	17.5	9.8	30.1
	(13.7)	(15.1)	(20.4)	(29.6)	(30.7)	(31.3)
Pre-claim earnings variability						
annual (CV)	0.6418	0.6393	-0.0025	-0.0027	0.0102	0.0044
	(0.0110)	(0.0115)	(0.0159)	(0.0230)	(0.0237)	(0.0242)
seasonal (CV)	0.5709	0.5837	0.0128	-0.0077	0.0027	-0.0066
	(0.0080)	(0.0084)	(0.0116)	(0.0169)	(0.0174)	(0.0177)

Table 2

	Claim year			Difference in differences between control and JSA groups (year t – claim year)		
Covariate	Control mean (1)	JSA mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
Quarters earnings >0	8.8572	8.7126	-0.1446	-0.0332	-0.0816	-0.0107
in 12 pre-claim quarters	(0.0698)	(0.0750)	(0.1025)	(0.1482)	(0.1529)	(0.1555)
< 6	0.1886	0.1890	0.0004	0.0045	0.0063	-0.0008
	(0.0083)	(0.0087)	(0.0120)	(0.0175)	(0.0179)	(0.0182)
6–8	0.1796	0.1934	0.0138	-0.0007	0.0031	0.0076
	(0.0081)	(0.0088)	(0.0120)	(0.0174)	(0.0180)	(0.0184)
> 8	0.6318	0.6176	-0.0142	-0.0038	-0.0095	-0.0068
	(0.0102)	(0.0108)	(0.0149)	(0.0216)	(0.0224)	(0.0228)
Pre-claim drop in earnings						
between pre-claim year 3	0.3426	0.3317	-0.0109	0.0059	0.0030	0.0043
and pre-clam year 2	(0.0101)	(0.0105)	(0.0145)	(0.0212)	(0.0219)	(0.0223)
between pre-claim year 2	0.3345	0.3158	-0.0188	0.0001	0.0030	-0.0007
and pre-clam year 1	(0.0100)	(0.0104)	(0.0144)	(0.0210)	(0.0217)	(0.0222)
Claimants with earnings >0						
Control group[d]	2,227	n/a	2,227	1,999	1,749	1,629
JSA group	n/a	2,011	2,011	1,798	1,585	1,474

Source: See Table 1.

Bold indicates p-value < .05 for test of difference between control and JSA groups (column 3).

Italic indicates p-value < .10 for test of difference between control and JSA groups (column 3).

a. Claimants were not required to search for work or participate in JSA if their job placement was handled by a union, or if they were on layoff with a set recall date.

b. Weekly benefit amount as a percentage of pre-claim average weekly earnings.

c. Combined wage claims use earnings from more than one state to calculate base period earnings.

d. The size of the control group in this table differs from size reported in Tables A-1, A-3, and A-4 because we drop claimants who never received benefits from the JSA group.

Table 2

Table 3
Characteristics of controls and Exception Reporting (ER) claimants with earnings in claim year, and differences in differences between controls and ER claimants (subsequent years against claim year) (standard errors in parentheses)

	Claim year			Difference in differences between control and ER groups (year *t* – claim year)		
Covariate	Control mean (1)	ER mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
Male	0.6935	0.7282	**0.0347**	-0.0139	-0.0190	-0.0170
	(0.0093)	(0.0102)	(0.0138)	(0.0202)	(0.0209)	(0.0214)
Age (years)						
≤ 24	0.2145	0.2078	-0.0068	-0.0042	-0.0058	0.0077
	(0.0083)	(0.0093)	(0.0125)	(0.0181)	(0.0188)	(0.0194)
25-34	0.3723	0.4145	**0.0422**	-0.0066	-0.0200	-0.0217
	(0.0098)	(0.0113)	(0.0149)	(0.0218)	(0.0225)	(0.0231)
35-44	0.2489	0.2183	**-0.0306**	0.0097	0.0128	0.0063
	(0.0087)	(0.0095)	(0.0129)	(0.0189)	(0.0195)	(0.0201)
45-54	0.1136	0.1097	-0.0040	-0.0029	0.0043	0.0091
	(0.0064)	(0.0072)	(0.0096)	(0.0141)	(0.0143)	(0.0143)
≥ 54	0.0507	0.0498	-0.0008	0.0040	0.0087	-0.0015
	(0.0044)	(0.0050)	(0.0067)	(0.0094)	(0.0088)	(0.0082)
Gender-age interactions						
male ≤ 24	0.1438	0.1401	-0.0038	0.0002	-0.0018	0.0063
	(0.0071)	(0.0080)	(0.0107)	(0.0155)	(0.0162)	(0.0167)
male 25-34	0.2726	0.3164	**0.0438**	-0.0125	-0.0228	-0.0226
	(0.0090)	(0.0107)	(0.0140)	(0.0203)	(0.0210)	(0.0217)
male 35-44	0.1626	0.1480	-0.0147	0.0020	0.0026	0.0033
	(0.0075)	(0.0081)	(0.0110)	(0.0161)	(0.0166)	(0.0171)
male 45-54	0.0756	0.0850	0.0094	-0.0085	-0.0039	-0.0047
	(0.0053)	(0.0064)	(0.0083)	(0.0121)	(0.0123)	(0.0122)
male ≥ 54	0.0388	0.0388	0.0000	0.0048	0.0069	0.0006
	(0.0039)	(0.0044)	(0.0059)	(0.0082)	(0.0076)	(0.0071)
female ≤ 24	0.0707	0.0677	-0.0030	-0.0044	-0.0040	0.0014
	(0.0052)	(0.0058)	(0.0077)	(0.0112)	(0.0116)	(0.0119)
female 25-34	0.0997	0.0981	-0.0016	0.0059	0.0027	0.0009
	(0.0061)	(0.0068)	(0.0091)	(0.0133)	(0.0139)	(0.0143)
female 35-44	0.0862	0.0703	*-0.0159*	0.0076	0.0102	0.0031
	(0.0057)	(0.0059)	(0.0082)	(0.0121)	(0.0126)	(0.0129)
female 45-54	0.0380	0.0247	**-0.0133**	0.0055	0.0082	*0.0138*
	(0.0039)	(0.0036)	(0.0053)	(0.0078)	(0.0080)	(0.0080)
female ≥ 54	0.0119	0.0110	-0.0008	-0.0008	0.0017	-0.0021
	(0.0022)	(0.0024)	(0.0032)	(0.0047)	(0.0046)	(0.0042)
Ethnicity						
white	0.8255	0.8221	-0.0034	-0.0115	0.0039	-0.0088
	(0.0077)	(0.0088)	(0.0116)	(0.0168)	(0.0172)	(0.0178)
black	0.0993	0.1070	0.0077	0.0086	-0.0096	0.0076
	(0.0060)	(0.0071)	(0.0093)	(0.0134)	(0.0135)	(0.0140)
other	0.0752	0.0708	-0.0044	0.0028	0.0057	0.0013

	Claim year			Difference in differences between control and ER groups (year t – claim year)		
Covariate	Control mean (1)	ER mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
	(0.0053)	(0.0059)	(0.0079)	(0.0115)	(0.0120)	(0.0123)
Schooling (years)						
less than high school	0.1406	0.1196	**-0.0210**	0.0007	-0.0012	0.0018
	(0.0070)	(0.0074)	(0.0102)	(0.0148)	(0.0151)	(0.0154)
high school	0.5386	0.5525	0.0138	0.0007	0.0059	0.0014
	(0.0101)	(0.0114)	(0.0152)	(0.0222)	(0.0229)	(0.0234)
some college	0.2395	0.2508	0.0113	-0.0030	-0.0038	-0.0074
	(0.0086)	(0.0099)	(0.0132)	(0.0192)	(0.0198)	(0.0202)
college graduate	0.0813	0.0771	-0.0042	0.0016	-0.0009	0.0042
	(0.0055)	(0.0061)	(0.0082)	(0.0122)	(0.0125)	(0.0130)
Marital status/gender						
married male	0.2628	0.2712	0.0085	-0.0166	-0.0154	-0.0253
	(0.0089)	(0.0102)	(0.0135)	(0.0196)	(0.0202)	(0.0206)
married female	0.0960	0.0771	**-0.0189**	0.0040	0.0093	0.0126
	(0.0060)	(0.0061)	(0.0085)	(0.0123)	(0.0128)	(0.0130)
Household status						
no dependents	0.3102	0.3106	0.0004	0.0064	0.0176	0.0141
	(0.0094)	(0.0106)	(0.0141)	(0.0206)	(0.0212)	(0.0216)
1 dependent	0.1524	0.1516	-0.0008	0.0001	0.0039	0.0005
	(0.0073)	(0.0082)	(0.0110)	(0.0160)	(0.0166)	(0.0170)
2 or more dependents	0.2321	0.2356	0.0035	-0.0024	-0.0087	-0.0124
	(0.0085)	(0.0097)	(0.0129)	(0.0189)	(0.0194)	(0.0200)
homeowner	0.2857	0.2796	-0.0060	0.0098	0.0212	0.0071
	(0.0091)	(0.0103)	(0.0138)	(0.0200)	(0.0207)	(0.0211)
Veteran	0.2342	0.2324	-0.0017	-0.0072	-0.0018	-0.0205
	(0.0086)	(0.0097)	(0.0129)	(0.0188)	(0.0193)	(0.0197)
Union/standby[a]	0.3727	0.3274	**-0.0453**	0.0105	0.0149	0.0130
	(0.0098)	(0.0108)	(0.0145)	(0.0212)	(0.0218)	(0.0224)
Occupation						
professional	0.0732	0.0735	0.0003	0.0032	0.0016	0.0091
	(0.0053)	(0.0060)	(0.0080)	(0.0118)	(0.0121)	(0.0124)
clerical	0.1381	0.1343	-0.0038	-0.0007	0.0001	-0.0011
	(0.0070)	(0.0078)	(0.0105)	(0.0153)	(0.0159)	(0.0163)
sales	0.0552	0.0567	0.0015	-0.0024	0.0011	-0.0024
	(0.0046)	(0.0053)	(0.0070)	(0.0102)	(0.0105)	(0.0107)
service	0.1103	0.0855	**-0.0248**	0.0061	0.0074	0.0101
	(0.0063)	(0.0064)	(0.0090)	(0.0130)	(0.0135)	(0.0135)
agric., fishery, forestry	0.0262	0.0231	-0.0031	0.0002	-0.0008	0.0040
	(0.0032)	(0.0034)	(0.0047)	(0.0067)	(0.0070)	(0.0072)
processing	0.0364	0.0357	-0.0007	-0.0003	-0.0011	-0.0065
	(0.0038)	(0.0042)	(0.0057)	(0.0085)	(0.0087)	(0.0088)
machine trades	0.0826	0.0892	0.0066	0.0011	-0.0049	-0.0044
	(0.0056)	(0.0065)	(0.0086)	(0.0126)	(0.0128)	(0.0134)
benchwork	0.0568	0.0593	0.0025	0.0040	0.0015	-0.0012
	(0.0047)	(0.0054)	(0.0072)	(0.0104)	(0.0107)	(0.0109)

Table 3

	Claim year			Difference in differences between control and ER groups (year t – claim year)		
Covariate	Control mean (1)	ER mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
structural work	0.2628	0.2907	**0.0279**	-0.0127	0.0064	-0.0043
	(0.0089)	(0.0104)	(0.0137)	(0.0199)	(0.0205)	(0.0211)
miscellaneous	0.1512	0.1459	-0.0054	0.0017	-0.0094	-0.0027
	(0.0072)	(0.0081)	(0.0109)	(0.0158)	(0.0164)	(0.0168)
Industry						
agriculture	0.0204	0.0220	0.0016	-0.0021	-0.0022	-0.0030
	(0.0029)	(0.0034)	(0.0044)	(0.0063)	(0.0065)	(0.0066)
mining	0.0020	0.0005	-0.0015	0.0003	0.0001	-0.0000
	(0.0009)	(0.0005)	(0.0011)	(0.0015)	(0.0016)	(0.0017)
construction	0.2084	0.2030	-0.0054	-0.0062	0.0100	-0.0008
	(0.0082)	(0.0092)	(0.0123)	(0.0179)	(0.0186)	(0.0190)
manufacturing	0.2272	0.2450	0.0178	0.0036	0.0001	0.0033
	(0.0085)	(0.0099)	(0.0130)	(0.0190)	(0.0196)	(0.0201)
transportation, utilities	0.0466	0.0540	0.0075	-0.0070	-0.0073	-0.0067
	(0.0043)	(0.0052)	(0.0067)	(0.0097)	(0.0099)	(0.0103)
wholesale trade	0.0597	0.0588	-0.0009	-0.0013	-0.0072	-0.0030
	(0.0048)	(0.0054)	(0.0072)	(0.0105)	(0.0110)	(0.0113)
retail trade	0.1655	0.1558	-0.0097	0.0007	0.0003	-0.0004
	(0.0075)	(0.0083)	(0.0112)	(0.0163)	(0.0169)	(0.0172)
finance, ins., real estate	0.0262	0.0294	0.0032	-0.0012	-0.0015	0.0002
	(0.0032)	(0.0039)	(0.0050)	(0.0072)	(0.0075)	(0.0077)
services	0.1778	0.1626	-0.0151	0.0156	0.0076	0.0130
	(0.0077)	(0.0085)	(0.0115)	(0.0168)	(0.0172)	(0.0177)
government	0.0499	0.0504	0.0005	-0.0024	-0.0018	-0.0023
	(0.0044)	(0.0050)	(0.0067)	(0.0096)	(0.0098)	(0.0102)
unclassified	0.0163	0.0184	0.0020	0.0002	0.0019	-0.0003
	(0.0026)	(0.0031)	(0.0040)	(0.0059)	(0.0062)	(0.0061)
Reason for job loss						
permanent layoff	0.1631	0.1511	-0.0120	-0.0009	0.0030	0.0178
	(0.0075)	(0.0082)	(0.0111)	(0.0162)	(0.0166)	(0.0172)
temporary layoff	0.2182	0.2513	**0.0331**	0.0063	0.0116	-0.0031
	(0.0084)	(0.0099)	(0.0130)	(0.0189)	(0.0195)	(0.0199)
contract/seasonal	0.1663	0.1584	-0.0079	0.0001	-0.0034	-0.0046
	(0.0075)	(0.0084)	(0.0113)	(0.0164)	(0.0169)	(0.0172)
quit	0.1716	0.1689	-0.0027	0.0031	0.0101	0.0011
	(0.0076)	(0.0086)	(0.0115)	(0.0168)	(0.0173)	(0.0177)
missing	0.2808	0.2702	-0.0106	-0.0085	-0.0214	-0.0112
	(0.0091)	(0.0102)	(0.0136)	(0.0198)	(0.0205)	(0.0210)
Prior claim						
none	0.8002	0.7943	-0.0058	0.0021	-0.0011	0.0034
	(0.0081)	(0.0093)	(0.0123)	(0.0179)	(0.0184)	(0.0189)
duration ≤ 15 weeks	0.1034	0.0986	-0.0048	0.0024	-0.0019	-0.0041
	(0.0062)	(0.0068)	(0.0092)	(0.0134)	(0.0138)	(0.0140)
duration > 15 weeks	0.0964	0.1070	0.0106	-0.0045	0.0030	0.0007
	(0.0060)	(0.0071)	(0.0093)	(0.0135)	(0.0138)	(0.0143)

Table 3

Covariate	Claim year: Control mean (1)	Claim year: ER mean (2)	Claim year: Difference (treatment – control) (3)	Difference in differences between control and ER groups (year *t* – claim year): Year 3 (4)	Year 6 (5)	Year 9 (6)
UI benefits/claim type						
weekly amount ($)	146.77	148.28	1.52	-0.80	-1.32	-1.63
	(1.03)	(1.13)	(1.52)	(2.21)	(2.28)	(2.33)
maximum amount ($)	3,888.1	3,943.0	54.9	-6.7	-37.2	-24.9
	(34.9)	(38.3)	(51.8)	(75.4)	(77.7)	(79.6)
potential benefit duration (weeks)	26.023	26.203	0.180	0.072	-0.034	0.097
	(0.100)	(0.112)	(0.150)	(0.215)	(0.221)	(0.228)
replacement rate (percent)[b]	61.197	60.188	-1.008	-0.369	0.132	-0.063
	(0.440)	(0.492)	(0.660)	(0.960)	(0.987)	(1.013)
combined wage claim[c]	0.0454	0.0414	-0.0039	0.0022	0.0028	0.0064
	(0.0042)	(0.0046)	(0.0062)	(0.0089)	(0.0093)	(0.0095)
ex-service member claim	0.0298	0.0299	0.0001	-0.0045	-0.0004	-0.0021
	(0.0034)	(0.0039)	(0.0052)	(0.0072)	(0.0073)	(0.0076)
federal employee claim	0.0168	0.0152	-0.0015	-0.0009	0.0006	0.0043
	(0.0026)	(0.0028)	(0.0038)	(0.0054)	(0.0054)	(0.0057)
Reservation wage (hourly)						
≤ $5.00	0.1814	0.1626	-0.0188	0.0104	0.0088	0.0105
	(0.0078)	(0.0085)	(0.0115)	(0.0167)	(0.0172)	(0.0175)
$5.01–$7.00	0.1373	0.1553	*0.0180*	-0.0043	0.0060	-0.0049
	(0.0070)	(0.0083)	(0.0108)	(0.0159)	(0.0164)	(0.0168)
$7.01–$10.00	0.1532	0.1668	0.0136	-0.0022	-0.0047	-0.0101
	(0.0073)	(0.0085)	(0.0112)	(0.0164)	(0.0170)	(0.0173)
$10.01–$20.00	0.1406	0.1443	0.0037	-0.0023	-0.0027	-0.0109
	(0.0070)	(0.0081)	(0.0107)	(0.0155)	(0.0160)	(0.0164)
> $20.00	0.0919	0.1049	0.0130	0.0042	0.0124	0.0179
	(0.0058)	(0.0070)	(0.0091)	(0.0133)	(0.0137)	(0.0141)
Pre-claim earnings ($)						
3 years before	10,669	11,160	491	312	77	112
	(232)	(271)	(357)	(519)	(531)	(546)
2 years before	11,919	12,188	269	43	-157	-111
	(228)	(260)	(345)	(504)	(517)	(533)
1 year before	14,073	14,204	131	97	-180	-37
	(200)	(230)	(305)	(448)	(457)	(469)
Pre-claim hours						
3 years before	939.1	950.0	11.0	18.6	0.4	0.3
	(16.1)	(18.1)	(24.3)	(35.3)	(36.3)	(37.2)
2 years before	1,095.2	1,095.4	0.2	4.9	-8.4	-9.7
	(15.2)	(17.2)	(23.0)	(33.4)	(34.5)	(35.3)
1 year before	1,340.7	1,350.3	9.6	13.8	4.7	8.9
	(13.2)	(15.3)	(20.2)	(29.2)	(30.1)	(30.9)
Pre-claim earnings variability						
annual (CV)	0.6470	0.6304	-0.0166	-0.0079	0.0037	0.0055
	(0.0106)	(0.0120)	(0.0160)	(0.0231)	(0.0237)	(0.0243)
seasonal (CV)	0.5729	0.5671	-0.0057	-0.0083	0.0018	0.0066
	(0.0077)	(0.0087)	(0.0116)	(0.0169)	(0.0174)	(0.0179)

Table 3

Covariate	Claim year: Control mean (1)	Claim year: ER mean (2)	Claim year: Difference (treatment – control) (3)	Difference in differences between control and ER groups (year *t* – claim year): Year 3 (4)	Year 6 (5)	Year 9 (6)
Quarters earnings >0	8.8038	8.7844	-0.0195	0.0687	-0.0390	-0.0067
in 12 pre-claim quarters	(0.0670)	(0.0771)	(0.1021)	(0.1474)	(0.1517)	(0.1546)
< 6	0.1913	0.2020	0.0107	-0.0096	-0.0040	-0.0039
	(0.0080)	(0.0092)	(0.0122)	(0.0175)	(0.0179)	(0.0184)
6–8	0.1843	0.1742	-0.0101	0.0012	0.0075	0.0157
	(0.0078)	(0.0087)	(0.0117)	(0.0170)	(0.0177)	(0.0181)
> 8	0.6244	0.6238	-0.0006	0.0084	-0.0036	-0.0118
	(0.0098)	(0.0111)	(0.0148)	(0.0215)	(0.0222)	(0.0227)
Pre-claim drop in earnings						
between pre-claim year 3	0.3367	0.3410	0.0043	0.0114	0.0089	0.0147
and pre-clam year 2	(0.0096)	(0.0109)	(0.0145)	(0.0211)	(0.0218)	(0.0223)
between pre-claim year 2	0.3327	0.3358	0.0031	0.0051	0.0021	-0.0002
and pre-clam year 1	(0.0095)	(0.0108)	(0.0144)	(0.0210)	(0.0217)	(0.0223)
Claimants with earnings >0						
Control group	2,447	n/a	2,447	2,195	1,930	1,785
ER group	n/a	1,906	1,906	1,692	1,522	1,388

Source: See Table 1.

Bold indicates *p* -value < .05 for test of difference between control and ER groups (column 3).

Italic indicates *p* -value < .10 for test of difference between control and ER groups (column 3), or for test of difference in differences between control and ER groups (year *t* – claim year) (column 6).

a. Johnson and Klepinger (1991, pp. 17-18) note that the difference between controls and ER claimants in union/standby is a reporting issue;

ER claimants did not need to submit continued claims, so there was no reason to record their union/standby status.

b. Weekly benefit amount as a percentage of pre-claim average weekly earnings.

c. Combined wage claims use earnings from more than one state to calculate base period earnings.

Table 3

Table 4
Characteristics of controls and New Work Search (NWS) claimants with earnings in claim year, and differences in differences between controls and NWS claimants (subsequent years against claim year) (standard errors in parentheses)

	Claim year			Difference in differences between control and NWS groups (year *t* – claim year)		
Covariate	Control mean (1)	NWS mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
Male	0.6935	0.7089	0.0154	-0.0086	-0.0119	-0.0176
	(0.0093)	(0.0109)	(0.0144)	(0.0210)	(0.0219)	(0.0224)
Age (years)						
≤ 24	0.2145	0.2072	-0.0074	0.0006	0.0005	-0.0027
	(0.0083)	(0.0098)	(0.0128)	(0.0187)	(0.0195)	(0.0200)
25-34	0.3723	0.3953	0.0230	-0.0027	-0.0133	-0.0050
	(0.0098)	(0.0118)	(0.0153)	(0.0224)	(0.0233)	(0.0239)
35-44	0.2489	0.2292	-0.0197	-0.0030	0.0063	-0.0007
	(0.0087)	(0.0101)	(0.0134)	(0.0195)	(0.0203)	(0.0209)
45-54	0.1136	0.1128	-0.0008	0.0029	0.0054	0.0146
	(0.0064)	(0.0076)	(0.0100)	(0.0147)	(0.0150)	(0.0151)
≥ 54	0.0507	0.0556	0.0049	0.0021	0.0011	-0.0062
	(0.0044)	(0.0055)	(0.0071)	(0.0100)	(0.0092)	(0.0087)
Gender-age interactions						
male ≤ 24	0.1438	0.1453	0.0014	-0.0029	-0.0052	-0.0081
	(0.0071)	(0.0085)	(0.0111)	(0.0161)	(0.0168)	(0.0173)
male 25-34	0.2726	0.2870	0.0145	-0.0049	-0.0100	-0.0072
	(0.0090)	(0.0109)	(0.0141)	(0.0207)	(0.0216)	(0.0223)
male 35-44	0.1626	0.1534	-0.0093	-0.0027	0.0070	0.0050
	(0.0075)	(0.0087)	(0.0114)	(0.0167)	(0.0174)	(0.0179)
male 45-54	0.0756	0.0799	0.0043	-0.0028	-0.0043	-0.0012
	(0.0053)	(0.0065)	(0.0084)	(0.0125)	(0.0125)	(0.0125)
male ≥ 54	0.0388	0.0434	0.0046	0.0047	0.0006	-0.0061
	(0.0039)	(0.0049)	(0.0063)	(0.0088)	(0.0080)	(0.0074)
female ≤ 24	0.0707	0.0619	-0.0088	0.0036	0.0057	0.0054
	(0.0052)	(0.0058)	(0.0078)	(0.0115)	(0.0120)	(0.0122)
female 25-34	0.0997	0.1082	0.0085	0.0022	-0.0033	0.0022
	(0.0061)	(0.0075)	(0.0096)	(0.0140)	(0.0147)	(0.0152)
female 35-44	0.0862	0.0758	-0.0104	-0.0003	-0.0007	-0.0057
	(0.0057)	(0.0064)	(0.0085)	(0.0125)	(0.0130)	(0.0134)
female 45-54	0.0380	0.0330	-0.0050	0.0057	0.0097	*0.0159*
	(0.0039)	(0.0043)	(0.0058)	(0.0086)	(0.0089)	(0.0090)
female ≥ 54	0.0119	0.0122	0.0003	-0.0026	0.0005	-0.0001
	(0.0022)	(0.0026)	(0.0034)	(0.0049)	(0.0048)	(0.0047)
Ethnicity						
white	0.8255	0.8264	0.0009	-0.0078	0.0002	0.0020
	(0.0077)	(0.0091)	(0.0119)	(0.0172)	(0.0178)	(0.0182)
black	0.0993	0.0926	-0.0067	0.0100	0.0019	0.0054
	(0.0060)	(0.0070)	(0.0092)	(0.0133)	(0.0137)	(0.0139)
other	0.0752	0.0810	0.0058	-0.0022	-0.0021	-0.0074

	Claim year			Difference in differences between control and NWS groups (year t – claim year)		
Covariate	Control mean (1)	NWS mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
	(0.0053)	(0.0066)	(0.0085)	(0.0122)	(0.0127)	(0.0130)
Schooling (years)						
less than high school	0.1406	0.1348	-0.0057	-0.0008	-0.0085	-0.0113
	(0.0070)	(0.0082)	(0.0108)	(0.0157)	(0.0160)	(0.0162)
high school	0.5386	0.5347	-0.0039	0.0066	0.0035	-0.0094
	(0.0101)	(0.0120)	(0.0157)	(0.0229)	(0.0237)	(0.0243)
some college	0.2395	0.2494	0.0099	-0.0002	0.0076	0.0178
	(0.0086)	(0.0104)	(0.0135)	(0.0198)	(0.0206)	(0.0212)
college graduate	0.0813	0.0810	-0.0003	-0.0057	-0.0026	0.0029
	(0.0055)	(0.0066)	(0.0086)	(0.0125)	(0.0130)	(0.0136)
Marital status/gender						
married male	0.2628	0.2483	-0.0145	-0.0041	-0.0001	-0.0093
	(0.0089)	(0.0104)	(0.0137)	(0.0200)	(0.0207)	(0.0211)
married female	0.0960	0.0880	-0.0081	0.0111	0.0212	0.0151
	(0.0060)	(0.0068)	(0.0091)	(0.0133)	(0.0139)	(0.0140)
Household status						
no dependents	0.3102	0.2975	-0.0127	0.0039	0.0056	0.0101
	(0.0094)	(0.0110)	(0.0144)	(0.0211)	(0.0217)	(0.0222)
1 dependent	0.1524	0.1667	0.0142	0.0007	0.0046	-0.0004
	(0.0073)	(0.0090)	(0.0115)	(0.0169)	(0.0176)	(0.0180)
2 or more dependents	0.2321	0.2187	-0.0134	-0.0003	0.0024	-0.0003
	(0.0085)	(0.0099)	(0.0131)	(0.0192)	(0.0199)	(0.0206)
homeowner	0.2857	0.2541	**-0.0316**	0.0095	0.0214	0.0172
	(0.0091)	(0.0105)	(0.0139)	(0.0203)	(0.0211)	(0.0215)
Veteran	0.2342	0.2465	0.0124	-0.0125	-0.0045	-0.0206
	(0.0086)	(0.0104)	(0.0134)	(0.0196)	(0.0202)	(0.0207)
Union/standby	0.3727	0.3767	0.0040	0.0037	0.0111	-0.0016
	(0.0098)	(0.0117)	(0.0152)	(0.0222)	(0.0230)	(0.0236)
Occupation						
professional	0.0732	0.0666	-0.0066	-0.0048	-0.0001	0.0085
	(0.0053)	(0.0060)	(0.0080)	(0.0117)	(0.0122)	(0.0125)
clerical	0.1381	0.1273	-0.0108	0.0038	0.0034	-0.0033
	(0.0070)	(0.0080)	(0.0106)	(0.0156)	(0.0163)	(0.0166)
sales	0.0552	0.0503	-0.0048	0.0009	0.0002	0.0024
	(0.0046)	(0.0053)	(0.0070)	(0.0102)	(0.0105)	(0.0109)
service	0.1103	0.1227	0.0123	0.0013	0.0032	0.0129
	(0.0063)	(0.0079)	(0.0101)	(0.0146)	(0.0152)	(0.0154)
agric., fishery, forestry	0.0262	0.0231	-0.0030	0.0016	0.0001	0.0011
	(0.0032)	(0.0036)	(0.0048)	(0.0070)	(0.0072)	(0.0074)
processing	0.0364	0.0347	-0.0016	-0.0055	-0.0072	-0.0037
	(0.0038)	(0.0044)	(0.0058)	(0.0085)	(0.0087)	(0.0091)
machine trades	0.0826	0.0874	0.0048	0.0028	0.0014	-0.0026
	(0.0056)	(0.0068)	(0.0088)	(0.0129)	(0.0133)	(0.0138)
benchwork	0.0568	0.0625	0.0057	0.0046	0.0063	0.0043
	(0.0047)	(0.0058)	(0.0075)	(0.0109)	(0.0114)	(0.0116)

Table 4

	Claim year			Difference in differences between control and NWS groups (year t – claim year)		
Covariate	Control mean (1)	NWS mean (2)	Difference (treatment – control) (3)	Year 3 (4)	Year 6 (5)	Year 9 (6)
structural work	0.2628	0.2812	0.0185	-0.0092	0.0011	-0.0234
	(0.0089)	(0.0108)	(0.0140)	(0.0204)	(0.0211)	(0.0216)
miscellaneous	0.1512	0.1343	-0.0169	0.0057	-0.0062	0.0061
	(0.0072)	(0.0082)	(0.0109)	(0.0161)	(0.0166)	(0.0172)
Industry						
agriculture	0.0204	0.0197	-0.0008	0.0021	0.0008	0.0014
	(0.0029)	(0.0033)	(0.0044)	(0.0065)	(0.0066)	(0.0069)
mining	0.0020	0.0023	0.0003	0.0006	0.0007	-0.0000
	(0.0009)	(0.0012)	(0.0015)	(0.0022)	(0.0024)	(0.0023)
construction	0.2084	0.1840	**-0.0244**	0.0004	0.0018	-0.0108
	(0.0082)	(0.0093)	(0.0124)	(0.0182)	(0.0187)	(0.0192)
manufacturing	0.2272	0.2517	*0.0245*	-0.0038	-0.0011	0.0012
	(0.0085)	(0.0104)	(0.0134)	(0.0197)	(0.0204)	(0.0209)
transportation, utilities	0.0466	0.0428	-0.0038	-0.0020	-0.0017	-0.0025
	(0.0043)	(0.0049)	(0.0065)	(0.0095)	(0.0098)	(0.0102)
wholesale trade	0.0597	0.0480	*-0.0116*	0.0062	0.0013	0.0038
	(0.0048)	(0.0051)	(0.0070)	(0.0105)	(0.0111)	(0.0113)
retail trade	0.1655	0.1615	-0.0041	-0.0042	-0.0080	0.0003
	(0.0075)	(0.0089)	(0.0116)	(0.0169)	(0.0175)	(0.0180)
finance, ins., real estate	0.0262	0.0301	0.0039	0.0042	0.0027	-0.0011
	(0.0032)	(0.0041)	(0.0052)	(0.0077)	(0.0080)	(0.0079)
services	0.1778	0.1875	0.0097	0.0018	0.0051	0.0128
	(0.0077)	(0.0094)	(0.0122)	(0.0177)	(0.0184)	(0.0189)
government	0.0499	0.0538	0.0040	-0.0018	-0.0006	-0.0066
	(0.0044)	(0.0054)	(0.0070)	(0.0101)	(0.0104)	(0.0106)
unclassified	0.0163	0.0185	0.0022	-0.0035	-0.0011	0.0016
	(0.0026)	(0.0032)	(0.0041)	(0.0059)	(0.0063)	(0.0065)
Reason for job loss						
permanent layoff	0.1631	0.1534	-0.0097	-0.0043	0.0026	0.0005
	(0.0075)	(0.0087)	(0.0114)	(0.0167)	(0.0173)	(0.0177)
temporary layoff	0.2182	0.2216	0.0034	0.0037	0.0031	-0.0026
	(0.0084)	(0.0100)	(0.0130)	(0.0190)	(0.0196)	(0.0201)
contract/seasonal	0.1663	0.1551	-0.0112	-0.0051	-0.0066	-0.0017
	(0.0075)	(0.0087)	(0.0115)	(0.0168)	(0.0173)	(0.0178)
quit	0.1716	0.1771	0.0054	0.0099	0.0079	0.0102
	(0.0076)	(0.0092)	(0.0119)	(0.0176)	(0.0181)	(0.0187)
missing	0.2808	0.2928	0.0121	-0.0042	-0.0071	-0.0064
	(0.0091)	(0.0110)	(0.0142)	(0.0208)	(0.0216)	(0.0222)
Prior claim						
none	0.8002	0.7882	-0.0120	0.0004	-0.0035	-0.0045
	(0.0081)	(0.0098)	(0.0127)	(0.0186)	(0.0193)	(0.0198)
duration ≤ 15 weeks	0.1034	0.1007	-0.0027	0.0028	-0.0002	0.0093
	(0.0062)	(0.0072)	(0.0095)	(0.0139)	(0.0144)	(0.0149)
duration > 15 weeks	0.0964	0.1111	0.0147	-0.0031	0.0037	-0.0047
	(0.0060)	(0.0076)	(0.0096)	(0.0141)	(0.0145)	(0.0149)

Table 4

Covariate	Claim year: Control mean (1)	Claim year: NWS mean (2)	Claim year: Difference (treatment – control) (3)	Difference in differences between control and NWS groups (year *t* – claim year): Year 3 (4)	Year 6 (5)	Year 9 (6)
UI benefits/claim type						
weekly amount ($)	146.77	145.82	-0.95	-0.71	-0.37	-0.97
	(1.03)	(1.22)	(1.59)	(2.32)	(2.40)	(2.46)
maximum amount ($)	3,888.1	3,839.1	-49.0	-23.6	-19.8	-13.7
	(34.9)	(41.5)	(54.2)	(79.0)	(81.7)	(84.0)
potential benefit duration (weeks)	26.023	25.840	-0.183	-0.009	-0.049	0.059
	(0.100)	(0.124)	(0.159)	(0.229)	(0.236)	(0.243)
replacement rate (percent)[a]	61.197	62.020	0.823	0.155	-0.144	-0.547
	(0.440)	(0.527)	(0.687)	(1.005)	(1.035)	(1.060)
combined wage claim[b]	0.0454	0.0503	0.0050	-0.0008	-0.0043	-0.0081
	(0.0042)	(0.0053)	(0.0067)	(0.0096)	(0.0099)	(0.0099)
ex-service member claim	0.0298	0.0376	0.0078	-0.0026	0.0030	-0.0019
	(0.0034)	(0.0046)	(0.0057)	(0.0081)	(0.0083)	(0.0085)
federal employee claim	0.0168	0.0145	-0.0023	0.0015	0.0002	0.0033
	(0.0026)	(0.0029)	(0.0039)	(0.0056)	(0.0055)	(0.0057)
Reservation wage (hourly)						
≤ $5.00	0.1814	0.1748	-0.0067	0.0089	-0.0045	0.0015
	(0.0078)	(0.0091)	(0.0120)	(0.0175)	(0.0179)	(0.0183)
$5.01–$7.00	0.1373	0.1325	-0.0048	-0.0032	0.0051	0.0015
	(0.0070)	(0.0082)	(0.0107)	(0.0158)	(0.0163)	(0.0169)
$7.01–$10.00	0.1532	0.1649	0.0117	-0.0098	-0.0041	-0.0059
	(0.0073)	(0.0089)	(0.0115)	(0.0168)	(0.0176)	(0.0180)
$10.01–$20.00	0.1406	0.1279	-0.0127	0.0097	0.0096	0.0039
	(0.0070)	(0.0080)	(0.0107)	(0.0157)	(0.0163)	(0.0168)
> $20.00	0.0919	0.0966	0.0047	-0.0037	0.0005	0.0065
	(0.0058)	(0.0071)	(0.0092)	(0.0133)	(0.0137)	(0.0141)
Pre-claim earnings ($)						
3 years before	10,669	10,558	-111	40	85	106
	(232)	(277)	(362)	(525)	(542)	(556)
2 years before	11,919	11,576	-343	-201	45	-166
	(228)	(263)	(348)	(507)	(526)	(537)
1 year before	14,073	13,625	-448	-80	144	132
	(200)	(235)	(309)	(453)	(470)	(479)
Pre-claim hours						
3 years before	939.1	939.4	0.4	2.9	2.9	13.2
	(16.1)	(19.3)	(25.2)	(36.7)	(37.9)	(38.8)
2 years before	1,095.2	1,079.1	-16.1	-11.0	4.8	-4.7
	(15.2)	(18.1)	(23.6)	(34.6)	(35.7)	(36.7)
1 year before	1,340.7	1,299.2	**-41.5**	0.3	19.1	16.0
	(13.2)	(16.1)	(20.8)	(30.4)	(31.4)	(32.2)
Pre-claim earnings variability						
annual (CV)	0.6470	0.6496	0.0026	-0.0070	-0.0116	-0.0100
	(0.0106)	(0.0126)	(0.0165)	(0.0240)	(0.0246)	(0.0253)
seasonal (CV)	0.5729	0.5812	0.0083	0.0016	-0.0025	-0.0063
	(0.0077)	(0.0092)	(0.0120)	(0.0176)	(0.0181)	(0.0186)

Table 4

Covariate	Claim year: Control mean (1)	Claim year: NWS mean (2)	Claim year: Difference (treatment – control) (3)	Difference in differences between control and NWS groups (year *t* – claim year): Year 3 (4)	Year 6 (5)	Year 9 (6)
Quarters earnings >0	8.8038	8.6973	-0.1065	0.0067	0.0706	0.0520
in 12 pre-claim quarters	(0.0670)	(0.0813)	(0.1053)	(0.1529)	(0.1573)	(0.1618)
< 6	0.1913	0.2083	0.0171	-0.0069	-0.0087	-0.0015
	(0.0080)	(0.0098)	(0.0126)	(0.0183)	(0.0187)	(0.0192)
6–8	0.1843	0.1777	-0.0066	0.0042	-0.0023	-0.0041
	(0.0078)	(0.0092)	(0.0121)	(0.0177)	(0.0183)	(0.0186)
> 8	0.6244	0.6140	-0.0104	0.0027	0.0110	0.0056
	(0.0098)	(0.0117)	(0.0153)	(0.0223)	(0.0230)	(0.0235)
Pre-claim drop in earnings						
between pre-claim year 3	0.3367	0.3437	0.0070	0.0023	-0.0078	0.0094
and pre-clam year 2	(0.0096)	(0.0114)	(0.0149)	(0.0218)	(0.0226)	(0.0232)
between pre-claim year 2	0.3327	0.3362	0.0036	-0.0016	-0.0034	-0.0150
and pre-clam year 1	(0.0095)	(0.0114)	(0.0148)	(0.0217)	(0.0224)	(0.0230)
Claimants with earnings >0						
Control group	2,447	n/a	2,447	2,195	1,930	1,785
NWS group	n/a	1,728	1,728	1,498	1,320	1,201

Source: See Table 1.

Bold indicates *p* -value < .05 for test of difference between control and NWS groups (column 3).

Italic indicates *p* -value < .10 for test of difference between control and NWS groups (column 3), or for test of difference in differences between control and NWS groups (year *t* – claim year) (column 6).

a. Weekly benefit amount as a percentage of pre-claim average weekly earnings.

b. Combined wage claims use earnings from more than one state to calculate base period earnings.

Table 4