The Long-Term Effects of Job Search Assistance for Displaced Workers

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Abstract

We examine the effects of randomly-assigned job search assistance (JSA) on the employment, hours, and earnings of displaced workers and UI claimants as a whole over the nine years following assignment to JSA. For displaced workers, JSA led to shorter spells of UI receipt in the short term, and to a higher probability of employment in the long run, and JSA led to employment with larger (and perhaps, better-paying) employers. This contrasts with the effects for UI claimants as a whole, for whom JSA also reduced UI receipt but had no effects on long-term outcomes. We interpret the positive effects of JSA on displaced workers' long-term outcomes as evidence that JSA enhanced the job-search human capital of these workers. For UI claimants as a whole, the pattern of UI exit coupled with negligible long-term effects of JSA is consistent with JSA raising the cost of continuing to claim UI—that is, with a so-called threat effect.

JEL codes: I38, J24, J64, J65, J68

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1. Introduction

Job search assistance (JSA) has been the main reemployment program in the United States since 1993, when Congress enacted Worker Profiling and Reemployment Services (WPRS). Adoption of JSA followed a series of random-assignment experiments conducted in the 1980s and 1990s showing that JSA reduced unemployment spells, normally without reducing earnings in the subsequent year.[1] But if the benefits (or costs) of JSA extend beyond one year, a complete evaluation needs to account for these long-term effects. Relatively little is known about JSA's effects on employment and earnings beyond one year (see below).

JSA may benefit job seekers through two possible mechanisms. The first—enhancement of job-seekers' search skills—would be expected to improve employment and earnings in the long term. For example, Manoli, Michaelides, and Patel (2018) find that JSA resulted in increased employment and higher earnings five years after treatment, consistent with a "JSA as human capital enhancement" interpretation. The second mechanism—the so-called threat effect—arises because JSA requires unemployment insurance (UI) claimants to attend one or more workshops and follow-up sessions or risk losing benefits. Black, Smith, Berger, and Noel (2003) have shown this increased cost of continuing to receive UI is the main reason JSA leads to higher UI exit rates and increased short-term earnings (the latter of which result from earlier

[1] On the history of WPRS, see Wandner (2010). JSA experiments were conducted in South Carolina (Corson, Long, and Nicholson 1985), New Jersey (Corson, Dunstan, Decker, and Gordon 1989; Anderson, Corson, and Decker 1991), Washington State (Johnson and Klepinger 1991, 1994), Maryland (Klepinger, Johnson, Joesch, and Benus 1998, 2002), and the District of Columbia and Florida (Decker, Olsen, and Freeman 2000). See O'Leary (2006) for a review, as well as Dickinson, Kreutzer, and Decker (1999) and Black, Smith, Berger, and Noel (2003). Overall, the experiments concluded that JSA reduced the unemployment insurance spell duration by about 0.5–0.75 weeks without reducing employment or earnings in the year following the experiment—the most commonly used post-experimental follow-up period.

return to work).[2] Unlike the human capital effect, the threat effect does not point to a clear long-term effect of JSA on employment or earnings.

Although estimates of JSA's influence on long-term outcomes may sort out the mechanism by which JSA is effective, few experiments have examined JSA's effects on employment and earnings beyond one year, and the existing studies have reached mixed conclusions. The two-site 1995–96 DC-Florida experiment (Decker, Olsen, and Freeman 2000) found that at one site (DC), UI claimants randomly assigned to JSA had significantly higher earnings and employment (by 10 percent and 6 percent) than controls after ten quarters. But at a second site (Florida), JSA had no statistically significant effect on earnings or employment after 12 quarters. A recent study by Manoli, Michaelides, and Patel (2018) analyzed the 5-year effects of JSA in a 2009 experiment in Nevada and found that, after five years, claimants assigned to JSA were 5 percent more likely to be employed and had higher average earnings (by 9 percent) than controls. These mixed findings about the long-term effects of JSA on employment and earnings suggest a need for additional evidence and a fuller understanding of JSA.

In this paper, we estimate the long-term effects of JSA on employment, hours, and earnings by matching nine years of follow-up administrative wage records to data from the 1986–1987 Washington Alternative Work Search (WAWS) experiment, whose short-term effects were first analyzed by Johnson and Klepinger (1991, 1994) (J&K). In the WAWS experiment, all eligible UI claimants were randomly assigned to a control group that was required only to be "able, available, and searching" for work, or to an intensive JSA group that was instructed to report to the public employment service for a JSA workshop and additional

[2] The threat effect might equally well be called a hassle or nuisance effect to emphasize the cost of attending the JSA workshop and complying with other requirements, as opposed to the "threat" of losing a week or more of benefits.

reemployment services. J&K found that JSA was successful in reducing the amount and weeks of UI receipt and increasing the probability of employment in the quarter following the experiment.

Our work differs from J&K's in two ways. First, we examine the long-term employment effects of JSA, whereas J&K studied only short-term effects. Second, our focus is on estimating the effects of JSA on displaced workers—workers who report having permanently lost their job, often because of a plant or company closure, whereas J&K considered all eligible UI claimants.[3] The rationale for shifting the focus to long-term effects on displaced workers is that JSA is intended primarily as a program for permanent job losers who are likely to exhaust benefits (Wandner 2010).

Our main findings suggest that JSA had positive long-term effects on the employment of displaced workers. Specifically, compared with displaced controls, displaced UI claimants assigned to JSA experienced a 6 percent higher employment probability averaged over the nine years following assignment to treatment. Displaced workers assigned to JSA also experienced an increase in work hours in some years. The estimated effects on long-term earnings are substantial but imprecisely estimated due to small sample size. Similar to Manoli, Michaelides, and Patel (2018), they point to a positive effect of JSA on the job-search human capital of displaced workers.[4]

To substantiate this interpretation, we present estimates of the effect of JSA on post-claim employer characteristics often associated with employer quality, such as employer size, the

[3] As described in Appendix D, we classify workers as displaced if they reported being permanently laid off when they filed their initial claim for UI.
[4] For evidence on the importance of improved job matches to the long-term recovery of displaced workers' earnings, see Lachowska, Mas, and Woodbury (2020).

average wage rate paid by the employer, and Abowd, Kramarz, and Margolis (1999) (AKM) employer fixed effects on wages. If JSA enhanced job-search human capital, we might expect displaced workers to be employed by larger and better-paying employers. We find that displaced workers assigned to JSA did tend to work for larger post-claim employers than the controls, and also that it led to employment with better-paying employers, although the latter estimates are again imprecise.

We contrast these long-term JSA effects for displaced workers to the long-term effects for all UI claimants—the sample originally analyzed by J&K. We find that for claimants as a whole, JSA had negligible long-term effects. These results are consistent with JSA acting mainly through a threat effect that leads to early UI exit and quicker reemployment in the short term, but not to any enhancement in job-search skills that might contribute to better job matches and improved long-term employment outcomes.

The contrast between the effects of JSA for displaced workers and for UI claimants as a whole offers a possible way to reconcile the varied estimates of JSA's effects on subsequent employment outcomes. Among displaced workers, we observe relatively few UI exits before the JSA workshop, a high JSA participation rate, and improved long-term employment outcomes—evidence consistent with JSA leading to improved job-search skills. Among UI claimants as a whole, JSA shortens UI spells mainly by increasing the cost of receiving UI benefits—that is, through a threat effect—and has no long-term effects. A corollary is that, when evaluating labor market policies, it is important to distinguish between workers for whom a program is intended—in this case, displaced workers—and other groups.

The estimates contribute to a growing literature using administrative data to estimate the long-term effects of various public policies. For example, Dahl, DeLeire, and Schwabish (2009)

find positive effects of the Earned Income Tax Credit (EITC) on earnings growth up to five years after the 1993–96 EITC expansions. Chetty et al. (2011) show that early childhood education interventions in the Student/Teacher Achievement Ratio (STAR) experiment had positive long-term impacts on earnings of students randomly assigned to various treatments. Price (2018) studies the long-term effects of the Seattle-Denver Income Maintenance Experiment and finds that, five years after the experiment, people randomly assigned to receive cash assistance had lower earnings and higher rates of disability applications. Hyman (2018) studies the long-run fadeout of earnings effects of Trade Adjustment Assistance (TAA) retraining. In addition to serving the obvious purpose of estimating whether interventions have effects over long periods, such studies can contribute to a fuller understanding of how the interventions work. In particular, by distinguishing the different ways interventions work in different populations, estimates of long-term effects can offer insights into the broader workings of the labor market.

The paper proceeds as follows. Section 2 reviews the design of the WAWS experiment, with attention to the services received by (and characteristics of) displaced claimants assigned to the JSA and control groups. Section 3 outlines the methods used to estimate the effects of assignment to JSA. Section 4 reports the estimated short-term effects of JSA on displaced workers, and Section 5 presents the long-term effects of JSA on displaced workers. In Section 6 we compare the estimates for displaced workers with estimates for UI claimants as a whole. Section 7 summarizes the findings and discusses implications for policy.

To keep the main discussion as direct as possible, we include additional results in appendices. Appendix A reports descriptive statistics of displaced workers. We relegate our replication of J&K's (1994) short-term analysis to Appendix B, which also includes the analysis of long-term employment outcomes for UI claimants as a whole. Appendix C contains the

analysis for claimants on temporary layoff. A detailed description of the data is presented in Appendix D.

2. The Washington Alternative Work Search Experiment

From July 1986 through August 1987, all eligible UI claimants at the Tacoma Employment Service Center were randomly assigned to a control group or a JSA treatment group, based on the last digit of their Social Security number.[5]

Claimants in the control group faced the usual requirement to be "able, available, and searching" for work—the so-called work search test. In practice, the work search test meant that the Employment Security Department (ESD) personnel told all new UI claimants to contact at least three employers per week and be prepared to show they had done so in an eligibility review interview (ERI). Claimants were usually sent a notice to report for an ERI in weeks 11–12 after the initial claim was filed, with the ERI usually conducted in weeks 12 or 13. For an ERI, a claimant reports to the public Employment Service for a one-hour group interview or lecture, possibly followed by a 15-minute individual interview during which employer contacts are checked. This is the "treatment" to which controls were subject.

In contrast, claimants assigned to the JSA group who were still unemployed about four weeks after their UI claim were sent a notice to attend a JSA workshop. The notice instructed them to report on a specified date in 2–3 weeks and indicated that failure to report could result in

[5] The WAWS experiment included two additional randomly assigned treatments, called exception reporting and the new work search policy (J&K1991, pp. 39). Lachowska, Meral, and Woodbury (2015, 2016) examine the long-term effects of exception reporting. Here, we consider only the JSA treatment.

loss of benefits.[6] The JSA program stretched over four weeks, starting with a two-day workshop that included discussions of skills assessment, interview and marketing techniques, use of the telephone to contact employers, completing applications, and résumé preparation. This was followed by 10 hours of twice-weekly sessions in a telephone room contacting potential employers. Those still unemployed 12 weeks after claiming received an additional notice to report for an ERI. (This was a week or two later than claimants in the control group.) JSA, then, included both an attempt to help claimants improve their job search skills and an effort to enforce the work search test more stringently.

For two reasons, it makes sense to focus on JSA's effects on displaced workers, rather than all UI claimants regardless of their reason for job loss, as J&K did. First, the JSA workshop and follow-up were intended primarily for displaced workers—permanent job losers who were likely to exhaust benefits and were in principle most in need of help finding a new job. Second, although all new UI claimants were randomly assigned to JSA, not all JSA-assigned claimants were called in for a JSA workshop—in particular, those temporarily laid-off with a definite recall date (about 20 percent of the sample).[7]

The inclusion of claimants who were not required to attend JSA may attenuate the estimated effects of assignment to JSA. For example, the 2009 Nevada experiment, which had

[6] The mean and median time from the start of the claim to the workshop was around seven weeks—that is, three weeks after receiving the call-in notice. Workshops were offered every other week, usually starting on a Monday, from September 1986 through September 1987, and typically enrolled between 10 and 20 claimants. (The smallest workshop enrolled 5, the largest enrolled 26.) At the time of the experiment, the labor market in the Tacoma region was weaker than that of the U.S. as a whole—the unemployment rate was 7.7 percent, compared with 5.7–6.6 percent nationally.

[7] Among these "standby" claimants only those who lost their temporary layoff status were referred to a JSA workshop (J&K 1994, p. 700). This accounts for the positive but low JSA participation rate among claimants on temporary layoff—about 12 percent; see Appendix Table C1 in Appendix C.

strong positive effects on employment probability and earnings, excluded claimants on temporary layoff (Michaelides and Mueser 2018; Manoli, Michaelides, and Patel 2018, 2023). For both reasons, we focus on displaced workers in the main body of the text and present an analysis of JSA's effects on UI claimants as a whole in Appendix B. Estimates for claimants on temporary layoff appear in Appendix C.

Table 1 shows the proportions of displaced UI claimants assigned to the control and JSA groups who attended a job search workshop, were called for an ERI, and received various employment services (a job referral, help in developing a job plan, or any other employment service).[8] About 30 percent of the displaced claimants who were assigned to JSA actually attended a JSA workshop.[9] Among the controls, one person attended a JSA workshop, suggesting that JSA "always-takers" were almost non-existent. Table 1 also shows that claimants in the JSA group were less likely than controls to be called for an ERI or to receive help with a job development plan (likely because they had exited UI already), although controls and those assigned to JSA were about equally likely to receive job referrals and other employment services.

[8] Other services included job consultations, 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 ESD.

[9] Evaluations of JSA experiments have not always reported JSA participation rates by claimant type, but the overall JSA participation rate in the South Carolina experiment (Corson, Long, and Nicholson 1985, p. 40) was 43 percent, in New Jersey (referenced in Decker, Olsen, and Freeman 2000, p. 42) 49.8 percent, in Maryland (Klepinger, Johnson, Joesch, and Benus 2002, p. 10) about 30 percent, and in DC and Florida (Decker, Olsen, and Freeman 2000, p. 42) 43.9 percent and 59.5 percent. The overall employment workshop participation rate in the Nevada experiment (Michaelides and Mueser 2018, p. 557) was about 14 percent; however, the take-up of other reemployment services was higher. Except for WAWS, these JSA demonstrations excluded claimants on temporary layoff from the initial sample selection.

Appendix Table A1 displays mean characteristics of the displaced workers in the control and JSA groups, as well as mean characteristics of the displaced JSA participants. Comparison of the control and JSA groups suggests that the randomization was generally successful: Of the 57 characteristics shown, four (the proportions of claimants who are white, black, worked in benchwork, worked in construction) differ between the control and JSA groups with $p \leq 0.05$.

In contrast, there are differences between JSA participants (that is, the "compliers" in this experiment) and controls. Participants were more likely to be female and married, homeowners, and somewhat older. They were also less likely have previous experience with the UI system.

3. Methods

We begin by discussing the OLS estimator of intention-to-treat (IT) effects of JSA assignment. We then argue that the JSA call-in notice may itself have affected outcomes, violating the exclusion restriction and limiting our ability to use JSA assignment as an instrument for JSA participation.

3.1 Effects of JSA assignment

The IT effects of JSA assignment can be obtained by pooling the control and JSA groups and estimating linear models of the following form:

$$y_{it} = \alpha + \delta JSA_i + \mathbf{x}_i\boldsymbol{\beta} + u_{it}, \qquad (1)$$

where y_{it} is an outcome for claimant i observed in period t relative to the claim year, JSA_i is an indicator for assignment to JSA, $\mathbf{x}_i$ denotes a row vector of observable characteristics of claimant i, and u_{it} denotes unobservable traits. The identifying assumption is that assignment to treatment is orthogonal to the error term: $E(u|JSA) = 0$, which is justified by random assignment. In this

case, the OLS estimator of δ is consistent for the causal effect of assignment to JSA on outcome y.

We begin by estimating the effect of JSA on the following short-term outcomes that were also studied by J&K (1994): UI benefits paid and number of weeks of benefits paid during the first spell of UI receipt; whether the claimant exhausted benefits; employment, hours, and earnings in quarter 1 (that is, the first quarter after the quarter of UI claim); employment, hours, and earnings during the first year after the claim (that is, the sum of quarters 1–4 after the UI claim quarter); and whether the claimant returned to a former employer or the industry of the former employer.

We also use equation (1) to estimate the effect of JSA on long-term worker outcomes (employment, work hours, earnings, and wage rates) and long-term employer-level outcomes (employer size, average employer wage rate, and AKM employer effects on wage rates) for each of the nine years following the experiment.[10] Employer size is often a proxy for better paying employers (Bloom et al. 2018), average employer wages may indicate the desirability of an employer, and AKM employer effects measure the systematic advantage (or disadvantage) from being employed by a given employer, after accounting for the composition of that employer's workforce (Kline 2025); Appendix E describes the procedure for estimating AKM employer effects.

[10] Because the follow-up administrative records available to us begin in the 1987:1, for claimants who claimed in 1986:3 we do not have employer information for the first quarter after the claim. Hence, employer characteristics in the claim year (year 0) are computed as averages using only the second, third, and fourth quarter after the claim. Also, because we do not have pre-claim employer-level information available, we cannot compute pre-claim employer size or average employer wage rates.

We also study whether assignment to JSA resulted in better long-term matches. The rationale for this is motivated by Lachowska, Mas, and Woodbury (2020), whose estimates suggest that lost match effects are a driving source of displaced workers' earnings losses. In this paper, our aim is to study whether JSA resulted in better employer-employee matches among workers assigned to the treatment, relative to those who were not. Appendix F describes the procedure for estimating match effects.

Inclusion of covariates **x** in (1) reduces sampling error and controls for observable differences between JSA and control groups that arise even under random assignment. The **x** vector includes the variables displayed in Appendix Table A1, the unemployment rate in the month in which the claim was filed, and indicators for the quarter in which the claim was filed (1986:3, 1986:4, 1987:1, 1987:2, or 1987:3).

3.2 The threat effect and the effects of JSA participation

To estimate the effect of participation, random-assignment is often used as an instrument for self-selected participation, producing a Local Average Treatment Effect (Bloom 1984; Angrist, Imbens, and Rubin 1996; Angrist and Pischke 2009). Table 1 shows that about 30 percent of displaced UI claimants assigned to JSA participated in a workshop, so instrumenting participation with assignment would magnify the IT effect by a factor of about $1/0.3 \approx 3.3$.

The above approach fails when assignment to treatment itself affects outcomes—that is, assignment has an effect outside the effect of program participation. As J&K discussed (1991, pp. 34–39), and as Black, Smith, Berger and Noel (2003) later showed, assignment to JSA can affect outcomes because receiving a notice to report for a two-day workshop raises the cost of

continuing to receive UI, creating an incentive to exit UI. If so, JSA could have a threat or hassle effect wholly apart from any effect it might have on improving job-search skills.

In the next section, we present evidence that the threat effect may play a role in JSA's estimated effects for displaced workers. Although the evidence is not strong, we refrain from using JSA assignment as an instrument for JSA participation, although we note that if we did, the treatment-on-the treated estimate would be about three times the IT estimate. Such a Wald-Bloom estimate of JSA participation represents an upper bound for the human capital effect of JSA.

4. Short-Term Effects of JSA on Displaced Workers

We begin this section by offering some evidence on the human capital and threat effects of JSA. To do this, we analyze the pattern of UI exit hazards for the JSA and control groups, in a manner similar to Black, Smith, Berger, and Noel (2003). We then present estimates of JSA effects on other short-term outcomes.

4.1 Effects of JSA on UI exit hazards and other short-term outcomes

The top panel of Figure 1 plots the two-week UI exit rates (that is, conditional probabilities of exiting the first post-claim UI spell within the next two weeks) for the control and JSA groups. The figure shows UI exit rates for two-week intervals because at the time of the experiment, Washington claimants received benefits every two weeks. For example, the hazard labeled "0–1" represents the conditional probability that claimants who received zero (to represent the so-called "waiting week" without any UI benefits) or one benefit payment exited UI and did not receive a second payment.

Recall that claimants assigned to JSA received a call-in notice four weeks after their claim (that is, in week 2–3 in the graph), and JSA services started on average about 2–3 weeks later and spanned four weeks. If JSA operated only through the workshop participation effect, we would expect the exit rates of the JSA claimants and the controls in Figure 1 to be indistinguishable from week 0–1 until about week 8–9 (when the JSA activities were over). Different exit rates after conclusion of the workshops in weeks 8–9 would then support the claim that JSA imparted job-search skills that helped claimants leave UI. If JSA operated through the threat or hassle effect, we would expect JSA claimants' hazards to be higher than the controls in week 2–3, as claimants assigned to JSA exited UI to avoid workshop attendance.

Figure 1 offers mixed evidence about the mechanism by which JSA operates for displaced workers. First, it shows that in all two-week periods until week 8–9 (that is, after the JSA activities), the UI-exit rate for JSA claimants exceeded that of controls. Although the differences (shown in the lower panel) are statistically insignificant, they are consistent with the JSA call-in notice having some effect on UI exit rates. Second, Figure 1 shows that in week 10–11 there is a large and statistically significant spike in the UI exit rate of JSA claimants, which would be consistent with JSA helping workers become reemployed. The pattern of UI exit hazards does not eliminate the possibility of a weak threat effect for displaced workers, but it does suggest that JSA participation per se improved the probability of reemployment.

Table 2 displays short-term treatment effects estimated using equation (1). For each of the ten short-term outcomes, the table shows the mean for the control group and the regression-adjusted difference between the control and JSA groups. Table 2 shows that the JSA group experienced reduced UI benefit receipt (by about \$192) in the first spell (p-value ≤ 0.10). For the remaining outcomes, JSA did not have an effect statistically different from zero.

5. Long-Term Effects of JSA on Displaced Workers

Figure 2(a) displays employment probabilities for displaced workers assigned to the JSA group (solid line) and the control group (dashed line).[11] Before the claim, there were no statistically significant differences in employment probability between the JSA and control groups, again consistent with successful randomization into treatment. Although the pre-experimental JSA-control differences are insignificant, it is worth noting that in years –3 to –1, the JSA group had slightly lower employment probabilities than the control group. After the experiment, the differences reverse, and the employment probabilities of the JSA group are always above those of controls.

The last row of Table 3, column (1), shows that on average, during the years 1–9, the JSA group's employment probability was 4-percentage points (or 6 percent; 0.042/0.68) higher than that of the control group. This percentage-point increase is similar to that reported by Manoli, Michaelides, and Patel (2018).[12]

Figure 2(b), shows the long-term pattern of work hours (unconditional on employment, so including zero hours) for the JSA group and controls, by year. The patterns in Figures 2(a) and 2(b) are similar: In the three years before the experiment, the JSA group worked slightly (and insignificantly) fewer hours, but after the experiment the pattern reverses, and JSA

[11] The lines representing the JSA group in Figure 2 are constructed by adding the estimated JSA effect to the control group mean for each outcome and year. The estimates of JSA effects are displayed in Table 3, columns (1)–(6), along with the control group mean for each year.
[12] To assess the statistical power of this point estimate, we applied the minimum detectable effects (MDE) method described in Bloom (1995). The MDE method provides a way to ex-post assess the power of an experiment. Applying the MDE method, we find that the average year 1–9 employment probability estimate is very close to the smallest positive estimate that, if true, will be statistically significant 70 percent of the time at the 10 percent level.

claimants work more hours. The differences are most pronounced (and have p-values ≤ 0.10) in years 6–7 after the experiment, after which the gap between the controls and JSA starts to close. Overall, the last row of Table 3, column (2), shows that in years 1–9 after the experiment, the JSA claimants worked on average 69 hours more per quarter (or 7 percent more; 68.6/941), but this point estimate is statistically insignificant (p-value ≈ 0.17).

Figure 2(c) shows mean earnings (unconditional on employment, so including zero earnings) for the JSA group and the controls, by year. Although after the experiment, JSA claimants consistently had higher earnings than controls, the differences are never statistically significant at conventional levels. The last row of Table 3, column (3), shows that in years 1–9 after the experiment, JSA claimants earned on average $695 more per quarter (or 6 percent; $695 /$11,571), but again this overall estimate is imprecise, and because the individual year-by-year estimated are imprecise as well, we cannot reject the hypothesis that JSA had no effect on post-treatment earnings.

The outcomes shown in Figures 2(a), 2(b), and 2(c) are unconditional on employment, defined as positive earnings, so they include zeros. A natural question is whether, conditional on employment, there are any differences between the JSA claimants and controls. (We do not refer to these differences as effects because they condition on employment—an outcome variable—and whenever JSA has a statistically significant effect on employment, there can be no claim of causality of JSA on any conditional-on-employment outcome). Figure 2(d) displays an event-study figure of log hourly wages in each year, constructed as the logarithm of the sum of earnings in a year divided by the sum of hours worked in that year. It shows no discernible difference between the log wages of the JSA and control groups. This finding suggests that the

(sometimes imprecisely estimated) increases in hours and earnings are driven by the employment margin, as shown in Figure 2(a).

Card, Kluve, and Weber (2010, 2018) have suggested that a pattern of treatment effects like that shown in Figure 2—where employment and earnings effects appear not immediately but over time—is the hallmark of a successful training program. This interpretation is consistent with JSA enhancing the job-search human capital of displaced workers and resulting in improved employability in the long term.

5.1 Long-term effects of JSA on employment quality

To study whether JSA is associated with employment with "better" employers, we examine the relationship between JSA assignment and three outcomes: employer size, average wage rates paid by employers, and AKM employer effect on wage rates. We also examine whether JSA-assigned workers obtained better employer-employee job matches than controls, as opposed to finding jobs with employers whose general characteristics are favorable. Figure 3 shows event-study plots of these outcomes for the JSA and control groups in years 0–9 after the experiment.

We find that post-treatment, displaced workers assigned to JSA tended to work for larger employers than the controls (Figure 3(a)). This employer-size difference emerges in Year 2 after the claim and is sustained in later years. It is worthwhile to note that in years 0–5 after the experiment, Because the JSA-control difference in employment *rates* is statistically zero in years 0–5 (Table 3, column 1), a statistically significant JSA-control difference in employer size in those years can more plausibly be interpreted as a causal effect of JSA on working for a larger employer.

Point estimates of the JSA-control differences in average employer wages (Figure 3(b)) are sizeable but not statistically different. Similarly, the JSA-control differences in AKM employer effects Figure 3(c)) are large, but except in year 5 for average wages, they are not statistically different from zero.

Finally, in order to measure whether assignment to JSA resulted in better idiosyncratic matches, we estimate the Woodcock (2015) orthogonal match effects model. We do not find statistically or economically significant differences in match effects—see Figure 3(d).

Together, these findings suggest that the modest increases in employment probability due to JSA could be associated with employment with "better" employers; however, the analysis is too underpowered to make strong claims. If we discount the noisiness of the estimates, the results seem to support the interpretation the JSA enhances the job-search human capital of displaced workers, as evidenced by employment with larger and better-paying employers.

6. Effects of JSA on UI Claimants as a Whole (Replication and Extension of J&K)

Our analysis has focussed on displaced workers, in contrast to J&K's, which focused on UI claimants as a whole. In this section, we contrast the short-term and long-term JSA estimates for displaced workers with estimates applied to the sample originally analyzed by J&K. These estimates are reported in Appendix B.

Appendix Figure B1 (which is analogous to Figure 1) shows that, for UI claimants as a whole in weeks 2–3, there is a clear spike in the difference between the exit rates of the JSA and control groups; that is, at the time claimants assigned to JSA received the call-in notices for the

workshop.[13] This contrasts with the UI exit pattern of displaced worker and suggests a significant threat effect of JSA for UI claimants as a whole.

Appendix Table B3 shows the estimated effects of JSA on other short-term outcomes for UI claimants as a whole. The estimates in this table come very close to the short-term estimates of J&K, who concluded that JSA led to a decrease in UI utilization and had a positive effect on employment and earnings, but only in the first quarter after the experiment.[14]

Most importantly, Appendix Figures B2 and B3 display the long-term effects of JSA for UI claimants as a whole. They show that assignment to JSA had no discernible effect on any long-term outcome. This is true even though, for claimants as a whole, the sample is larger and should provide more power to discern treatment effects. We also find a lack of any statistically significant long-term effects if we exclude displaced workers from the overall group of UI claimants.[15]

In summary, for the group of claimants originally studied by J&K, JSA appears to have worked mainly by increasing the cost of continuing to claim UI, motivating them either to

[13] As in Figure 1, there is also a spike in week 10-11, which may suggest a human capital effect of JSA, but this interpretation is tenuous because of dynamic selection out of UI—that is, more JSA claimants than controls exited UI in week 2-3, so by week 10-11 we cannot claim that the JSA and the control groups are on average comparable. The UI exit patterns in Appendix Figure B1 are almost identical if we exclude displaced workers from the group of UI claimants as a whole. Further inspection suggests that the threat effect is driven mainly by claimants on temporary layoff; see Appendix Figure C1.

[14] We find that for the full group of claimants, JSA led to a $96 decrease in UI benefit receipt in the first spell; the corresponding estimate in J&K is $101. Appendix Table B3 shows that JSA also led to a decrease in duration of UI receipt by about a 0.5 week; the corresponding estimate in J&K is 0.66 weeks. Finally, in quarter 1, it led to a 1.7 percentage point increase employment probability; the corresponding estimate in J&K is 1.6 percentage points.

[15] In Appendix C we present the short-term and long-term JSA effects for claimants on temporary layoff. In the short-term, no point estimate is statistically different from zero (Appendix Table C3). Although the point estimates in Appendix Table C4 show that these claimants had higher employment probability in year 3 and 6 after the claim, Appendix Figures C2 and C3 show no pattern of a sustained increase for any of the long-term outcomes.

become reemployed quickly or exiting UI even without reemployment, but leading to no permanent increase in employability.

7. Summary and Discussion

We have used data from the Washington Alternative Work Search experiment (Johnson and Klepinger 1994), merged with nine years of follow-up administrative records, to estimate the long-term effects of assignment to JSA on employment, hours, earnings, and employer characteristics of displaced workers. The main findings show that displaced UI claimants assigned to JSA experienced a 6 percent increase in their long-term employment probability on average over the nine years following the experiment. We also examine the relationship between JSA and employer characteristics typically associated with employer quality and find that displaced workers assigned to JSA tended to work for larger post-claim employers and better-paying employers (although the latter estimates are noisy). This contrasts with the experience of UI claimants as a whole, for whom JSA had an effect only on employment and earnings in the quarter following assignment to JSA.

The results for displaced workers and for UI claimants as a whole may throw light on differences in the mechanisms by which JSA affects these two groups of workers. For displaced workers, the results point to the possibility that JSA enhances the job-search human capital of workers, making them better able to find jobs with better long-term prospects. In contrast, the evidence for UI claimants as a whole suggests strongly that JSA operates mainly through a threat effect, increasing the cost of continuing to claim UI benefits and motivating workers to exit UI quickly. The contrast between the effects of JSA on displaced workers and UI claimants as a

whole also highlights the importance of focusing evaluations on the population for whom the policy is intended—in this case, the displaced workers for whom JSA was intended.

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Results

Table 1:
Job search assistance, eligibility review interviews, and employment services received by displaced claimants assigned to the control and JSA groups

	(1)	(2)	(3)	(4)
Service	Assigned to Control	Assigned to JSA	JSA participant	*p*-value of test for difference between: (1) and (2)
Job search assistance (JSA) workshop	0.002	0.302	1.000	0.000
Eligibility review interview (ERI)	0.353	0.233	0.650	0.000
Employment services[a]				
job referral/placement	0.216	0.202	0.256	0.591
job development plan	0.188	0.090	0.222	0.000
other employment service[b]	0.122	0.090	0.128	0.140
Sample size	485	387	117	

Notes: The sample consists of displaced eligible UI claimants assigned to the control and JSA groups.
a. A claimant may receive services from more than one category.
b. Job consultation, receipt of or referral to training, testing, support services, job development (contacting an employer on the claimant's behalf), or any other contact with the Employment Service.

Table 2:
Estimated short-term effects of JSA assignment for displaced claimants on selected outcomes

Outcome	Control group mean	JSA effect (robust std. error)	
UI outcomes (first spell)			
Benefits paid ($)	2,165	-192*	(111)
Weeks paid	16.39	-0.98	(0.80)
Exhausted benefits (proportion)	0.336	-0.045	(0.033)
Short-term employment outcomes (unconditional)			
Quarter 1 outcomes[a]			
Employed (proportion)	0.577	0.000	(0.034)
Hours worked	154.5	13.7	(14.3)
Earnings ($)	1,659	8.58	(159.24)
Year 0 outcomes[a]			
Employed (proportion)	0.825	0.027	(0.026)
Hours worked	821.3	75.2	(51.6)
Earnings ($)	8,920	555	(594)
Other outcomes			
Returned to same employer (proportion)	0.128	0.003	(0.024)
Returned to same industry (proportion)	0.258	0.014	(0.030)
Sample size	485	387	

Notes: The sample consists of displaced claimants assigned to the control and JSA groups. Estimated effects are regression-adjusted differences (based on equation 1) controlling for all variables displayed in Table A1 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed. Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10.

a. Quarter 1 is the quarter following the quarter in which the UI claim was filed. Year 0 refers to the sum of quarters 1–4. Earnings are in 1988:4 dollars.

Table 3 Estimated long-term effects of JSA assignment on employment outcomes for displaced workers, by year

Dependent variable	(1) Employment		(2) Hours worked		(3) Earnings		(4) log wage rates	
	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect
Year since claim								
–3	0.761	-0.013	916	-14.1	10,261	-277	2.196	-0.035
		(0.029)		(51.9)		(652)		(0.027)
–2	0.874	-0.029	1,138	-24.2	12,186	-744	2.148	0.005
		(0.021)		(44.4)		(549)		(0.031)
–1	0.965	-0.015	1,377	17.9	10,261	-277	2.225	-0.054**
		(0.013)		(27.7)		(652)		(0.024)
0	0.825	0.027	821	75.2	8,920	555	2.231	-0.014
		(0.026)		(51.6)		(594)		(0.039)
1	0.798	0.010	1,028	44.4	11,326	541	2.264	-0.017
		(0.029)		(58.8)		(701)		(0.040)
2	0.761	0.029	1,028	41.6	12,189	293	2.320	-0.033
		(0.030)		(60.8)		(745)		(0.042)
3	0.74	0.025	1,001	53.0	11,989	401	2.326	-0.011
		(0.031)		(61.8)		(786)		(0.044)
4	0.711	0.025	1,006	33.6	12,497	-22	2.356	-0.060
		(0.032)		(64.4)		(825)		(0.036)
5	0.678	0.033	944	96.0	12,011	733	2.349	-0.008
		(0.032)		(63.4)		(835)		(0.038)
6	0.637	0.069**	880	132.1**	11,179	1,260	2.354	-0.029
		(0.032)		(63.5)		(877)		(0.041)
7	0.619	0.069**	866	107.1*	10,913	1,363	2.365	0.001
		(0.033)		(62.1)		(852)		(0.061)
8	0.588	0.057*	844	87.9	10,767	1,098	2.359	-0.019
		(0.033)		(64.0)		(875)		(0.047)
9	0.586	0.058*	872	22.1	11,268	587	2.350	0.056
		(0.033)		(62.6)		(889)		(0.054)
Years 1–9 (average)	0.68	0.042*	941	68.6	11,571	695	2.374	-0.032
		(0.025)		(49.7)		(697)		(0.038)

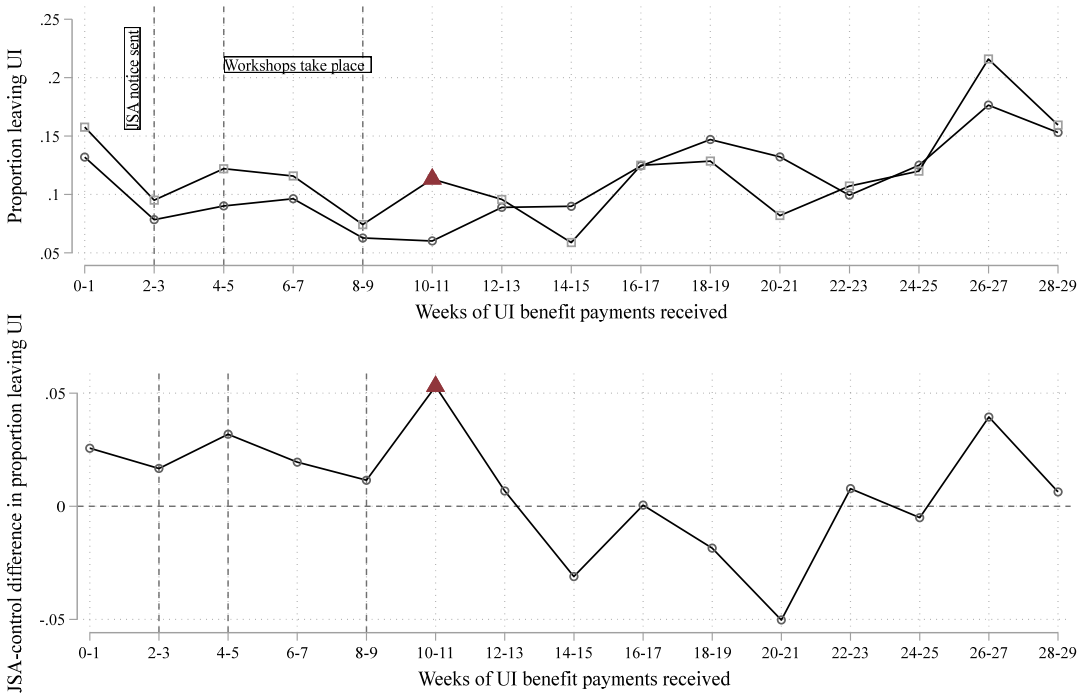
Notes: Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Sample consists of displaced claimants assigned to the control and JSA groups. A separate regression is estimated for each year and for each outcome. Estimated effects are regression-adjusted differences controlling for all variables displayed in Table A1 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed. Earnings are in 1988:4 dollars.

Table 4 Estimated long-term effects of JSA assignment on employment quality for displaced workers, by year

Dependent variable	(1) log employer size		(2) Average log wage rate of coworkers		(3) Average firm effect		(4) Match effect	
	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect
Year since claim								
0	4.375	0.252	2.222	-0.003	-0.00262	0.004	-0.0248	0.016
		(0.182)		(0.029)		(0.017)		(0.022)
1	4.468	0.222	2.236	0.015	0.00120	0.023	-0.00674	0.012
		(0.187)		(0.034)		(0.019)		(0.021)
2	4.583	0.377*	2.287	-0.022	0.0212	0.013	0.00540	0.009
		(0.195)		(0.037)		(0.020)		(0.021)
3	4.604	0.659***	2.278	0.004	0.0185	0.015	0.0153	0.006
		(0.209)		(0.039)		(0.020)		(0.018)
4	4.607	0.677***	2.267	0.034	0.0109	0.027	0.00737	-0.011
		(0.222)		(0.042)		(0.021)		(0.015)
5	4.741	0.407*	2.276	0.042	0.0135	0.043**	-0.00331	0.002
		(0.231)		(0.041)		(0.020)		(0.015)
6	4.618	0.512**	2.252	0.047	0.00373	0.035	-0.00703	-0.001
		(0.231)		(0.044)		(0.022)		(0.017)
7	4.601	0.476**	2.264	0.038	0.00649	0.030	-0.00625	0.007
		(0.218)		(0.043)		(0.020)		(0.015)
8	4.701	0.498**	2.279	0.048	0.0183	0.025	-0.00349	-0.014
		(0.240)		(0.048)		(0.022)		(0.018)
9	4.820	0.451*	2.314	0.031	0.0303	0.020	-0.0129	0.006
		(0.248)		(0.053)		(0.023)		(0.022)
Years 1–9 (average)	4.566	0.368**	2.244	0.006	0.00136	0.012	-0.00821	0.017
		(0.150)		(0.029)		(0.015)		(0.012)

Notes: Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Sample consists of displaced claimants assigned to the control and JSA groups. A separate regression is estimated for each year and for each outcome. Estimated effects are regression-adjusted differences controlling for all variables displayed in Table A1 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed. Earnings are in 1988:4 dollars. Employment-quality outcomes are not available in the pre-claim years; see the text for details.

Figure 1: Biweekly UI exit rates (hazards) for displaced claimants assigned to the control and JSA groups (first spell)

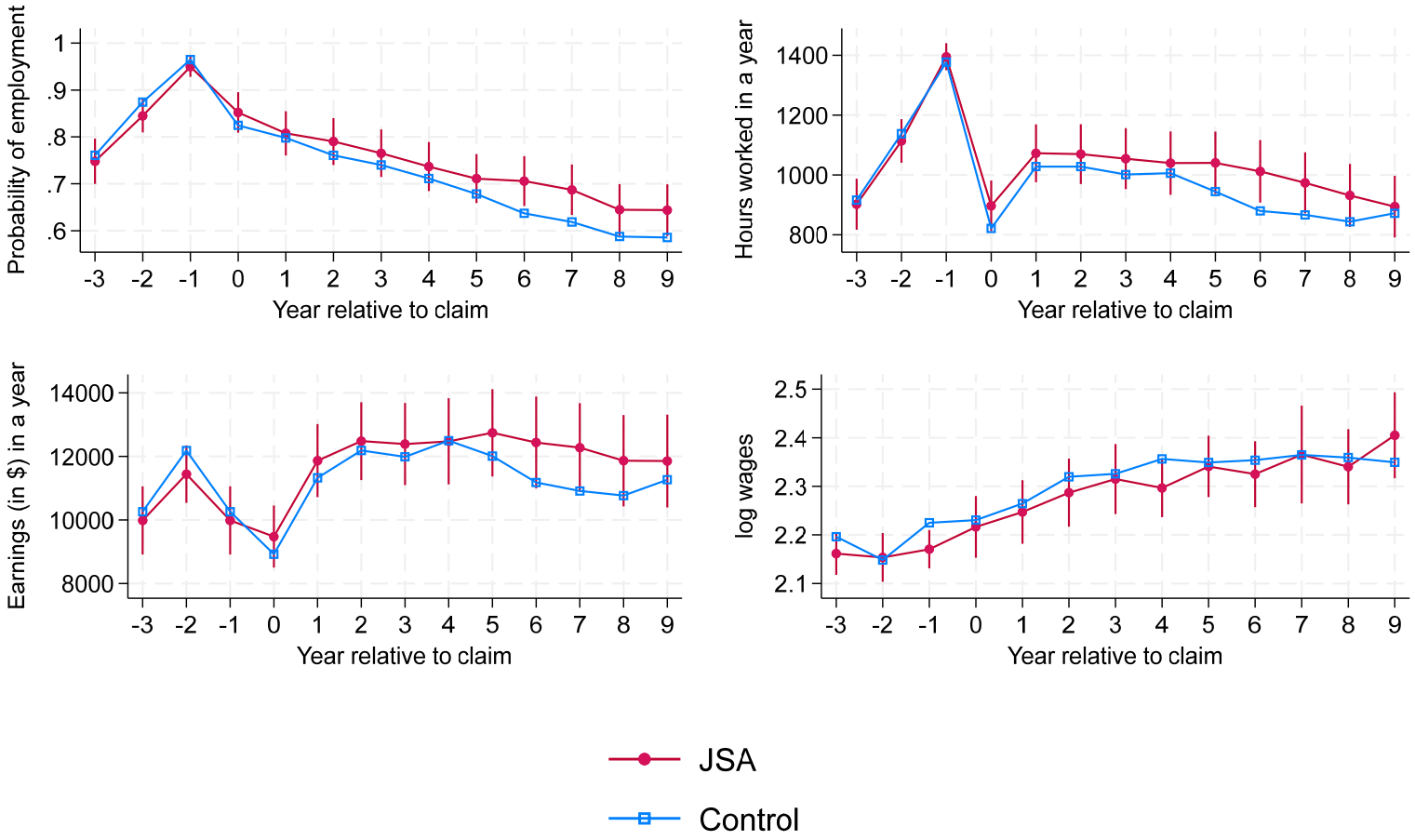


Notes: The sample consists of displaced claimants assigned to the control and JSA treatment groups. The top figure shows UI exit rates for the two groups—the squares denote the JSA group and the circles denote the controls. The bottom figure shows the difference between the exit rates of the JSA and control groups. Black triangles denote statistically significant differences between the JSA and control groups at the 5-percent level; black dots (not shown) denote statistically significant differences at the 10-percent level.

The UI exit rates are plotted as functions of bi-weekly UI payments because at the time of the experiment, Washington claimants "certified" for two weeks at a time. UI exit rates labeled "0-1" represent the conditional probability that claimants who experienced zero or one week of insured unemployment exited UI before receiving a second benefit check; UI exit hazards labeled "2-3" represent the conditional probability that claimants who experienced two or three weeks of insured unemployment exited UI before receiving a third benefit check, etc.

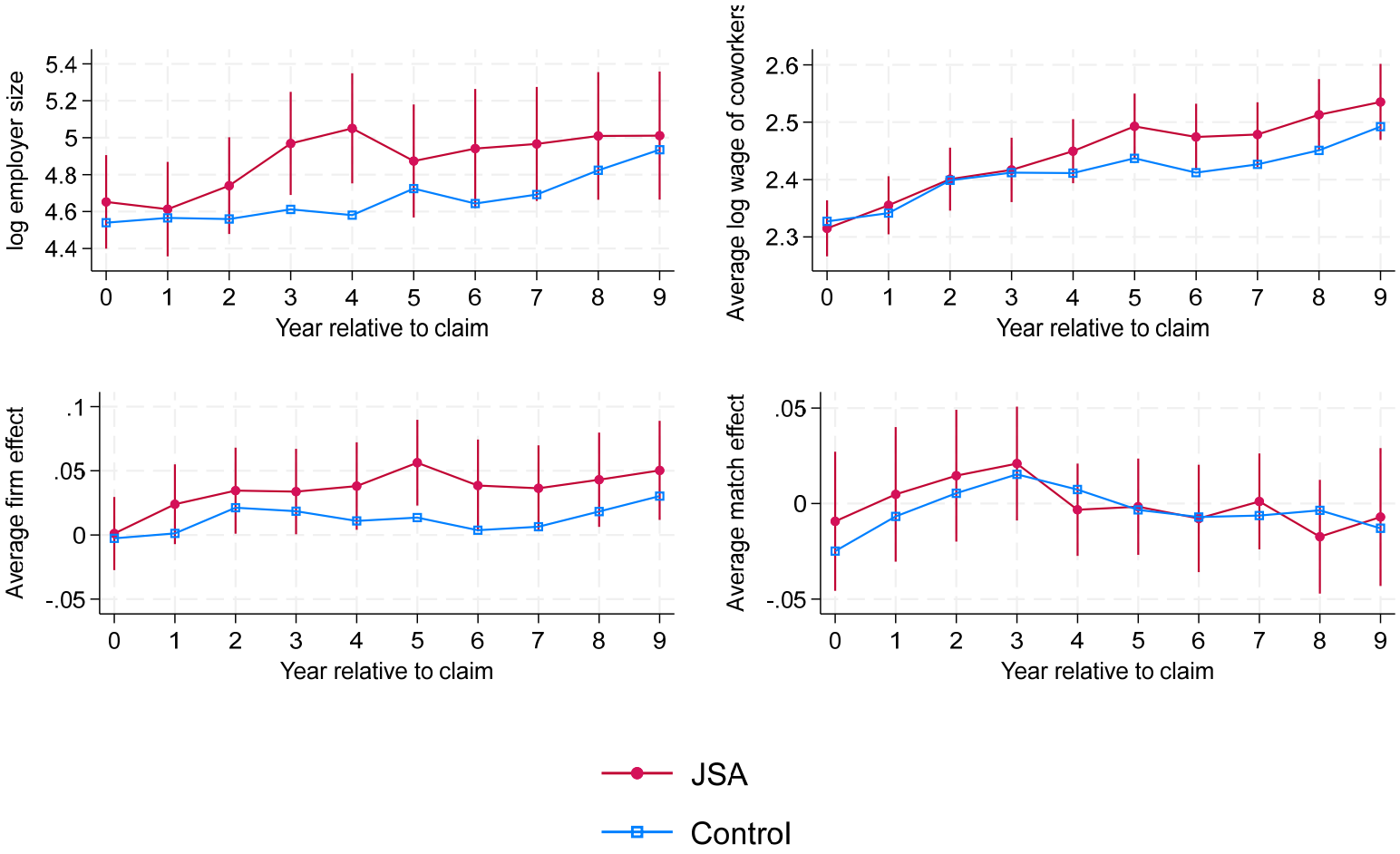
The JSA workshop call-in notice was sent about four weeks following the claim—that is, around the second UI payment, or weeks 2-3 in the graph. Most JSA workshops occurred between weeks 4-5 and 8-9. ERI call-in notices were sent to controls in weeks 10-11.

Figure 2: Long-term employment outcomes for displaced JSA claimants and controls before and after the claim year



Notes: The sample consists of displaced claimants assigned to the control and JSA group. The blue line with square markers represents the outcome (by year) of claimants assigned to the control group. The red line with circle markers represents the estimated effect of JSA on the outcome added to the control group average. The vertical bars are 90-percent confidence intervals, based on heteroskedasticity-adjusted robust standard errors. The estimated effect is from regressions that control for all variables displayed in Table A1 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed. Earnings are in 1988:4 dollars. The point estimates are presented in Table 3.

Figure 3: Long-term employment-quality outcomes for displaced JSA claimants and controls after the claim year



Notes: The sample consists of displaced claimants assigned to the control and JSA group. The blue line with square markers represents the outcome (by year) of claimants assigned to the control group. The red line with circle markers represents the estimated effect of JSA on the outcome added to the control group average. The vertical bars are 90-percent confidence intervals, based on heteroskedasticity-adjusted robust standard errors. The estimated effect is from regressions that control for all variables displayed in Table A1 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed. Earnings are in 1988:4 dollars. The point estimates are presented in Table 4.

The Long-Term Effects of Job Search Assistance for Displaced Workers: Appendices

Marta Lachowska, Merve Meral, and Stephen A. Woodbury

2025-10-14

Appendices

Appendix A: Other Results for Displaced Claimants

This appendix presents the summary statistics (referenced in the main text) for the sample of displaced claimants.

Table A1:
Sample descriptive statistics and tests of differences between displaced claimants assigned to the control group or JSA, and JSA participants

	(1)	(2)	(3)	(4)
	Sample proportions and means			
Covariate	Assigned to Control	Assigned to JSA	JSA participants	*p*-value of test for difference between (1) and (2)
Male	0.614	0.594	0.479	0.546
Race				
white	0.839	0.902	0.906	**0.007**
black	0.089	0.052	0.060	**0.036**
other	0.072	0.047	0.034	0.115
Age				
≤ 24	0.198	0.178	0.162	0.462
25-34	0.346	0.351	0.265	0.877
35-44	0.258	0.282	0.350	0.429
45-54	0.126	0.124	0.162	0.938
≥ 54	0.072	0.065	0.060	0.661
Schooling				
less than high school	0.132	0.106	0.103	0.241
high school	0.493	0.517	0.530	0.482
some college	0.262	0.269	0.239	0.819
college graduate	0.113	0.109	0.128	0.820
Veteran	0.214	0.243	0.214	0.319
Marital status/gender				
married male	0.328	0.318	0.248	0.754
married female	0.171	0.178	0.265	0.782
Household status				
no dependents	0.468	0.463	0.444	0.871
1 dependent	0.216	0.181	0.205	0.192
2 or more dependents	0.262	0.295	0.299	0.284
homeowner	0.396	0.416	0.462	0.548
Occupation				
professional	0.194	0.222	0.248	0.304
clerical	0.194	0.178	0.214	0.560
sales	0.099	0.083	0.085	0.409
service	0.107	0.134	0.145	0.220
agric., fishery, forestry	0.010	0.010	0.000	0.997

processing	0.039	0.013	0.017	**0.019**
machine trades	0.074	0.072	0.060	0.916
benchwork	0.014	0.049	0.043	**0.003**
structural work	0.167	0.163	0.077	0.868
miscellaneous	0.101	0.075	0.111	0.180
Industry				
agriculture	0.008	0.013	0.009	0.498
mining	0.000	0.000	0.000	–
construction	0.138	0.096	0.077	0.054
manufacturing	0.179	0.181	0.145	0.954
transportation, utilities	0.045	0.036	0.017	0.499
wholesale trade	0.078	0.088	0.128	0.613
retail trade	0.237	0.251	0.248	0.644
finance, ins., real estate	0.033	0.057	0.060	0.087
services	0.235	0.225	0.274	0.722
government	0.023	0.031	0.017	0.446
unclassified	0.023	0.023	0.026	0.955
Prior UI claim				
none	0.887	0.886	0.932	0.989
duration ≤ 15 weeks	0.033	0.026	0.034	0.538
duration > 15 weeks	0.080	0.088	0.034	0.694
Employer-attached/placed by union	0.181	0.191	0.085	0.713
UI benefits/claim type				
weekly amount ($)	145.61	140.34	142.79	0.128
maximum amount ($)	3,931	3,780	4,029	0.195
potential duration	26.51	26.44	27.79	0.841
replacement rate (percent)[2]	61.46	59.97	55.81	0.313
combined wage claim[3]	0.054	0.044	0.034	0.512
ex-service member claim	0.006	0.008	0.000	0.781
federal employee claim	0.012	0.021	0.009	0.333
Reservation wage (hourly)				
≤ $5.00	0.268	0.271	0.282	0.914
$5.01–$7.00	0.206	0.199	0.222	0.793
$7.01–$10.00	0.188	0.212	0.248	0.373
$10.01–$20.00	0.167	0.178	0.162	0.661
> $20.00	0.113	0.080	0.051	0.102
Sample size	485	387	117	

Notes: The sample consists of displaced claimants assigned to the control and JSA groups.
1. Bold denotes *p*-values for the test of mean differences between groups < .05.
2. The replacement rate is the weekly benefit amount as a percentage of average weekly earnings before the UI claim.
3. Combined wage claims use earnings from more than one state to calculate base period earnings.

Appendix B: Results for UI Claimants as a Whole (Regardless of Reason for Job Loss)

This appendix presents summary statistics and estimates from our replication of Johnson and Klepinger's (J&K) (1994) main results. It additionally shows estimates of long-term effects of JSA, not studied by J&K. Note that the sample used by J&K included all claimants, regardless of reason for job loss (displaced claimants, claimants on temporary layoff, contract or seasonal workers, workers who quit for good cause, and workers whose reason for job loss is unknown; see Table B2 for the relative proportions of different groups of claimants).

Table B1:
Job search assistance, eligibility review interviews, and employment services received by any claimant assigned to the control group and the JSA group

Service	(1) Assigned to Control	(2) Assigned to JSA	(3) JSA participant	(4) *p*-value of test for difference between (1) and (2)
Job search assistance (JSA) workshop	0.001	0.184	1.000	0.000
Eligibility review interview	0.248	0.159	0.649	0.000
Employment services[a]				
job referral/placement	0.191	0.165	0.219	0.013
job development plan	0.140	0.055	0.174	0.000
other employment service[b]	0.105	0.065	0.111	0.000
Sample size	2,852	2,548	470	

Notes: The sample consists of all claimants (regardless of reason for job loss) who were assigned to the control and JSA groups.
a. A claimant may receive services from more than one category.
b. Job consultation, receipt of or referral to training, testing, support services, job development (contacting an employer on the claimant's behalf), or any other contact with the Employment Service.

Table B2:
Sample descriptive statistics and tests of differences between all claimants (regardless of reason for job loss) assigned to the control group or JSA, and JSA participants

	(1)	(2)	(3)	(4)
	Sample proportions and means			
Covariate	Assigned to Control	Assigned to JSA	JSA participant	*p*-value of test for difference between (1) and (2)
Male	0.694	0.703	0.626	0.472
Race				
white	0.824	0.838	0.811	0.184
black	0.098	0.090	0.111	0.318
other	0.078	0.072	0.079	0.434
Age				
≤ 24	0.208	0.205	0.168	0.806
25-34	0.369	0.389	0.349	0.115
35-44	0.250	0.241	0.285	0.405
45-54	0.113	0.108	0.130	0.592
≥ 54	0.061	0.057	0.068	0.559
Schooling				
less than high school	0.147	0.128	0.119	**0.039**
high school	0.527	0.546	0.519	0.163
some college	0.238	0.239	0.228	0.909
college graduate	0.088	0.087	0.134	0.904
Veteran	0.242	0.232	0.211	0.387
Marital status/gender				
married male	0.260	0.256	0.217	0.767
married female	0.098	0.097	0.157	0.878
Household status				
no dependents	0.307	0.310	0.334	0.773
1 dependent	0.150	0.129	0.128	**0.027**
2 or more dependents	0.228	0.232	0.223	0.748
homeowner	0.282	0.263	0.277	0.119
Occupation				
professional	0.124	0.134	0.202	0.288
clerical	0.140	0.132	0.177	0.436
sales	0.055	0.055	0.077	0.987
service	0.109	0.125	0.157	0.065
agric., fishery, forestry	0.021	0.019	0.006	0.575
processing	0.038	0.031	0.026	0.169
machine trades	0.084	0.071	0.074	0.071
benchwork	0.041	0.048	0.038	0.198
structural work	0.254	0.257	0.123	0.810
miscellaneous	0.135	0.128	0.119	0.494
Industry				

agriculture	0.020	0.021	0.019	0.686
mining	0.002	0.000	0.002	0.134
construction	0.196	0.177	0.085	0.086
manufacturing	0.225	0.220	0.168	0.642
transportation, utilities	0.046	0.051	0.045	0.419
wholesale trade	0.061	0.049	0.068	0.064
retail trade	0.162	0.164	0.170	0.811
finance, ins., real estate	0.026	0.033	0.055	0.108
services	0.183	0.191	0.268	0.466
government	0.060	0.070	0.096	0.126
unclassified	0.020	0.022	0.023	0.547
Prior UI claim				
none	0.812	0.817	0.902	0.611
duration ≤ 15 weeks	0.092	0.081	0.032	0.153
duration > 15 weeks	0.096	0.102	0.066	0.493
Reason for job loss				
permanent layoff	0.170	0.152	0.249	0.070
temporary layoff with recall date	0.205	0.220	0.140	0.178
contract/seasonal	0.157	0.141	0.096	0.112
quit for good cause	0.177	0.195	0.234	0.090
Employer-attached/placed by union	0.342	0.343	0.119	0.951
UI benefits/claim type				
weekly amount ($)	147	146	145	0.407
maximum amount ($)	3,871	3,829	3,815	0.377
potential duration	26	26	26	0.973
replacement rate (percent)[2]	62	62	62	0.965
combined wage claim[3]	0.049	0.055	0.062	0.334
ex-service member claim	0.038	0.042	0.057	0.483
federal employee claim	0.018	0.020	0.026	0.501
Reservation wage (hourly)				
≤ $5.00	0.179	0.181	0.198	0.870
$5.01–$7.00	0.138	0.148	0.179	0.286
$7.01–$10.00	0.149	0.150	0.170	0.926
$10.01–$20.00	0.136	0.120	0.096	0.088
> $20.00	0.092	0.078	0.060	0.072
Sample size	2,852	2,548	470	

Notes: The sample consists of all claimants (regardless of reason for job loss) assigned to the control and JSA groups.
1. Bold denotes *p*-values for the test of mean differences between groups < .05.
2. The replacement rate is the weekly benefit amount as a percentage of average weekly earnings before the UI claim.
3. Combined wage claims use earnings from more than one state to calculate base period earnings.

Table B3:
Estimated short-term effects of JSA assignment for all claimants on selected outcomes

Outcome	Control group mean	JSA effect (robust std. error)	
UI outcomes (first spell)			
Benefits paid ($)	1,762	-96**	(42)
Weeks paid	14.02	-0.54*	(0.29)
Exhausted benefits (proportion)	0.26	-0.009	(0.012)
Short-term employment outcomes (unconditional)			
Quarter 1 outcomes[a]			
Employed (proportion)	0.653	0.006	(0.013)
Hours worked	191.3	5.6	(5.7)
Earnings ($)	2,108	63.10	(65.44)
Year 0 outcomes[a]			
Employed (proportion)	0.859	0.017*	(0.009)
Hours worked	937.7	23.8	(19.5)
Earnings ($)	10,606	408*	(230)
Other outcomes			
Returned to same employer (proportion)	0.285	0.003	(0.011)
Returned to same industry (proportion)	0.396	0.011	(0.012)
Sample size	2,852	2,548	

Notes: Sample consists of all claimants (regardless of reason for job loss) assigned to the control and JSA groups. Estimated effects are regression-adjusted differences (based on equation 1) controlling for all variables displayed in Table B2 plus the quarter in which the claim was filed and the unemployment rate in month in which the claim was filed. Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10.

a. Quarter 1 is the quarter following the quarter in which the UI claim was filed. Year 0 refers to the sum of the first, second, third, and fourth quarters after the claim quarter. Earnings are in 1988:4 dollars.

Table B4: Estimated long-term effects of JSA assignment on employment outcomes for all claimants, by year

Dependent variable	(1) Employment		(2) Hours worked		(3) Earnings		(4) log wage rates	
	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect
Year since claim								
–3	0.761	-0.003	906	-2.4	10,307	184	2.215	0.005
		(0.011)		(18.8)		(246)		(0.011)
–2	0.864	-0.003	1076	-6.8	11,762	56	2.185	0.012
		(0.008)		(16.5)		(209)		(0.009)
–1	0.944	0.004	1322	-2.2	10,307	184	2.242	-0.006
		(0.005)		(11.8)		(246)		(0.010)
0	0.859	0.017*	937.7	23.8	10,606	408*	2.290	0.019
		(0.009)		(19.5)		(230)		(0.017)
1	0.825	0.000	1088	21.7	12,621	376	2.339	-0.009
		(0.010)		(22.3)		(272)		(0.015)
2	0.796	0.006	1098	4.2	13,367	108	2.377	-0.008
		(0.011)		(23.3)		(291)		(0.016)
3	0.771	0.006	1070	9.2	13,261	24	2.401	-0.013
		(0.011)		(23.6)		(305)		(0.017)
4	0.745	-0.003	1036	-4.7	13,129	-150	2.395	-0.002
		(0.012)		(23.7)		(321)		(0.017)
5	0.712	-0.003	993	5.4	12,705	214	2.408	-0.012
		(0.012)		(24.1)		(330)		(0.017)
6	0.678	0.012	944.8	20.4	12,015	213	2.405	-0.008
		(0.012)		(24.3)		(334)		(0.017)
7	0.658	0.014	941	15.4	11,896	249	2.390	0.013
		(0.013)		(24.4)		(337)		(0.018)
8	0.641	0.001	902.5	17.1	11,722	260	2.430	0.004
		(0.013)		(24.7)		(345)		(0.021)
9	0.627	0.008	910.9	10.2	11,994	137	2.425	0.032
		(0.013)		(24.8)		(353)		(0.020)
Years 1–9 (average)	0.717	0.005	998.3	11.0	12,523	159	2.398	0.003
		(0.009)		(19.0)		(269)		(0.013)

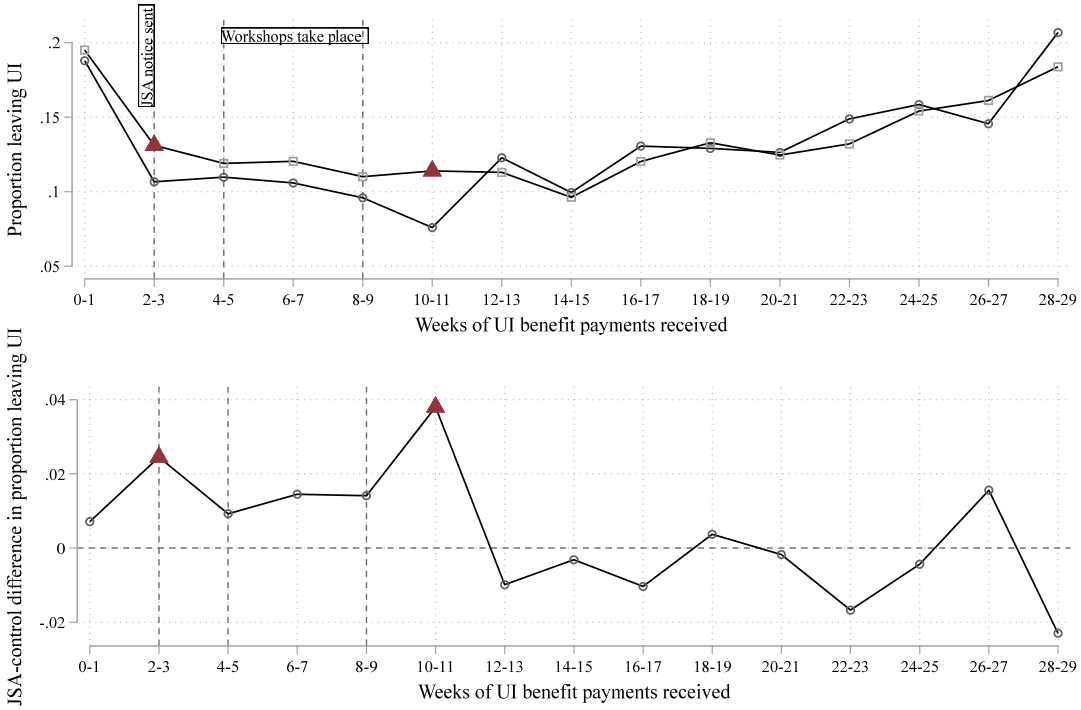
Notes: Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Sample consists of all claimants (regardless of reason for job loss) assigned to the control and JSA groups. A separate regression is estimated for each year and for each outcome. Estimated effects are regression-adjusted differences controlling for all variables displayed in Table B2 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed. Earnings are in 1988:4 dollars.

Table B5: Estimated long-term effects of JSA assignment on employment quality for all claimants, by year

Dependent variable	(1) log employer size		(2) Average log wage rate of coworkers		(3) Average firm effect		(4) Match effect	
	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect
Year since claim								
0	4.570	-0.027	2.292	0.007	0.0546	-0.001	-0.00962	-0.001
		(0.063)		(0.013)		(0.007)		(0.008)
1	4.747	-0.033	2.310	0.009	0.0645	0.001	-0.00106	-0.003
		(0.066)		(0.012)		(0.007)		(0.006)
2	4.869	-0.045	2.337	0.004	0.0752	0.003	0.0108	-0.003
		(0.071)		(0.013)		(0.007)		(0.006)
3	4.941	-0.035	2.353	-0.012	0.0793	-0.004	0.0210	-0.008
		(0.075)		(0.013)		(0.007)		(0.005)
4	4.928	0.066	2.353	0.002	0.0765	-0.003	0.0107	-0.003
		(0.079)		(0.014)		(0.008)		(0.005)
5	4.944	0.041	2.353	0.008	0.0760	0.003	0.00466	0.002
		(0.082)		(0.013)		(0.008)		(0.005)
6	4.958	0.039	2.359	-0.007	0.0768	-0.009	0.00349	-0.004
		(0.084)		(0.014)		(0.008)		(0.006)
7	4.972	-0.037	2.351	0.009	0.0648	0.004	0.00136	-0.000
		(0.082)		(0.015)		(0.008)		(0.006)
8	4.983	-0.041	2.364	0.019	0.0727	0.003	-0.00214	0.001
		(0.084)		(0.015)		(0.008)		(0.006)
9	5.023	0.062	2.377	0.013	0.0771	0.002	-0.00395	0.004
		(0.088)		(0.016)		(0.008)		(0.007)
Years 1–9 (average)	4.808	-0.012	2.310	0.001	0.0532	-0.000	0.00157	-0.000
		(0.056)		(0.010)		(0.006)		(0.006)

Notes: Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Sample consists of all claimants (regardless of reason for job loss) assigned to the control and JSA groups. A separate regression is estimated for each year and for each outcome. Estimated effects are regression-adjusted differences controlling for all variables displayed in Table B2 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed. Earnings are in 1988:4 dollars. Employment quality outcomes are not available in the pre-claim years; see the text for details.

Figure B1: Biweekly UI exit rates (hazards) for all claimants assigned to the control and JSA groups (first spell)

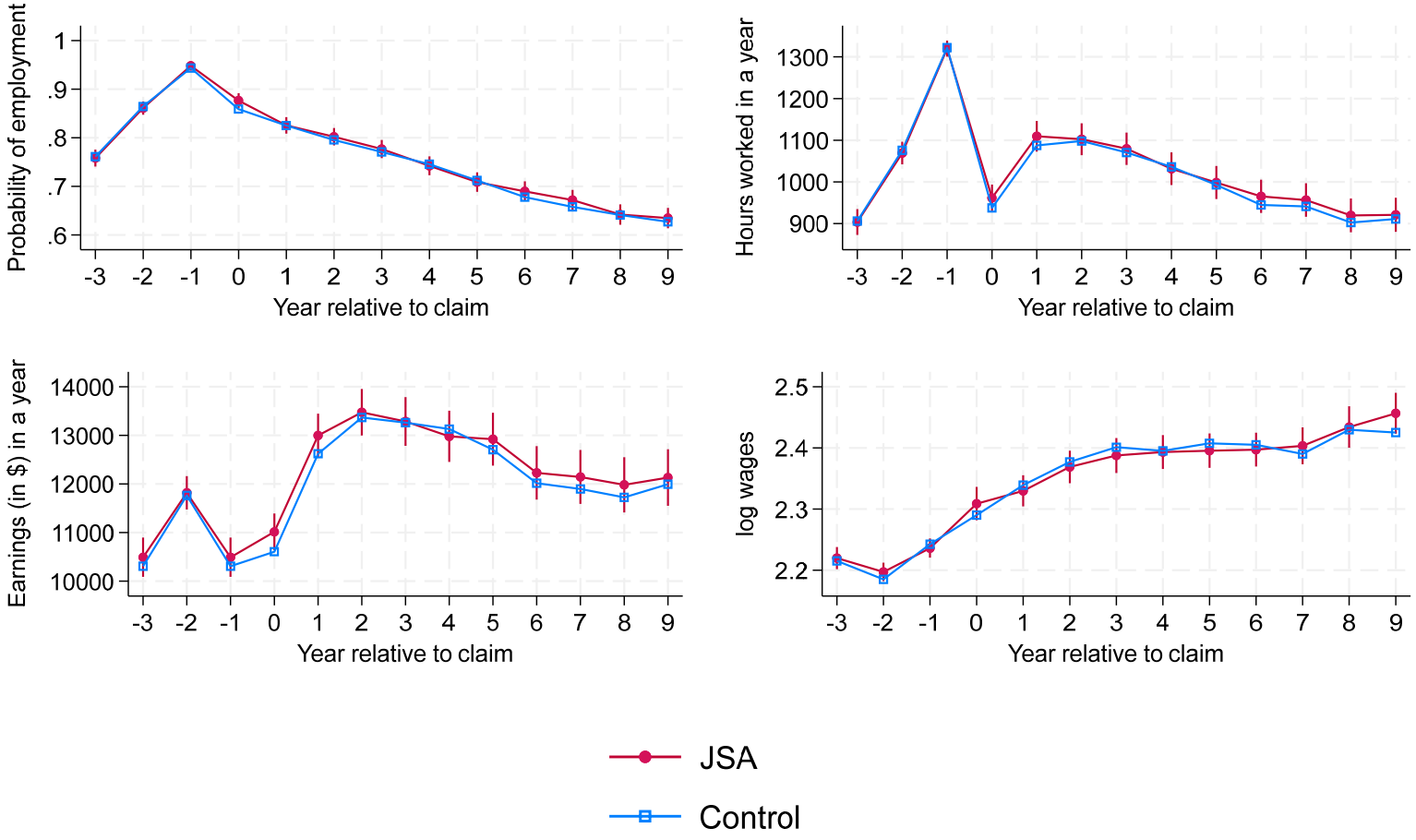


Notes: The sample consists of displaced claimants assigned to the control and JSA group. The top figure shows the exit rate (hazard) for the two groups—the squares denote the JSA group and the circles denote the controls. The bottom figure shows the difference in exit rate between JSA and the controls. Black triangles denote statistically significant differences between JSA and the controls at the 5-percent level. Black dots (not shown) denote statistically significant differences between JSA and the controls at the 10-percent level.

The hazards are plotted as functions of bi-weekly UI payments because in Washington state, claimants "certify" for two weeks at a time. UI exit hazards labeled "0-1" represent the conditional probability that claimants who experienced zero or one week of insured unemployment exited UI before receiving a second benefit check; UI exit hazards labeled "2-3" represent the conditional probability that claimants who experienced two or three weeks of insured unemployment exited UI before receiving a third benefit check, etc.

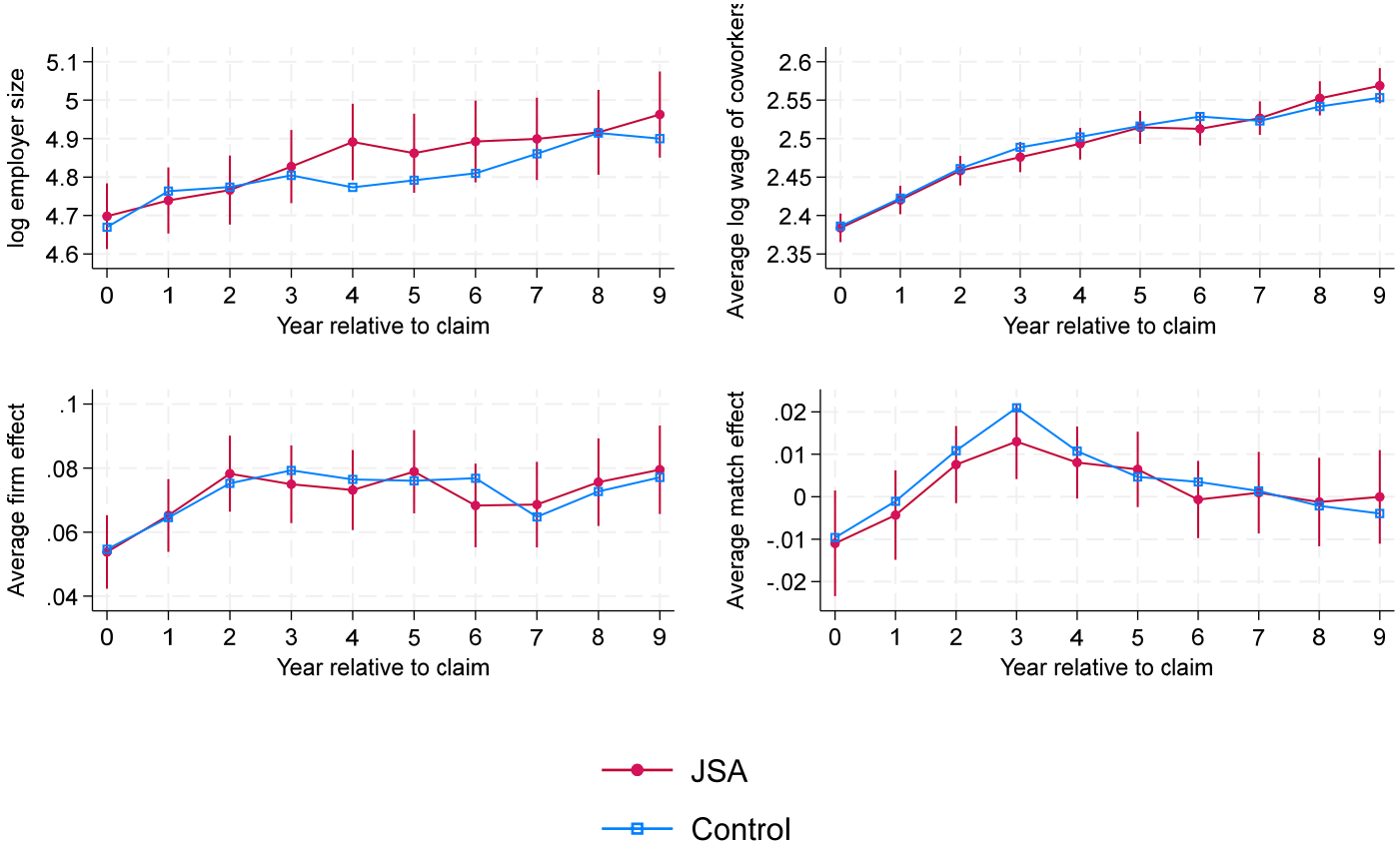
The JSA workshop call-in notice was sent about four weeks following the claim—that is, around the second UI payment, or week 2-3 in the graph. Most JSA workshops occurred between weeks 4-5 and 8-9 in the graph. ERI call-in notices were sent to controls in weeks 10-11.

Figure B2: Long-term outcomes for all JSA claimants and controls before and after the claim year



Notes: The sample consists of all claimants (regardless of reason for job loss) assigned to the control and JSA group. The blue line with square markers represents the outcome (by year) of claimants assigned to the control group. The red line with circle markers represents the estimated effect of JSA on the outcome added to the control group average. The vertical bars are 90-percent confidence intervals, based on heteroskedasticity-adjusted robust standard errors. The estimated effect is from regressions that control for all variables displayed in Table B2 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed. Earnings are in 1988:4 dollars. The point estimates are presented in Table B4.

Figure B3: Long-term employment-quality outcomes for all JSA claimants and controls after the claim year



Notes: The sample consists of all claimants (regardless of reason for job loss) assigned to the control and JSA group. The blue line with square markers represents the outcome (by year) of claimants assigned to the control group. The red line with circle markers represents the estimated effect of JSA on the outcome added to the control group average. The vertical bars are 90-percent confidence intervals, based on heteroskedasticity-adjusted robust standard errors. The estimated effect is from regressions that control for all variables displayed in Table B2 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed. Earnings are in 1988:4 dollars. The point estimates are presented in Table B5.

Appendix C: Results for UI Claimants on Temporary Layoff

This appendix presents summary statistics and estimates of JSA effects for the sample of claimants on temporary layoff.

Table C1:
Job search assistance, eligibility review interviews, and employment services received by claimants on temporary layoff assigned to the control group and the JSA group

	(1)	(2)	(3)	(4)
Service	Control	JSA	JSA participant	*p*-value of test for difference between (1) and (2)
Job search assistance (JSA) workshop	0.002	0.118	1.000	0.000
Eligibility review interview	0.171	0.113	0.667	0.004
Employment services[a]				
job referral/placement	0.135	0.111	0.182	0.207
job development plan	0.096	0.030	0.136	0.000
other employment service[b]	0.060	0.027	0.076	0.006
Sample size	584	560	66	

Notes: The sample consists of claimants on temporary layoff who were assigned to the control and JSA groups.
a. A claimant may receive more than one category of services.
b. Job consultation, receipt of or referral to training, testing, support services, job development (contacting an employer on the claimant's behalf), or any other contact with the Employment Service.

Table C2:

Sample descriptive statistics and tests of differences between claimants on temporary layoff assigned to the control group or JSA, and JSA participants

	(1)	(2)	(3)	(4)
	Sample proportions and means			
Covariate	Assigned to Control	Assigned to JSA	JSA participants	*p*-value of test for differences between (1) and (2)
Male	0.747	0.754	0.788	0.785
Race				
white	0.836	0.829	0.864	0.750
black	0.096	0.098	0.091	0.895
other	0.069	0.073	0.046	0.756
Age				
≤ 24	0.178	0.191	0.167	0.572
25-34	0.404	0.395	0.424	0.744
35-44	0.253	0.229	0.152	0.326
45-54	0.101	0.111	0.152	0.595
≥ 54	0.063	0.075	0.106	0.438
Schooling				
less than high school	0.146	0.155	0.197	0.643
high school	0.567	0.588	0.682	0.479
some college	0.238	0.207	0.061	0.210
college graduate	0.050	0.050	0.061	0.979
Veteran	0.277	0.261	0.242	0.525
Marital status/gender				
married male	0.387	0.395	0.303	0.791
married female	0.127	0.104	0.091	0.221
Household status				
no dependents	0.397	0.407	0.500	0.734
1 dependent	0.205	0.175	0.121	0.190
2 or more dependents	0.329	0.330	0.242	0.954
homeowner	0.373	0.395	0.348	0.458
Occupation				
professional	0.051	0.061	0.061	0.492
clerical	0.070	0.088	0.091	0.278
sales	0.029	0.030	0.015	0.901
service	0.086	0.111	0.212	0.154
agric., fishery, forestry	0.019	0.020	0.015	0.921
processing	0.057	0.043	0.046	0.289
machine trades	0.104	0.091	0.136	0.447
benchwork	0.081	0.066	0.030	0.351
structural work	0.344	0.330	0.242	0.622
miscellaneous	0.159	0.161	0.152	0.946

Industry				
agriculture	0.012	0.013	0.015	0.937
mining	0.007	0.000	0.000	0.050
construction	0.235	0.230	0.212	0.866
manufacturing	0.341	0.341	0.242	0.991
transportation, utilities	0.058	0.066	0.091	0.583
wholesale trade	0.048	0.043	0.061	0.680
retail trade	0.116	0.116	0.106	0.985
finance, ins., real estate	0.014	0.021	0.030	0.319
services	0.147	0.154	0.227	0.766
government	0.009	0.005	0.000	0.516
unclassified	0.014	0.011	0.015	0.647
Prior UI claim				
none	0.750	0.762	0.803	0.623
duration ≤ 15 weeks	0.151	0.129	0.046	0.281
duration > 15 weeks	0.099	0.109	0.152	0.595
Employer-attached/placed by union	0.553	0.580	0.318	0.353
UI benefits/claim type				
weekly amount ($)	148.80	147.20	140.60	0.607
maximum amount ($)	3,997.00	3,966.00	3,799.00	0.761
potential duration	26.34	26.40	26.26	0.816
replacement rate (percent)[2]	60	60	63	0.945
combined wage claim[3]	0.022	0.032	0.061	0.304
ex-service member claim	0.002	0.002	0.000	0.976
federal employee claim	0.010	0.007	0.000	0.570
Reservation wage (hourly)				
≤ $5.00	0.199	0.177	0.227	0.345
$5.01–$7.00	0.161	0.212	0.288	0.025
$7.01–$10.00	0.250	0.241	0.242	0.726
$10.01–$20.00	0.202	0.170	0.121	0.159
> $20.00	0.120	0.102	0.061	0.331
Sample size	584	560	66	

Notes: The sample consists of claimants on temporary layoff assigned to the control and JSA groups.
1. Bold denotes *p*-values for the test of mean differences between groups < .05.
2. The replacement rate is the weekly benefit amount as a percentage of average weekly earnings before the UI claim.
3. Combined wage claims use earnings from more than one state to calculate base period earnings.

Table C3:
Estimated short-term effects of assignment to JSA for claimants on temporary layoff on selected outcomes

Outcome	Control group mean	JSA effect (robust std. error)	
UI outcomes (first spell)			
Benefits paid ($)	1,405	-106	(86)
Weeks paid	11.890	-0.51	(0.62)
Exhausted benefits (proportion)	0.183	-0.026	(0.022)
Short-term employment outcomes (unconditional)			
Quarter 1 outcomes[a]			
Employed (proportion)	0.757	0.036	(0.024)
Hours worked	257	9.7	(13.2)
Earnings ($)	2,972	61.85	(146.01)
Year 0 outcomes[a]			
Employed (proportion)	0.916	0.025	(0.016)
Hours worked	1,178	7.2	(41.8)
Earnings ($)	13,941	305	(522)
Other outcomes			
Returned to same employer (proportion)	0.519	0.008	(0.028)
Returned to same industry (proportion)	0.601	0.042	(0.028)
Sample size	584	560	

Notes: The sample consists of claimants on temporary layoff assigned to the control and JSA groups. Estimated effects are regression-adjusted differences (based on equation 1) controlling for all variables displayed in Table C2 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed. Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10.
a. Quarter 1 is the quarter following the quarter in which the UI claim was filed. Year 0 refers to the sum of the first, second, third, and fourth quarters after the claim quarter. Earnings are in 1988:4 dollars.

Table C4: Estimated long-term effects of JSA assignment on employment outcomes for claimants on temporary layoff, by year

Dependent variable	(1) Employment		(2) Hours worked		(3) Earnings		(4) log wage rates	
	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect
Year since claim								
–3	0.805	0.006	1,014	-2.1	11,971	7	2.259	0.020
		(0.022)		(40.3)		(543)		(0.024)
–2	0.887	-0.007	1,085	75.7**	12,461	892**	2.238	0.018
		(0.018)		(35.0)		(444)		(0.018)
–1	0.961	0.008	1,351	-20.0	11,971	7	2.291	0.008
		(0.010)		(19.9)		(543)		(0.017)
0	0.916	0.025	1,178	7.2	13,941	305	2.327	0.036
		(0.016)		(41.8)		(522)		(0.030)
1	0.865	0.002	1,263	7.8	15,109	355	2.388	-0.004
		(0.020)		(48.0)		(605)		(0.028)
2	0.822	0.029	1,238	7.4	15,260	477	2.424	0.017
		(0.022)		(50.6)		(636)		(0.037)
3	0.798	0.043*	1,159	30.8	14,705	472	2.443	-0.024
		(0.023)		(51.2)		(660)		(0.035)
4	0.776	0.003	1,089	15.3	14,286	204	2.446	0.002
		(0.025)		(51.8)		(693)		(0.034)
5	0.745	0.006	1,045	36.6	13,612	681	2.461	-0.053
		(0.026)		(52.9)		(712)		(0.037)
6	0.699	0.046*	1,031	71.0	13,198	821	2.436	-0.038
		(0.027)		(54.6)		(722)		(0.033)
7	0.683	0.033	1,018	59.4	13,057	559	2.405	0.008
		(0.027)		(53.9)		(717)		(0.036)
8	0.673	0.010	986	67.0	13,113	671	2.446	-0.010
		(0.027)		(54.2)		(742)		(0.037)
9	0.661	0.017	974	80.9	13,146	944	2.444	0.005
		(0.028)		(55.7)		(771)		(0.039)
Years 1–9 (average)	0.747	0.021	1,089	41.8	13,943	576	2.432	0.010
		(0.019)		(41.8)		(585)		(0.029)

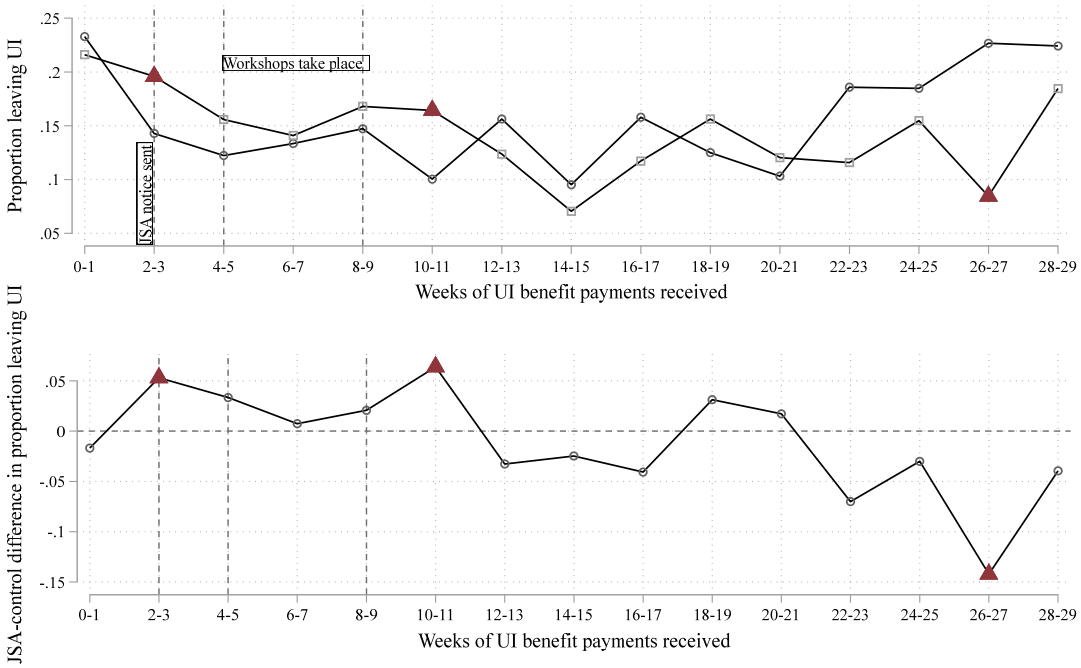
Notes: Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Sample consists of claimants on temporary layoff assigned to the control and JSA groups. A separate regression is estimated for each year and for each outcome. Estimated effects are regression-adjusted differences controlling for all variables displayed in Table C2 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed. Earnings are in 1988:4 dollars.

Table C5: Estimated long-term effects of JSA assignment on employment quality for claimants on temporary layoff, by year

Dependent variable	(1) log employer size		(2) Average log wage rate of coworkers		(3) Average firm effect		(4) Match effect	
	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect	Control group mean	JSA effect
Year since claim								
0	4.732	-0.086	2.348	0.032	0.0965	0.010	0.00889	0.000
		(0.126)		(0.024)		(0.015)		(0.014)
1	4.874	-0.091	2.369	0.002	0.105	-0.004	0.0140	0.013
		(0.136)		(0.023)		(0.014)		(0.014)
2	4.946	-0.049	2.388	0.010	0.103	0.011	0.0124	0.005
		(0.144)		(0.026)		(0.015)		(0.010)
3	5.056	-0.156	2.382	-0.017	0.102	-0.015	0.0160	-0.007
		(0.150)		(0.026)		(0.015)		(0.010)
4	4.968	-0.064	2.383	0.002	0.0993	-0.012	0.00905	0.001
		(0.163)		(0.028)		(0.017)		(0.009)
5	4.967	0.046	2.394	-0.016	0.102	-0.017	0.00169	0.003
		(0.167)		(0.027)		(0.017)		(0.010)
6	5.002	-0.067	2.408	-0.038	0.103	-0.028*	-0.00992	0.007
		(0.179)		(0.029)		(0.017)		(0.010)
7	5.048	-0.238	2.394	0.017	0.0836	0.012	-0.00760	0.007
		(0.175)		(0.031)		(0.018)		(0.014)
8	4.954	-0.022	2.429	-0.015	0.103	-0.004	-0.00935	0.011
		(0.176)		(0.031)		(0.018)		(0.013)
9	4.868	0.233	2.421	-0.016	0.100	0.001	-0.0143	0.019
		(0.187)		(0.031)		(0.018)		(0.012)
Years 1–9 (average)	4.828	-0.071	2.361	-0.007	0.0845	-0.004	-0.000144	-0.004
		(0.118)		(0.021)		(0.013)		(0.013)

Notes: Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Sample consists of claimants on temporary layoff assigned to the control and JSA groups. A separate regression is estimated for each year and for each outcome. Estimated effects are regression-adjusted differences controlling for all variables displayed in Table C2 plus the quarter in which the claim was filed and the unemployment rate in the county and month in which the claim was filed. Earnings are in 1988:4 dollars. Employment-quality outcomes are not available in the pre-claim years; see the text for details.

Figure C1: Biweekly UI exit rates (hazards) for claimants on temporary layoff assigned to the control and JSA groups (first spell)

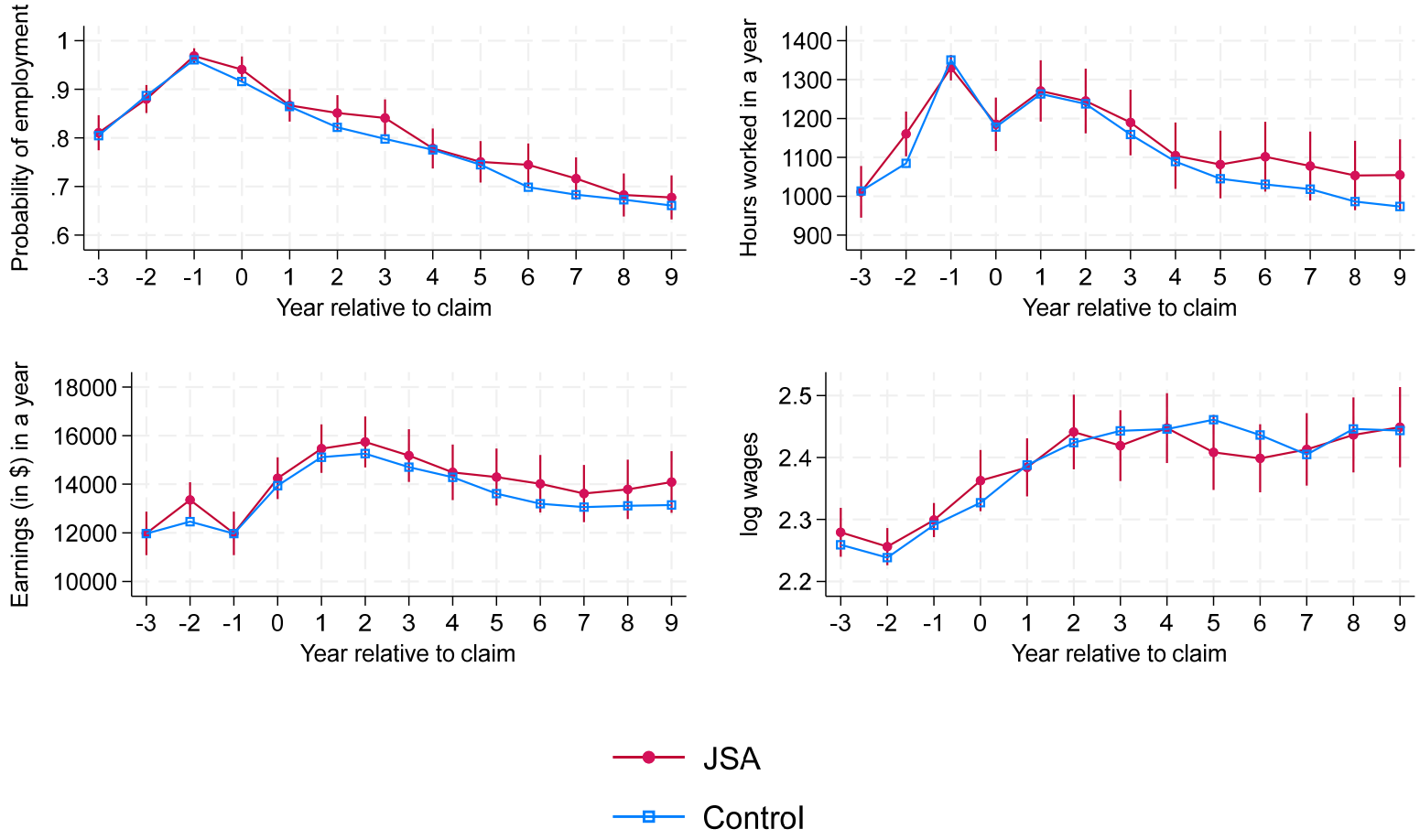


Notes: The sample consists of displaced claimants assigned to the control and JSA group. The top figure shows the exit rate (hazard) for the two groups—the squares denote the JSA group and the circles denote the controls. The bottom figure shows the difference in exit rate between JSA and the controls. Black triangles denote statistically significant differences between JSA and the controls at the 5-percent level. Black dots (not shown) denote statistically significant differences between JSA and the controls at the 10-percent level.

The hazards are plotted as functions of bi-weekly UI payments because in Washington state, claimants "certify" for two weeks at a time. UI exit hazards labeled "0-1" represent the conditional probability that claimants who experienced zero or one week of insured unemployment exited UI before receiving a second benefit check; UI exit hazards labeled "2-3" represent the conditional probability that claimants who experienced two or three weeks of insured unemployment exited UI before receiving a third benefit check, etc.

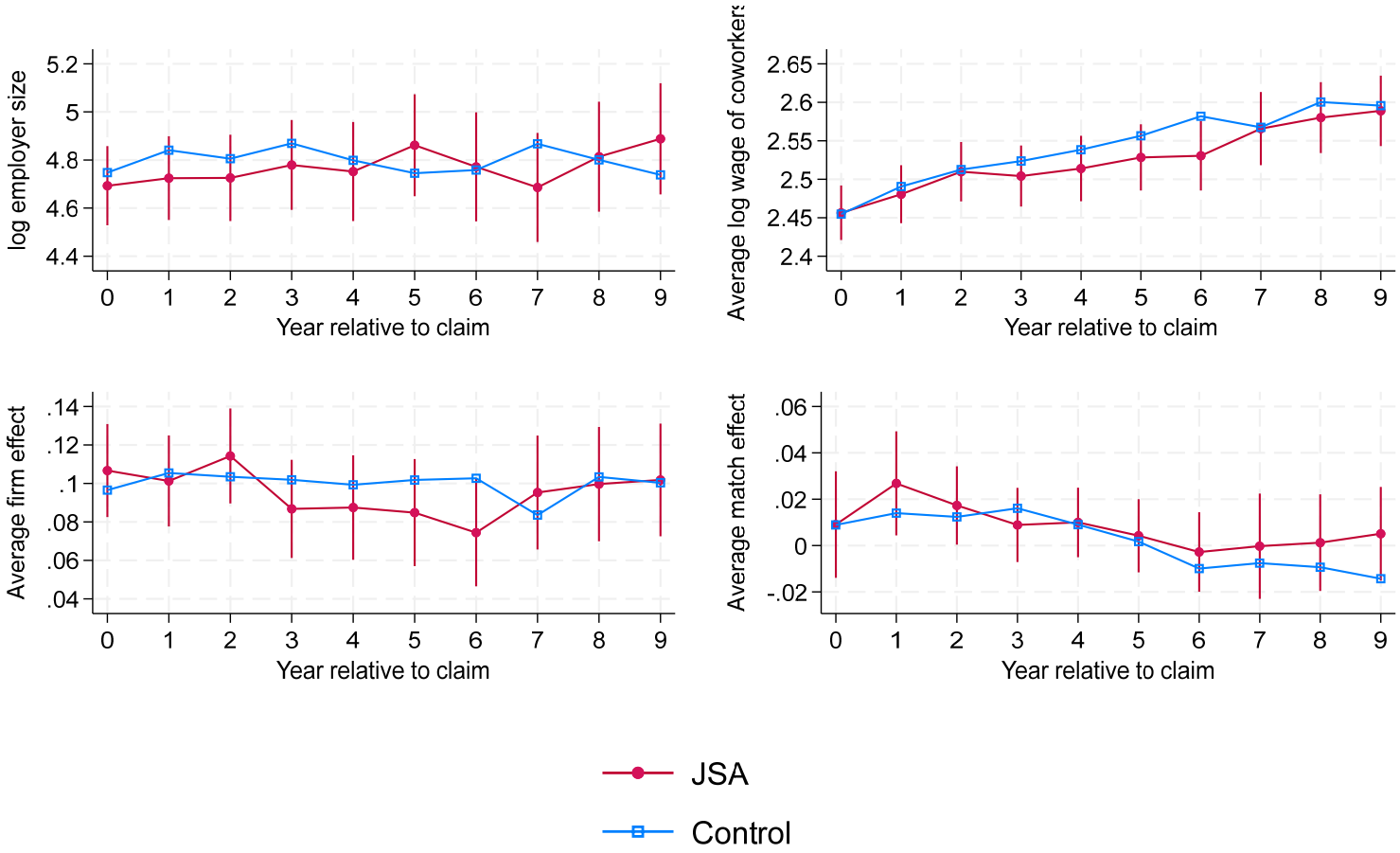
The JSA workshop call-in notice was sent about four weeks following the claim—that is, around the second UI payment, or week 2-3 in the graph. Most JSA workshops occurred between weeks 4-5 and 8-9 in the graph. ERI call-in notices were sent to controls in weeks 10-11.

Figure C2: Long-term employment outcomes for temporarily laid off JSA claimants and controls before and after the claim year



Notes: The sample consists of claimants on temporary layoff assigned to the control and JSA group. The blue line with square markers represents the outcome (by year) of claimants assigned to the control group. The red line with circle markers represents the estimated effect of JSA on the outcome added to the control group average. The vertical bars are 90-percent confidence intervals, based on heteroskedasticity-adjusted robust standard errors. The estimated effect is from regressions that control for all variables displayed in Table B2 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed. Earnings are in 1988:4 dollars. The point estimates are presented in Table C4.

Figure C3: Long-term employment-quality outcomes for temporarily laid off JSA claimants and controls after the claim year



Notes: The sample consists of claimants on temporary layoff assigned to the control and JSA group. The blue line with square markers represents the outcome (by year) of claimants assigned to the control group. The red line with circle markers represents the estimated effect of JSA on the outcome added to the control group average. The vertical bars are 90-percent confidence intervals, based on heteroskedasticity-adjusted robust standard errors. The estimated effect is from regressions that control for all variables displayed in Table B2 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed. Earnings are in 1988:4 dollars. The point estimates are presented in Table C5.

Appendix D: Data Appendix

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

1. Description of the Experimental Data

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

The experimental data combine data from three sources:

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

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

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

Details of the assignment protocol

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

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

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

Replicating Johnson and Klepinger's sample

Claimants who failed the separation requirement for UI need to be dropped from the sample, but the experimental data do not include a binary indicator of whether the claimant satisfied that requirement. The data do include two "separation issue" variables, one for the total number of separation issues raised during the benefit year, the other a set of weekly variables indicating whether a separation issue was raised in that week. The two are inconsistent, and our solution was to drop all workers with at least one separation issue during the benefit year and zero benefits paid, on the assumption that they were ineligible due to a separation issue. This reduces the sample to 9,620 observations.

Although the sample we use is not identical to that used by Johnson and Klepinger (1991, 1994), both our short-term treatment effects and theirs are essentially similar, as can be seen by comparing Appendix Tables A2 and A3 with Tables 1, 2, and 3 in Johnson and Klepinger (1994).

[1] These proportions are with respect to the full sample of 10,635 monetarily eligible claimants.

Claimants with two benefit years

Each observation in the experimental data represents a UI claimant's "benefit year"—that is, the year following the initial UI claim, during which the claimant could receive UI benefits. For 9,211 initial UI claimants, one observation (that is, one benefit year) appears in the experimental data; however, for 400 claimants, two observations appear. These latter claimants each started a benefit year in the early months of the WAWS experiment (in July or August 1986). This benefit year lapsed a year later, and these 400 claimants established a new benefit year in July or August 1997, while the Washington experiment was still in progress. As a result, they were again assigned to a treatment or to the control group and are included in our sample.

Benefit duration

Another key variable we constructed is the *duration of the first UI claim spell*. The experimental data include a variable for the amount of UI benefit paid in each of the 52 weeks of the benefit year, so we followed each claimant's benefit payments from the time of the initial claim until the series lapsed for at least four weeks. This follows the procedure used by Spiegelman, O'Leary and Kline (1992, 1995) and Corson, Decker, Dunstan, and Kerachsky (1992) in a similar setting, although appears to differ from Johnson and Klepinger (1991, 1994), who ended the first spell with a lapse of one week. Inspection of the data shows that many claimants with a lapse of three or fewer weeks had a subsequent claim series of at least another four weeks, which suggested a single spell of insured unemployment briefly interrupted by time out of the labor force, rather than reemployment. We have checked the sensitivity of the findings and hazards we report to

different definitions of a first spell (a one-week lapse, two-weeks, and so on) and obtain similar findings in each case.

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

For 7,054 claimants, the first spell of UI accounts for all benefits paid in the benefit year. The rest received additional benefits later in the benefit year. We refer to the total number of weeks in which a claimant received benefits as the "compressed UI spell." Although useful as a measure of the total effect of a treatment, a compressed spell does not correspond to a true duration because it combines spells of benefit receipt separated by at least four weeks. Accordingly, the hazard functions we display (and our interpretation of the treatment effects on search behavior) focus on the duration of the first claim spell.

Reason for job loss

Reason for job loss is a key claim characteristic. To construct this variable, we create five mutually exclusive categories by using "unemployed due to lack of work" and "reason for lack of work" indicators from the Employment Service records. We define *displaced workers* as "unemployed due to permanent job loss" if a claimant was either laid off permanently because of a plant or company closure; "unemployed due to temporary layoff" if a claimant was laid off temporarily either with a known recall date or without a recall date; "unemployed due to contract

completion or seasonal layoff" if a claimant was laid off because of job or contract completion or because of a seasonal layoff; "unemployment not due to job loss," which we call "quit for good cause" for short, if the reason for lack of work was missing and the claimant was unemployed not due to lack of work.

Note that, typically, if a worker voluntarily quit his or her job, he or she is not eligible for UI benefits. Such a worker might still receive benefits, if the reason for the quit meets the "standard for good cause," such as showing that the claimants left due to a hostile work environment (e.g., because of discrimination or sexual harassment). Finally, we define "reason for unemployment unknown" if there is no information regarding if the claimant was unemployed due to lack of work or if the reason for lack of work is missing. This last group is the largest.

2. Constructing a Long-Term Panel

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

- a worker identifier
- the year and quarter
- an employer identifier and earnings received from the principal employer in that quarter, and
- hours worked in the quarter (again, for each principal employer)

Coverage of the UI system is nearly universal (self-employed workers are the only significant group of "above-ground" workers who are not covered), and any UI-covered worker who has

earnings in a given quarter from an employer in the state appears in the wage records. As a result, wage records can be used to construct an earnings history of most workers who were in the WAWS experiment.

The wage records also allow us to track which worker works for what employer in any given quarter, and as a result can be used to construct employer-level variables such as employer size, measured by the number of workers of a given employer, and average wages paid by the employer. These employer-level variables are employer-specific means, averaged over all workers and all years.

For this study, we had available the population of Washington administrative wage records for quarters 1987:I through 1997:IV inclusive, so we can observe up to nine years of earnings following the Washington experiment by matching workers in the experiment with their wage records and characteristics of their current employer.

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

earnings outcomes of training programs (Kornfeld and Bloom 1999, p. 171). Wallace and Haveman's (2007) validation study focused on welfare recipients in Wisconsin and found that, despite discrepancies, wage records and survey data gave similar results on employment and earnings outcomes. Wallace and Haveman conclude that, given their availability, low cost, and similarity across states, UI wage records are preferable to surveys for monitoring labor market outcomes of low-wage workers.

Appendix Table D1 shows the results of matching workers enrolled in the Washington experiment with the Washington wage records. The table shows the number of matches for the all claimants in the experiment (the "Total" column) and each treatment group (including controls) by year.[2] A "match" occurs when a claimant is observed with earnings in the Washington administrative wage records in a given year. The table also shows "match rates" (in parentheses) defined as the proportion of claimants initially enrolled in the experiment (or each treatment) observed with positive earnings (or "matched") in a given year. In the table, "year 0" refers to the claim year, defined as the quarter in which the initial UI claim was filed and the three following quarters.

The Washington experiment enrolled new claimants between July 1986 and August 1987, so it should be possible to match most of the enrolled claimants with their 1987 wage records. (Claimants would not have a 1987 match if they claimed benefits in 1986 or early 1987 and never found reemployment, withdrew from the labor force, or for a few other reasons — see the following section.) As Appendix Table D1 shows, 86.5 percent of claimants enrolled in the

[2] The "Total" column gives the sum of the full control, Exception Reporting, New Work Search, and JSA groups. The full control group is used with the Exception Reporting and New Work Search treatments. The restricted control group, which drops claimants who never received a benefit, is used with the JSA treatment because only JSA assignees who received a first benefit payment received a JSA call-in notice.

experiment could be matched with wage records at some time during the claim year. This match rate falls unevenly to 62.5 percent in year 9, or at an average of just under 3 percentage points per year. Specifically, the match rate falls by about 4 percentage points between years 0 and 1, then by 3 percentage points per year until year 5, after which it falls somewhat more slowly.

3. Attrition from the Long-term Sample

The match rates shown in Appendix Table D1 decline over time for two reasons. First, workers may remain unemployed or drop out of the labor force after claiming UI, so they will have no earnings and none will appear in the wage records. Second, workers could become self-employed, leave the formal labor force for the underground economy, or leave Washington State and find employment elsewhere.[3] In these latter cases, a worker will have earnings, but those earnings will not be recorded in the UI wage records of Washington State. (Self-employed workers are not covered by UI, earnings in the underground economy are not reported, and out-of-state earnings will be picked up in the wage records of another state.)

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

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

Attrition of participants from a long-term panel poses a threat to the validity of an experimental study if the subjects who leave the sample differ systematically and in unobserved ways from those who remain (see for example, the discussion and references in Murnane and Willett 2011). Sample attrition is usually considered as a problem of nonresponse: experiments conducted with household surveys depend on both a survey center's ability to find participants and on participants' cooperation over a long period of time (Hausman and Wise 1979, McFadden 1985). Administrative data have an advantage over survey data because they are potentially less vulnerable to attrition: anyone who receives covered earnings in given state in a given quarter should be observed in UI administrative wage records, regardless of whether that individual can be found or is willing to cooperate.[4]

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

To investigate the extent of sample attrition, we follow the approach described in Chyn (2018) and based on Grogger (2013). First, for each person and each post-claim year, 0–9, we define "attrition" as a dummy that equals one if the person is never observed with positive

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

earnings again. For example, in year 0, attrition equals one if the person is never observed with positive earnings in any of the years 0–9. In year 1, attrition equals one if the person is never observed with positive earnings in any of the years 1–9, etc. Second, we use the attrition dummy as an outcome variable in equation (1), which we estimate separately for each post-claim year. The idea is to examine whether assignment to JSA has a statistical effect on the probability of attriting from the long-term panel.

Table D2 presents the results. Column (1) shows the attrition rate for the control group and column (2) shows the estimated differences in probability of attriting between the JSA group and the control group. We find little evidence that JSA had an impact on attrition from the long-term sample.

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Table D1:
Matches[1] and match rates[2] (in parentheses) between population wage records and claimants enrolled in the WAWS experiment, by year and treatment group

	Control group	JSA
Year 0	2,450	2,235
	(0.859)	(0.877)
Year 1	2,353	2,105
	(0.825)	(0.826)
Year 2	2,269	2,048
	(0.796)	(0.804)
Year 3	2,198	1,984
	(0.771)	(0.779)
Year 4	2,126	1,899
	(0.745)	(0.745)
Year 5	2,031	1,813
	(0.712)	(0.712)
Year 6	1,933	1,756
	(0.678)	(0.689)
Year 7	1,876	1,714
	(0.658)	(0.673)
Year 8	1,828	1,647
	(0.641)	(0.646)
Year 9	1,788	1,625
	(0.627)	(0.638)
Number enrolled	2,852	2,548

Source: Tabulated from the WAWS experimental data base and population wage records from the Washington State Department of Employment Security.
1. A match occurs for a claimant in a given year if positive earnings were found in at least one quarter of the year for the claimant.
2. The match rate is the proportion of claimants initially enrolled in the experiment (or in a treatment) who were observed with earnings in a given year.

Table D2:
Testing for differential attrition by year

Dependent variable	Probability of attrition by year t	
	(1) Control group mean	(2) Difference between JSA and control groups
Year 0	0.0519	-0.007
		(0.006)
Year 1	0.0912	-0.008
		(0.008)
Year 2	0.116	-0.004
		(0.009)
Year 3	0.139	0.001
		(0.009)
Year 4	0.168	0.007
		(0.010)
Year 5	0.202	0.005
		(0.011)
Year 6	0.243	-0.005
		(0.011)
Year 7	0.278	-0.005
		(0.012)
Year 8	0.317	0.001
		(0.012)
Year 9	0.373	-0.008
		(0.013)

Notes: Standard errors robust to heteroskedasticity are in brackets. ***, **, and * indicate $p <$ 0.01, 0.05, or 0.10. The sample consists of all claimants assigned to the control and JSA groups. A separate regression (based on equation 1) is estimated for each year. Estimated effects are regression-adjusted differences controlling for all variables displayed in Table A1 plus the quarter in which the claim was filed and the unemployment rate in the month in which the claim was filed.

Appendix E: The AKM Model[5]

This appendix describes estimation of the Abowd, Kramarz, Margolis (1999) (AKM) employer fixed effects for hourly wages used in the main text. Raw data for the analysis come from administrative wage records of Washington state. The records available to us provide information on the earnings and paid work hours of workers employed by UI-covered employers in Washington during 1987:I–1997:IV.

For the purposes of this analysis, for each quarter, we identify the employers from whom the worker earned the largest share of his/her earnings. We drop observations with zero or missing hours or earnings as well as employers with fewer than five employees. Next, we annualize the data by focusing on each worker's principal employer, defined as the highest-paying employer in that calendar year. After restricting the analysis to principal-employer job spells, we calculate the hourly wage rate by dividing earnings by hours. We drop observations with wages below \$2/hour and hours above the 99th percentile.

We estimate the AKM model for log wage rates as follows:

$$\log y_{ijt} = \alpha_i + \psi_{j(i,t)} + \theta_t + u_{ijt} \quad \text{(E1)}$$

where y_{ijt} denotes the wage rate of worker i with employer j in year t; α_i is a worker-specific fixed effect (the productive characteristics of the worker that is portable between employers); $\psi_{j(i,t)}$ is an employer-specific fixed effect (employer characteristic that results in above- or below-average wage rates for all workers at employer j); θ_t is a vector of calendar year effects; and u_{ijt} is an error that may include idiosyncratic worker-employer match effects. The function $j(i,t)$ indexes the employer j effect for worker i in year t.

[5] This section draws heavily on the description of the AKM model in Lachowska, Mas, and Woodbury (2020).

Table E1 shows that the largest connected set for estimating employer effects includes includes about 8 million workers-year observations and about 151, 000 employers. The table displays the resulting variance decomposition, with naïve plug-in variance estimators in column (1) and estimators corrected for limited mobility bias using the Kline-Saggio-Solvesten estimator (KSS) in column (2). The variance of each outcome is decomposed into worker effects, employer effects, and the covariance between worker and employer effects (measuring the sorting of workers and employers), and the residual.

Table E1: Variance decomposition of log hourly wage rates

	Plug-in variance estimates		*KSS-corrected variance estimates*	
Variance of log wages and its components	(1)	Share of variance explained	(2)	Share of variance explained
Variance of log wage	0.354	*100%*	0.354	*100%*
Worker effects	0.169	*48%*	0.110	*31%*
Employer effects	0.059	*17%*	0.057	*16%*
2*cov(worker effs, employer effs)	0.071	*20%*	0.072	*20%*
AKM model fit				
Adj. R^2	0.842		0.677	
Summary statistics				
Mean log wage	2.425		2.425	
Number of worker-years	81,371,763		81,371,763	
Number of movers	3,028,428		3,028,428	
Number of employers	151,072		151,072	

Note: Authors' tabulations of Washington administrative earnings records, 1987:I–1997:IV. Column (1) shows the naive (plug-in) variance decompositions. Column (2) shows the KSS-corrected variance decompositions after leaving out a worker-firm match. The decompositions include covariances between worker- and employer effects and year effects. Because these covariances explain at most 2 percent of the variation, they are omitted from the table.

Worker fixed effects explain about 31 percent of the variation in wages (=0.11/0.35) and employer effects explain about 16 percent (=0.057/0.35). Overall, the observables explain about

68 percent of total variation. There is positive sorting of between workers and employer in terms of wages – the correlation equals about 0.454.

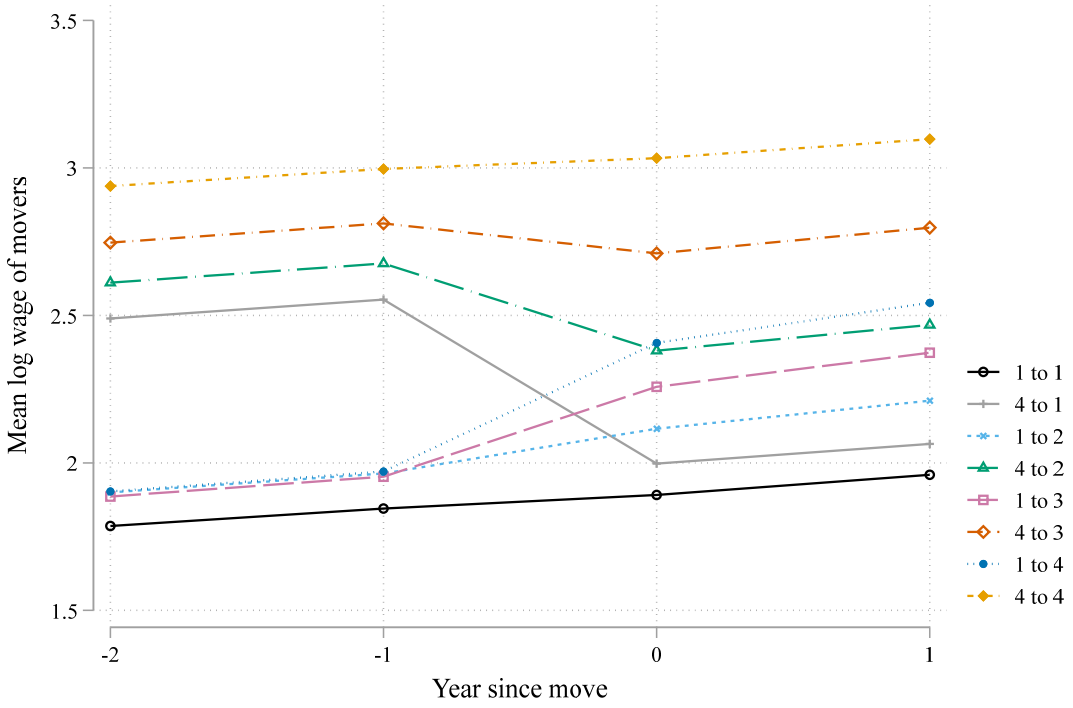
Estimates of the AKM model will be biased for the employer effects (ψ) if worker mobility among employers is endogenous or correlated with time-varying components of the residual in equation (E1). This problem would arise, for example, if workers moved to take advantage good specific employer-employee matches.

To examine the importance of endogenous mobility, Card, Heining, and Kline (2013) (CHK) developed an event study analysis of the movement of average wages when workers move between employers. If the AKM model is a correct description of wage determination, then workers who move from low-ψ to high-ψ employers should on average see their pay rise, and conversely. Further, workers who move from low-ψ to high-ψ employers should receive (on average) pay increases equal and opposite those of workers who move from high-ψ to low-ψ employers. In contrast, the presence of specific employer-employee match effects would lead to average pay increases for workers moving in any direction, as they take advantage of favorable matches.

Following CHK, we conduct an event-study analysis of how wage rates change when workers move between employers of different types. To do so, we classify each worker *i*'s employer into quartiles of *i*'s coworkers' wages. Next, for a given year t, we select workers in each quartile who have been with the employer at least two years, change employers (i.e., are observed with a different primary employer in year t+1), and remain with the subsequent employer for at least two quarters. Finally, we calculate the average outcome before and after the move for each possible type of interquartile move (1→1, 1→2, ..., 4→3, and 4→4).

Figure E1 shows the event-study figure. We note two main points. First, workers who move from lower-paying to higher-paying employers tend to improve their earnings, and conversely. Second, the approximate symmetry of gains and losses suggests that idiosyncratic match effects are not of great importance (CHK, p. 990). If employer-employee match effects were important, we would observe average pay increases for workers moving in any direction, but this is not the case.

Figure E1:
Mean log hourly wage rates of movers, classified by quartile of coworker wages at origin (quarter = –1) and destination (quarter = 0) employer



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Appendix F: Woodcock Match Effects Model[6]

We estimate time-invariant worker-employer match effects for wage rates using Woodcock's (2015) fixed effects estimator. Woodcock's approach estimates worker-employer match effects in two steps. First, we compute the average wage rate ($\overline{\log y}_{ij}$) for each *ij* worker-employer match.[7] Second, using the sample of worker-employer matches, we regress each average *ij* match outcome on a worker fixed effect (w_i) and a firm fixed effect ($f_{j(i,t)}$):

(F1) $\quad \overline{\log y}_{ij} = w_i + f_{j(i,t)} + \mu_{ij}$

The regression is weighted by match duration, and the error μ_{ij} is assumed mean independent of the worker and employer effects, which is a strong assumption. The residuals from equation (F1) represent the variation remaining in $\overline{\log y}_{ij}$ after accounting for worker and employer effects:

(F2) $\quad \hat{\mu}_{ij} = \overline{\log y}_{ij} - \hat{w}_i - \hat{f}_{j(i,t)}$

It follows that the $\hat{\mu}$s can be interpreted as estimated time-invariant worker-employer match effects, averaged over the years we observe a given match. Note that because $\hat{w}_i$ and $\hat{f}_j$ are time-invariant, and $\hat{\mu}_{ij}$ does not vary within a job match, some idiosyncratic individual-level variation in the $\log Y_{ijt}$ remains after accounting for worker, employer, and match effects.

To obtain the sample of worker-employer matches used to estimate equation (F1), we start with the same sample used to estimate the AKM model (described in Appendix E).

[6] This section draws heavily on the description of the Woodcock model in Lachowska, Mas, and Woodbury (2020).

[7] In practice, for each $\log y_{ijt}$, we net out calendar-year effects. Specifically, we first remove year effects from the wage rate, then regress these adjusted wages on worker-employer match indicators. Finally, we compute within-match averages of wages.

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