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# Effects of the unemployment insurance work test on long-term employment outcomes☆

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## HIGHLIGHTS

- We examine the effects of the work test on long-term employment outcomes.
- We add nine years of data to the Washington Alternative Work Search experiment.
- The work test improves employment outcomes for lower-wage, permanent job losers.

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## ABSTRACT

Does requiring job seekers to be available and searching for work affect job quality? We examine the effects of this unemployment insurance (UI) work test on long-term employment outcomes. Adding administrative wage records to the Washington Alternative Work Search (WAWS) experiment, we examine effects on earnings, hours worked, employment, and job match quality in the nine years following the experiment. Among UI recipients as a whole, the effects of the work test were negligible, counter to the hypothesis that the work test may harm long-term earnings. But for permanent job losers, the work test reduced time to reemployment by 1–2 quarters, and increased job tenure with the first post-claim employer by about 2 quarters. Also, we find that the work test selected lower-wage workers into reemployment. Accordingly, the work test may be an important policy for improving the reemployment prospects of lower-wage, permanent job losers.



## 1. Introduction

The work test for unemployment insurance (UI) recipients has been a central part of UI in the United States since the system began in the 1930s. In general, 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 test.

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This paper examines the intent-to-treat effects of the work test on long-term employment outcomes, such as long-term earnings, hours worked, probability of employment, and post-unemployment match quality (proxied by job tenure). To do this, we add nine years of quarterly administrative wage records to the short-term data from the Washington Alternative Work Search (WAWS) experiment previously analyzed by Johnson and Klepinger (1991, 1994). The WAWS experiment randomly assigned eligible UI claimants to a treatment that effectively eliminated the work test (*no work test* group); to a treatment with a *standard work test*, which usually called UI recipients for an Eligibility Review Interview (ERI) 12–15 weeks after the initial UI claim; and to a treatment with a *modified work test*, which usually called UI recipients for an ERI about the *fourth* week following the initial claim. (We describe the treatments further in Section 2.)

### 1.1. Previous literature

The effects of the work test on post-unemployment outcomes have been much debated. Policymakers and economists alike have expressed concerns that, although necessary to reduce moral hazard, the work test could pressure a worker to accept a relatively poor job match, undermining UI's objective of enabling the worker "to search longer for a suitable job that is in line with previous earnings and experience" (Blaustein, 1993, p. 61).

Appropriately, then, a relatively small but growing international empirical literature has examined the effects of the work test and related UI policies (such as monitoring and sanctions) on post-unemployment outcomes, particularly in European countries. Arni et al. (2013) provide a detailed analysis of warnings and sanctions in the Swiss UI system and find that benefit sanctions lower post-unemployment earnings, and that the adverse effects persist for at least two years. Similarly, van den Berg and Vikström (2014) find that in Sweden, sanctions for not meeting the work test lead to lower wage rates and hours worked, and that the adverse effects persist at least 4 years. Accordingly, these studies suggest that stringent monitoring of the work test may push claimants to accept poor job matches.

Van den Berg and van der Klaauw (2006, 2013) study a random-assignment experiment in Holland and find relatively little effect of counseling and monitoring on exit to work or post-unemployment earnings. They explain their findings in light of a structural model and show that monitoring causes claimants to substitute formal for informal job search in order to satisfy monitoring requirements.

In a random-assignment experiment in the United States (Maryland), Klepinger et al. (2002) found that a relaxed work search requirement led to higher earnings in the third and fourth quarters following the quarter in which new UI claimants were assigned to treatment.[1]

The work most closely related to ours is by Dolton and O'Neill (2002), who examine the long-term employment outcomes of tighter monitoring of benefit eligibility rules and increased job search assistance in the UK Restart program during 1989–1994. Their analysis finds that monthly unemployment rates among males in the randomized treatment group (that is, those subject to closer monitoring) were lower than controls over the 5-year follow-up period, but there was no effect among women.

The evidence, then, is quite mixed regarding the effect of the work test on post-unemployment outcomes. We can only speculate on the reasons for these diverse findings, but a common thread in the policies that reduced post-unemployment earnings is their apparent emphasis on sanctions, whereas the treatments that improved post-unemployment outcomes purported to place more emphasis on reemployment services. The speculative nature of these comments highlights the importance of looking inside the "black box" of policy interventions.

### 1.2. Main findings

The WAWS experiment was based on random assignment, allowing us to rely mainly on straightforward estimators of the intent-to-treat effects of the standard and modified work tests on long-term post-unemployment outcomes, with no work test as the comparison group. For reasons discussed in Sections 2 and 4, the work test may have different effects on different groups of UI claimants, so we also estimate separate models for subgroups of claimants: permanent job losers, those who quit for good cause, and those temporarily laid off.

For UI claimants as a whole, we find little evidence that the long-term earnings or employment probabilities of workers in the standard and modified work test groups differed from those of workers who faced no work test; however, we do find differences among subgroups. For permanent job losers, the work test resulted in improved employment outcomes: greater earnings in the year following job loss, a shorter spell of nonemployment, and longer tenure with the first post-claim employer. Pre-treatment outcome tests in Section 6.1 suggest these gains accrued disproportionately to workers who had earned lower wages before their permanent job loss. Given that permanent job losers have increased as a share of all job losers during the past 20 years, the findings suggest the importance of maintaining the work test as a way of improving the reemployment prospects of low-wage, permanent job losers.

For claimants who quit for "good cause," the effects of the work test were minimal. Although the modified work test increased the probability of reemployment in the year following the UI claim, neither work test treatment had any long-term effect on the employment, earnings, or other observable long-term outcomes of claimants who quit.

Finally, for claimants on temporary layoff, the work test treatments reduced UI benefit receipt and UI durations, but had virtually no impact on employment outcomes. The results for claimants on temporary layoff imply that the work test plays a role in reducing moral hazard—without it, claimants drew more UI benefits without any improvement in employment outcomes.

The paper is organized as follows. After a description of the WAWS experimental design (Section 2), we describe the data and the setup of the long-term panel (Section 3). [Additional information on the data is included in an online Data Appendix (Lachowska et al., 2015.)] We then briefly consider the theoretical links between the UI work test and post-claim employment outcomes (Section 4). Section 5 reviews the estimation methods, and Section 6 presents the results of estimation. Section 7 offers further discussion and summarizes the findings.

## 2. Institutional background: the UI work test and the WAWS experiment

### 2.1. UI and the work test in the State of Washington

To be eligible for UI in Washington, a claimant must have worked at least 680 hours in approximately the year before claiming UI, must have

[1] Several papers examine the impact of sanctions on the transition out of unemployment or the duration of unemployment, but for lack of data they do not examine post-unemployment outcomes: Gorter and Kalb (1996), Abbring et al. (2005), Ashenfelter et al. (2005), Lalive et al. (2005), Micklewright and Nagy (2010), Rosholm and Svarer (2008), Svarer (2011), Cockx and Dejemeppe (2012), and Toohey (2014).
A different literature examines the effects of other aspects of UI (such as benefit generosity or potential benefit duration) on post-unemployment outcomes. For example, Centeno (2004), McCall and Chi (2008), Tatsiramos (2009), Caliendo et al. (2013), and Nekoei and Weber (2015a) find a positive relationship between more generous UI and post-unemployment earnings. In contrast, Addison and Portugal (1989), Gregory and Jukes (2001), and Schmieder et al. (2016) find a negative relationship. Finally, some research has found little convincing relationship between post-unemployment earnings and UI benefit generosity (Addison and Blackburn, 2000; Belzil, 2001; Centeno and Novo, 2009), longer potential duration of UI benefits (Lalive, 2007; Card et al., 2007), or subsequent job tenure (Belzil, 2001; Card et al., 2007; van Ours and Vodopivec, 2008). See also the reviews by Fredriksson and Holmlund (2006) and Tatsiramos and van Ours (2014).

been laid off for lack of work and through no fault of her own,[2] and must be "able, available, and searching" for work. The states use different standards in applying this work test, but in Washington (as in most states), UI claimants are required to register with the public Employment Service (ES) unless they are on temporary layoff (in which case they are attached to an employer) or obtain employment through a union hiring hall. The ES offers a range of job-matching and reemployment services, the most-used of which are job referral (and possible placement) and job development (Balducchi et al., 1997).[3] A job referral occurs when the ES directs a claimant to interview for a "suitable" job opening that an employer has listed with the ES.[4] If a claimant refuses to follow-up on a referral or refuses an offer of suitable work, the ES reports the issue to UI personnel to determine whether benefits should be terminated. In this way, the ES performs a central role in enforcing the work test.

Job development is similar to job referral, but occurs when ES personnel make an effort to place a claimant with an employer who has not listed a vacancy. In Washington and most states, job development is preceded by a planning process that appraises the claimant's job skills and employment goals and provides information about employers and their needs. It is worth noting that the effectiveness of these and other services provided by the ES has long been a subject of debate.[5]

At the time of the WAWS experiment, Washington was one of two states where UI claimants could receive up to 30 weeks of benefits during the 52 weeks following the claim. (In other states the maximum was 26 weeks.) Both the weekly benefit amount and the maximum number of weeks payable varied with earnings during the year preceding the claim. The weekly benefit amount was calculated so as to replace 52% of average weekly earnings in the *two high-earning quarters* of the year before the UI claim, up to $197 per week[6]; and maximum payable benefits ranged from 13 weeks to 30 weeks, calculated as the lesser of one-third of earnings in the year before the claim or 30 times the weekly benefit amount. In the estimation sample, the average weekly benefit amount is $146, and the average weeks of benefits payable is 26 weeks; the maximum weekly benefit amount ($197 per week, received by one-third of the analysis sample) and maximum weeks of benefits payable (30 weeks, applying to 47% of the sample) are also the modal values. (Means for each treatment group are displayed in Table 2.)

### 2.2. Description of the treatments

Enrollment in the WAWS experiment took place at the Tacoma Employment Service Center between July 1986 and August 1987, at which time new UI claimants were randomly assigned to a group subject to the standard work test, a modified work test, and no work test.[7] At the time of the experiment, the Tacoma Service Center combined the functions of the UI agency and the ES under one roof, as was done in many states until UI claims stopped being taken in-person during the late 1990s. This "co-location" of UI and ES functions was intended to facilitate use of the ES by UI claimants and to coordinate UI and ES activities so as to improve enforcement of the work test.

Claimants in the standard and modified work test groups were given an application to complete and told to return the following week for a so-called Presentation of Benefits Rights. During this presentation, UI personnel told claimants about their responsibilities under the UI law, which included (i) searching for work (although they were not told how many weekly contacts to make *at this time*), (ii) following subsequent directives to report to the ES that would arrive in the mail, and (iii) filing a "continued claim" every two weeks in order to receive benefits. To file a continued claim, a claimant needed to complete and mail a form to the UI agency every two weeks "certifying" that she was actively engaged in work search and listing her job contacts.

Claimants in the no work test group were also given an application and told to return for a Presentation of Benefits Rights, but the content of their presentation differed in one important way from the one attended by the standard and modified work test groups: no work test claimants were *not* told to file a bi-weekly continued claim. Instead, they were told to call the Service Center if they stopped looking for work or took a job. Otherwise, weekly UI benefits would continue to be mailed. Hence, the no work test treatment was effectively an honor system. Like the other groups, the no work test group was told to actively seek work (but like the other groups, they were not told how many weekly contacts to make), and that the Service Center's services would be available to them.

Following the Presentations of Benefits Rights, the treatment of the standard and modified work test groups diverged. The standard work test group received a letter (about one week after the presentation) telling them to contact at least three employers per week and to report these contacts on their continued claims forms. Also, claimants in the standard work test group received a directive to report for an ERI about 12–15 weeks after the claim for benefits. For an ERI, a claimant reported for a one-hour group "interview" or lecture followed by (in some cases) a 15-minute individual interview during which employer contacts were checked. The penalty for failure to report for the ERI or failure to provide evidence of job adequate search was denial of benefits for the week(s) in question.

In contrast to the standard work test, the modified work test typically called claimants for an ERI in week 4, with the likelihood of an early ERI being highest for permanent job losers and seasonal workers.[8] Also, descriptions of the modified work test suggest that ERIs for these claimants were intended to focus more on job development planning than those for the standard work test group (Johnson and Klepinger,

[2] A claimant who voluntarily quit her job would still be eligible for UI benefits if the quit was for "good cause"—that is, "connected with the work, attributable to the employer, or involving fault on the part of the employer" (U.S. Department of Labor, 2015, p. 5-2). Examples of good cause include hazardous working conditions and being required to work a different shift than the worker was hired for. Washington also considers illness of the claimant and quitting to accept another job that subsequently does not materialize to be good causes.

[3] Balducchi et al. (1997) also discuss additional services, such as testing and support services (counseling). These more expensive services are usually reserved for long-term unemployed workers.

[4] "Suitable work" is defined differently in different states (U.S. Department of Labor, 2015, pp. 5-26–5-28). Washington's criteria for suitable work are fairly standard and include prior work experience, education, and training, as well as risk to a claimant's health and safety, the claimant's duration of unemployment, and distance between the work and the claimant's home (Revised Code of Washington, Title 50, chapter 20, section 100, "Suitable work factors").

[5] Although the two most extensive studies of ES job referrals ( Johnson et al., 1985; Jacobson and Petta, 2000) conclude that referrals shorten the duration of unemployment spells (the estimates vary between the studies and across groups), both studies faced difficulties in identifying the effects of ES referrals: neither was based on randomized trials, and both relied mainly on controlling for observables to select comparison groups for individuals who received ES referrals. See the comments on Jacobson and Petta (2000) by Smith (2000).

[6] Note that the replacement rate based on mean weekly earnings in the *full year* before the claim will exceed the statutory 52% for any claimant whose quarterly earnings were not constant during the year before the claim. For this reason, the sample mean replacement rate based on earnings in the full year before the claim, which we use as a control variable in the analysis, is 61.6% (see Table 2).

[7] The WAWS experiment also included an "intensive services" treatment, in which claimants were assigned to job search assistance (see Johnson and Klepinger, 1991). This treatment poses analytical issues that do not come up with work test treatments; accordingly, we are examining its long-term effects elsewhere.

[8] Descriptions of the modified work test are unclear about the number of job contacts required for these claimants, and the data do not include an indicator of the week in which each was to be called for an ERI. However, inspection of the data confirms that permanent job losers and seasonal workers in the modified work test group were indeed more likely to receive an ERI.

**Table 1**
Proportions of claimants receiving eligibility review interviews (ERIs) and employment services, by treatment group and number of weeks of UI benefits received during the benefit year. *Source*: Authors' tabulations of the Washington Alternative Work Search experimental data, from UI claims records, administrative wage records, and Employment Service records. See Section 3 and the Data Appendix (Lachowska et al., 2015) for details.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Sample proportions | | | | | |
| | | | | *p*-value of test for difference between: | | |
| Service[a] | No work test | Standard work test | Modified work test | (1) and (2) | (1) and (3) | (2) and (3) |
| *Panel A: All claimants* | | | | | | |
| Eligibility review interview (ERI) | 0.004 | 0.250 | 0.322 | 0.000 | 0.000 | 0.000 |
| Employment services | | | | | | |
| Job development plan | 0.007 | 0.114 | 0.182 | 0.000 | 0.000 | 0.000 |
| Job referral/placement | 0.155 | 0.185 | 0.160 | 0.028 | 0.720 | 0.105 |
| Other employment service[b] | 0.062 | 0.107 | 0.116 | 0.000 | 0.000 | 0.506 |
| Sample size | 1606 | 1539 | 1073 | | | |
| *Panel B: Claimants who received 1–7 weeks of UI benefits* | | | | | | |
| Eligibility review interview (ERI) | 0.000 | 0.004 | 0.055 | 0.157 | 0.000 | 0.000 |
| Employment services | | | | | | |
| Job development plan | 0.000 | 0.006 | 0.053 | 0.083 | 0.000 | 0.000 |
| Job referral/placement | 0.116 | 0.127 | 0.124 | 0.625 | 0.738 | 0.897 |
| Other employment service[b] | 0.064 | 0.045 | 0.063 | 0.250 | 0.991 | 0.248 |
| Sample size | 362 | 528 | 379 | | | |
| *Panel C: Claimants who received 8–15 weeks of UI benefits* | | | | | | |
| Eligibility review interview (ERI) | 0.003 | 0.162 | 0.330 | 0.000 | 0.000 | 0.000 |
| Employment services | | | | | | |
| Job development plan | 0.006 | 0.092 | 0.225 | 0.000 | 0.000 | 0.000 |
| Job referral/placement | 0.158 | 0.183 | 0.174 | 0.387 | 0.617 | 0.784 |
| Other employment service[b] | 0.059 | 0.095 | 0.138 | 0.085 | 0.003 | 0.133 |
| Sample size | 323 | 327 | 218 | | | |
| *Panel D: Claimants who received > 15 weeks of UI benefits* | | | | | | |
| Eligibility review interview (ERI) | 0.005 | 0.466 | 0.524 | 0.000 | 0.000 | 0.047 |
| Employment services | | | | | | |
| Job development plan | 0.009 | 0.202 | 0.263 | 0.000 | 0.000 | 0.130 |
| Job referral/placement | 0.166 | 0.225 | 0.185 | 0.003 | 0.374 | 0.088 |
| Other employment service[b] | 0.061 | 0.255 | 0.151 | 0.000 | 0.000 | 0.863 |
| Sample size | 967 | 729 | 502 | | | |

*Notes*: Sample consists of claimants in the no work test, standard work test, and modified work test groups during fall 1986, winter 1987, and spring 1987.
[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.

1991, p. 4), but whether this was the case in practice is unclear (see the discussion of Table 1 below).

Unlike the standard and modified work test groups, the no work test group did not hear from the Service Center after their Presentations of Benefits Rights, except in regard to services such as job referral and placement; that is, they received no directives about the number of weekly job contacts to make or to report for an ERI, consistent with the "honor system" approach of this treatment.

To summarize, the three treatments differed from each other in two key ways. First, the standard and modified work test groups were required to file continued claims every two weeks as a condition of receiving benefits, whereas claimants in the no work test group received benefits automatically until they called the UI agency to report they had stopped looking for work or found a job. Second, the timing of ERIs differed between the standard and modified work test groups: claimants in the standard work test group were called in weeks 12–15, whereas the bulk of claimants in the modified work test group were called in week 4. Claimants in all three groups were eligible to receive various services from the ES.

### 2.3. Services received by the treatment groups

Table 1 offers information on how the treatments worked in practice by showing proportions of the three treatment groups receiving an ERI and various reemployment services. The top panel shows receipt of ERIs and services for all claimants in the experiment, whereas the bottom three panels show the same information for three mutually exclusive groups of claimants: those who received 1–7 weeks of UI benefits in total during the year following the initial claim, those who received 8–15 weeks of UI, and those who received >15 weeks of UI.[9]

Virtually none of the no work test claimants were subjected to an ERI or received job development planning (which often went with an ERI), consistent with the treatment protocol (top panel of Table 1). Similar proportions of the three groups received job referrals and placements, which are usually initiated by the ES; however, fewer in the no work test group received other reemployment services, which are usually initiated by the claimant.

The modified work test group was more likely to receive an ERI and a job development plan than was the standard work test group, which makes sense because modified work test claimants were called relatively early for an ERI. As can be seen in the bottom three panels of Table 1, modified work test claimants who received 1–7 weeks and 8–15 weeks of UI were substantially more likely than standard work test claimants to be called for an ERI.

As mentioned above, source descriptions of the treatments (Johnson and Klepinger, 1991, pp. 4, 9) suggest that the modified work test would place greater emphasis on job development planning and other services. The figures in Table 1 do suggest that, conditional of ERI receipt,

[9] Data from the experiment do not indicate the date on which services were received. Examining claimants by the total number of weeks of UI benefits received is a second-best way of examining the timing of services received by each treatment group.

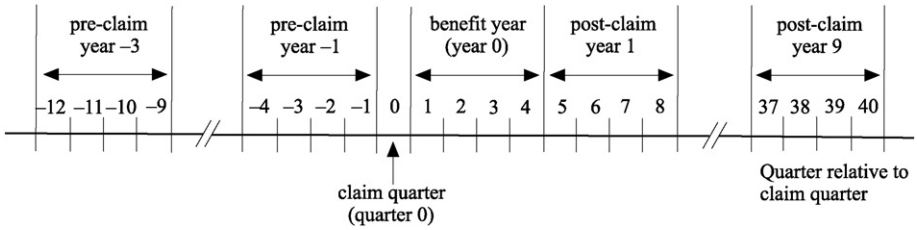


**Fig. 1.** Structure of the long-term panel *Notes*: The claim quarter (defined as quarter 0) is the quarter in which the worker claims UI. The benefit year (defined as year 0) is the four quarters following the claim quarter, during which up to 30 weeks of benefits may be drawn. As shown, pre-claim quarters and post-claim quarters are defined relative to the claim quarter, and pre-claim years and post-claim years are defined relative to the benefit year.

modified work test claimants were somewhat more likely to receive job development planning than were standard work test claimants. However, referral and placement through the ES appear to have proceeded almost independent of treatment assignment: only for claimants with UI spells exceeding 15 weeks did differences among the treatments reach statistical significance, and even among these claimants the differences are not great (16.6%, 22.5%, and 18.5%—see the bottom panel of Table 1).[10]

## 3. Description of the data

The data we use come from two sources. First, the data used originally by Johnson and Klepinger (1991, 1994) to analyze the WAWS short-term intent-to-treat effects (which we refer to as the WAWS experimental data) include (i) observable characteristics of each claimant in the experiment; (ii) information about UI benefits paid to each claimant during the year following enrollment; and (iii) quarterly observations of each worker's earnings and hours of work in the 12 quarters before the experiment (*pre-claim quarters*), the quarter in which the worker claimed UI (the *claim quarter*, defined as quarter 0), and the subsequent four quarters (which together comprise the *benefit year* during which claimants may draw benefits)—see Fig. 1, which shows the structure of the long-term panel. Note that the benefit year is defined as year 0. The original sources of these data are ES records, UI claims records, and UI administrative wage records.

Second, to construct long-term employment outcomes, we merge the WAWS experimental data on each claimant with quarterly UI administrative wage records for the nine years following the benefit year, which we refer to as *post-claim years* 1 through 9—see again Fig. 1. These wage records give quarterly observations on each claimant's earnings and hours of work, and in addition the identity of the claimant's principal employer (defined as the employer from whom the worker had the most earnings in the quarter).[11] These wage records come from the Washington Employment Security Department, and their coverage is nearly universal—self-employed workers are the only "above-ground" workers not included (because they are not covered by UI).

### 3.1. Short-term outcome variables

For each claimant, we construct variables related to UI outcomes and short-term employment outcomes. The UI outcomes include total UI *benefits paid* during the benefit year, the number of *weeks paid* (number of weeks during the benefit year in which the claimant received a payment), an indicator for whether the claimant *exhausted benefits*, and the *number of conditional payments* of UI benefits (payments made when there was a question about the claimant's continuing eligibility). Conditional payments occur in the process of monitoring claims, so they give an indication of how strictly the work test is being enforced.

We examine three short-term employment outcomes for both quarter 1 following the claim quarter and for the benefit year (year 0): *earnings* received from all employers (unconditional on employment, so including $0), number of *hours worked* for those employers (again unconditional), and whether *employed* (indicating whether the claimant had positive earnings. Two other short-term employment outcomes we examine are *returned to same employer* and *returned to same industry*, which indicate whether the first post-claim employer (or industry) was the same as the primary employer (or employer) during pre-claim year − 1.

### 3.2. Long-term outcome variables

We focus on six long-term outcomes of interest. The first four are averages calculated over post-claim years 1–9 (that is, the nine years following the benefit year). *Average annual earnings* and *average annual hours* are annual averages of the worker's earnings and work hours over post-claim years 1–9. *Average employment probability* is the proportion of post-claim years 1–9 in which the claimant had any earnings. *Average log wage rate* (*conditional on employment*) is the log of the annual wage rate (annual earnings divided by annual hours, conditional on positive earnings) averaged over the nine post-claim years. Of the long-term outcomes, the log wage rate is the only one that is conditional on an outcome (employment) and hence may be subject to dynamic selection. (That is, if the treatment affects employment, we would be comparing self-selected samples if we compared the wages of employed workers in the treatment group with employed controls.) Note that these variables are averaged over post-claim years 1–9, so the benefit year is dropped from the long-term outcomes.[12]

A fifth long-term outcome we construct is *quarters until reemployment*—the number of consecutive quarters (following the claim quarter) in which a claimant is observed without earnings. This variable allows us to examine whether the work test treatments influenced the time to reemployment beyond what we can infer from UI claim duration, which measures only the duration of *insured* unemployment. If the claimant was not reemployed during the 40 quarters following the claim quarter, we define that claimant as having 40 quarters of nonemployment (4% of the WAWS sample is censored in this way).

A sixth long-term outcome is *tenure with first post-claim employer*, which is a proxy for match quality. For each claimant, we compute the number of quarters (following the claim quarter) in which a claimant

[10] The differences between the standard and modified work test groups in receipt of the six "other services" are not statistically significant.

[11] The presence of the principal employer's identity is the key difference between the WAWS experimental data (in which the employer's identity does not appear) and the follow-up administrative wage records (in which it does). Because the follow-up wage records available to us begin in the first quarter of 1987, we do not have data on the employer's identity for post-claim quarter 5 for WAWS claimants from the third quarter (July, August, and September) of 1986. Due to this data limitation, we use a smaller sample than Johnson and Klepinger, as described in the Data Appendix (Lachowska et al., 2015).

[12] We also examine the difference between the average log wage rate in the pre-claim years (that is, the log of the wage rate averaged over the three pre-claim years) and the average log wage in the post claim years (just defined in the text). This outcome appears as "Δ Average log wage rate (conditional on employment)" in the tables. Estimated treatment effects for this differenced outcome are similar to those for the long-term average log wage, as discussed in Section 6.2.

**Table 2**
Sample descriptive statistics and tests of differences between groups.
Source: Authors' tabulations of the Washington Alternative Work Search experimental data.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Sample proportions and means | | | *p*-value for test of difference between: | | |
| Covariate | No work test | Standard work test | Modified work test | (1) and (2) | (1) and (3) | (2) and (3) |
| *Demographics* | | | | | | |
| Male | 0.717 | 0.718 | 0.713 | 0.935 | 0.834 | 0.779 |
| Race | | | | | | |
| White | 0.828 | 0.819 | 0.829 | 0.487 | 0.931 | 0.479 |
| Black | 0.099 | 0.097 | 0.087 | 0.883 | 0.287 | 0.350 |
| Other | 0.073 | 0.084 | 0.084 | 0.256 | 0.302 | 0.996 |
| Age | | | | | | |
| ≤24 | 0.210 | 0.218 | 0.192 | 0.589 | 0.265 | 0.111 |
| 25–34 | 0.404 | 0.389 | 0.391 | 0.393 | 0.511 | 0.909 |
| 35–44 | 0.207 | 0.240 | 0.222 | **0.029** | 0.378 | 0.285 |
| 45–54 | 0.111 | 0.103 | 0.129 | 0.468 | 0.153 | **0.040** |
| ≥54 | 0.068 | 0.006 | 0.066 | **0.044** | 0.857 | 0.093 |
| Schooling | | | | | | |
| Less than high school | 0.123 | 0.159 | 0.148 | **0.004** | 0.064 | 0.471 |
| High school | 0.566 | 0.537 | 0.542 | 0.099 | 0.229 | 0.774 |
| Some college | 0.240 | 0.225 | 0.242 | 0.305 | 0.907 | 0.298 |
| College graduate | 0.071 | 0.080 | 0.067 | 0.336 | 0.706 | 0.220 |
| Veteran | 0.190 | 0.196 | 0.215 | 0.657 | 0.107 | 0.235 |
| Marital status/gender | | | | | | |
| Married male | 0.264 | 0.270 | 0.242 | 0.718 | 0.210 | 0.116 |
| Married female | 0.094 | 0.099 | 0.096 | 0.609 | 0.866 | 0.772 |
| Household status | | | | | | |
| No dependents | 0.329 | 0.309 | 0.322 | 0.213 | 0.669 | 0.485 |
| 1 dependent | 0.148 | 0.155 | 0.169 | 0.550 | 0.140 | 0.360 |
| 2 or more dependents | 0.229 | 0.236 | 0.207 | 0.622 | 0.190 | 0.081 |
| Homeowner | 0.285 | 0.286 | 0.253 | 0.933 | 0.078 | 0.067 |
| *Pre-claim observables* | | | | | | |
| Pre-claim earnings ($) | | | | | | |
| Pre-claim year −1 | 13,559 | 13,841 | 13,531 | 0.438 | 0.944 | 0.447 |
| Pre-claim year −2 | 11,571 | 11,900 | 11,639 | 0.416 | 0.880 | 0.563 |
| Pre-claim year −3 | 10,737 | 10,744 | 10,801 | 0.988 | 0.892 | 0.904 |
| Pre-claim hours | | | | | | |
| Pre-claim year −1 | 1313 | 1334 | 1286 | 0.375 | 0.317 | 0.073 |
| Pre-claim year −2 | 1064 | 1101 | 1076 | 0.176 | 0.690 | 0.414 |
| Pre-claim year −3 | 931 | 946 | 964 | 0.600 | 0.302 | 0.583 |
| Occupation | | | | | | |
| Professional | 0.102 | 0.105 | 0.106 | 0.773 | 0.732 | 0.936 |
| Clerical | 0.133 | 0.122 | 0.116 | 0.374 | 0.216 | 0.661 |
| Sales | 0.059 | 0.058 | 0.050 | 0.932 | 0.366 | 0.407 |
| Service | 0.101 | 0.101 | 0.123 | 0.989 | 0.069 | 0.073 |
| Agric., fishery, forestry | 0.028 | 0.026 | 0.021 | 0.719 | 0.291 | 0.455 |
| Processing | 0.033 | 0.038 | 0.035 | 0.419 | 0.741 | 0.698 |
| Machine trades | 0.090 | 0.086 | 0.107 | 0.707 | 0.126 | 0.066 |
| Benchwork | 0.048 | 0.046 | 0.049 | 0.812 | 0.863 | 0.700 |
| Structural work | 0.265 | 0.266 | 0.274 | 0.911 | 0.592 | 0.667 |
| Miscellaneous | 0.143 | 0.151 | 0.116 | 0.476 | 0.056 | **0.011** |
| Industry | | | | | | |
| Agriculture | 0.025 | 0.025 | 0.021 | 0.969 | 0.565 | 0.588 |
| Mining | 0.001 | 0.001 | 0.001 | 0.538 | 0.799 | 0.785 |
| Construction | 0.196 | 0.205 | 0.190 | 0.518 | 0.702 | 0.338 |
| Manufacturing | 0.232 | 0.237 | 0.263 | 0.780 | 0.070 | 0.125 |
| Transportation, utilities | 0.054 | 0.038 | 0.034 | **0.023** | **0.010** | 0.577 |
| Wholesale trade | 0.060 | 0.070 | 0.048 | 0.222 | 0.229 | **0.023** |
| Retail trade | 0.158 | 0.159 | 0.158 | 0.938 | 0.950 | 0.994 |
| Finance, ins., real estate | 0.031 | 0.028 | 0.031 | 0.599 | 0.955 | 0.674 |
| Services | 0.172 | 0.174 | 0.172 | 0.866 | 0.970 | 0.909 |
| Government | 0.057 | 0.045 | 0.054 | 0.137 | 0.712 | 0.318 |
| Unclassified | 0.014 | 0.018 | 0.027 | 0.426 | 0.018 | 0.129 |
| Prior UI claim | | | | | | |
| None | 0.804 | 0.804 | 0.791 | 0.969 | 0.423 | 0.408 |
| Duration ≤15 weeks | 0.100 | 0.104 | 0.106 | 0.689 | 0.581 | 0.852 |
| Duration >15 weeks | 0.097 | 0.092 | 0.103 | 0.642 | 0.606 | 0.353 |
| *Claim-related observables* | | | | | | |
| Reason for job loss | | | | | | |
| Permanent layoff | 0.153 | 0.172 | 0.157 | 0.147 | 0.815 | 0.291 |
| Temporary layoff with recall date | 0.265 | 0.231 | 0.253 | **0.028** | 0.514 | 0.179 |
| Contract/seasonal | 0.154 | 0.155 | 0.156 | 0.908 | 0.897 | 0.981 |
| Quit for good cause | 0.177 | 0.172 | 0.167 | 0.696 | 0.476 | 0.719 |

(continued on next page)

**Table 2** (*continued*)

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Sample proportions and means | | | *p*-value for test of difference between: | | |
| Covariate | No work test | Standard work test | Modified work test | (1) and (2) | (1) and (3) | (2) and (3) |
| *Claim-related observables* | | | | | | |
| Employer-attached/placed by union[a] | 0.286 | 0.355 | 0.371 | **0.000** | **0.000** | 0.418 |
| UI benefits/claim type | | | | | | |
| Weekly amount ($) | 145 | 146 | 145 | 0.566 | 0.963 | 0.640 |
| Maximum amount ($) | 3830 | 3868 | 3849 | 0.530 | 0.779 | 0.776 |
| Potential duration | 25.9 | 26.0 | 26.0 | 0.887 | 0.774 | 0.875 |
| Replacement rate (percent)[b] | 61.6 | 61.7 | 61.4 | 0.841 | 0.869 | 0.733 |
| Combined wage claim[c] | 0.044 | 0.049 | 0.045 | 0.490 | 0.889 | 0.635 |
| Ex-service member claim | 0.035 | 0.034 | 0.034 | 0.868 | 0.957 | 0.923 |
| Federal employee claim | 0.018 | 0.009 | 0.014 | **0.031** | 0.374 | 0.241 |
| Reservation wage (hourly) | | | | | | |
| ≤$5.00 | 0.181 | 0.190 | 0.175 | 0.477 | 0.725 | 0.325 |
| $5.01–$7.00 | 0.164 | 0.151 | 0.142 | 0.289 | 0.110 | 0.519 |
| $7.01–$10.00 | 0.161 | 0.138 | 0.157 | **0.066** | 0.740 | 0.180 |
| $10.01–$20.00 | 0.130 | 0.143 | 0.138 | 0.273 | 0.534 | 0.717 |
| >$20.00 | 0.106 | 0.110 | 0.117 | 0.722 | 0.376 | 0.581 |
| Sample size | 1606 | 1539 | 1073 | | | |

*Notes*: Sample consists of claimants in the no work test, standard work test, and modified work test groups during fall 1986, winter 1987, and spring 1987.
1. Bold denotes *p*-value <0.05 for test of mean difference between groups.
[a] 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.
[b] The replacement rate is the weekly benefit amount as a percentage of average weekly earnings before the UI claim.
[c] Combined wage claims use earnings from more than one state to calculate base period earnings.

is observed with earnings from the first principal post-claim employer. If the claimant is observed with the same employer throughout the 40 follow-up quarters, we assign a value of 40 to that claimant's employment tenure. (The variable is unconditional on finding employment during the follow-up period—if the claimant is not observed with any employer, we assign a value of zero.)[13] We observe 96% of WAWS claimants employed with at least one employer following the claim quarter.

### 3.3. Descriptive statistics and randomization

Table 2 displays various mean characteristics of the claimants in the three treatment groups, and the differences among them. In general, randomization appears to have been successful: excluding the "employer-attached" variable, which differs across the groups due to reporting issues (Johnson and Klepinger, 1991; Johnson and Klepinger, 1994), 11 of the 198 *p*-values shown (or 5.6%) are less than 0.05, which is about what we would expect. Also, if random assignment was successful, linear probability models that regress a treatment indicator on the covariates shown in Table 2 should return regression *F*-statistics with *p*-values greater than 0.05, which is the case. Although there are relatively few observable differences among the groups, the existing differences provide a rationale for using regression-adjustment to estimate treatment effects.

## 4. Theoretical background: UI and post-unemployment outcomes

Much of the empirical literature on post-unemployment outcomes frames its hypotheses within the basic job search model; see Rogerson et al. (2005). This model considers an individual job seeker who maximizes utility by weighing the current and future benefits of accepting a wage offer, arriving with probability $\alpha$, drawn at random from a known and stationary distribution of wage offers, $F(w)$. If an unemployed job seeker finds a suitable job, she receives $w$ per period until the match breaks up, which happens with probability $q$. If the unemployed job seeker does not find a suitable job, she receives unemployment benefits and continues to sample from $F(w)$ in hopes of drawing a higher $w$. The problem of the unemployed job seeker—whether to accept or reject a wage offer—can be expressed in terms of the reservation wage, $w_r$, the lowest wage acceptable to the job seeker. The reservation wage can be implicitly defined as $w_r = b + \frac{\alpha}{r+q}\int_{w_r}^{+\infty}(w - w_r)dF(w)$, where $b$ is the value of remaining unemployed (the sum of UI benefits and the value of leisure), and $r$ is the discount rate. In this setup, the exit rate of unemployment can be defined as $\theta = \alpha[1 - F(w_r)]$.

The basic job search model allows us to derive implications about the effects of UI and the UI work test on post-unemployment outcomes. Comparative statics using the two equations above show that UI effectively raises the worker's reservation wage, $\partial w_r/\partial b > 0$, and increases the expected duration of unemployment, $\partial\theta/\partial b < 0$. The resulting increase in the reservation wage also increases the expected post-unemployment wage.

What are the implications of the work test in the basic job search model? On one hand, introducing a work test increases job search monitoring and the risk of sanctions, making it more difficult to receive UI benefits and lowering the value of unemployment, $b$. Accordingly, the work test reduces the reservation wage and increases the exit rate from unemployment. Hence, in the process of reducing the moral hazard associated with UI (i.e., countering UI's incentive to take longer to become reemployed) the work test may reduce UI's effectiveness as a job search subsidy (Burdett, 1979) and lead to lower expected post-unemployment earnings.[14]

On the other hand, a work test with reemployment services may improve a job seeker's wage offer arrival rate, $\alpha$. For example, van den Berg and van der Klaauw (2006) model the counseling aspect of the work test as an increase in the wage offer arrival rate facing job seekers. Comparative statics show that an increase in the offer

[13] Both quarters to first post-claim reemployment and job tenure are right-censored variables, so in addition to estimating treatment effects using Eq. (1), we estimate treatment effects on these outcomes using Weibull accelerated failure-time models. See Section 5.2.

[14] If the model is extended to allow for endogenous search effort, a decrease in $b$ will decrease the reservation wage and increase job search effort, which in turn unambiguously decreases the exit rate. The effect of a decrease in $b$ on expected post-unemployment earnings will depend on both the decrease in the reservation wage and the increase in search effort; see Cahuc et al. (2014).

arrival rate will increase the job seeker's reservation wage, $\partial w_r/\partial\alpha > 0$, which in turn leads to higher expected post-unemployment earnings. However, the effect of an increase in $\alpha$ on the exit rate from unemployment, $\theta$, is ambiguous: it could either increase the exit rate, or decrease it by inducing job seekers to become more selective about accepting wage offers (van den Berg, 1994; van den Berg and van der Klaauw, 2006).[15]

Relaxing the assumptions of the basic job search model can lead to different predictions about the effects of UI and the work test on post-unemployment outcomes. This is clear in the models developed by Marimon and Zilibotti (1999), van den Berg and van der Klaauw (2006, 2013), Toohey (2014), and Nekoei and Weber (2015a), as well as in the less formal discussions of Fredriksson and Holmlund (2006); Arni et al. (2013), and Tatsiramos and van Ours (2014). For example, one can relax the basic model's assumption of a stationary wage offer distribution, which could shift and become less favorable as the spell of unemployment lengthens if employers view unemployment duration as a quality signal, or if skills depreciate with unemployment.[16] In this case, the work test would result in a higher post-unemployment wage because job seekers would be sampling from the relatively favorable wage-offer distribution they face early in their unemployment spell. Ultimately, then, the effect of the work test on post-unemployment outcomes is an empirical question.

The basic job search model also assumes homogeneous job seekers, and hence a homogeneous response to policies like the work test. But in practice, low-wage workers with less-specialized skills are more likely to be affected by the work test (that is, to find a job sooner as a result of the work test) than are high-wage workers whose skills are more specialized. That is, at least in a non-recessionary labor market, the suitable work requirements of low-wage workers are easier to meet because a wider range of jobs is likely to be "suitable." In contrast, high-wage workers with specialized skills face a relatively narrow set of job possibilities, and as a result, the work test is less likely to have an effect on their time to reemployment or post-unemployment wage.

## 5. Estimation methods

### 5.1. Effects of the work test

The approach to estimation relies on random assignment to the three treatments. We estimate the intent-to-treat effects of the standard and modified work test (compared with no work test) on both short- and long-term outcomes by estimating linear models of the form:

$$y_{it} = \beta_0 + \beta_1 SWT_i + \beta_2 MWT_i + X_i\lambda + u_{it}, \quad (1)$$

where $y_{it}$ is an outcome for individual $i$ in year $t$ following enrollment in the experiment, $SWT_i$ indicates assignment to the standard work test group, and $MWT_i$ indicates assignment to the modified work test group. (The reference treatment is no work test.) $X_i$ includes all variables listed in Table 2, plus the unemployment rate in the county where the claim was filed and indicators for the quarter the individual claimed benefits.

The identifying assumption is that assignment to treatment is independent of any individual characteristics, including unobservables: $E(u|SWT, MWT, X) = 0$. This assumption is reasonable because the randomization appears to have been successful. In this case, the OLS estimators of $\beta_1$ and $\beta_2$ are consistent for the intent-to-treat effect on outcome $y$. Including the demographic variables ($X$) reduces sampling variation and controls for the few observable differences between treatment and control groups noted earlier.

### 5.2. Identifying the work test effect on long-term outcomes

For most of the long-term employment outcomes we examine—employment, earnings and hours worked (unconditional on employment), and tenure with the first post-claim employer, the OLS estimators of $\beta_1$ and $\beta_2$ are unbiased for the intent-to-treat effect of the work test on $y$. This is because none of these outcomes is conditioned on a post-treatment outcome; we compare all claimants assigned at baseline to a treatment group with all claimants assigned at baseline to the control group.

However, one outcome—the hourly wage rate—is observed only if the claimant is employed (that is, it is necessarily conditional on employment), so we face a problem of dynamic selection if the treatment affected employment. If dynamic selection exists, the treatment effect on wages cannot be estimated without further assumptions. In subsection 6.1, we describe and implement a form of pre-treatment outcome test (Heckman and Hotz, 1989) to check whether the selection of claimants into employment after the UI claim differed by treatment. As will be seen, these tests do detect the presence of such selection.

A second potential issue in estimating long-term effects pertains to the censoring of long-term employment outcomes—in particular, quarters until first post-claim reemployment and tenure with the first post-claim employer. The tenure variable in particular may be a problem because claimants assigned to the work test groups exited UI earlier than claimants assigned to the no work test group, and we can observe post-claim employment outcomes only for a fixed number of quarters after the claim (the data are right-censored at 40 quarters). As a result, there may be a mechanical negative correlation between unemployment duration and post-claim tenure.[17] For three reasons, this appears to be a minor concern. First, we have 40 quarters post-claim data, and 96% of the sample is employed at least once during this period, so censoring of quarters to reemployment applies to only 4% of the sample. Also, only 4% of claimants in the sample were reemployed in a job with tenure as long as 40 quarters, and the average post-unemployment tenure was 8.1 quarters, so censoring of the tenure variable only applies to a small fraction of the sample. This is quite similar to the situation in Dolton and O'Neill (2002, pp. 394 − 395). Second, the work test reduced UI duration by about 3 weeks, so for the job tenure variable, the mechanical "advantage" experienced by claimants in the standard and modified work test groups is minor in comparison to the length of the follow-up period. Third, estimates from Weibull accelerated failure-time models, which we describe below, yield estimates that are qualitatively very similar to those obtained by OLS (see Section 6.2).

### 5.3. Effects of the work test by reason for job loss

The effects of the work test on employment outcomes may differ across workers who are unemployed for different reasons. Accordingly, we estimate separate models for three mutually exclusive groups: permanent job losers (16.1% of the overall sample); workers who quit for reasons considered "good cause" (17.3% of the overall sample); and workers temporarily laid off (24.9% of the overall sample).[18] In each

[15] In a model with endogenous search effort, an increase in the offer arrival rate will increase both the reservation wage and job search effort. The result is an ambiguous effect on the exit rate and an increase in expected post-unemployment earnings.

[16] Another possibility is that the wage-offer distribution is narrow or even degenerate, as in trade-frictions models.

[17] For example, if claimants in all treatment groups found jobs lasting ten or more years, but claimants in the no work test group took longer to find these jobs, we could erroneously conclude that the no work test group found jobs with shorter tenure due to the treatment, when in fact this "effect" would be the result of right censoring at 40 quarters.

[18] In the Appendix, we present results for the remaining two categories: unemployed due to seasonal layoff (15.5%) and unemployed for unknown reasons (26.2%).

**Table 3**
Estimated effects of the standard and modified work tests on selected outcomes, all claimants.

| | No work test | | OLS estimated effects (robust std. error) | | | |
|---|---|---|---|---|---|---|
| Outcome | Mean (std. dev.) | | Standard work test | | Modified work test | |
| *UI outcomes* | | | | | | |
| Benefit paid ($) | 2411 | (1797) | −429*** | (53) | −484*** | (59) |
| Weeks paid | 17.56 | (10.32) | −3.28*** | (0.35) | −3.24*** | (0.39) |
| Exhausted benefits (proportion) | 0.354 | (0.478) | −0.116*** | (0.015) | −0.111*** | (0.017) |
| Number of conditional payments (incl. zeros) | 0.518 | (1.47) | 0.313*** | (0.054) | 0.391*** | (0.061) |
| *Short-term employment outcomes (unconditional)* | | | | | | |
| Quarter 1 outcomes[a] | | | | | | |
| Earnings ($) | 2123 | (2549) | 86 | (81) | 80 | (85) |
| Hours worked | 189 | (205) | 6.2 | (7.0) | 8.9 | (7.6) |
| Employed (proportion) | 0.653 | (0.476) | 0.028* | (0.016) | 0.038** | (0.018) |
| Benefit year (year 0) outcomes[a] | | | | | | |
| Earnings ($) | 11,064 | (9976) | 320 | (289) | 216 | (307) |
| Hours worked | 974 | (745) | 25.6 | (24.6) | 33.9 | (26.7) |
| Employed (proportion) | 0.867 | (0.34) | 0.016 | (0.011) | 0.025** | (0.012) |
| Other outcomes | | | | | | |
| Returned to same employer (proportion) | 0.342 | (0.475) | −0.033** | (0.015) | −0.022 | (0.017) |
| Returned to same industry (proportion) | 0.448 | (0.497) | −0.029* | (0.016) | −0.004 | (0.018) |
| *Employment outcomes over post-claim years 1–9 (unconditional)* | | | | | | |
| Average annual earnings ($)[b] | 12,659 | (11,124) | −18 | (340) | −441 | (372) |
| Average annual hours[b] | 1009 | (737) | −4.8 | (24.0) | −13.1 | (27.0) |
| Average employment probability[b] | 0.711 | (0.353) | 0.009 | (0.012) | −0.001 | (0.013) |
| Average log wage rate (conditional on employment) | 2.472 | (0.408) | −0.022 | (0.017) | −0.057*** | (0.019) |
| Δ Average log wage rate (conditional on employment) | 0.208 | (0.385) | −0.015 | (0.019) | −0.036* | (0.021) |
| Quarters until reemployment | 3.83 | (8.27) | −0.102 | (0.279) | −0.138 | (0.323) |
| Tenure with first post-claim employer (quarters) | 8.10 | (10.96) | −0.428 | (0.368) | 0.057 | (0.420) |
| Sample size | 1606 | | 1539 | | 1073 | |

*Notes*: The sample consists of claimants in the no work test, standard work test, and modified work test groups during fall 1986, winter 1987, and spring 1987. Estimated effects are regression-adjusted differences controlling 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. See Eq. (1).
*p*-values are for tests of the difference between the standard (or modified) work test group and the no work test group.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.
[a] "Quarter 1" refers to the quarter following the quarter in which the UI claim was filed. "Benefit year" (year 0) is the sum of the four quarters following the claim quarter. See Fig. 1.
[b] Averages are taken over post-claim years 1–9 (see Fig. 1). Earnings are expressed in 1988:4 US dollars.

case, we estimate Eq. (1), comparing those in the standard and modified work test groups with those in no work test group. Reason for job loss is pre-determined with respect to treatment assignment, so again, $\beta_1$ and $\beta_2$ are unbiased for the intent-to-treat effects of the standard and modified work tests for a given reason-for-job-loss category.[19]

### 5.4. Threats to validity

Spillover effects have long been recognized as a potential threat to the internal validity of random-assignment experiments—see, for example, Spiegelman and Woodbury (1990) for a discussion in the context of UI. If a treatment indirectly affects the outcomes of the control group, then the treatment-control comparison is clearly compromised. In the case of the WAWS experiment, if the search intensities of the standard and modified work test groups increased so as to "crowd out" the job opportunities of the no work test group, the estimator we use would overstate the effect of the work test on UI durations.

For example, one experiment where young job-seekers in France were offered career counseling found that improvements in employment outcomes of the treatment group were due mainly to worse employment outcomes of the controls (Crépon et al., 2013). However, this spillover effect may have occurred because counselors were paid about $2000 for placing a worker, creating an incentive to "push" job-seekers assigned to the treatment at the expense of job-seekers in the control group. No such incentives existed in the WAWS experiment.

Lalive et al. (2015) also find evidence of substantial externalities in analyzing an Austrian natural experiment that lengthened the potential of UI benefits and had the effect of increasing the average UI duration by about 43 weeks. This in turn reduced the UI duration of a comparison group by nearly 9 weeks. This is a substantial effect, although it implies fairly small spillovers in the context of the WAWS experiment: the WAWS work tests reduced UI durations by about 3 weeks, which would imply a 0.7-week increase in the UI duration of controls.

Studies of crowd-out associated with reemployment bonuses and ES referral and placement activities in the United States have concluded that neither is likely to result in crowd-out, partly because in a general equilibrium setting, such activities may lead to job creation, and partly because the proportion of all seekers who use these incentives and services is small (Davidson and Woodbury, 1993, 2000). Clearly, we cannot rule out spillover effects as a possible reason for the size of the estimated effects in the WAWS experiment, but the available evidence suggests that, in the case of the WAWS experiment, spillovers seem likely to be small.

External validity will be compromised if an experiment cannot be generalized from the population, setting, or scale studied in an experiment. Regarding the WAWS experiment, Washington State does not appear to be an outlier with respect to the characteristics

[19] In an attempt to improve efficiency, we have also estimated a model over the entire sample, fully interacting the work test treatment indicators and the reason-for-job loss indicators. The point estimates and inferences drawn about treatment effects for each reason for job loss are very similar to those obtained from the separate reason-for-job-loss subsamples. The estimates are available from the authors.

of its population or industrial mix. Also, as Johnson and Klepinger (1994) note, the work test implemented in Washington at the time of the experiment was similar to that in most other states at that time, although as we discuss in the conclusion (Section 7) it seems likely that enforcement of the work test has changed in the years since the WAWS experiment. Also, the average unemployment rate in Tacoma, the site of the WAWS experiment, was about 8% at the time of the experiment. It follows that the estimated effects pertain to relatively slack labor markets.

## 6. Results of estimation

### 6.1. Short-term effects of the work test and pre-treatment outcome tests of selection into reemployment

Table 3 displays results from estimating Eq. (1) over the full sample. The first two columns show the mean and standard deviation of each outcome for the no work test group. Subsequent columns show estimated effects of the standard and modified work tests (and standard errors of those estimated effects) relative to the no work test group.

The short-term treatment effects complement the long-term estimates, so the top two panels of Table 3 show estimated treatment effects on UI outcomes and short-term employment outcomes. Like Johnson and Klepinger (1994), we find that claimants in the standard and modified work test groups received less in UI benefits (by about $430–$480 during the benefit year, on a base of $2411), claimed benefits for a shorter time (by about 3 weeks, on a base of about 17 weeks), and were less likely to exhaust their benefits (by about 11 percentage points) than claimants not subject to the work test. Consistent with these UI outcomes, claimants in both work test groups had a higher probability of employment in the first post-claim quarter (by 3–4 percentage points), and the modified work test group had a higher employment probability over the benefit year (by 2.5 percentage points).

Given that the standard and modified work test groups had shorter UI spells and increased employment probabilities, we would expect their (unconditional) earnings and work hours to be higher following treatment. But in fact, the earnings and work hours of the standard and modified work test groups, both in the first post-claim quarter and over the benefit year, are statistically indistinguishable from the no work test group.

An explanation of this finding (shortened UI spells and higher employment probability without an increase in hours or earnings) is that claimants in the standard and modified work test groups tended to be selected into reemployment by the work test treatments; that is, lower-wage workers found jobs sooner when they were in the work test groups than when they were in the no work test group. Three pieces of evidence support this explanation. First, we observe the wage rates of claimants for three years *before* the experiment, so we can perform pre-treatment outcome tests (Heckman and Hotz, 1989) to check whether the selection of claimants into employment after the UI claim differed by treatment. The question here is, did the average pre-claim wage rates of *reemployed* claimants in the standard and modified work test groups differ from the pre-claim wage rates of *reemployed* claimants in the no work test group? If they were less, we would infer that the work test selected lower-wage workers into reemployment—that is, claimants in the work test groups who earned lower wage rates before the treatment were disproportionately affected by the work test, and were relatively more likely to be employed after the UI spell.

The first column of Table 4 (headed "Year 0") displays estimates of $\alpha_1$ and $\alpha_2$ from the following model of the log hourly wage rate in each of the three years *before* the experiment [$log(wage_{i-t})$], estimated over the sample of claimants who were employed at some time during the benefit year (Year 0):

$$log(wage_{i-t}) = \alpha_0 + \alpha_1 SWT_i + \alpha_2 MWT_i + X_i\theta + u_{it}. \quad (2)$$

The reference treatment is no work test, and $X_i$ includes all variables listed in the footnote to Table 3 except for pre-claim earnings and pre-claim hours.

The "Year 0" estimates in Table 4 provide evidence that, compared with reemployed claimants in the no work test group, reemployed

**Table 4**
Pre-treatment outcome tests of selection into reemployment based on Eq. (2): estimated differences between pre-claim wage rates of the standard (or modified) work test group and the no work test group, conditional on post-claim reemployment (robust standard errors in parentheses).

| | Year 0 | Year 1 | Year 2 | Year 3 | Year 4 | Year 5 | Year 6 | Year 7 | Year 8 | Year 9 |
|---|---|---|---|---|---|---|---|---|---|---|
| *Panel A: Outcome variable is log(wage in year –3), sample conditional on employment in year:* | | | | | | | | | | |
| Standard work test | −0.032** | −0.037** | −0.035** | −0.038*** | −0.031** | −0.029* | −0.028* | −0.038** | −0.045*** | −0.045*** |
| | (0.014) | (0.015) | (0.014) | (0.015) | (0.015) | (0.016) | (0.016) | (0.016) | (0.016) | (0.016) |
| Modified work test | −0.032** | −0.041** | −0.037** | −0.032** | −0.040** | −0.032* | −0.023 | −0.044** | −0.051*** | −0.052*** |
| | (0.016) | (0.016) | (0.016) | (0.016) | (0.017) | (0.018) | (0.017) | (0.018) | (0.018) | (0.018) |
| Sample size | 2939 | 2820 | 2732 | 2619 | 2507 | 2424 | 2336 | 2256 | 2172 | 2130 |
| *Panel B: Outcome variable is log(wage in year –2), sample conditional on employment in year:* | | | | | | | | | | |
| Standard work test | −0.029** | −0.025* | −0.025* | −0.024* | −0.023* | −0.014 | −0.012 | −0.019 | −0.026* | −0.024* |
| | (0.012) | 0.006 | (0.013) | (0.013) | (0.013) | (0.013) | (0.014) | (0.014) | (0.014) | (0.014) |
| Modified work test | −0.012 | −0.021 | −0.021 | −0.01 | −0.009 | −0.002 | 0 | −0.005 | −0.015 | −0.016 |
| | (0.014) | (0.014) | (0.015) | (0.014) | (0.015) | (0.015) | (0.016) | (0.016) | (0.016) | (0.016) |
| Sample size | 3244 | 3102 | 2997 | 2863 | 2743 | 2642 | 2540 | 2462 | 2366 | 2321 |
| *Panel C: Outcome variable is log(wage in year –1), sample conditional on employment in year:* | | | | | | | | | | |
| Standard work test | −0.014 | −0.01 | −0.006 | −0.005 | −0.009 | −0.004 | 0.003 | −0.003 | −0.011 | −0.008 |
| | (0.010) | (0.010) | (0.010) | (0.009) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) |
| Modified work test | −0.002 | −0.007 | −0.004 | 0.004 | 0.004 | 0.002 | 0.002 | 0.004 | −0.002 | −0.003 |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.012) | (0.012) |
| Sample size | 3487 | 3322 | 3184 | 3043 | 2915 | 2804 | 2692 | 2605 | 2504 | 2460 |

*Notes*: Each coefficient is the regression-adjusted difference between the log pre-claim wage rate of claimants in the work test groups (standard or modified) who were reemployed and claimants in the no work test group who were reemployed in the year indicated. See Eq. (2). Other covariates include the variables listed in the notes to Table 3 (except for pre-claim earnings and pre-claim hours).
*p*-values are for tests of the difference between the standard (or modified) work test group and the no work test group.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

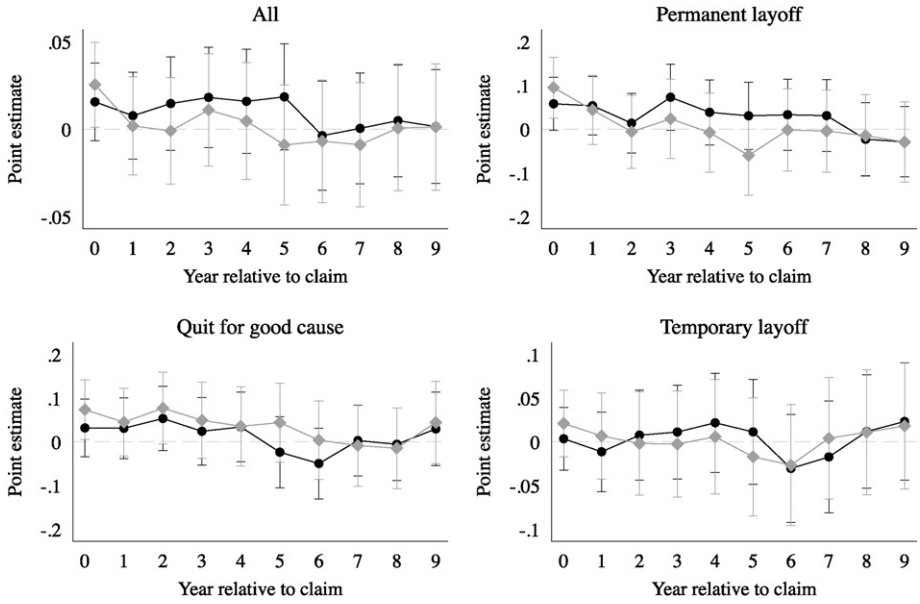


**Fig. 2.** Estimated effects of standard work test and modified work test on employment (proportion), by year and reason for job loss *Notes*: The black circles represent the estimated effect of the standard work test and the gray diamonds represent the estimated effect of the modified work test. The vertical whiskers are 95% confidence intervals. Year 0 is the benefit year; see notes to Table 3.

claimants in the standard and modified work test groups earned lower hourly wage rates *before* the WAWS experiment. For example, three years before the experiment, the average wage rates of claimants in both the standard and modified work test groups who were employed at some time during the benefit year (year 0) were about 3% lower than the average wage rates of claimants in the no work test group (who were also employed at some time during the benefit year). The evidence is stronger for the claimants in the standard work test group than for those in the modified work test group, and the differences are greater for wage rates two and three years before the experiment than for wage rates in the year before the claim. (The latter suggests that more recent labor force entrants were less likely to be affected by the work test treatments.)

The evidence in Table 4 suggests that, rather than pushing UI claimants to accept poorer job matches than otherwise, *the work test treatments altered the composition of reemployed workers by having a relatively strong effect on lower-wage workers*. That is, lower-wage workers accepted jobs earlier if they were subject to the work test than if they were not.

A second piece of evidence consistent with the work test selecting lower-wage workers into post-claim employment pertains to the likelihood of returning to a previous employer. Specifically, claimants in the standard work test group were about 3 percentage points less likely to return to a previous employer, and hence were less likely to reestablish a prior job match. (Similarly, the claimants in the modified work test group were about 2 percentage points less likely to return to a former employer, although this point estimate is imprecise. Note that these two point estimates are not statistically different from one another.) It is well-known that recalled workers tend to earn higher wage rates than not-recalled workers (Nekoei and Weber, 2015b).

Third, because the average effect of a variable $x_i \in X$ on unconditional earnings $y$ can be written as $E(y|X) = \Pr(y > 0|X) \times E(y|y > 0, X)$, the treatment effect of $x_i$ on unconditional earnings can be written as:

$$dE(y|X)/dx_i = d\Pr(y{>}0|X)/dx_i \times E(y|y{>}0, X) + \Pr(y{>}0|X) \times dE(y|y{>}0, X)/dx_i. \quad (3)$$

The terms on the right-hand-side of Eq. (3) denote (a) the treatment effect of $x_i$ on the probability of positive earnings [i.e., employment, $d\Pr(y > 0|X)/dx_i$], (b) mean earnings, conditional on reemployment [$E(y|y > 0, X$], (c) the probability of employment [$\Pr(y > 0|X)$], and (d) the treatment effect of $x_i$ on earnings, conditional on positive earnings [or employment, $dE(y|y > 0, X)/dx_i$]. We can either observe or obtain consistent estimates of all the terms in Eq. (3) except the last (the conditional treatment effect, due to dynamic selection). For a case in which $dE(y|X)/dx_i$ is not statistically different from zero, but the terms (a), (b), and (c) are positive, the remaining term, $dE(y|y > 0, X)/dx_i$ must be negative. This again suggests that claimants in the work test groups who became reemployed were more likely to be lower-wage workers.[20]

The first panel of Table 3 also shows that claimants in the standard and modified work test groups were more likely than those in the no work test group to have at least one conditional payment. This suggests that monitoring and enforcement of the work test really was more stringent in the work test groups. The preceding discussion suggests that lower-wage workers in the standard and modified work test groups responded to this monitoring and found reemployment earlier than otherwise. Again, it does not appear that the additional monitoring pushed claimants to accept less favorable jobs than they would otherwise have accepted.

### 6.2. Long-term effects of the work test averaged over all workers

The third panel of Table 3 ["Employment outcomes over years 1–9 (unconditional)"] reports the estimated effects of the standard and modified work tests for all groups of workers, averaged over years 1–9 following the benefit year. Most of these estimates are imprecise, suggesting that the work test treatments had no long-term effect on average annual earnings, average annual hours, or average employment probability. The exception is the estimated "effect" of the modified work test on log wage rates, which is negative and significant, but here we need to consider the potential endogeneity problems arising from conditioning on an outcome (dynamic selection). We consider this issue below.

[20] We are grateful to Henry Farber for suggesting this way of handling "conditional-on-positive" estimates. See also Angrist and Pischke (2009, pp. 98–102).

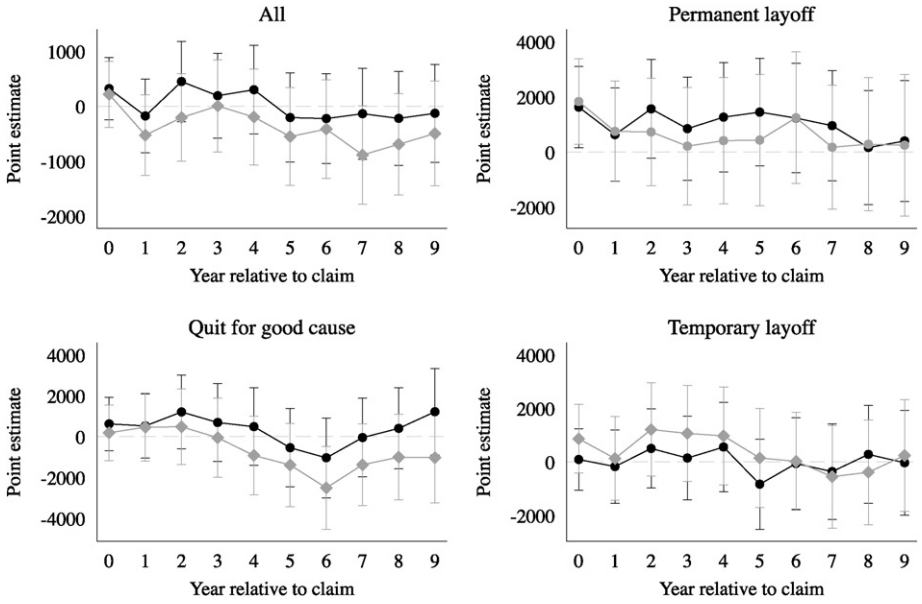


**Fig. 3.** Estimated effects of standard work test and modified work test on (unconditional) earnings, by year and reason for job loss *Notes*: The black circles represent the estimated effect of the standard work test and the gray diamonds represent the estimated effect of the modified work test. The vertical whiskers are 95% confidence intervals. Year 0 is the benefit year; see notes to Table 3.

Figs. 2, 3 and 4 show year-by-year estimated treatment effects of the standard and modified work tests on the probability of employment (Fig. 2), unconditional earnings (Fig. 3), and log wage rates (Fig. 4). Each figure shows these effects for the full sample (labeled "All") and for three subgroups of claimants (which we consider in the next section). The top left graph in Fig. 2 ("All") shows that the modified work test had a statistically significant effect on employment only in the benefit year not in years 1–9, and the standard work test never had a statistically significant effect on employment.[21] The top left graph in Fig. 3 ("All") shows that neither work test ever had a statistically significant effect on earnings, either in the benefit year or in any subsequent year. Overall, then, it seems reasonable to conclude that the long-term effects of the work test on employment and earnings were negligible. That is, in the long-term, the work test treatments neither helped nor harmed the employment or earnings of claimants.

What about wage rates? If we believed there were no compositional differences among the three treatment groups (an important caveat to which we return), then the finding that the standard and modified work tests had no effect on employment or earnings would allow us to obtain treatment effects on wages for years 1–9 in a straightforward way: by comparing the wage rates of the standard and modified work test groups with the wage rates of the no work test group.[22] As Table 3 (bottom panel) shows, the standard work test did not have a statistically significant effect on long-term wage rates (although the point estimate suggests a 2% decrease), but the modified work test reduced the average wage rate by more than 5% over the long term.[23] Fig. 4 shows that most of this latter negative "effect" (correctly, an observed wage-rate differential) did not appear until Year 5, although point estimates for the preceding years were negative (if imprecise).

The question is whether these apparently negative effects of the modified work test on wage rates are treatment effects, compositional effects, or noise. Our preferred interpretation is that they are either compositional effects or noise. We have already laid the groundwork for the compositional interpretation: the evidence suggests that the work test treatments selected lower-wage UI claimants into reemployment in year 0—see the first column of Table 4. Further evidence is shown in Table 4, in the columns labeled "Year 1" through "Year 9." These estimates show that claimants from both the standard and modified work test groups who were reemployed in years 1–9 tended to be lower-wage workers; that is, they had wages that were 3–5% lower than claimants in the no work test group three years before the WAWS experiment. The evidence continues to be stronger for claimants in the standard work test group, and the differences are again largest for earnings two and three years before the experiment. But overall, they are consistent with the view that, rather than pushing UI claimants to accept worse jobs, the work test treatments changed the composition of reemployed UI claimants.[24]

Table 3 also shows long-term treatment effects on quarters until first reemployment and tenure (in quarters) with the first post-claim employer. The work test treatments had no statistically significant effect on either of these outcomes.[25] Because quarters until first reemployment and job tenure are both right-censored (as discussed in Sections 3.2 and 5.2), Table A.3 shows the estimates of $\beta_1$ and $\beta_2$ from Weibull accelerated-failure time models for the same outcomes. For both, the estimated treatment effects are again small and statistically insignificant

[21] The estimated effect of the modified work test on earnings is negative and significant at the 10% level in year 7. Because we are estimating 36 treatment effects (9 years × 2 treatments × 2 outcomes), it would not be surprising to obtain three or four estimates with *p*-values of 0.10, even in the absence of any treatment effect.

[22] We are grateful to Arash Nekoei for pointing this out.

[23] Estimated treatment effects for the difference between the average log wage rate in the pre-claim years and the average log wage in the post claim years are somewhat smaller, but are similar to those for the long-term average log wage, presumably because the latter estimates are conditioned on pre-treatment earnings and hours worked.

[24] One could also make a case that the estimated wage "effects" are largely noise. Although it is difficult to ignore 5 estimates out of 20 with *p*-values ≥0.10, it is hardly overwhelming evidence.

[25] Column (1) in Table 3 shows that, on average, claimants spent about eight quarters with their first employer (standard deviation ≈ 11 quarters). This is comparable to the estimates in Centeno (2004), who uses data from the NLSY79 from 1979 until 1998 (a period spanning the time we are examining, 1986–1996). Centeno reports that the average post-unemployment job tenure in the NLSY79 is about 95 weeks (standard deviation ≈ 125 weeks), which, converted to quarters, equals about 8 quarters (standard deviation ≈ 10.4 quarters).

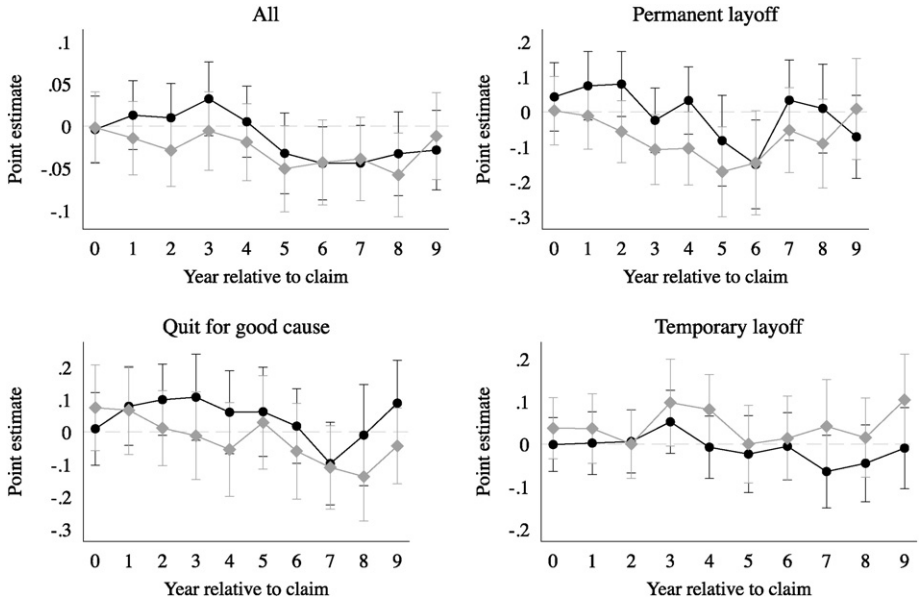


**Fig. 4.** Estimated effects of standard work test and modified work test on log wage rates (conditional on employment), by year and reason for job loss *Notes*: The black circles represent the estimated effect of the standard work test and the gray diamonds represent the estimated effect of the modified work test. The vertical whiskers are 95% confidence intervals. Year 0 is the benefit year; see notes to Table 3.

(see the column labeled "All"). Also, Figs. A.1 and A.2 plot the Kaplan–Meier survivor functions of these two outcomes for each of the three groups: no work test, standard work test, and the modified work test. The survivor functions give no indication of differences between the work test and no work test groups.

To summarize, most of the evidence in Table 3 points to little if any effect of the work test on long-term outcomes. The work test treatments had virtually no effect on earnings, hours, employment, quarters until reemployment, or job tenure. The work test treatments appear to have selected lower-wage UI claimants into reemployment, so that observed hourly wage rates in the years following the experiment were lower for the work test groups than for the no work test group, but overall, the estimates seem counter to the hypothesis that the work test had a negative effect on UI claimants' job match quality.

### 6.3. Effects of the work test by reason for job loss

We are also interested in the effects of the work test for subgroups of claimants who lost their job for different reasons. Tables 5–7 report

**Table 5**
Estimated effects of the standard and modified work tests on selected outcomes, permanent job losers.

| | No work test | | OLS estimated effects (robust std. error) | | | |
|---|---|---|---|---|---|---|
| Outcome | Mean (std. dev.) | | Standard work test | | Modified work test | |
| *UI outcomes* | | | | | | |
| Benefits paid ($) | 2677 | (1759) | −443*** | (132) | −574*** | (152) |
| Weeks paid | 20.13 | (9.8) | −3.44*** | (0.91) | −3.96*** | (1.07) |
| Exhausted benefits (proportion) | 0.459 | (0.499) | −0.151*** | (0.043) | −0.168*** | (0.049) |
| Number of conditional payments (incl. zeros) | 0.272 | (0.941) | 0.637*** | (0.116) | 0.828*** | (0.152) |
| *Short-term employment outcomes (unconditional)* | | | | | | |
| Quarter 1 outcomes[a] | | | | | | |
| Earnings ($) | 1250 | (1840) | 454** | (187) | 437** | (205) |
| Hours worked | 138 | (186) | 31.1* | (18.3) | 30.3 | (20.1) |
| Employed (proportion) | 0.553 | (0.498) | 0.084* | (0.043) | 0.086* | (0.050) |
| Benefit year (year 0) outcomes[a] | | | | | | |
| Earnings ($) | 7819 | (8230) | 1626** | (751) | 1829** | (792) |
| Hours worked | 841 | (731) | 88.2 | (66.2) | 134.1* | (71.8) |
| Employed (proportion) | 0.821 | (0.384) | 0.058* | (0.031) | 0.095*** | (0.036) |
| Other outcomes | | | | | | |
| Returned to same employer (proportion) | 0.106 | (0.308) | 0.008 | (0.030) | 0.033 | (0.034) |
| Returned to same industry (proportion) | 0.211 | (0.409) | 0.008 | (0.039) | 0.106** | (0.047) |
| *Employment outcomes over post-claim years 1–9 (unconditional)* | | | | | | |
| Average annual earnings ($)[b] | 11,053 | (9847) | 943 | (827) | 497 | (979) |
| Average annual hours[b] | 974 | (733) | 33.9 | (60.8) | 11.5 | (73.0) |
| Average employment probability[b] | 0.699 | (0.362) | 0.025 | (0.030) | −0.006 | (0.035) |
| Average log wage rate (conditional on employment) | 2.378 | (0.412) | −0.022 | (0.047) | −0.079 | (0.053) |
| Δ Average log wage rate (conditional on employment) | 0.265 | (0.420) | −0.060 | (0.049) | −0.058 | (0.059) |
| Quarters until reemployment | 5.01 | (9.66) | −1.084 | (0.785) | −2.038** | (0.910) |
| Tenure with first post-claim employer (quarters) | 6.00 | (9.16) | 1.299 | (0.862) | 2.249** | (1.054) |
| Sample size | 246 | | 265 | | 168 | |

*Notes*: See Table 3.

**Table 6**
Estimated effects of the standard and modified work tests on selected outcomes, claimants who quit for good cause.

| | No work test | | OLS estimated effects (robust std. error) | | | |
|---|---|---|---|---|---|---|
| Outcome | Mean (std. dev.) | | Standard work test | | Modified work test | |
| *UI outcomes* | | | | | | |
| Benefits paid ($) | 2350 | (1728) | −532*** | (131) | −338** | (152) |
| Weeks paid | 18.22 | (10.28) | −4.64*** | (0.91) | −2.97*** | (1.07) |
| Exhausted benefits (proportion) | 0.407 | (0.492) | −0.137*** | (0.041) | −0.085* | (0.047) |
| Number of conditional payments (incl. zeros) | 0.498 | (1.30) | 0.367*** | (0.129) | 0.253* | (0.145) |
| *Short-term employment outcomes (unconditional)* | | | | | | |
| Quarter 1 outcomes[a] | | | | | | |
| Earnings ($) | 1383 | (1993) | 234 | (189) | 84 | (190) |
| Hours worked | 147 | (201) | 28.1 | (18.9) | −4.8 | (20.1) |
| Employed (proportion) | 0.54 | (0.499) | 0.054 | (0.043) | 0.054 | (0.049) |
| Benefit year (year 0) outcomes[a] | | | | | | |
| Earnings ($) | 7671 | (8761) | 620 | (665) | 186 | (697) |
| Hours worked | 783 | (748) | 97.7 | (68.8) | −6.5 | (72.0) |
| Employed (proportion) | 0.786 | (0.411) | 0.032 | (0.034) | 0.074** | (0.035) |
| Other outcomes | | | | | | |
| Returned to same employer (proportion) | 0.161 | (0.369) | −0.015 | (0.031) | 0.020 | (0.036) |
| Returned to same industry (proportion) | 0.263 | (0.441) | −0.000 | (0.038) | 0.069 | (0.045) |
| *Employment outcomes over post-claim years 1–9 (unconditional)* | | | | | | |
| Average annual earnings ($)[b] | 10,244 | (10,194) | 320 | (831) | −817 | (837) |
| Average annual hours[b] | 926 | (760) | −15.5 | (62.4) | −42.2 | (69.4) |
| Average employment probability[b] | 0.654 | (0.382) | 0.011 | (0.032) | 0.031 | (0.035) |
| Average log wage rate (conditional on employment) | 2.347 | (0.384) | 0.054 | (0.050) | −0.076 | (0.059) |
| Δ Average log wage rate (conditional on employment) | 0.193 | (0.369) | 0.111* | (0.065) | −0.026 | (0.081) |
| Quarters until reemployment | 5.67 | (10.66) | −0.118 | (0.870) | −1.229 | (0.884) |
| Tenure with first post-claim employer (quarters) | 5.59 | (8.77) | 0.605 | (0.740) | 1.186 | (0.945) |
| Sample size | 285 | | 265 | | 179 | |

*Notes*: See Table 3.

results from estimating Eq. (1) for permanent job losers, workers who quit for good cause, and workers temporarily laid off. For the first three UI outcomes (shown in the top panels of Tables 5–7), estimated effects of the standard and modified work tests are qualitatively similar to each other and to the overall estimated effects in Table 3: claimants assigned to the work test groups received less in UI benefits, claimed benefits for fewer weeks, and were less likely to exhaust their benefits than claimants not subject to the work test.[26] However, the treatment effects on conditional payments vary across the three groups: they are substantial for permanent job losers and those who quit, but minimal for claimants on temporary layoff (who had the highest baseline incidence of conditional payments among the three groups).

The effects of the standard and modified work tests on both short-term (quarter 1 and year 0) and long-term (years 1–9) employment outcomes vary substantially across the subgroups, as we now discuss. For permanent job losers (Table 5), the standard and modified work tests generally increased short-term unconditional earnings, work hours, and the probability of employment, compared with the no work test group. However, these improvements appear to be transitory, as long-term earnings, hours worked, and employment probabilities for the work test groups are statistically indistinguishable from the no work test group. (In some cases, however, the point estimates are economically substantial—see the bottom panel, "Employment outcomes over years 1–9").

The top right panels of Figs. 2 and 3 show year-by-year estimates of the work test treatment effects on long-term employment probabilities and earnings of permanent job losers. Although most of the point estimates are positive, most are also imprecise, suggesting that the improved short-term outcomes of permanent job losers in the work test groups do not translate into convincingly better long-term outcomes. But in light of the positive point estimates, they also suggest rather strongly that the work test does no long-term harm to permanent job losers.

But the improved short-term outcomes of permanent job losers in the modified work test groups do translate into fewer quarters until reemployment and longer tenure with the first post-claim employer (point estimates for the standard work test are in the same direction but are imprecise)—see the bottom two rows of Table 5. The former result suggests that a shorter duration of *insured* unemployment due to the work test implies a shorter duration of actual non-employment. The latter result suggests claimants subject to the work test obtained a better job match than claimants in the no work test group.

Because quarters to reemployment and job tenure are censored at 40 quarters, we have estimated Weibull accelerated-failure time models of Eq. (1) for each. The estimates of $\beta_1$ and $\beta_2$ (see Table A.3) show that the standard work test reduces the number of quarters until reemployment by about 22.6% [$=exp.(-0.257)-1$], and increased tenure with the first post-claim employer by about 32.3% [$=exp.(0.280)-1$]. Also, the modified work test reduced time to reemployment by about 34.3%, and increased tenure with the first post-claim employer by about 35.5. These estimates are similar to the OLS estimates in Table 5.[27]

The important points here are that (a) the short-term employment outcomes of permanent job losers were improved by both work test treatments, (b) there is no evidence that permanent job losers were harmed in the long run by either form of the work test, and (c) although the estimates are sometimes imprecise due to small sample size, permanent job losers appear to have returned to work more quickly and had longer job tenure with their first post-claim employer

[26] Note that the baseline averages for the no work test group differ depending on the reason for job loss.

[27] Fig. A.1 (top right panel) shows Kaplan–Meier survivor functions for quarters until reemployment for the three treatment groups of permanent job losers. Fig. A.2 (top right panel) shows survivor functions for job tenure with the first post-claim employer. The *p*-values testing for equality among the three survivor functions indicate that the estimated survivor functions for job tenure are statistically different from each other at the 10% level, but the estimated survivor functions for quarters until reemployment are not.

**Table 7**
Estimated effects of the standard and modified work tests on selected outcomes, claimants unemployed due to temporary layoff.

| | No work test | | OLS estimated effects (robust std. error) | | | |
|---|---|---|---|---|---|---|
| Outcome | Mean (std. dev.) | | Standard work test | | Modified work test | |
| *UI outcomes* | | | | | | |
| Benefits paid ($) | 2160 | (1793) | −320*** | (113) | −544*** | (124) |
| Weeks paid | 15.4 | (10.72) | −2.29*** | (0.72) | −3.79*** | (0.79) |
| Exhausted benefits (proportion) | 0.249 | (0.433) | −0.087*** | (0.028) | −0.135*** | (0.029) |
| Number of conditional payments (incl. zeros) | 0.685 | (1.82) | 0.185 | (0.115) | 0.177 | (0.116) |
| *Short-term employment outcomes (unconditional)* | | | | | | |
| Quarter 1 outcomes[a] | | | | | | |
| Earnings ($) | 3107 | (2882) | −135 | (181) | 160 | (186) |
| Hours worked | 259 | (206) | −14.4 | (14.0) | 10.0 | (15.0) |
| Employed (proportion) | 0.774 | (0.419) | −0.013 | (0.030) | 0.060* | (0.031) |
| Benefit year (year 0) outcomes[a] | | | | | | |
| Earnings ($) | 14,487 | (10,443) | 88 | (589) | 864 | (655) |
| Hours worked | 1232 | (710) | −44.2 | (46.9) | 25.2 | (51.7) |
| Employed (proportion) | 0.927 | (0.26) | 0.003 | (0.018) | 0.021 | (0.019) |
| Other outcomes | | | | | | |
| Returned to same employer (proportion) | 0.539 | (0.499) | −0.057 | (0.035) | −0.026 | (0.038) |
| Returned to same industry (proportion) | 0.64 | (0.481) | −0.056 | (0.034) | −0.030 | (0.036) |
| *Employment outcomes over post-claim years 1–9 (unconditional)* | | | | | | |
| Average annual earnings ($)[b] | 14,750 | (11,214) | 1 | (711) | 315 | (784) |
| Average annual hours[b] | 1131 | (727) | 3.7 | (50.5) | −12.8 | (55.2) |
| Average employment probability[b] | 0.759 | (0.328) | 0.003 | (0.023) | −0.000 | (0.026) |
| Average log wage rate (conditional on employment) | 2.511 | (0.369) | 0.002 | (0.029) | 0.015 | (0.037) |
| Δ Average log wage rate (conditional on employment) | 0.175 | (0.402) | −0.003 | (0.035) | −0.008 | (0.042) |
| Quarters until reemployment | 2.66 | (6.49) | −0.397 | (0.400) | −0.136 | (0.533) |
| Tenure with first post-claim employer (quarters) | 10.34 | (12.3) | −1.264 | (0.857) | −1.135 | (0.893) |
| Sample size | 425 | | 355 | | 272 | |

*Notes*: See Table 3.

**Table 8**
Pre-treatment outcome tests of selection into reemployment in year 0 based on Eq. (2): estimated differences between pre-claim wage rates of the standard (or modified) work test group and the no work test group, conditional on post-claim reemployment, by reason for job loss (robust standard errors in parentheses).

| | Outcome variable | | |
|---|---|---|---|
| | Log(wage in year −1) | Log(wage in year −2) | Log(wage in year −3) |
| *Panel A: permanent job losers* | | | |
| Standard work test | −0.005 | −0.007 | 0.027 |
| | (0.023) | (0.036) | (0.035) |
| Modified work test | −0.046* | −0.068* | −0.049 |
| | (0.027) | (0.038) | (0.041) |
| Sample size | 556 | 517 | 454 |
| *Panel B: quit for good cause* | | | |
| Standard work test | −0.023 | −0.012** | −0.062 |
| | (0.026) | 0.006 | (0.038) |
| Modified work test | 0.030 | 0.022 | 0.032 |
| | (0.026) | (0.033) | (0.037) |
| Sample size | 552 | 512 | 443 |
| *Panel C: unemployed due to temporary layoff* | | | |
| Standard work test | −0.023 | −0.006 | −0.006*** |
| | (0.021) | (0.023) | (0.027) |
| Modified work test | −0.018 | 0.022 | −0.003 |
| | (0.023) | (0.027) | (0.031) |
| Sample size | 942 | 869 | 809 |

*Notes*: Each coefficient is the regression-adjusted difference between the log pre-claim wage rate in year − *t* of claimants in the standard (or modified) work test group who were reemployed and claimants in the no work test group who were reemployed, by reason for job loss. See Eq. (2). Other covariates include the variables listed in the notes to Table 3 (except for pre-claim earnings and pre-claim hours).
*p*-values are for tests of the difference between the standard (or modified) work test group and the no work test group.
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

(that is, they obtained a better job match) when they were subjected to a work test than when they were not.

For claimants who quit for good cause (Table 6), the only short-term effect that is estimated precisely is for the effect of the modified work test on employment probability in Year 0 (the benefit year). However, this improvement in employment probability is not reflected in other improved outcomes over the short term. In the long-term, there is little evidence of any effect of the work test for claimants who quit for good cause.

The bottom left panels of Figs. 2 and 3 show year-by-year estimates of the work test treatments on long-term employment probabilities and unconditional earnings of claimants who quit for good cause. These figures tend to confirm that the work test treatments had little long-term effect on employment or earnings. Unlike permanent job losers, workers who quit for good cause had statistically insignificant work test effects on quarters to reemployment, and the effects on tenure with the first post-claim employer are statistically insignificant.[28]

For workers on temporary layoff (Table 7), the only notable short-term employment effect is a possible increase in employment probability associated with the modified work test in quarter 1. In the long-term, none of the estimated work test treatment effects is statistically significant (see the bottom panel of Table 7 and the lower-right graphs in Figs. 2 and 3).

### 6.4. Pre-treatment outcome tests by reason for job loss

We concluded in Section 6.2 that the work test treatments may have had a relatively strong effect on lower-wage workers, selecting them into reemployment and altering the composition of the reemployed

[28] The findings for quarters until reemployment and tenure with the first post-claim employer are confirmed by Weibull accelerated failure-time estimates and Kaplan–Meier survivor functions—see Table A.3 and Figs. A.1 and A.2.

across treatment groups. Is this effect more pronounced for some subgroups than for others?

Fig. 4 shows year-by-year estimates of wage rate differentials between the work test and no work test groups for the three subgroups we are examining. For permanent job losers, several negative differentials appear between the modified work test and no work test groups (four have $p$-values ≥0.10), and the average differential over the nine post-claim years is negative (although imprecisely estimated—see Table 5). The pattern is less consistent for the standard work test group, but on average, the differential is again negative.

Table 8 displays estimates of Eq. (2), testing for selection into reemployment in the benefit year (year 0). For permanent job losers, the modified work test treatment clearly selected lower-wage workers into reemployment. This suggests that the negative wage "effects" of the modified work test are likely the result of a stronger treatment effect on lower-wage claimants who were permanent job losers (a selection effect).

Table A.4 provides further evidence on this point by displaying estimated long-term effects of the work test treatments after partitioning the sample of permanent job losers into those whose pre-claim wage rate was less than the median (top panel) and greater than or equal to the median (bottom panel). The resulting samples are notably small and subject to sampling error, but the estimates are consistent with the hypothesis that the work test treatments affected primarily lower-wage permanent job losers. For example, the standard work test lengthened post-claim job tenure only for lower-wage permanent job losers, and the modified work test reduced quarters until reemployment only for lower-wage permanent job losers. Also, the negative wage differential associated with the modified work test (in Table 3) appears to be driven by lower-wage permanent job losers. (Recall that this negative differential is not a treatment effect, but results from negative selection into reemployment.)

For claimants who quit for good cause and temporary layoffs, Fig. 4 shows little or no systematic pattern to the post-claim wage "effects" of the work test treatments, and in both cases, the average long-term wage rate estimates are statistically insignificant (bottom panels of Tables 6 and 7). Further, the estimates in Table 8 point to no effect of either work test on selection into reemployment for claimants who quit or were temporarily laid off.

Overall, the results shown in Fig. 4 and Table 8 suggest that effect of the work test treatments on selection into reemployment was probably confined to permanent job losers.

## 7. Summary and discussion

A long-standing concern about strict enforcement of the UI work test is that it may pressure UI recipients to accept a job sooner than they otherwise would, 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 tenure (Farber, 1999).

The Washington Alternative Work Search experiment was designed to examine the effects of the work test by randomly assigning new UI claimants to groups subject to a standard work test, a modified work test (which scheduled earlier ERIs), and an honor system that effectively eliminated the work test. By appending nine years of administrative wage records to the original data from the WAWS experiment, we are able to examine the long-term intent-to-treat effects of the standard and modified work tests on employment outcomes like earnings, probability of employment, work hours, time to reemployment, and job match quality (tenure with the first post-claim employer).

For UI claimants as a whole, the standard and modified work tests reduced benefits received, the duration of UI spells, and the probability of exhausting benefits, and they increased the probability of employment in the year following the UI claim. In light of these findings, it is surprising that the work test treatments did not increase these claimants' earnings or work hours in the year following the claim. Evidence from pre-treatment outcome tests (Section 6.1) suggest an explanation: claimants in the work test treatments who became reemployed following the experiment had earned relatively lower wages *before* the experiment; that is, the work test treatments selected lower-wage workers into reemployment. This disproportionate effect of the work test on lower-wage workers makes sense given that lower-wage workers tend to have less-specialized skills and can be suitably matched to a wider range of jobs. For these workers, being pushed to accept a job earlier has no deleterious effect because the gains from earlier reemployment outweigh the gains from a good match, which are relatively small.

Evidence in Section 6.2 shows that, averaged over the full sample, the effects of the work test treatments on earnings and employment over the subsequent nine years were negligible. Put more positively, the work test appears to be benign in the long run, contrary to the hypothesis that the work test could harm long-term earnings.

Our analysis of treatment effects by reason for job loss (Section 6.3) focuses mainly on permanent job losers, for whom the work test was beneficial in the short run: during the year of the experiment, these claimants were more likely to be reemployed, worked more hours, and had higher earnings. We find no evidence that the work test harmed permanent job losers' long-run earnings or employment (indeed, point estimates for most post-claim years were positive, albeit imprecise). Also, although some of the estimates are imprecise, permanent job losers subject to the work test appear to have returned to work 1–2 quarters sooner than those not subject to the work test, and they had longer job tenure with their first post-claim employer by as much as two quarters (on a base of six quarters). Combined with the evidence on pre-claim outcomes from Section 6.1, the findings suggest the work test is a potentially important policy for improving the prospects of low-wage, permanent job losers.

Extending Johnson and Klepinger's (1991, 1994) short-term analysis of the WAWS experiment is useful both in light of the diverse findings on the effects of the UI work test and benefit sanctions on post-unemployment outcomes, and because enforcement of the work test in the United States appears to have fallen since the 1980s. Evidence on the latter can be obtained from the annual reports filed by states with the U.S. Department of Labor about "nonmonetary determination activities" (that is, the states' investigations of UI claims for various reasons) (U.S. Department of Labor, no date). Our tabulations of these data suggest that, nationally, the percentage of continued claims investigated for failure to meet the work test (the "able, available, and actively seeking" criterion for eligibility) has fallen substantially since the 1980s.[29] Specifically, during 1980–1984, 1.65% of continued claims were investigated for failing to meet the work test. In subsequent five-year periods, this percentage fell unevenly to 1.33 (1985–1989), 1.01 (1990–1994), 1.13 (1995–1999), 0.76 (2000–2004), 0.70 (2005–2005), and 0.77 (2010–2014).

Two possible reasons for this decline in work test enforcement have been discussed. First, during the 1990s, the states came under pressure from the U.S. Department of Labor to reduce their UI payment error rates. When the states found that failure to enforce the work test was a key source of payment errors, they relaxed their work test standards (O'Leary, 2004). Second, since the mid 1990s, nearly all states have moved from taking claims in-person to accepting claims by telephone and on-line, leading to a more impersonal or "hands-off" approach to administering UI (Ebenstein and Stange, 2010). But regardless of the reasons, the available evidence shows that work test enforcement in the United States has become less stringent, and the findings presented here suggest this may be to the detriment of lower-wage, permanent job losers.

[29] A continued claim is a claim filed for payment for one or (in most states) two more weeks of benefit payments in an ongoing claim series.

## Appendix A

**Table A.1**
Estimated effects of the standard and modified work tests on selected outcomes, claimants unemployed due to seasonal layoff.

| | No work test | | OLS estimated effects (robust std. error) | | | |
|---|---|---|---|---|---|---|
| Outcome | Mean (std. dev.) | | Standard work test | | Modified work test | |
| *UI outcomes* | | | | | | |
| Benefits paid ($) | 2789 | (1807) | −454*** | (138) | −608*** | (145) |
| Weeks paid | 18.74 | (9.45) | −3.27*** | (0.88) | −3.40*** | (0.94) |
| Exhausted benefits (proportion) | 0.336 | (0.473) | −0.097** | (0.041) | −0.095** | (0.043) |
| Number of conditional payments (incl. Zeros) | 0.571 | (1.639) | −0.016 | (0.129) | 0.291* | (0.157) |
| *Short-term employment outcomes (unconditional)* | | | | | | |
| Quarter 1 outcomes[a] | | | | | | |
| Earnings ($) | 2362 | (2756) | 17 | (243) | 7 | (256) |
| Hours worked | 171 | (197.7) | 3.5 | (17.6) | 18.1 | (20.2) |
| Employed (proportion) | 0.7 | (0.459) | −0.006 | (0.042) | 0.008 | (0.046) |
| Benefit year (year 0) outcomes[a] | | | | | | |
| Earnings ($) | 13,248 | (10,554) | 16 | (842) | 42 | (852) |
| Hours worked | 924 | (670) | 24.3 | (57.8) | 48.6 | (69.4) |
| Employed (proportion) | 0.915 | (0.279) | 0.008 | (0.025) | 0.003 | (0.028) |
| Other outcomes | | | | | | |
| Returned to same employer (proportion) | 0.372 | (0.484) | −0.042 | (0.044) | −0.080 | (0.049) |
| Returned to same industry (proportion) | 0.518 | (0.501) | −0.074 | (0.047) | −0.065 | (0.049) |
| *Employment outcomes over post-claim years 1–9 (unconditional)* | | | | | | |
| Average annual earnings ($)[b] | 14,272 | (12,031) | 61 | (897) | −1005 | (987) |
| Average annual hours[b] | 984 | (682) | 40.8 | (57.4) | 5.9 | (64.9) |
| Average employment probability[b] | 0.744 | (0.329) | 0.022 | (0.028) | −0.017 | (0.033) |
| Average log wage rate (conditional on employment) | 2.620 | (0.458) | −0.016 | (0.048) | −0.062 | (0.052) |
| Δ Average log wage rate (conditional on employment) | 0.211 | (0.389) | −0.024 | (0.056) | −0.044 | (0.066) |
| Quarters until reemployment | 2.99 | (6.84) | −0.307 | (0.625) | −0.092 | (0.772) |
| Tenure with first post-claim employer (quarters) | 8.45 | (11.04) | −0.723 | (0.988) | −0.158 | (1.170) |
| Sample size | 247 | | 239 | | 167 | |

*Notes*: See Table 3.

**Table A.2**
Estimated effects of the standard and modified work tests on selected outcomes, claimants unemployed for unknown reasons.

| | No work test | | OLS estimated effects (robust std. error) | | | |
|---|---|---|---|---|---|---|
| Outcome | Mean (std. dev.) | | Standard work test | | Modified work test | |
| *UI outcomes* | | | | | | |
| Benefits paid ($) | 2325 | (1816) | −510*** | (103) | −450*** | (116) |
| Weeks paid | 17.08 | (10.31) | −3.41*** | (0.68) | −2.86*** | (0.78) |
| Exhausted benefits (proportion) | 0.372 | (0.484) | −0.131*** | (0.029) | −0.113*** | (0.034) |
| Number of conditional payments (incl. zeros) | 0.474 | (1.305) | 0.437*** | (0.104) | 0.550*** | (0.122) |
| *Short-term employment outcomes (unconditional)* | | | | | | |
| Quarter 1 outcomes[a] | 1996 | (2394) | 100 | (160) | −12 | (163) |
| Earnings ($) | 188 | (204.8) | 0.8 | (13.4) | 11.8 | (14.9) |
| Hours worked | 0.638 | (0.481) | 0.046 | (0.032) | 0.037 | (0.035) |
| Employed (proportion) | | | | | | |
| Benefit year (year 0) outcomes[a] | | | | | | |
| Earnings ($) | 10,497 | (9404) | −243 | (538) | −640 | (572) |
| Hours worked | 947 | (759.6) | 11.4 | (48.2) | 45.6 | (52.1) |
| Employed (proportion) | 0.859 | (0.349) | −0.008 | (0.023) | 0.002 | (0.026) |
| Other outcomes | | | | | | |
| Returned to same employer (proportion) | 0.39 | (0.488) | −0.052* | (0.031) | −0.041 | (0.033) |
| Returned to same industry (proportion) | 0.479 | (0.500) | −0.031 | (0.032) | −0.049 | (0.035) |
| *Employment outcomes over post-claim years 1–9 (unconditional)* | | | | | | |
| Average annual earnings ($)[b] | 12,152 | (11,320) | −759 | (691) | −1261* | (727) |
| Average annual hours[b] | 975 | (751) | −44.9 | (49.1) | −37.2 | (52.2) |
| Average employment probability[b] | 0.685 | (0.358) | 0.001 | (0.024) | −0.007 | (0.026) |
| Average log wage rate (conditional on employment) | 2.467 | (0.401) | −0.058* | (0.034) | −0.100** | (0.039) |
| Δ Average log wage rate (conditional on employment) | 0.218 | (0.342) | −0.049 | (0.040) | −0.050 | (0.043) |
| Quarters until reemployment | 3.58 | (7.58) | 0.833 | (0.582) | 1.211* | (0.682) |
| Tenure with first post-claim employer (quarters) | 8.59 | (11.29) | −1.051 | (0.772) | −0.631 | (0.844) |
| Sample size | 403 | | 415 | | 287 | |

*Notes*: See Table 3.

**Table A.3**
Weibull accelerated failure-time estimated effects of the standard and modified work test on tenure with first post-claim employer and quarters until reemployment (standard errors in parentheses).

| Dependent variable | Tenure with first post-claim employer | | | | Quarters until reemployment | | | |
|---|---|---|---|---|---|---|---|---|
| Estimation sample | All | Permanent layoff | Quit | Temporary layoff | All | Permanent layoff | Quit | Temporary layoff |
| Standard work test | −0.041 | 0.280** | 0.157 | −0.224** | −0.047 | −0.257** | −0.109 | −0.028 |
| | (0.051) | (0.122) | (0.112) | (0.096) | (0.062) | (0.128) | (0.135) | (0.096) |
| Modified work test | 0.027 | 0.304** | 0.194 | −0.142 | −0.000 | −0.420*** | −0.214 | 0.042 |
| | (0.055) | (0.133) | (0.121) | (0.105) | (0.073) | (0.146) | (0.153) | (0.119) |

*Notes*: See Table 3.

**Table A.4**
Estimated effects of the standard and modified work tests on selected long-term employment outcomes of permanent job losers, by pre-treatment wage rate.

| | Outcome variable | | | | | |
|---|---|---|---|---|---|---|
| | Average annual earnings ($)[a] | Average annual hours[a] | Average employment probability[a] | Average log wage rate[a] | Quarters until reemployment | Tenure (quarters)[b] |
| *Panel A: Subsample of claimants with pre-treatment wage rate < median* | | | | | | |
| Standard work test | 495 | 84 | 0.037 | −0.079 | −1.310 | 3.024** |
| | (840) | (76.3) | (0.038) | (0.080) | (1.074) | (1.180) |
| Modified work test | −227 | 40.4 | 0.002 | −0.135* | −2.724** | 1.959 |
| | (1057) | (96.0) | (0.046) | (0.081) | (1.178) | (1.276) |
| Sample size | 393 | 393 | 393 | 148 | 393 | 393 |
| *Panel B: Subsample of claimants with pre-treatment wage rate ≥ median* | | | | | | |
| Standard work test | 1012 | −10.3 | 0.024 | 0.121 | −0.208 | 0.303 |
| | (1738) | (109.3) | (0.053) | (0.081) | (1.438) | (0.592) |
| Modified work test | −253 | −90.2 | −0.054 | 0.052 | 3.123 | −0.680 |
| | (1981) | (134.4) | (0.064) | (0.099) | (2.069) | (0.606) |
| Sample size | 286 | 286 | 286 | 123 | 286 | 286 |

*Notes*: Pre-treatment wage is the average wage rate in the three pre-claim years. See Fig. 1 and notes to Table 3.
[a] Averages are taken over post-claim years 1–9.
[b] Tenure with first post-claim employer in quarters.

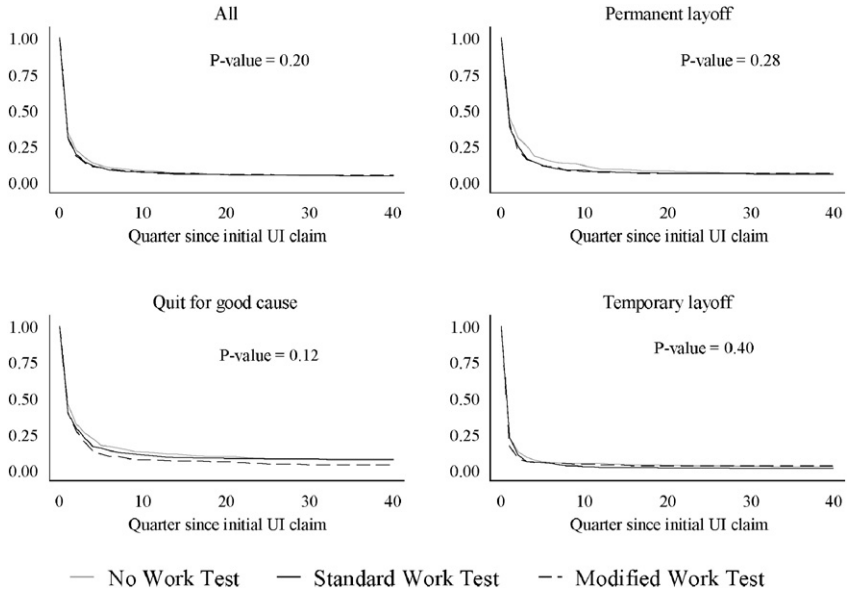


**Fig. A.1.** Kaplan–Meier survivor functions (unadjusted conditional probabilities) of quarters until reemployment, by reason for job loss and treatment group *Notes*: The *y*-axes show the probability of survival in nonemployment following the initial UI claim. The data account for right-censoring occurring at the 40th quarter after the claim. The *p*-values in each panel come from a log-rank test for equality of the three survivor functions.

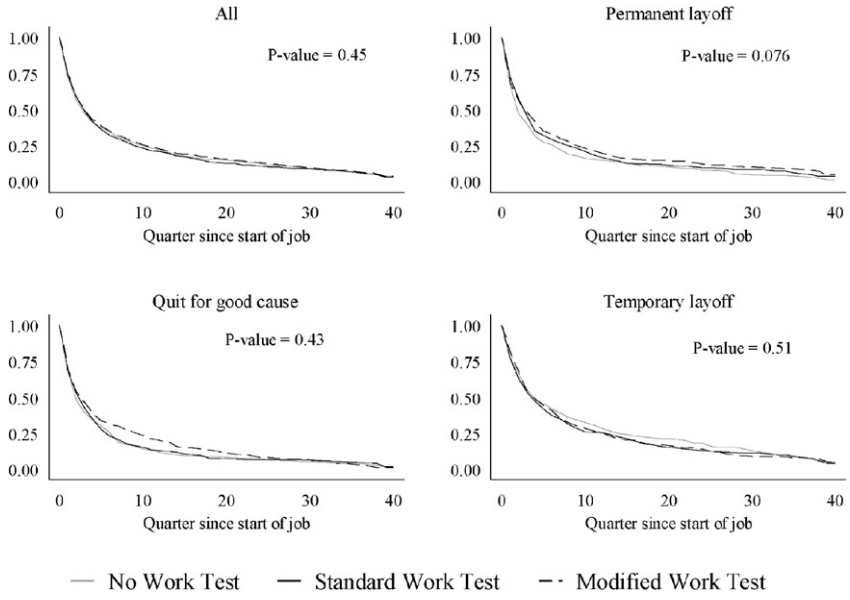


**Fig. A.2.** Kaplan–Meier survivor functions (unadjusted conditional probabilities) of job tenure with the first post-claim employer, by reason for job loss and treatment group *Notes*: The *y*-axes show the probability of survival with the first post-claim employer. The data account for right-censoring occurring at the 40th quarter after the claim. The *p*-values in each panel come from a log-rank test for equality of the three survivor functions.

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