Job Search and Unemployment Insurance: New Evidence from One Million audits

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Abstract

I study unemployment insurance claimants using one million eligibility audits spanning 1987 to the present. The audits include a verified, one-time survey of active claimants giving their weekly benefit amount, reservation wage, targeted occupation, and number of job contacts made that week. Using a regression kink design, I find that monetary weekly benefits have a positive impact on unemployment duration. However, the same design finds no effects of unemployment benefits on any of the recorded search behaviors. Despite this null finding, the search behaviors respond to other factors in the expected ways: claimants later in their spells have a lower reservation wage, are more likely to be switching occupations, and make slightly more job contacts; also, claimants have lower reservation wages when the unemployment rate is higher. These results suggest that search behaviors may not explain the duration response to benefits.

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1 Introduction

Understanding behavioral responses to unemployment insurance (UI) levels is key to inferring the optimal level of benefits (Chetty, 2008). However, the existing quasi-experimental evidence on this question is restricted to data from more than three decades ago (Landais, 2015). Moreover, studies quantifying behavioral responses to benefits are typically forced to focus exclusively on time spent unemployed as the outcome, having no additional information on the behavior of claimants (Johnston and Mas, 2018; Card et al., 2015a; Rothstein, 2011).

This paper analyzes close to a million audits performed by the Department of Labor as part of its Benefit Accuracy Management (BAM) program from 1987 to the present. BAM audits are meant to determine whether randomly sampled benefit payments are valid according to state eligibility criteria. Importantly, claimants are questioned on their reservation wage, the number of jobs applied to that week, and the occupation they are seeking. These underutilized data present a unique opportunity to study search behaviors, the duration response to UI generosity, and the relationship between the two.

Claimants are selected based on a random draw from those who received benefits each week, with each state required to select a fixed number that roughly scales with population. The interviews are primarily performed in person or over the phone, and have a response rate above 90 percent (Potter et al., 2014). BAM investigators contact employers and state employment offices in order to verify survey responses, and claimants face potential monetary penalties if their answers are found to be inaccurate.

I first use this data to measure the duration response to monetary UI benefits. I exploit features of each state's benefit schedule in a regression kink framework, combining the BAM data with newly digitized panels of benefit formulas across all states from 1987 to the present. This extends existing quasi-experimental evidence in the US, which has been limited to the five states included in the Continuous Wage and Benefit History Project (Meyer, 1990; Landais, 2015; Card et al., 2015a).

I find a positive elasticity of duration at audit (or interrupted duration) to the UI monetary benefit amount, with an increase of roughly 3 percent for every 10 percent increase in benefit levels. When I impute expected completed duration using the distribution of interrupted spell links, the effect shrinks to 1.5 percent. These elasticities are similar to although broadly lower than other estimates in the United States; Landais (2015) finds an elasticity between .2 and .7 and Card et al. (2015a), using administrative data from Missouri, find an elasticity between .4 and .9.

I next turn to the unique feature of the data: measures of search effort and reservation wage. While the responses are not a panel as in Krueger and Mueller (2010), the size of the sample affords a similar causal exercise using the regression kink design. I find no evidence that reservation wages, job contacts, and occupation choice respond to benefits. The null results persist using both claimants in the first week and the entire sample, where estimates could be affected by length-biased sampling. The results suggest that, although greater benefits prolong unemployment spells, this is not due to any of the measured search behaviors.

These findings present some of the first large-scale evidence in the US evaluating the effects of benefits on search. Existing studies in the US have been mixed. Feldstein and Poterba (1984) find a positive association between the reservation wage ratio and the benefit replacement ratio in a relatively small sample unemployed workers, but Krueger and Mueller (2016) find no association using a longitudinal survey of UI claimants in New Jersey. Using the same survey, Krueger et al. (2011) find no effect of UI benefits on job search activity.[1] Marinescu (2017), using data from CareerBuilder.com, finds that a one percent increase in potential benefit duration decreases job applications by 0.4 percent. Finally, Le Barbanchon et al. (2017) find a precise zero effect of unemployment benefits on the reservation wage using administrative data from France.

While the search behaviors do not respond to benefits, they exhibit the predicted associations across several validation exercises. I match the audits to Missouri claim and wage records, which allows for a comparison reservation wages and eventual reemployment earnings. This important validation exercise is not possible in Le Barbanchon et al. (2017), and just one other US study has connected reported reservation wages and accepted wages, using only survey data (Krueger and Mueller, 2016). I find that reservation wages in BAM are a meaningful proxy of eventual earnings,

[1] However, using the American Time Use survey and cross-state variation in UI schedules, Krueger and Mueller (2010) find a strong negative response of search effort to unemployment benefits.

with a significant elasticity of .4 even controlling for past wages.[2]

Next, I study how search behavior evolves with unemployment duration. Claimants later in their spells have lower reservation wages, increased job contacts, and increased probability of switching occupations. Only the reservation wage response survives the addition of controls and person fixed effects for a limited sample of twice-audited claimants, suggesting that differences in job contacts and occupational choice are due to dynamic selection over the spell.

Finally, I find that the search behaviors exhibit cyclical qualities: across several specifications with flexible controls, and including all or only first week claimants to account for length-biased sampling, claimants lower their reservation wages by 0.5 percent for every 1 percent increase in the state unemployment rate. This reservation wage response outstrips the new hire wage elasticity to the state unemployment rate found in recent work (Gertler et al., 2016). In contrast, the decisions to switch occupations or make more job contacts appear unrelated to local labor market conditions.

Taken together, these findings challenge the view that UI duration depends on the explicit search activities undertaken by claimants. While UI increases unemployment duration, the behavioral measures provided in the data—the reservation wage, job choice, and number of job contacts—are unlikely to drive the response.

2 Data

The data covers 913,729 paid claims audits from 1987 to 2019 from the Department of Labor's Benefit Accuracy Management (BAM) program (USDOL, 2018). The BAM program seeks to measure the accuracy of paid and denied claims.[3] Interviews are mostly conducted in-person or over the phone. Across all audits, 25 percent uncover an erroneous payment, which half of the time results in a change to their benefit amount. Top reasons for overpayment concern work search, benefit calculations, separation issues, and availability for work.

Audits are based on random samples of benefit payments: Each week, claimants are randomly

[2]These results are broadly similar to recent work showing that the perceived job finding probability is predictive of actual job finding (Mueller et al., 2018).

[3]The most recent BAM Annual report is available here.

chosen from a list a claimants with positive benefit amounts in that state. The target number of audits scales loosely with state population, ranging from 6 per week in Delaware to 15 per week in California.[4] Importantly, claimants are not followed after the audit, so it is not possible from BAM data alone to know their ultimate unemployment duration or employment outcome.

The survey includes questions about job search behavior. The text of the reservation wage question is: "What is the lowest rate of pay you will accept for a job?" Claimants can give any time period in their response ($X per Y). It is then converted to an hourly wage by the state (the original response is not available). In Figure 1, I plot the distribution of the reservation wage ratio, the reservation wage over the claimant's previous hourly wage. Following Krueger and Mueller (2016), I drop respondents with ratios below .3 or above 3. Compared to Krueger and Mueller (2016) and Le Barbanchon et al. (2017), the reservation wage ratio in this context is less dispersed and much less likely to exceed one.

Contacts are recorded in a worksheet with spaces for the employer name and address, the contact date and method of contact, the type of work applied for, and whether a job was offered; some states also accept electronic proof of applications. The distribution of the contacts variable is shown in Figure A.1, split by whether the claimant was required to search for a job. The variation in this figure assuages one concern about the contacts measure—that claimants exclusively report the number of job contacts required to maintain eligibility.[5]

Finally, claimants are asked to report which occupation they had previously and which occupation they are now seeking, recorded using 3-digit O*NET codes. I use these two variables together to create the switching occupations variable, equal to one if the two do not match. Means for these search variables, benefit parameters, and other recorded demographics are reported in the descriptive statistics in Table 1.

An advantage of the audit context is that answers to these questions may be investigated by the examiner. The state UI office investigates almost all contacts reported by the claimant, and some auditees are cited for refusal of acceptable work, albeit rarely.

[4]See the BAM operations guide for more details.

[5]In another check on this issue, I find that more than 20 percent of claimants report more contacts that the state-year mode for claimants required to search, which I presume to be the statutory requirement.

2.1 Matched Missouri sample

In order to partially validate the reservation wage measure, I also match BAM claimants from Missouri to UI claims data and quarterly wages from Missouri using unique combinations of highest quarter earnings, base period earnings, and week of claim.[6] This matched sample is useful because it allows me to observe the total number of weeks claimed and eventual reemployment wages for claimants. As evidence that the match is successful, I find that claimants who reported that they were expecting to be recalled in the BAM data have significantly shorter durations in the Missouri claims data (Appendix Table A.1)

2.2 Duration measures

I employ two outcomes to measure the duration response: weeks claimed and unemployment duration, both measured at audit. The unemployment duration variable measures the days between the audit and the date the claim was started. Duration is different from weeks claimed because of lapses in the submitting weekly claims, which could be due to part-time employment or temporary non-participation.

These duration outcomes taken at the audit are lengths of interrupted spells, but are most useful as proxies for eventual completed duration or spell. Since a claimant is equally likely to be sampled at any week in their spell, audited claimants should on average be halfway through their full completed duration at the time of the survey assuming the composition of active claimants does not change substantially across weeks (Salant, 1977).

Figure A.2 shows a histogram of the ratio of weeks claimed at audit to weeks claimed total for each of the matched Missouri claimants. The plot suggests that, as expected, claimants are equally likely to be selected at any point in their claim.[7] However, the relationship between completed and interrupted duration does not appear well approximated with a linear term and no constant. Figure A.3(a) plots the completed duration against the interrupted duration for the same sample

[6] This analysis was facilitated by Andrew Johnston.

[7] A more efficient way to measure the duration response could be to explicitly model the probability of selection for audit. This is complicated by the facts that many claims are interrupted and the DOL does not keep records of which claimants were at risk of being selected each week.

of Missouri claimants. The slope of the trendline is 0.57, with a constant of 13.

This means that elasticites based on the duration at audit may not be equivalent to completed duration elasticities. I use a non-parametric method as in Sider (1985) for calculating the expected completed durations from the interrupted durations using the audit data. Assume some distribution w of the interrupted spell lengths (in weeks), $F(w)$, and a survival function $S(w) = 1 - F(w)$. Then the expected duration S conditional on observing a claimant with an interrupted spell $T = w$ is

$$E[S|T = w] = \frac{1}{S(w)} \int_{w}^{w_{max}} f(t) t dt. \tag{1}$$

This is simply a weighted average taken across the weeks greater than or equal to w.

I validate this method using the Missouri sample. Figure A.3(b) shows that the actual completed duration lines up closely with the predicted duration based on Equation 1 and using the whole sample, although not perfectly. The slope of the trend line is 1.09. In the duration results below, I present effects on weeks claimed and duration at audit, as well as the expected weeks measure. This method requires a stationary assumption because $f(t)$ is assumed to be stable; in practice, restricting to states or small time periods did not affect the fit between expected and actual duration in the Missoui sample.

Inferring completed duration from interrupted duration has been a longstanding challenge in studies of unemployment (Salant, 1977; Marston et al., 1975; Baker and Trivedi, 1985; Heckman and Singer, 1984). This technique may have efficiency losses compared to a maximum likelihood approach, but it has the advantage of not imposing parametric assumptions on the distribution of completed durations, as previous research has found that parameter estimates are sensitive to the specified distribution (Kiefer et al., 1985; Kiefer, 1988).

3 Regression kink evidence on the effect of UI benefits on unemployment duration

I first assess the impact of unemployment insurance benefits on unemployment duration and search behavior. In most states, the benefit amount is a fixed fraction of the claimant's highest quarterly wages from the past year, up to a maximum weekly amount. Thus a natural way to investigate the causal effects of benefits is with a regression kink design (RKD), which tests for a significant change in the slope of an outcome variable at the point at which the treatment variable sharply changes slope (Card et al., 2015b; Landais, 2015).[8]

This analysis cannot use all claimants in the audit data because states vary in whether they employ a formula based on highest quarter earnings (which is included in BAM) or some other function based on multiple quarters of earnings (which are not). For the states using only highest quarter earnings, I hand-coded each benefit schedule in order to identify where benefits hit their maximum as a function of previous earnings. These schedules were collected from the Department of Labor and are updated every six months.[9] In several cases I manually inputted more exact start dates for changes in the benefit schedules based on a visual inspection of the first stage figure.

To restrict to states and periods where benefits closely track the reported schedule, I drop schedules where a simple regression of benefits on highest quarter earnings interacted with a post-kink indicator yields an r-squared lower than 0.90.[10] Finally, as in Landais (2015), I drop claimants who have below the maximum number of weeks of eligibility, as this confounds the kink in benefits. In Figure A.7, I show the sample sizes for each state employed in the RKD analysis.

3.1 Regression kink results

Figure 2 shows the first stage figure combining all claimants in the RKD sample. The figure shows that, after the sample restrictions, the benefits in the analysis sample closely match the

[8] I originated digitizations of state UI laws from 1978 to the present. They are available at this link.

[9] This data is made available at http://maximmassenkoff.com/data.html.

[10] All analyses are robust to the choice of r-squared threshold.

benefit schedule. Figure 3 shows the reduced form figures, with weeks claimed and unemployment duration on the y-axis. The plots show evidence of a discontinuous change in the slope at the kink point, indicated by the red line. The two grey lines show linear fits of the data on either side of the kink point.

Next, I turn to estimating the responses in a regression framework. Since the first stage in Figure 2 shows that not all claimants get the exact benefit amount predicted by their schedule, I employ a fuzzy RKD as in Card et al. (2015b).[11] I use the bias-corrected estimator described in Calonico et al. (2014) in order to calculate optimal bandwidths, and cluster standard errors at the benefit schedule level. Across specifications, I vary the bandwidth and polynomial order to gauge the stability of the estimates.

The results with logs of weeks claimed, duration, and expected duration as the outcomes are shown in Table 2. The duration measures taken at audit suggest an elasticity between .14 and .53. This implies that for every 10 percent increase in weekly benefit levels, weeks claimed at audit increases by 1 to 5 percent. These elasticities broadly overlap with the estimates from Landais (2015), who found elasticities between .2 and .7 using data from the early 1980s on Idaho, Louisiana, Missouri, New Mexico, and Washington, and are at the lower end of estimates from Card et al. (2015a), using data from Missouri. The estimates for the expected duration measure are smaller, suggesting an elasticity between .04 and .14.

A valid RKD estimate requires that the density and covariates evolve smoothly over the kink point in highest quarter earnings. Unlike Landais (2015) and Card et al. (2015a), these tests cannot be performed on the full analysis data due to the nature of the sample: Claimants with longer spells are more likely to get audited. If any groups are more likely to respond to benefits, the audit sample in the present data will display a kink in covariates. For instance, if only men respond to benefit levels, the share of men in the sample will change discontinuously above the kink point as men decrease in their relative probability of being audited compared to women.

I can perform similar tests of the assumptions restricting to claimants audited in the first weeks of their spells, since these claimants are uncontaminated by the dynamic selection issue.

[11]In practice, the estimates are quite similar between sharp and fuzzy.

Restricting to this sample I plot a histogram of highest quarter earnings around the kink point in Figure A.4, which shows that the density evolves smoothly around the kink point at 0. Next, as in Card et al. (2015a), I use all the pre-determined covariates in the sample to predict the two main outcomes, log weeks claimed and log duration, using OLS. I plot these against the centered highest quarter earnings measure in Figure A.6. The fitted values evolve smoothly over the kink point, suggesting that there is no discontinuous change in the derivative of covariates at the kink point.

3.2 Effects on search

In Figure 4, I show similar reduced-form plots using the full sample for the four main search behaviors: the log reservation wage, the reservation ratio, and indicator for switching occupations, and the number of contacts. In each case, the plots suggest no obvious effects of benefits on these search outcomes. The reservation ratio gives a visual indication of a kink, but it is in the opposite direction than would be expected, suggesting that benefits decrease the reservation ratio.

As with the tests of the RKD assumptions above, these could be confounded with duration when estimated on the whole sample. For instance, it could be that people with low reservation wages are most affected by benefit levels. As highest quarter earnings increase and claimants reach the maximum level of benefits, those with low reservation wages will constitute relatively less of the sample, resulting in a spurious kink in the reservation wage. In theory, it should be feasible to counteract these effects using weights. However, due to limitations in the BAM data, it is difficult to precisely know each claimant's probability of having been selected. Thus, in the analysis below, I present estimates using the potentially contaminated full sample, and only claimants in their first week. The results are the same across the two samples.

The reduced-form results for the four main search measures using only claimants in their first week are given in Figure A.5. In each case, the search measures show no sign of a kink where benefit levels reach their maximum. The coefficient estimates from the regressions, reported in Table 4 are similar. The results for the reservation wage outcomes, for instance, flip between positive and negative and significantly detect a kink in the linear specifications, where the effect suggests that the reservation ratio *decreases* with benefits.

These findings contrast with Feldstein and Poterba (1984), who find that reservation wages increase as much as 4 percent for every 10 percent increase in benefits. Still, the implied confidence intervals encompass potentially significant responses. The first specification in column (1) cannot reject an elasticity as large as .12 and .13 for the reservation ratio and reservation wage, respectively. For comparison, Nekoei and Weber (2017) find a 0.5 percent increase in wages in response to a substantial 30 percent increase in potential benefit duration. This implies a small elasticity that is certainly consistent with these findings.

4 Validation of search measures

The previous results suggest that claimants spend more time unemployed when monetary benefits are more generous. However, it finds no effects of unemployment benefits on the recorded search behaviors. While this is consistent with findings on the reservation wage in Le Barbanchon et al. (2017), this section presents some validation exercises to show that the measures do vary in the expected ways with other economic factors.

4.1 Relationship between reservation wages and reemployment wages

First, I test whether the reported reservation wages in the BAM data are related to the ultimate reemployment wages of the claimants. This is possible using the matched Missouri sample described in Section 2.1. I regress log reemployment earnings—i.e., the log of the claimant's first positive quarterly earnings following the claim, derived from the Missouri wage data—on the log reservation wage, with fixed effects for the month the claim was started. The results are in Table 5, where across the columns (1)-(4) I incrementally include person-level covariates. Importantly, the log of the previous hourly wage enters beginning in column (2).

In each specification, the coefficient on the reservation wage is positive with $p < .01$. The coefficient drops by roughly half when I control for the log of the previous hourly wage and drops slightly more as I add controls for demographics and industry, indicating a .4 percent increase in reemployment earnings for every 1 percent increase in the reservation wage in the most restrictive specification. This echoes the findings in Krueger and Mueller (2016) and DellaVigna and Paserman (2005), where reservation wages are predictive of accepted job offers and reemployment earnings. However, it presents the first evidence to my knowledge connecting administrative wage records to survey responses on the reservation wage.

These results suggest that answers to the reservation wage question in the BAM data are not meaningless, containing information about the claimant's eventual employment. Despite the correlation, the r-squared in the last column, which removes the reservation wage from the most controlled specification, demonstrates that not much information is added once accounting for occupation, previous wage, and the other controls.

4.2 Duration dependence

Next I study how the reported search behaviors vary with weeks claimed. The results in Table 6 show regressions with the different search behaviors on the left hand side and, along with state-by-year fixed effects, three sets of regressors on the right: weeks claimed, weeks claimed with controls, and weeks claimed with person fixed effects for a small subsample of people who were audited twice in the same claim.[12]

The results in column (1) show how search behaviors associate with spell duration at audit. For ease of interpretation, the coefficients are scaled to represent the change in the outcome for every additional 10 weeks of UI duration. By four out of the five measures, search intensity is higher as the spell progresses: after 10 weeks on UI, the reservation wage is lower by 1-2 percent. Claimants also report slightly more contacts and are 2 percentage points more likely to be looking for a different occupation.

Except for the reservation wage regressions, every coefficient gets smaller with the addition of detailed controls on the claimant including past earnings, education, and demographics. In particular, the confidence interval in the number of work contacts specification rejects very small responses. This contrasts with Skandalis and Marinescu (2019), who, using online search job

[12] These qualitative findings do not change when using share of eligible weeks claimed as the measure of duration.

application data, document a spike in applications close to benefit exhaustion.

The one result that remains fairly consistent across spacifications is that the reservation wage declines moderately with duration. The best other source for this relationship comes from the longitudinal survey in Krueger and Mueller (2016), where the estimates are quite similar: they find that reservation wages decline 0.5 to 1.4 percent for every ten weeks of unemployment, compared to my estimates of 1-2 percent. This consonance is interesting in part because of the differing samples: Krueger and Mueller followed all claimants from their first week, while the audit data oversamples those with longer durations.

4.3 Cyclicality

Finally, I study how the search measures vary with the state unemployment rate. Little is known about how job search behaviors vary over the business cycle, although the cyclicality of continuing and new hire wages have long been a focus in macroeconomics (Keynes, 1936; Bils, 1985; Gertler et al., 2016). The coverage of the data provides a unique opportunity to study how job search behaviors vary with local labor market conditions.

Table 7 shows how the different search measures vary with the state unemployment rate. Each point estimate reports a β from the equation below estimated using OLS:

$$y_{its} = \alpha + \beta u_{st} + \delta_t + \Omega_s + \epsilon_{its} \tag{2}$$

where y_{its} is the reservation ratio, log reservation wage, log sought wage, switching jobs indicator, or number of job contacts for claimant i in month-year t and state s, u_{st} is the unemployment rate in percent in that month-year, δ_t indicates month-year fixed effects, and Ω_s indicates state fixed effects. Standard errors are clustered at the state level.

As discussed in Section 3.2, the sample selection inherent to the audit process could confound these estimates. Separate from this issue, any results on cyclicality will capture a mix of selection into unemployment and actual changes in search behaviors caused by the business cycle (Krueger et al., 2011). I add increasingly stringent controls across columns to partially address these potential sources of bias: Column (1) uses the full sample; column (2) adds fixed effects for weeks claimed as controls. Column (3) uses only first week claimants and column (4) adds the full set of claimant-level controls including age, education, ethnicity.

The results show a stable and strong correlation between the reservation wage measures and the state unemployment rate. The first point estimate in column (1) implies that the reservation ratio decreases by half a percent for every one percent increase in the state unemployment rate. Columns (2) and (3) suggest that this result is not simply due to claimants being further along in their spells on average when unemployment is higher, and column (4), which adds tight controls including fixed effects for state-industry-occupation, suggests that selection into unemployment is also not driving the finding.

The results show no correlation between job contacts and the unemployment rate. This contrasts with Mukoyama et al. (2018), which finds a positive association between time spent searching for a job and the state unemployment rate, but the confidence intervals do not rule out the slight (but significant) effects found in their analyses.

5 Discussion

The preceding sections find clear evidence using large-scale data and a quasi-experimental design that unemployment durations increase in the monetary level of benefits, complementing existing work from Landais (2015) and Card et al. (2015a). The unique data allows me the probe the mechanism further by estimating the effects of benefits on search behaviors. None of the search behaviors appear to respond to benefits. This is despite the fact that the reservation wage measures appear reliable across several validation exercise comparing them to the reemployment wage, unemployment duration, and local unemployment rate.

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Figure 1: Reservation ratio

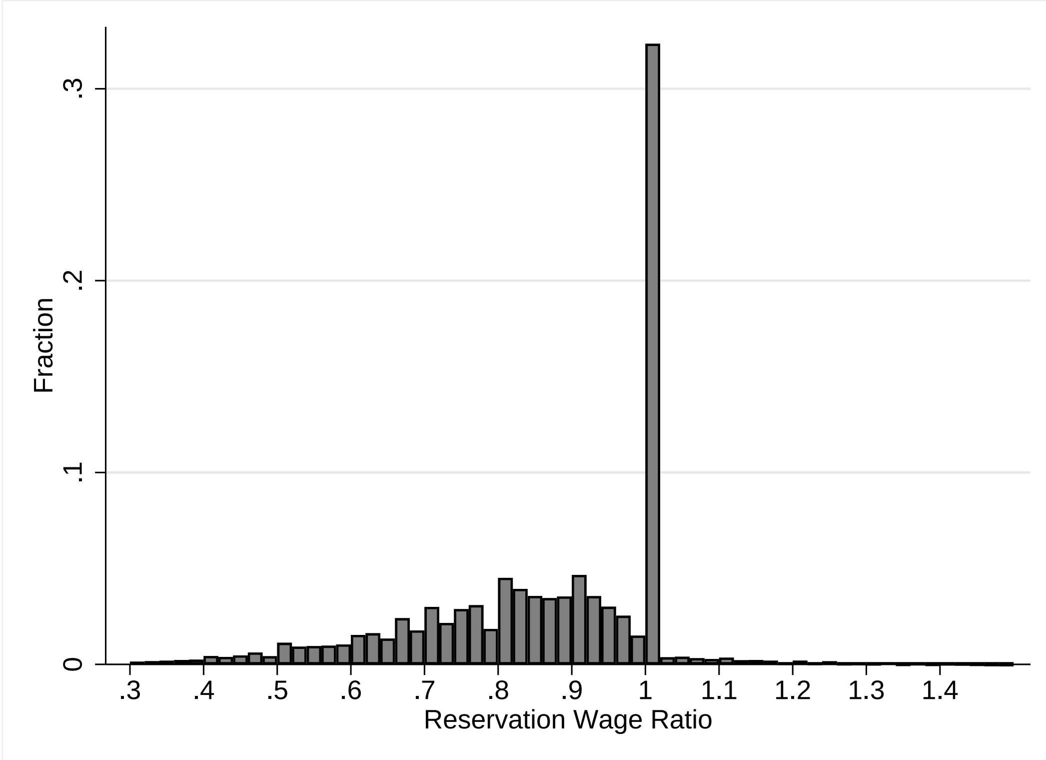
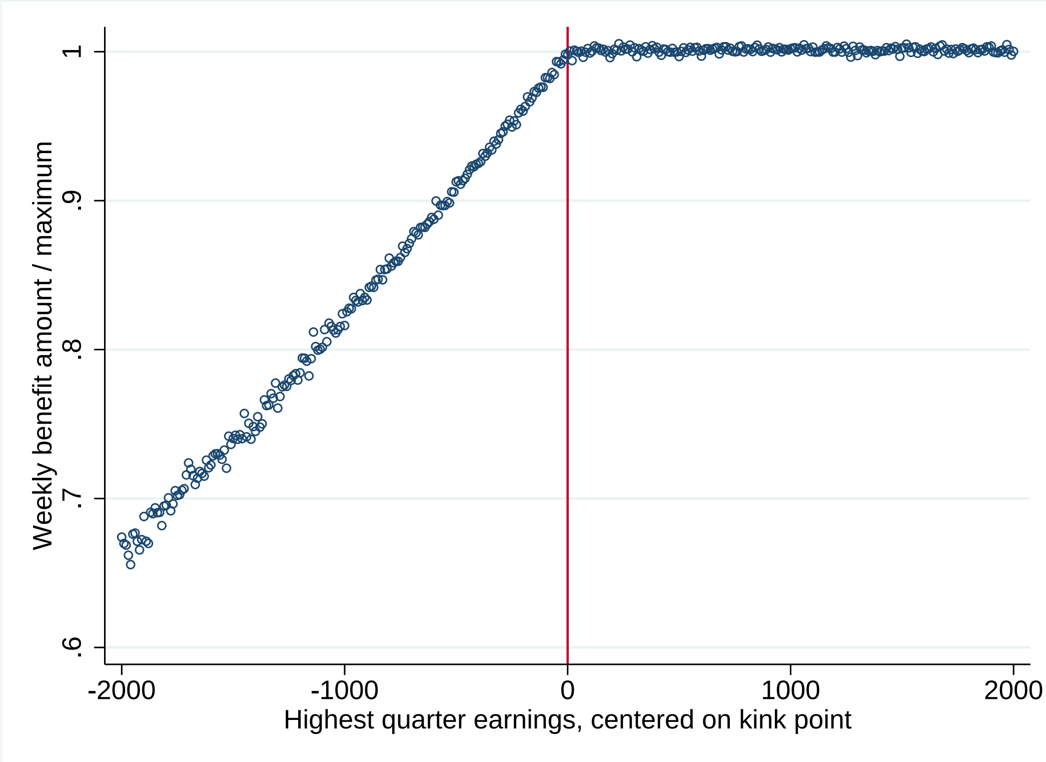


Figure 2: Regression kink design first stage



N=84,684. The y-axis gives claimant weekly benefit payments divided by the maximum weekly benefit amount in their benefit schedule. The x-axis gives claimant highest quarters earnings minus the point in their benefit schedule where the benefit amount reaches its maximum. Each dot represents an average using $10 bins of highest quarter earnings.

Figure 3: RKDs showing effect of UI benefits on duration

(a) Weeks claimed at audit

(b) Ln(unemployment duration) at audit

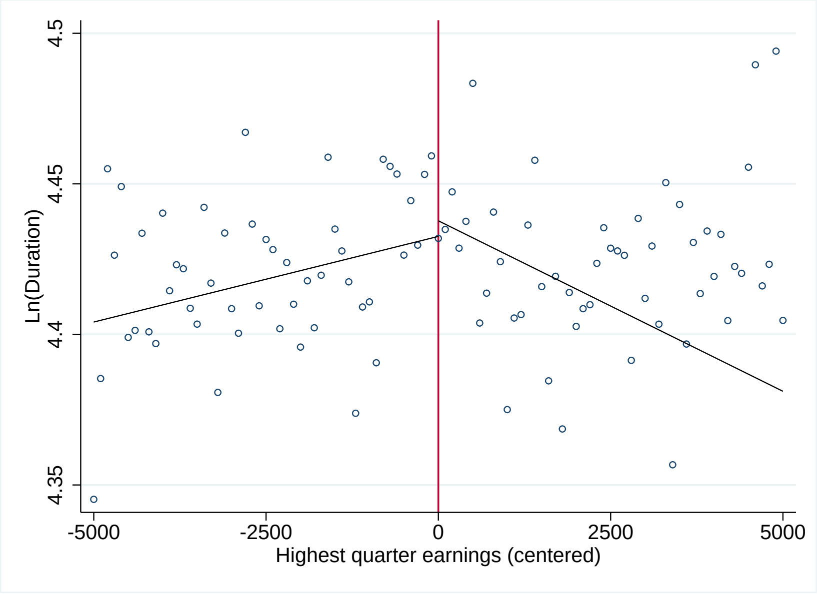
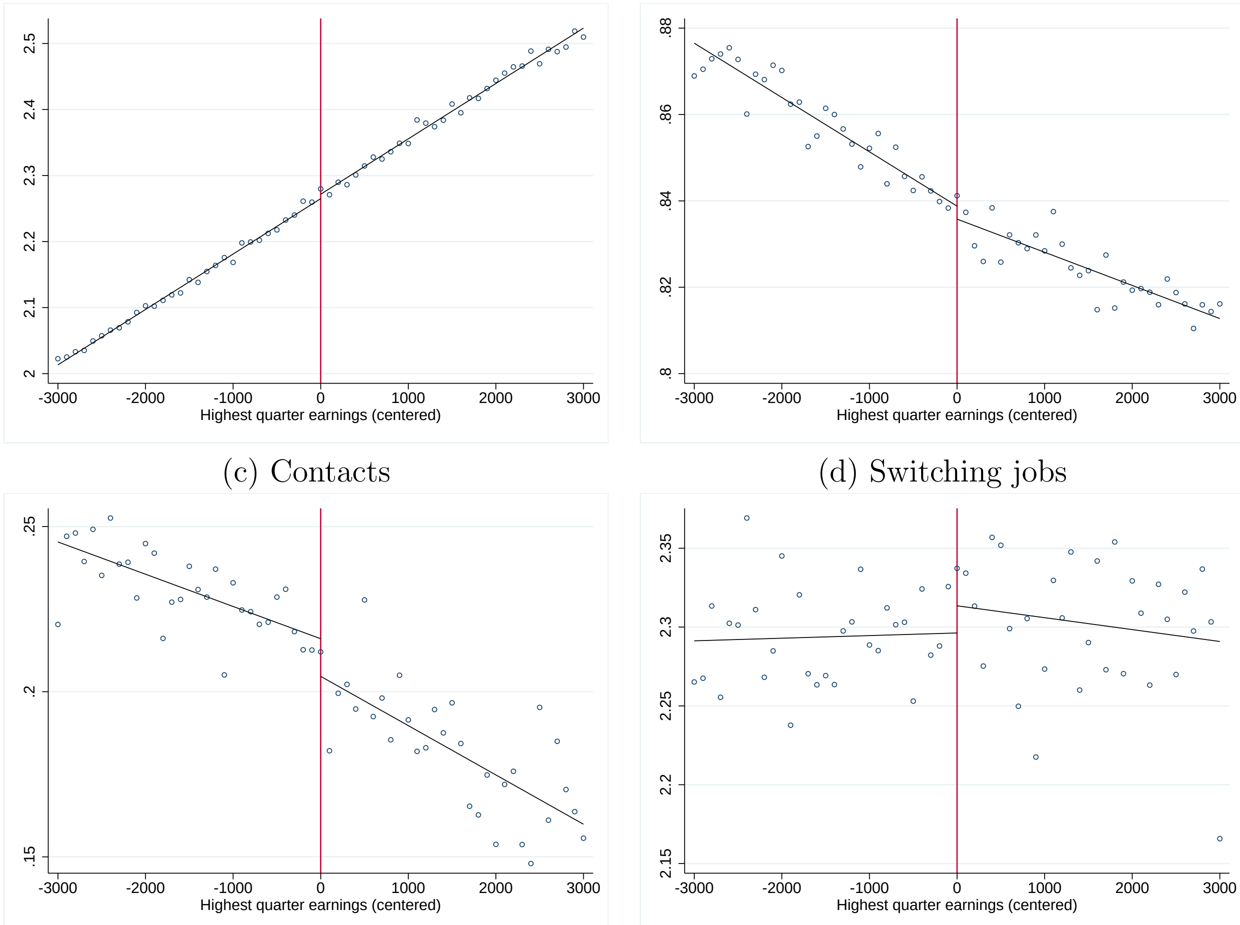


Figure 4: RKDs showing no effect of UI benefits on search behaviors



(a) Ln(reservation wage)

(b) Reservation ratio

(c) Contacts

(d) Switching jobs

Table 1: Descriptive statistics

Variable	(1) All	(2) First week	(3) Long term (≥20 weeks)
Weeks claimed	10.832	1.231	23.329
	(7.369)	(0.440)	(3.023)
Weekly benefit amount	230.204	222.045	238.825
	(114.490)	(110.872)	(116.645)
Reserve ratio	0.859	0.875	0.839
	(0.175)	(0.168)	(0.184)
Switching occupation	0.200	0.174	0.216
	(0.400)	(0.379)	(0.411)
Job contacts	2.145	2.104	2.092
	(1.575)	(1.646)	(1.511)
Male	0.581	0.598	0.544
	(0.493)	(0.490)	(0.498)
Any dependents	0.083	0.084	0.093
	(0.276)	(0.277)	(0.290)
Black	0.157	0.141	0.161
	(0.364)	(0.349)	(0.367)
Age	40.703	39.850	41.809
	(12.518)	(12.335)	(12.711)
College	0.124	0.118	0.137
	(0.329)	(0.323)	(0.343)
Recall	0.252	0.348	0.157
	(0.434)	(0.476)	(0.364)
Definite recall date	0.166	0.255	0.093
	(0.372)	(0.436)	(0.291)
Layoff	0.695	0.711	0.639
	(0.460)	(0.453)	(0.480)
Quit	0.040	0.032	0.048
	(0.195)	(0.177)	(0.214)
Discharge	0.188	0.155	0.208
	(0.391)	(0.362)	(0.406)
Vocational certification	0.175	0.172	0.170
	(0.380)	(0.378)	(0.376)
Employer tax rate	3.545	3.600	3.389
	(2.720)	(2.738)	(2.624)
Numer of employers	1.659	1.659	1.593
	(1.002)	(1.006)	(0.952)
Observations	913,729	80,807	144,922

Standard deviations shown in parentheses. The mean for the first week sample in column (2) is greater than one because claimants in states where two weeks are claimed at a time get the first week label in their first two weeks.

Table 2: RKD results for duration outcomes

	(1)	(2)	(3)	(4)
Ln(Weeks claimed at audit)	0.285***	0.534***	0.126***	0.610***
	(0.0953)	(0.135)	(0.0347)	(0.144)
N	66,049	125,536	125,579	188,115
Bandwidth	1446	2892	2893	5200
Ln(Duration claimed at audit)	0.409***	0.451***	0.139***	0.397***
	(0.109)	(0.165)	(0.0392)	(0.123)
N	59,485	114,395	114,944	208,771
Bandwidth	1304	2594	2608	6664
Ln(Expected weeks claimed)	0.107***	0.138***	0.0440***	0.172***
	(0.0283)	(0.0450)	(0.0104)	(0.0411)
N	62,839	116,254	120,397	187,105
Bandwidth	1375	2643	2751	5146
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Standard errors clustered at the benefit schedule level. Each coefficient gives the treatment effect of logged weekly benefit amount on the outcome indicated the leftmost column, estimated using 2SLS. MSE-optimal bandwidths are calculated using the `rdrobust` package (Calonico et al., 2017)

Table 3: RKD results for search outcomes, all claimants

	(1)	(2)	(3)	(4)
Ln(reservation wage)	0.0202	-0.0220	0.0388**	0.0352
	(0.0500)	(0.0669)	(0.0191)	(0.0632)
N	45,767	100,534	89,794	201,500
Bandwidth	1059	2395	2117	7192
Reserve ratio	-0.0594***	-0.0328	-0.0404***	0.0107
	(0.0229)	(0.0352)	(0.00959)	(0.0335)
N	56,312	106,377	108,257	197,296
Bandwidth	1316	2579	2631	6891
Switching occupation	0.0224	-0.0581	0.0180	-0.0336
	(0.0364)	(0.0802)	(0.0144)	(0.0512)
N	63,911	99,901	121,214	197,500
Bandwidth	1455	2327	2910	6411
Contacts	0.252	0.525*	0.0676	-0.0262
	(0.214)	(0.274)	(0.0769)	(0.166)
N	38,275	77,381	73,780	133,057
Bandwidth	1242	2637	2485	6977
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Standard errors clustered at the benefit schedule level. Each coefficient gives the treatment effect of logged weekly benefit amount on the outcome indicated the leftmost column, estimated using 2SLS. MSE-optimal bandwidths are calculated using the `rdrobust` package (Calonico et al., 2017)

Table 4: RKD results for search outcomes, only first week claimants

	(1)	(2)	(3)	(4)
Ln(reservation wage)	-0.0134	-0.115	0.0744**	-0.00154
	(0.0809)	(0.0962)	(0.0348)	(0.0829)
N	6,289	13,905	11,772	18,866
Bandwidth	1718	4311	3435	9384
Reserve ratio	0.0404	-0.0423	-0.0493**	0.0108
	(0.0642)	(0.0444)	(0.0211)	(0.0946)
N	5,391	15,125	10,432	16,607
Bandwidth	1488	5021	2977	6152
Switching occupation	0.0870	0.00701	0.0226	0.118
	(0.0648)	(0.0460)	(0.0275)	(0.140)
N	7,944	18,069	14,091	17,705
Bandwidth	2124	7338	4247	6862
Contacts	0.109	0.167	0.101	0.0611
	(0.413)	(0.407)	(0.143)	(1.390)
N	4,494	9,071	8,003	9,729
Bandwidth	1867	4589	3735	5349
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Standard errors clustered at the benefit schedule level. Each coefficient gives the treatment effect of logged weekly benefit amount on the outcome indicated the leftmost column, estimated using 2SLS. MSE-optimal bandwidths are calculated using the `rdrobust` package (Calonico et al., 2017)

Table 5: Reemployment wage vs. reservation wage in the matched Missouri sample

	(1)	(2)	(3)	(4)	(5)
Log Reservation Wage	0.963***	0.659***	0.467***	0.436***	
	(0.0490)	(0.0880)	(0.0899)	(0.0946)	
Log Prev Wage		0.340***	0.0328	0.0328	0.288***
		(0.0819)	(0.0965)	(0.102)	(0.0860)
Expecting recall			0.135***	0.151***	0.167***
			(0.0519)	(0.0530)	(0.0533)
Month FEs	Yes	Yes	Yes	Yes	Yes
Weekly benefits	No	No	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Industry	No	No	Yes	Yes	Yes
Last Occ	No	No	No	Yes	Yes
R-squared	0.270	0.278	0.336	0.386	0.377
N	1,673	1,673	1,673	1,673	1,673

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Duration dependence results

	(1) Res ratio	(2) Ln(Res wage)	(3) Contacts	(4) Ln(Seek)	(5) Occ switch
No controls					
Weeks claimed	-0.0176***	-0.0181***	0.0247***	0.00317***	0.0214***
	(0.000268)	(0.000665)	(0.00240)	(0.00100)	(0.000599)
r2	0.071	0.327	0.335	0.972	0.070
N	833,663	843,615	601,423	471,992	855,278
Full controls					
Weeks claimed	-0.0143***	-0.0208***	-0.0138***	-0.00395***	0.0136***
	(0.000260)	(0.000357)	(0.00251)	(0.000924)	(0.000634)
r2	0.232	0.843	0.400	0.980	0.119
N	758,942	761,830	530,233	425,050	760,640
Restricted sample with multiple audits					
Weeks claimed	-0.00809***	-0.0107***	-0.0259	-0.00431	0.00431
	(0.00244)	(0.00296)	(0.0286)	(0.00958)	(0.00691)
r2	0.903	0.984	0.839	0.998	0.833
N	7,589	7,680	5,966	3,948	7,786

Robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Coefficients are multiplied by 10 for interpretability. The first two rows include state-by-month fixed effects. Full controls includes ethnicity, sex, dependents, a quadratic in age, education indicators, two indicators for recall status, reason for layoff, employer tax rate, an indicator for work search required, weeks eligible, and the log of base period earnings and the previous wage.

Table 7: Cyclicality results

	Full sample		Just first week	
	(1)	(2)	(3)	(4)
Reserve ratio	-0.005***	-0.005***	-0.007***	-0.007***
	(0.001)	(0.001)	(0.002)	(0.001)
R-squared	0.026	0.031	0.036	0.443
N	803,196	803,196	71,477	34,291
Ln(Reservation wage)	-0.006**	-0.006**	-0.009**	-0.007***
	(0.003)	(0.003)	(0.004)	(0.002)
R-squared	0.276	0.277	0.265	0.861
N	812,542	812,542	72,207	34,690
Switch occupation	-0.001	-0.001	-0.001	-0.003
	(0.003)	(0.003)	(0.004)	(0.003)
R-squared	0.028	0.030	0.034	0.406
N	820,639	820,639	72,756	34,971
Job contacts	-0.001	-0.001	0.008	0.016
	(0.031)	(0.031)	(0.032)	(0.031)
R-squared	0.187	0.188	0.200	0.591
N	581,348	581,348	48,508	18,997
Weeks claimed dummies	No	Yes	No	No
Full controls	No	No	No	Yes
Month FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	No
State x Industry x Occupation	No	No	No	Yes

Standard errors clustered at the state level in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each point estimate is the coefficient on the unemployment rate (in percent) from a separately run specification. Observations vary across specifications because of missing values in the outcomes.

A Appendix figures

Figure A.1: Histogram of job contacts

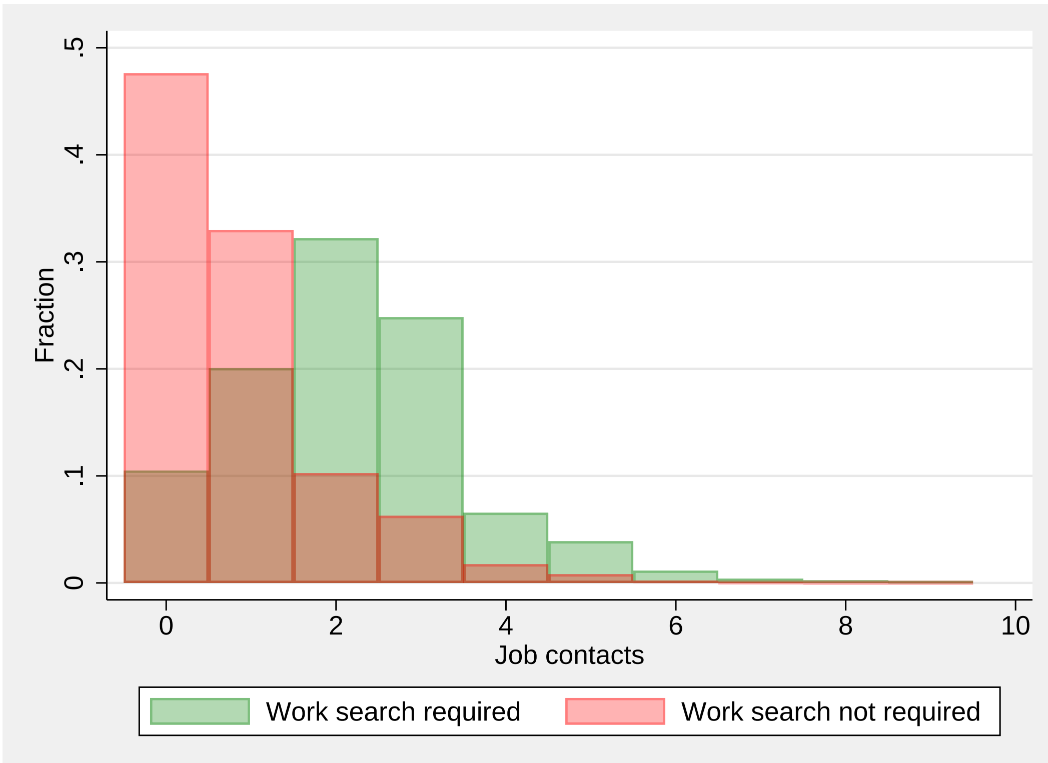


Figure A.2: Histogram of percent spell completed at audit

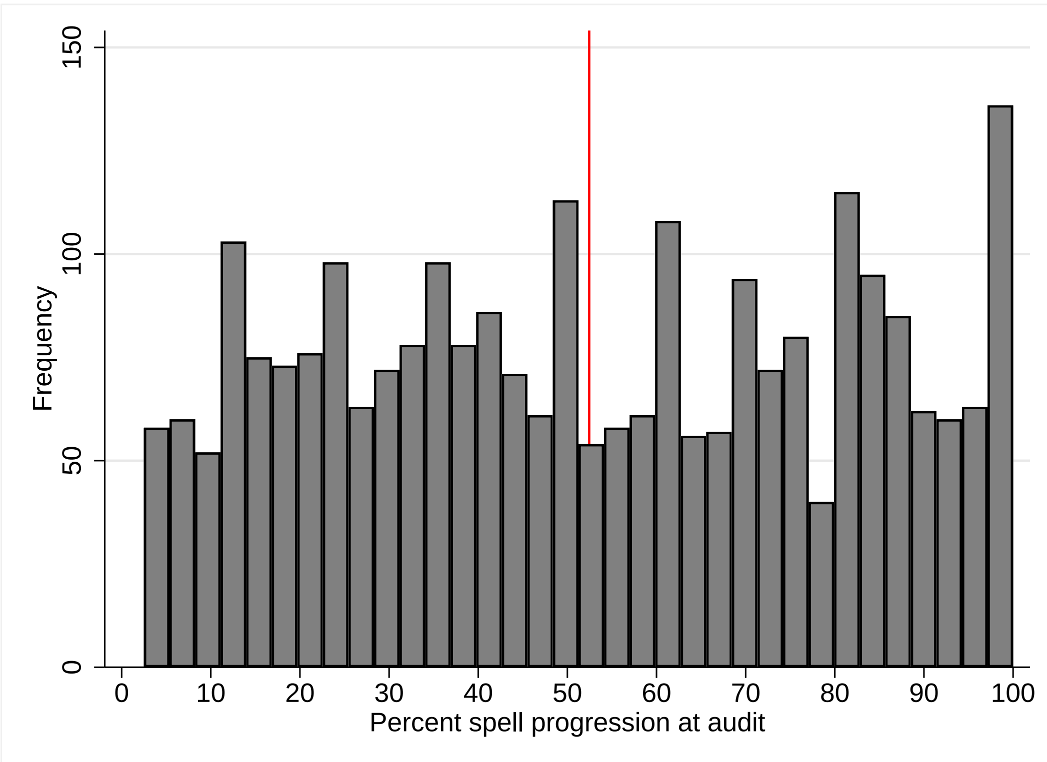
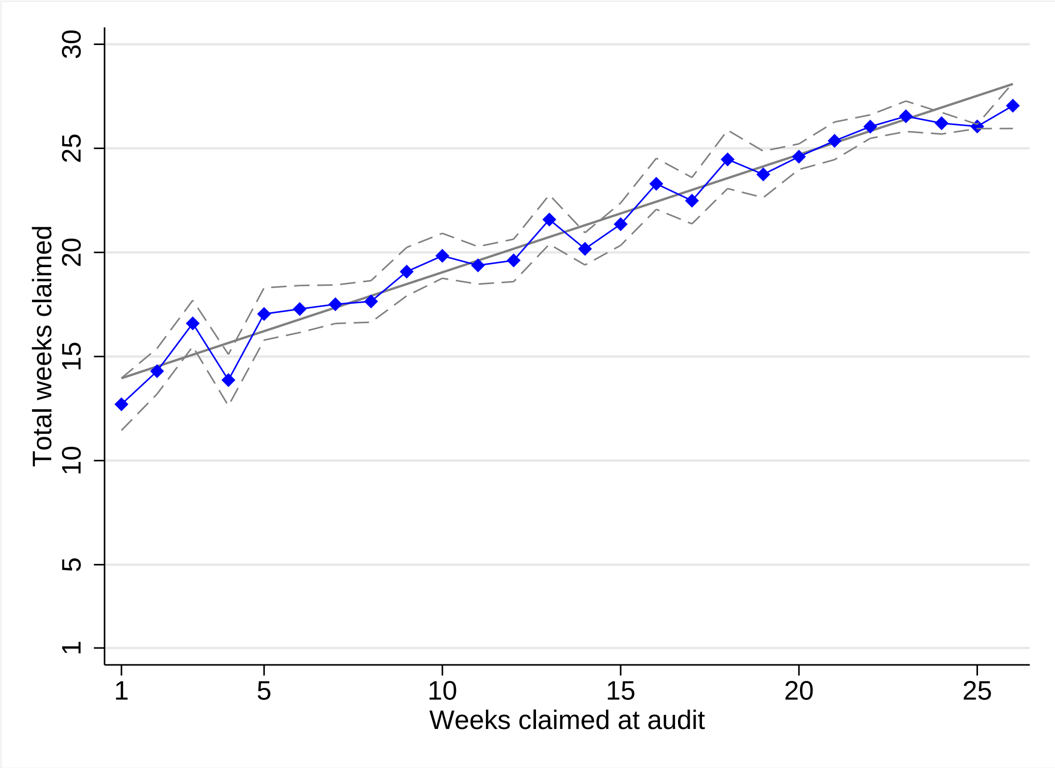
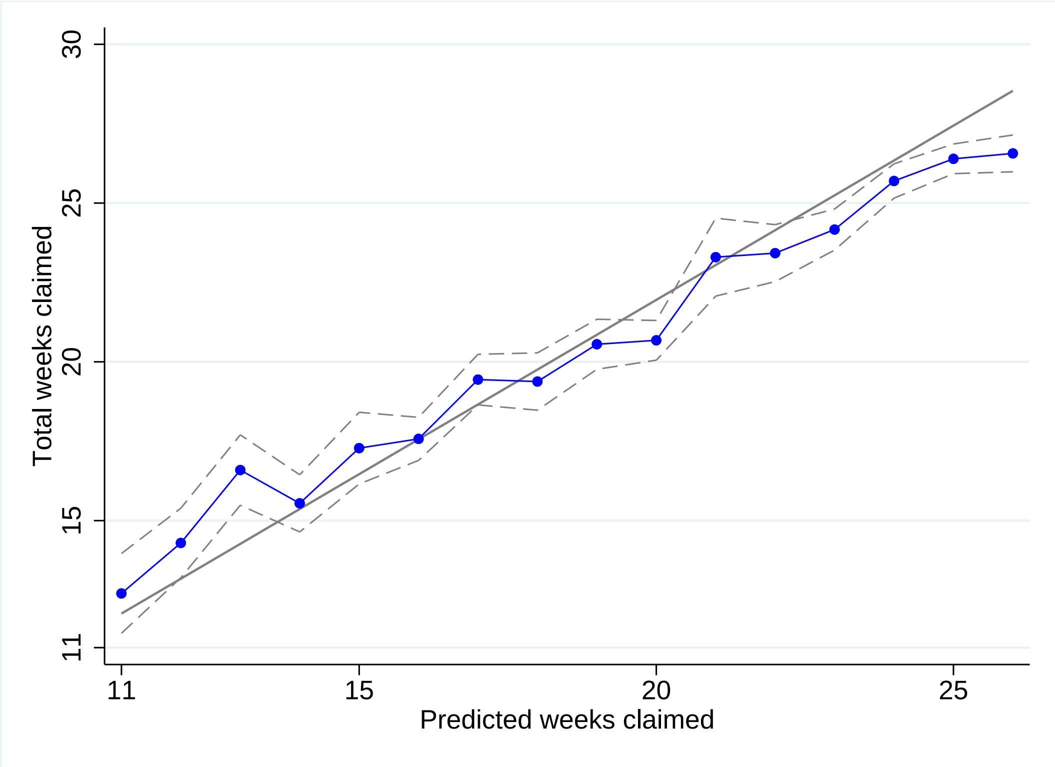


Figure A.3: Calibration using Missouri sample

(a) Total weeks claimed vs. weeks claimed at audit



(b) Total weeks claimed vs. predicted total weeks claimed



These plots show binned averages of completed duration against (a) weeks claimed at audit and (b) the expectation of completed duration using the method described in Section 2.2 for the 2,611 matched claimants from Missouri. The straight red line shows the fitted values from the estimated bivariate regression.

Figure A.4: Density check around kink point

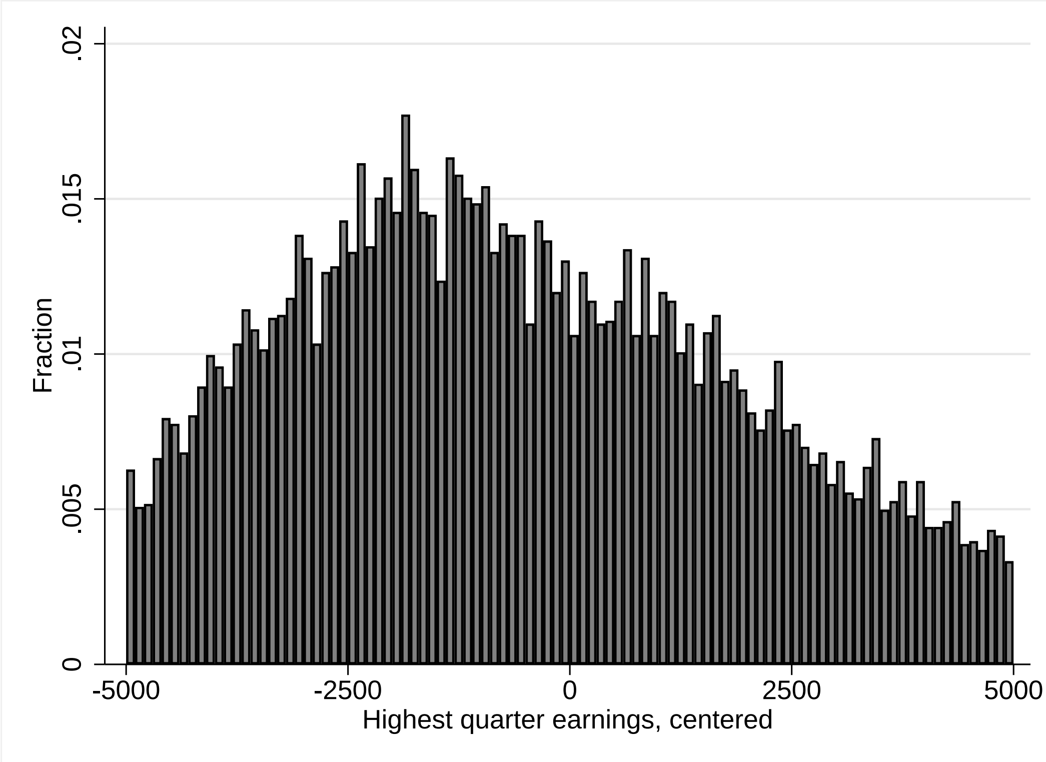
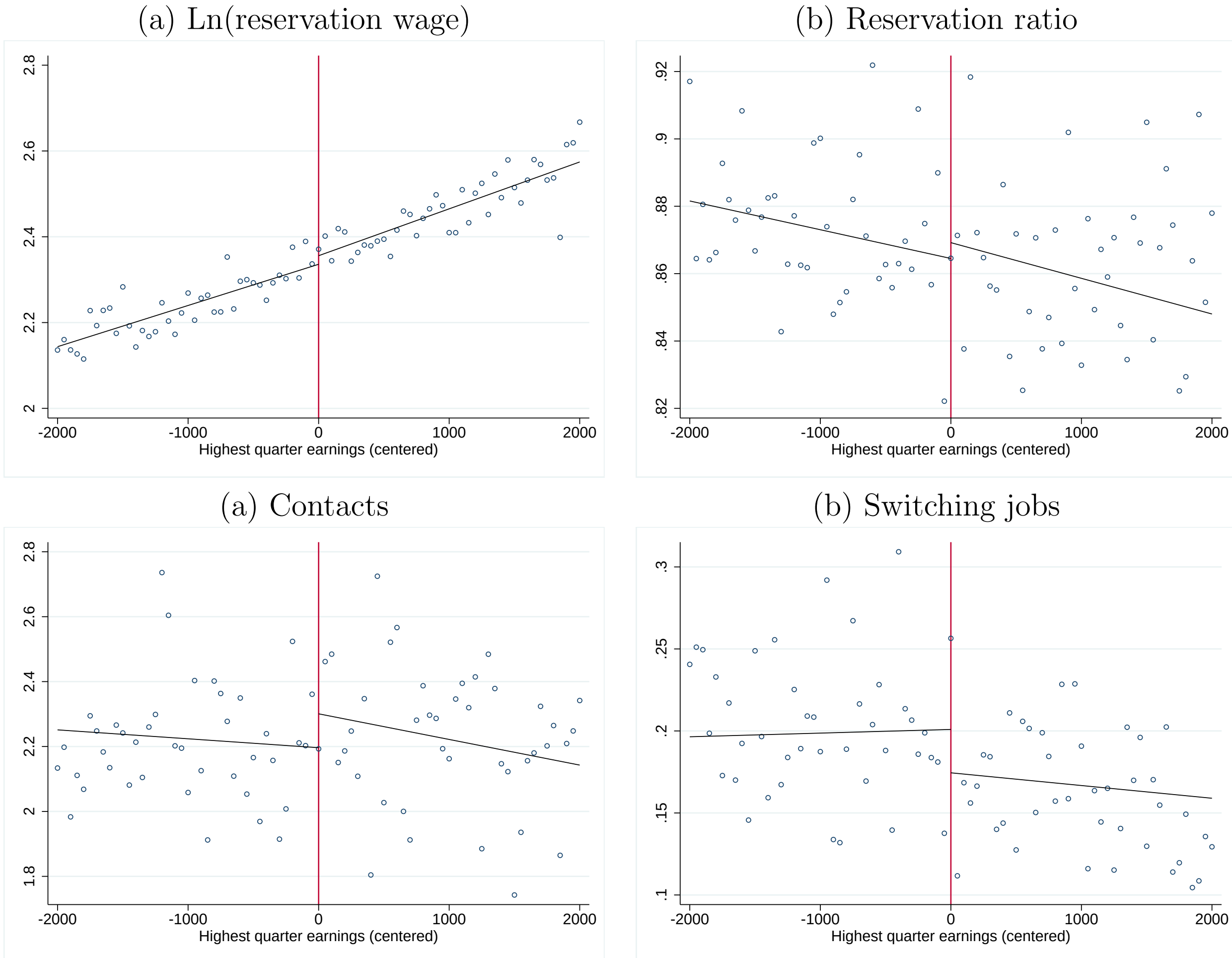


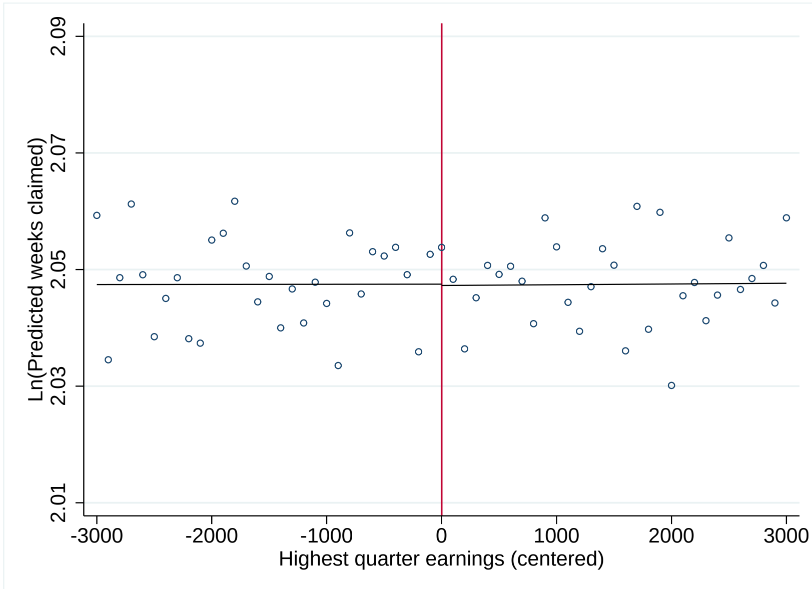
Figure A.5: RKDs showing no effect of UI benefits on search behaviors, first week sample



Notes:

Figure A.6: RKDs showing covariate balance

(a) Predicted weeks claimed at audit



(b) Predicted Ln(unemployment duration) at audit

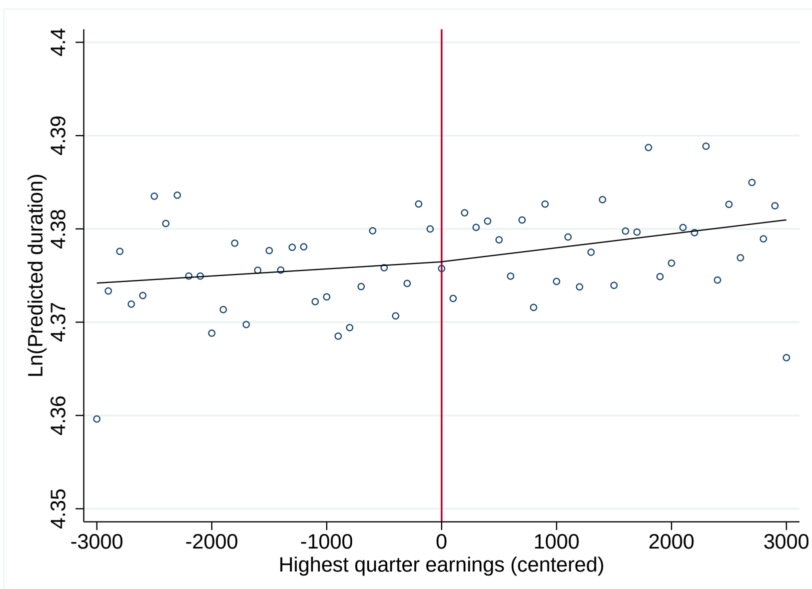


Figure A.7: State sample sizes in RKD

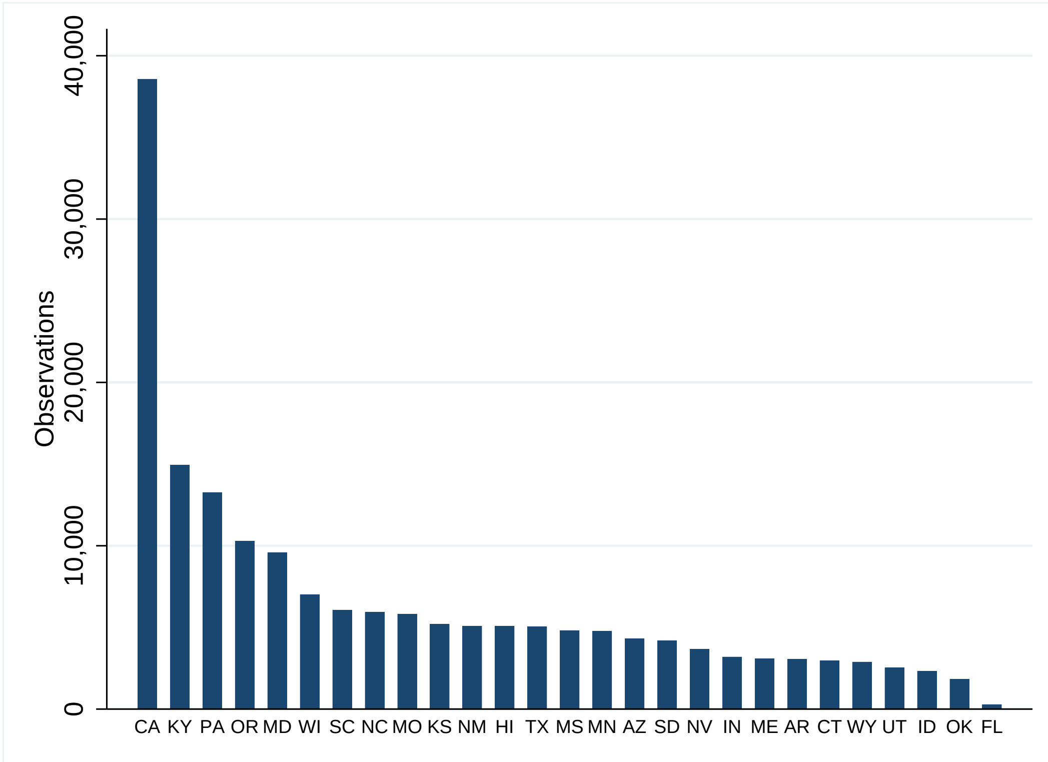


Table A.1: Weeks claimed vs. recall status

	(1) UI Duration
Expecting recall	-1.816***
	(0.328)
Definite recall date	-5.898***
	(0.476)
Month FEs	Yes
Demographics	Yes
Industry	Yes
Last Occ	Yes
Observations	2,475

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2: Weeks claimed vs. reservation wage

	(1)	(2)	(3)	(4)	(5)
Log Reservation Wage	-1.469***	-4.471***	-3.633***	-3.592***	
	(0.313)	(0.570)	(0.548)	(0.577)	
Log Prev Wage		3.317***	0.251	0.0474	-2.223***
		(0.528)	(0.612)	(0.646)	(0.538)
Expecting recall			-1.734***	-1.631***	-1.699***
			(0.317)	(0.325)	(0.328)
Month FEs	Yes	Yes	Yes	Yes	Yes
Weekly benefits	No	No	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Industry	No	No	Yes	Yes	Yes
Last Occ	No	No	No	Yes	Yes
R-squared	0.160	0.174	0.295	0.324	0.312
N	2,475	2,475	2,475	2,475	2,475

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$