Job Search and Unemployment Insurance: New Evidence from Claimant Audits

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December 21, 2020

Abstract

Do higher unemployment insurance benefits reduce job search among the unemployed? I examine this question using data from verified audits of US claimants giving their weekly benefit amount, reservation wage, targeted occupation, and number of job contacts made. In a regression kink design, I find that weekly monetary benefits increase unemployment duration but have no impact on search behaviors. As evidence against misreporting, I show that reservation wages predict reemployment wages and decrease with unemployment duration and the local unemployment rate. These results suggest that explicitly measured search behaviors may not explain the duration response to benefits.

*Naval Postgraduate School, maxim.massenkoff@nps.edu. For thoughtful discussions and comments, I thank David Card, Stefano DellaVigna, Andrew Johnston, Marta Lachowska, Camille Landais, Evan Rose, Nicholas Li, Jesse Rothstein, Yotam Shem-Tov, Daphne Skandalis, Johannes Spinnewijn, Chris Walters, Rob Valetta, Nathan Wilmers, Steve Woodbury, and participants at the LERA session on unemployment insurance at the 2019 ASSA meetings. Special thanks to Cristobal Young, who provided an earlier version of the BAM data, and Andrew Spisak and Ross Miller at the Department of Labor for help understanding and accessing the BAM data. All errors are my own.

1 Introduction

Understanding the behavioral response to unemployment insurance (UI) is crucial for determining the optimal level of benefits (Chetty, 2008). However, due to data limitations, the key quasi-experimental evidence on this question in the United States is restricted to claimants from more than three decades ago (Landais, 2015). Moreover, researchers quantifying behavioral responses to benefits can typically study only impacts on unemployment duration, with no additional information on the behavior of claimants (Johnston and Mas, 2018; Card et al., 2015; 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. Further, the data include administrative information on the claimant's exact monetary benefit amount and inputs into their benefit formula. These data present a unique opportunity to study job search, the duration response to UI generosity, and the relationship between the two.[1]

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

[1] These data are also used in two working papers, Young (2012) and Ferraro et al. (2020), discussed more below.

et al., 2015).

Throughout, a key issue in the BAM data is that the only duration measure available is the *interrupted* duration—the length of unemployment at the time that the claimant was selected for audit. In the main analyses I use outcomes directly taken from the data: weeks claimed and duration at audit (which is different from weeks claimed due to lapses in claiming). I augment this with a measure of expected duration, which is non-parametrically imputed using the distribution of interrupted spell lengths as in Salant (1977) and Baker and Trivedi (1985). I show using a subsample of claimants for which I know completed duration that this method provides a good approximation.

I find a positive elasticity of duration at audit to the UI monetary benefit amount, with an increase of roughly 4-9 percent for every 10 percent increase in benefit levels. The effects on my proxy for completed duration are somewhat smaller, ranging from to 1-3 percent. This latter finding is similar to although broadly lower than other estimates in the United States studying the effects of benefits on completed duration; Landais (2015) finds an elasticity between .2 and .7 and Card et al. (2015), 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 the 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, or occupation choice respond to benefits. The 95% confidence interval rules out elasticities above .1 for the reservation wage and suggests that a 10% increase in benefits causes no more than a 0.05 decrease in the number of job contacts. These results persist using both claimants in the entire sample, where estimates could be affected by length-biased sampling, and claimants in their first week, who are uninfluenced by such selection.

These findings present some of the first quasi-experimental evidence in the US evaluating the effects of UI benefits on search behavior and the reservation wage. Young (2012) and a contemporaneous paper, Ferraro et al. (2020), use older BAM data and a different identification strategy to find that benefits *increase* the probability of job search. While my results cannot rule out some small increases in search behavior, the identification assumptions in Ferraro et al. (2020) are arguably more demanding, requiring that the distribution of earnings across quarters is exogenous conditional on total earnings and a set of controls.

Apart from these studies, the most comparable US papers study search behaviors using aggregated online search data. Marinescu (2017), using state-level variation in potential benefit duration from the Great Recession extensions and data from CareerBuilder.com, finds that a one percent increase in potential benefit duration decreases job applications by 0.4 percent, but Baker and Fradkin (2017), using a similar methodology with Google search data, find effects closer to zero. The most similar work uses administrative data from France. Le Barbanchon et al. (2017) find a precise zero effect of unemployment benefits on the reservation wage, concluding that the duration response to potential benefits must be due to search effort. Marinescu and Skandalis (2019) record distinct spikes in online job applications around benefit exhaustion.

Smaller survey-based studies have similarly had mixed results. 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 such 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, but, 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. Separate from these studies, several experiments consider the consequences of work search requirements for the unemployed, which is a key motivation for the audits (e.g. Lachowska et al., 2016; Johnson and Klepinger, 1994).

While search behaviors in the present context 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 between 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 been able to connect 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 as recorded in the administrative quarterly wage records, with a significant elasticity of .4 even controlling for past wages.

Next, I show 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. These findings complement recent work using surveys (Krueger et al., 2011; DellaVigna et al., 2020) and online job boards (Marinescu and Skandalis, 2019; Faberman and Kudlyak, 2019) to study job search over the unemployment spell, with the advantage that responses in this context are investigated.

I also explore the cyclical qualities of the responses. Claimants lower their reservation wages by 0.6 to 1 percent for every 1 percent increase in the state unemployment rate, a result that holds using either all claimants or only claimants in their first week. Interestingly, 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 analysis records effects on job choice and work contacts that are close to zero and precisely estimated. The non-response in job contacts contrasts with recent work documenting increased search intensity in weaker labor markets using survey data (Mukoyama et al., 2018) and online job boards (Faberman and Kudlyak, 2019).

While the job choice and contacts measures show no response to benefits, duration, or the local unemployment rate, they both exhibit clear negative relationships with the internal measure of reservation wages, consistent with a notion of selectivity driving the three behaviors: Claimants with lower reservation wages are more likely to be switching occupations and make more job contacts. Also, as in DellaVigna et al. (2020), I find that reported job contacts are lower during holidays.

Taken together, these findings challenge the view that UI duration depends on the measurable 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.

One potential explanation is that claimants wield storable job offers that they can time to begin when their benefits elapse (Boone and van Ours, 2012). If this is what drives the duration response to benefits, reported search activities have limited relevance. However, evidence from existing studies argues that this mechanism is not core to job search patterns. DellaVigna et al. (2020) find that job seekers do not time their start dates to coincide with UI exhaustion. Further, although more indirectly, several studies (e.g. Card et al., 2007) find that recalls, which might have more negotiable start dates given an existing relationship with the employer, do not show a markedly higher exit spike at exhaustion.

2 Data

The data covers 921,801 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; this paper uses the paid claims component.[2] Interviews are mostly conducted in-person or over the phone and focus on the respondent's activities in a week for which they claimed benefts (the "key week"; US Dept of Labor, 2009). Across all audits, 25 percent uncover an erroneous payment, which half of the time results in a change to the 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 chosen from a list all 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.[3] 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 (the original

[2] The most recent BAM Annual report is available here.
[3] See the BAM operations guide (US Dept of Labor, 2009) for more details.

response is not available). In Figure 1(a), 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 and Le Barbanchon et al. (2017), the reservation wage ratio in this context is less dispersed and much less likely to exceed one, which could reflect reporting issues that are unique to this context. In all results, I use both the logged reservation wage and the reserve ratio as outcomes to ensure that no results hinge on potential idiosyncrasies in the reserve ratio.

Contacts are recorded in a worksheet with spaces for the organization's 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. Job contacts can include inquires made with unions or private employment agencies and do not necessarily need to result in applications. The distribution of the contacts variable is shown in Figure 1(b), split by whether the claimant reported to be awaiting a job recall.

One concern with this measure is that claimants may simply report the statutorily required number of contacts. However, the contacts are investigated by BAM employees at the state office, and a variable records the number of contacts that were deemed legitimate. All the results presented below are qualitatively the same using only the number of acceptable contacts,[4] and as a further check on the reliability of the measure I show in Section 4.4 that reported contacts are lower during holidays as in DellaVigna et al. (2020).

Finally, claimants are asked to report which occupation they had previously and which occupation they are now seeking, recorded using 3-digit SOC 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.

A strength of the audit context is that answers to these questions are investigated by the examiner. The data shows that the state UI office investigates almost all contacts reported by the claimant, and some claimants are cited for refusal of acceptable work, albeit rarely. This could

[4] Results available on request.

make the data especially reliable compared to other unvalidated survey-based studies of UI (e.g. DellaVigna et al., 2020; Krueger and Mueller, 2010).

However, the data are not without limitations. The reservation wage and targeted occupation measures are based on self-report; data on job applications may give a more accurate picture of job search activities (e.g. Marinescu and Rathelot, 2018). Further, while claimants might honestly report excess contacts in order to make sure that they meet the requirement in the event that one contact is unverifiable, they may also restrict themselves to the exact amount mandated if they perceive that excess contacts would increase the chance of sanction.[5]

Another limitation is that the data are length-biased: a claimant with 10 weeks of total duration is twice as likely to be audited as a claimant with 5 weeks of total duration. This means that, like any point-in-time sample, the full BAM sample will not be representative of the population of people who ever claim UI. More problematically, this could lead to spurious effects on the search outcomes. For example, if only claimants with low reservation wages drive the duration response to benefits, a regression kink specification on the full sample would suggest that benefits increase reservation wages. In practice, this appears not to be a significant issue as robustness checks using only first-week claimants confirm the main findings.

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. 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). For instance, in the first specification, claimants expecting recall spend 1.9 fewer weeks on UI ($se = 0.3$).

[5] I thank a referee for noticing this.

2.2 Duration measures

These duration outcomes taken at the audit are lengths of interrupted spells, but are arguably most useful as proxies for eventual completed duration. 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.1 shows a histogram of the ratio of weeks claimed at audit to total weeks claimed 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.

At the claimant level, however, there is a more complicated relationship between completed and interrupted spell length. Figure A.2(a) plots the completed duration against the interrupted duration for the same sample of Missouri claimants. The linear fit has a positive constant and a slope close to 0.5. 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 of the interrupted spell lengths (in weeks), $F(w)$, and a survival function $S(w) = 1 - F(w)$. Then the expected duration D_e conditional on observing a claimant with an interrupted spell T equal to w is

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

where w_{max} is the maximum number of weeks available, usually 26. This is simply a weighted average taken across the weeks greater than or equal to w.

I validate that my method of expected duration is a good proxy for completed duration using the Missouri sample. Figure A.2(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 linear fit is 1.09. In the duration results below, I present effects on weeks claimed and duration at audit, as well as this expected weeks measure. This method requires a stationary assumption because $f(t)$ is assumed to be stable; in practice, restricting to smaller time periods did not affect the fit between expected and actual duration in the Missouri 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., 2015; Landais, 2015).

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. 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 postkink indicator yields an r-squared lower than 0.50.[6] Finally, as in Landais (2015), I drop claimants who have below the maximum number of weeks of eligibility, as this kink in duration confounds the kink in benefits. In Figure A.3, 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 benefit schedule.

The main RKD specification stacks variation at each kink point in the sample in order to estimate a single treatment effect of benefits on the duration and search outcomes. To account for the fact that most benefit schedules have distinct maximums, and to absorb variation across time and space, I employ cell fixed effects in the RKD specifications, where each cell is a unique combination of state, replacement rate, and benefit maximum. Further, since the first stage in Figure 2 shows that not all claimants get the exact benefit amount predicted by their schedule, I use a fuzzy RKD as in Card et al. (2015), which can be estimated using 2SLS and a uniform kernel. The formal first stage and second stage equations are given below:

$$b_{ic} = \gamma_c + \gamma_0 D_{ic} + \sum_{p=1}^{\overline{p}} (\sigma_p HQE_i^p + \eta_p HQE_i^p D_{ic}) + e_{ic} \tag{2}$$

$$y_{ic} = \alpha_c + \alpha_0 D_{ic} + \tau b_{ic} + \sum_{p=1}^{\overline{p}} (\delta_p HQE_i^p) + \epsilon_{ic} \tag{3}$$

In this setup, b_{ic} gives claimant i's weekly benefit amount in benefit schedule cell c. Next, γ_c and α_c denote cell fixed effects, D_{ic} is an indicator for being above the kink point, HQE_i is highest centered quarter earnings, $\overline{p}$ denotes polynomial order, and τ measures the causal effect of monetary benefits on the outcome y_{ic}. The excluded instrument is $HQE_i D_{ic}$, and for $\overline{p} > 1$, the interaction terms $\sum_{p=2}^{\overline{p}} (\beta_p HQE_i^p D_{ic})$ are included in the second stage equation. Standard errors

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

are clustered at the cell level.[7]

I test for visual evidence of a kink in the outcomes in Figure 3. The outcomes are constructed by taking the residual from the linear reduced form equation and adding the global mean and the two highest quarter earnings terms. They are plotted as binned averages against highest quarter earnings, centered on the kink point. The grey lines show linear fits of the data on either side of the kink point. The plots show that each of the duration measures stays flat or slightly increases as a function of highest quarter earnings as claimants approach the kink point. Once benefits hit their maximum, the measures begin declining with past earnings, suggesting that duration is positively impacted by benefits.

Next I report formal regression estimates of the causal effect of benefits, using the bias-corrected estimator described in Calonico et al. (2014) in order to calculate optimal bandwidths. In the inset boxes on each plot in Figure 3, and in columns (1) and (2) of Table 2, I report the implied elasticities from linear ($\overline{p} = 1$) and quadratic ($\overline{p} = 2$) specifications. In Table 2, I also show estimates for the linear specification with double the optimal bandwidth (column 3) and $\overline{p} = 3$ (column 4). In Figure A.4, I probe the importance of bandwidth selection by plotting the 2SLS estimates for the linear and quadratic specifications for a variety of bandwidths. While the estimates are noisy below the optimal linear bandwidth, they are otherwise fairly stable around the reported point estimates.

The estimates suggest an elasticity between .2 and .9. This implies that for every 10 percent increase in weekly benefit levels, weeks claimed at audit increases by 2 to 9 percent. The estimates for the expected duration measure are smaller, ranging from .1 to .3. The small effect sizes for the imputed measure are mechanical in that my calculations will compress differences in duration at audit: claimants audited at one week are assigned 11 weeks of completed duration while claimants audited in their last week of UI are assigned that same number.

The elasticity of imputed duration is more relevant to policy (since, for example, total weeks claimed maps directly to UI expenditure) and provides a means of comparison with existing studies using similar methodologies and the actual full duration. My estimates are somewhat

[7] A more flexible estimation strategy would allow the coefficients σ_p, η_p, and δ_p to also vary by c as in Rose and Shem-Tov (2018), but this introduces noise in the estimation for smaller benefit cells.

smaller. Landais (2015) found elasticities between .2 and .7 using data from the early 1980s covering claimants in Idaho, Louisiana, Missouri, New Mexico, and Washington. Card et al. (2015), using more recent data from Missouri, found an elasticity between .35 and .9.

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. (2015), these tests cannot be performed on the full 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 dynamic selection. Restricting to this sample I plot a histogram of highest quarter earnings around the kink point in Figure A.5, which shows that the density evolves smoothly around the kink point at 0. Next, as in Card et al. (2015), I use all the pre-determined covariates in the sample to predict the three duration outcomes using ordinary least squares. I plot these against the centered highest quarter earnings measure in Figure A.6. The fitted values vary smoothly over the kink point, suggesting that there is no discontinuous change in the derivative of covariates. Formal regression estimates reported in the inset boxes using the same approach as in Table 2 find point estimates close to zero and that switch signs between linear and quadratic specifications.

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, an 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. Table 3 shows the corresponding point estimates.

The effects on contacts reject quite small responses. According to the linear estimate in column (1), an increase of 10 percent in the weekly benefit amount decreases contacts by no more than .02. Moving across specifications, the confidence intervals reject similarly small decreases (or increases).

As with the tests of the RKD assumptions above, these could be confounded with duration when estimated on the whole sample, where the probability of audit is mechanically related to duration. 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, the claimant's probability of having been selected is not recoverable. To address this concern, coefficient estimates from the same regressions for the four main search measures using only claimants in their first week are given in Table A.2, with the reduced form plots in Figure A.7. In each case, the search measures show no sign of a kink where benefit levels reach their maximum. The results for the reservation wage outcomes, for instance, are consistently small and flip between positive and negative.

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, but corroborate a precise zero effect on reservation wages found in Le Barbanchon et al. (2017). In the present context, however, the implied confidence intervals encompass potentially meaningful responses. For instance, the linear RKD for the reserve ratio cannot reject an elasticity as large as .08. 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 would imply a small elasticity that is not ruled out by my 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 4, 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 contain information about the claimant's eventual employment. Despite the correlation, however, a comparison of the r-squared in columns (4) and (5), 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 of benefits claimed. The results in Table 5 Panel A show regressions with the different search behaviors in the columns and, along with state-by-year fixed effects, three sets of regressors across rows: 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.[8]

The results in the top row 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: for every 10 weeks on UI, the reservation wage decreases 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 in 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 suggests that dynamic selection drives much of the differences in search behavior observed at different durations. It also contrasts with Skandalis and Marinescu (2019), who, using online search job application data, document a spike in applications close to benefit exhaustion.

The one result that remains fairly consistent across specifications 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.

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

4.3 Cyclicality

Finally, I study how the search measures vary with the local labor market conditions, proxied for by the state unemployment rate. Recent work studies the evolution of search intensity over the business cycle (Mukoyama et al., 2018; Faberman and Kudlyak, 2019). However, while the cyclicality of continuing and new hire wages have long been a focus in macroeconomics (Keynes, 1936; Bils, 1985; Gertler et al., 2016), less is known about reservation wages.

Table 5 Panel B shows how the different search measures vary with the state unemployment rate. Each point estimate reports an OLS estimate of β from the equation below:

$$y_{its} = \alpha + \beta u_{st} + \delta' \boldsymbol{X}_{its} + \epsilon_{its} \tag{4}$$

where y_{its} is the search measure for claimant i in year-by-month t and state s, u_{st} is the unemployment rate in percent in that month, and $\boldsymbol{X}_{its}$ denotes a matrix of controls. 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). The first row uses the full sample and includes a stringent set of controls in $\boldsymbol{X}_{its}$ including all available demographics and fixed effects for the month the claim began, state-by-industry-by-occupation, past earnings, and indicators for number of weeks claimed.[9] The next row includes only the claimants in their first week of benefits to gauge the robustness to length-biased sampling, retaining the same controls (except for number of weeks claimed).

The results show a strong relationship between the two reservation wage measures and the state unemployment rate. The point estimates in columns (1) and (2) for all claimants imply that the reservation ratio decreases by 0.6-0.7 percent for every one percent increase in the state unemployment rate, and increases slightly higher when restricting to claimants in their first week in the row below. This suggests that claimants in the sample update their job preferences in

[9] The full list is: ethnicity, sex, dependents, a quadratic in age, indicators for education and probable and definite recall, layoff reason, employer tax rate, an indicator for work search required, the log of base period earnings and the previous wage, and fixed effects for the month the claim began and state-by-industry-by-occupation.

response to local labor market conditions.

On the other hand, I find no relationship between job contacts and the unemployment rate. This contrasts with Mukoyama et al. (2018), who find a positive association between time spent searching for a job and the state unemployment rate, but the confidence intervals do not rule out the small effects found in their analyses.

4.4 Internal validation of search measures

Since contacts and job choice show no relationship with the factors studied to this point, I next present a brief validation exercise showing their relationship with the reservation wage. Table 6 shows regressions of contacts and the switching occupations indicator on the full set of controls from Table 5 and the reservation ratio. In both cases, a clear and significant relationship is recorded using either all claimants or the first week sample.

The relationship between contacts and the reservation wage is slight, suggesting that a ten percentage point increase in the reservation ratio predicts a decrease in contacts by 0.02 ($se =$ 0.001) in the full sample, but the direction is consistent with selectivity causing higher reservation wage claimants to apply to slightly fewer jobs. The job choice measure is also negatively related to the reservation wage; a 10 percentage point increase in the reservation ratio is associated with a decrease in the probability of switching jobs by 2 percent ($se = 0.0003$), a more considerable effect since only 20 percent of claimants report seeking to switch jobs. The latter finding is consistent with evidence that wage losses in recessions are concentrated among those who switch occupations (Huckfeldt et al., 2016).

Finally, I test how reported search behavior is affected by holidays, as in DellaVigna et al. (2020). If claimants always report the required number of contacts, the contacts measure should be unaffected when the audited week includes federal holidays such as Thanksgiving or Christmas Day, when employers may be harder to reach and claimants may be engaged in holiday activities. DellaVigna et al. (2020) find a 40 percent decrease in minutes spent searching on national holidays.[10] In the BAM context, this test is more indirect since the survey applies to a whole week.

[10] That is, a decrease of 30 minutes from an average of 70-80 minutes per day.

Around holidays, claimants may shift their search behaviors to other days of the week, muting any responses detectable at the week level.

I used data on federal holiday dates to create an indicator for whether the key week—i.e., the week being audited—includes a holiday and regressed contacts on this indicator along with a set of controls. Most importantly, the regression includes a fixed effect for year × month, which would remove any seasonality by comparing weeks containing holidays to weeks in the same month and year. The results suggest that claimants report 0.018 fewer contacts, or a 1 percent decrease in the number of contacts, for weeks that included a holiday. As a placebo check, the other search behaviors are included as well. In each case the point estimate is quite small and the confidence interval rules out any meaningful effects, as expected. This provides further evidence that the contacts measure tracks actual behavior.

5 Discussion

The preceding sections use large-scale data and a quasi-experimental design to show that unemployment durations increase in the monetary level of benefits, extending existing work from Landais (2015) and Card et al. (2015), which are based on more limited samples. The unique data allows me the probe the mechanism further by estimating the effects of benefits on search behaviors. I find that none of the search behaviors appear to respond to benefits.

One potential limitation of survey-based responses is that the answers to the job search questions may not accurately measure the actual job preferences and search activities of the claimants. I argue that several features make the BAM survey data uniquely good: First, auditors investigate whether the reported job contacts are legitimate, and claimants are subject to potential monetary penalties for inaccurate answers. Next, I provide evidence that the self-reported reservation wage is predictive of the reemployment wage found in administrative wage records. Finally, the data replicates existing evidence on the decline of the reservation wage over the unemployment spell (Krueger and Mueller, 2016).

Still, it is possible that important components of job search are left out of the survey. For instance, my data cannot show whether claimants time "storable offers" to start right when benefits lapse, which may explain the duration response to benefits (Van Ours and Vodopivec, 2008). And BAM data contains no information on time spent searching for jobs and uncovering good matches, which could be as important as the number of jobs applied to. Indeed, some studies find that when states experimentally vary the number of job contacts required, the main effect is to deter claimants through the non-monetary burden, with no detectable impacts on employment outcomes (Johnson and Klepinger, 1994).

This study builds on Le Barbanchon et al. (2017), who found a precise zero effect of potential benefit duration on reservation wages using administrative data from France, but extends the results to explicitly study search intensity. Search behaviors are clearly not irrelevant to UI responses given the well-documented spike in the exit hazard (Card et al., 2007), and the recent finding that this corresponds with a surge in job applications (Marinescu and Skandalis, 2019). More detailed data on job search activities in the US might further illuminate what causes claimants to stay unemployed longer when benefits are higher.

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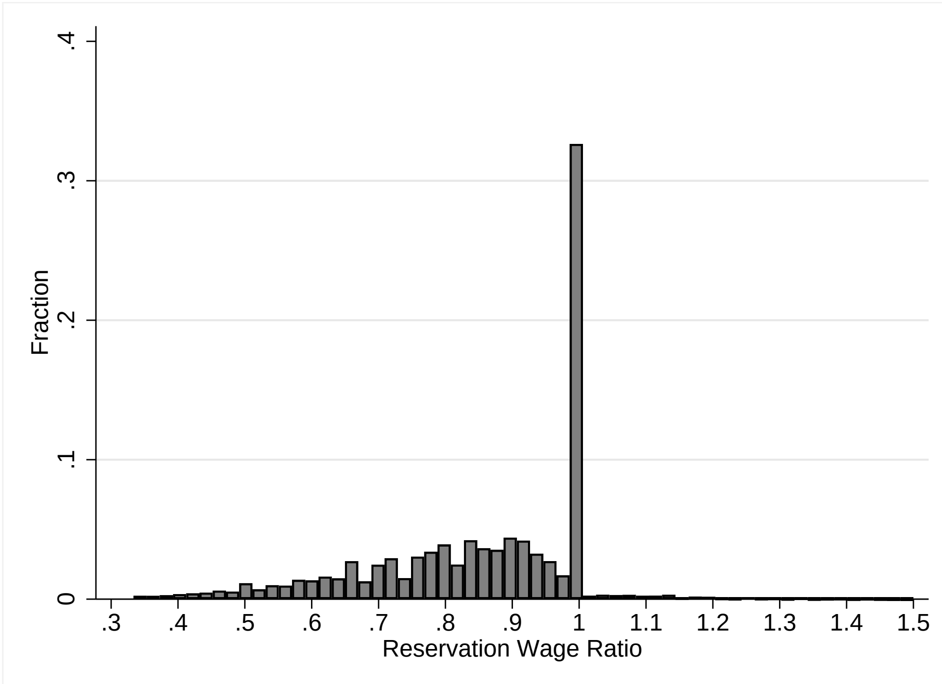
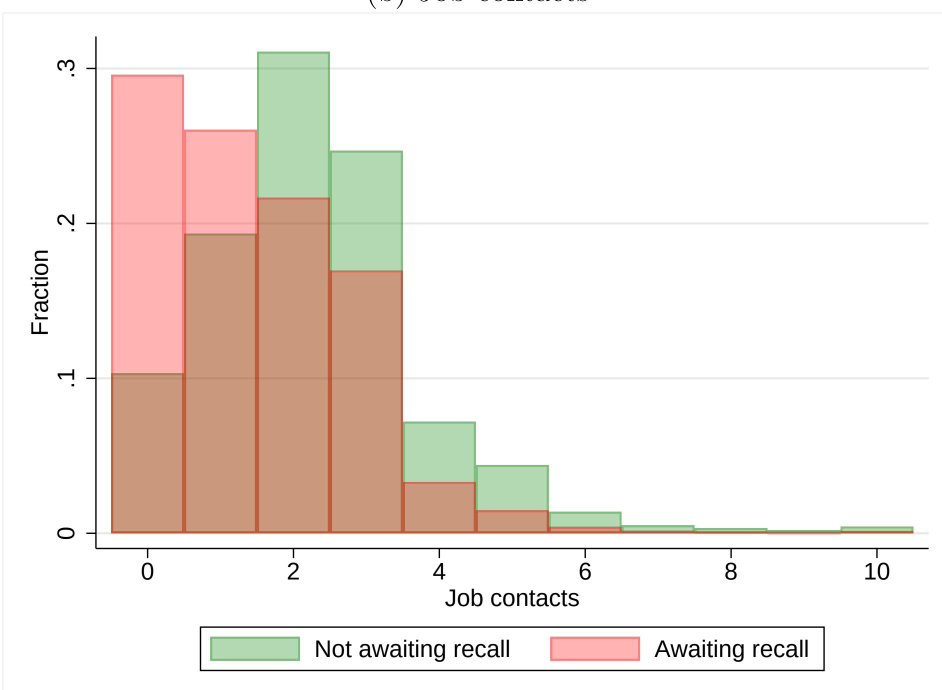
Figure 1: Distributions of two search measures

(a) Reservation ratio

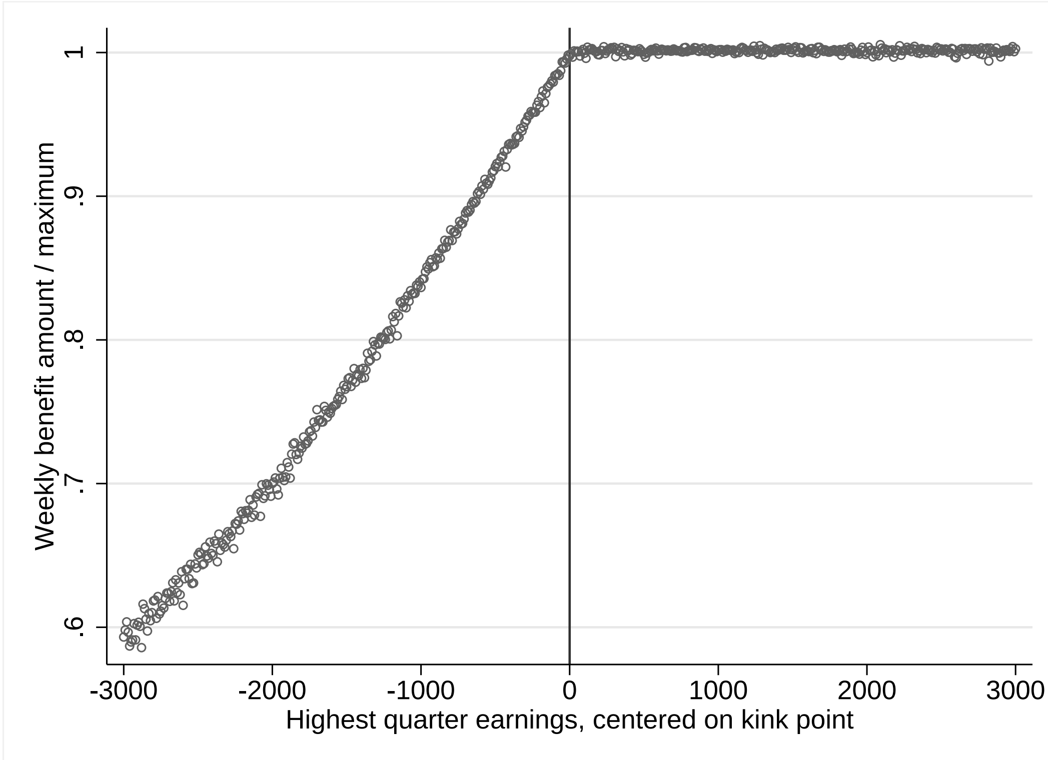


(b) Job contacts



Notes: (a) Histogram of the reservation ratio (reservation wage / previous wage) in the BAM audit sample for all claimants with non-missing values (N=860,216). For legibility, responses are capped at 1.5. (b) Histogram number of work contacts made last week for claimants in the BAM sample, for all claimants with non-missing values (N=612,422). Responses are capped at 10.

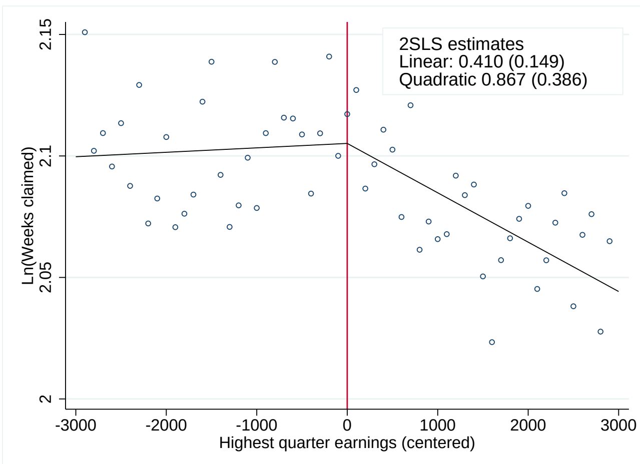
Figure 2: Regression kink design first stage



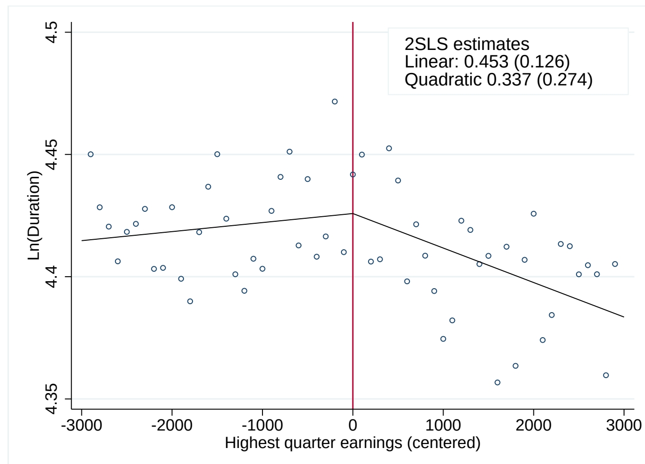
Notes: This plot shows the first stage relationship for all claimants in the RKD sample within $3,000 of their benefit schedule kink (N=132,337). The y-axis gives claimant weekly benefit payments divided by the maximum amount in their benefit schedule. The x-axis gives highest quarter earnings minus the point in their schedule where the benefit amount reaches its maximum. Each dot represents an average using $10 bins of highest quarter earnings.

Figure 3: Graphical evidence on the effect of UI benefits on duration

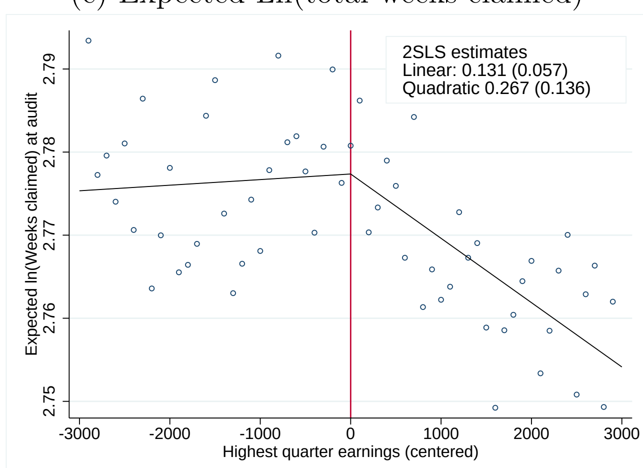
(a) Ln(weeks claimed at audit)



(b) Ln(unemployment duration at audit)

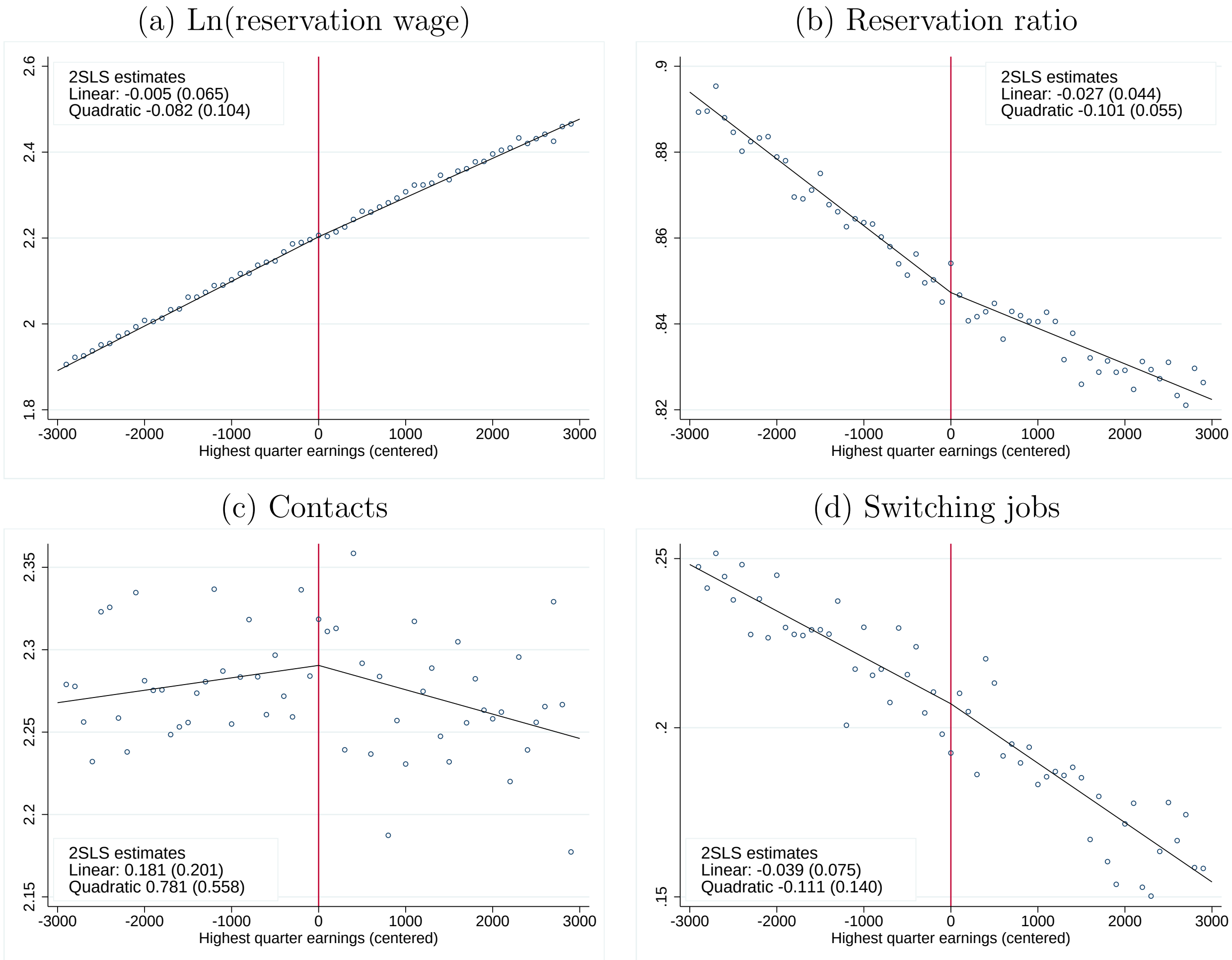


(c) Expected Ln(total weeks claimed)



Note: These show visual evidence of a kink for the three duration outcomes. The textbox in each plot reports the 2SLS results, interpretable as elasticities, from the regression specification outlined in Section 3.1 using optimal bandwidths reported in Table 2. In each case, the outcome on the y-axis is the from the reduced form equation plus the global mean, the main effect of highest quarter earnings, and the effect of highest quarter earnings interacted with an indicator for being above the kink. Each dot represents a binned average, with a bin size of $100.

Figure 4: Graphical evidence on the effect of UI benefits on search



Note: These show limited evidence of a kink for the four search outcomes. The textbox in each plot reports the 2SLS results from the regression specification outlined in Section 3.1 using optimal bandwidths reported in Table 3. In each case, the outcome on the y-axis is the residual from the reduced form equation plus the global mean, the main effect of highest quarter earnings, and the effect of highest quarter earnings interacted with an indicator for being above the kink. Each dot represents a binned average, with a bin size of $100.

Table 1: Descriptive statistics

Variable	(1) All	(2) First week	(3) Long term (>20 weeks)
Reserve ratio	0.860	0.875	0.841
	(0.175)	(0.168)	(0.184)
Switching occupation	0.201	0.175	0.221
	(0.401)	(0.380)	(0.415)
Job contacts	2.162	2.121	2.104
	(1.596)	(1.665)	(1.527)
Weeks claimed	10.818	1.233	23.266
	(7.349)	(0.448)	(2.586)
Weekly benefit amount	231.711	223.457	239.352
	(115.719)	(112.031)	(117.526)
Male	0.581	0.598	0.549
	(0.493)	(0.490)	(0.498)
Any dependents	0.083	0.083	0.096
	(0.276)	(0.276)	(0.294)
Black	0.158	0.142	0.165
	(0.365)	(0.349)	(0.371)
Age	40.759	39.905	41.809
	(12.539)	(12.350)	(12.720)
College	0.123	0.117	0.135
	(0.328)	(0.321)	(0.341)
Recall	0.252	0.347	0.164
	(0.434)	(0.476)	(0.370)
Definite recall date	0.166	0.255	0.097
	(0.372)	(0.436)	(0.296)
Layoff	0.697	0.710	0.657
	(0.459)	(0.454)	(0.475)
Quit	0.040	0.032	0.049
	(0.196)	(0.177)	(0.216)
Discharge	0.190	0.157	0.214
	(0.392)	(0.364)	(0.410)
Vocational certification	0.176	0.172	0.175
	(0.381)	(0.378)	(0.380)
Employer tax rate	3.538	3.591	3.389
	(2.722)	(2.740)	(2.628)
Numer of employers	1.659	1.659	1.595
	(1.001)	(1.006)	(0.954)
Observations	921,801	81,693	145,440

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)	0.410	0.867	0.240	0.714
	(0.149)	(0.386)	(0.0568)	(0.310)
N	51,207	72,677	100,965	152,855
Bandwidth	1057	1503	2115	3429
Ln(Duration)	0.453	0.337	0.175	0.659
	(0.126)	(0.274)	(0.0490)	(0.303)
N	55,865	88,804	109,577	153,855
Bandwidth	1154	1843	2308	3459
Expected Ln(weeks claimed)	0.131	0.267	0.0955	0.282
	(0.0568)	(0.136)	(0.0209)	(0.111)
N	51,607	73,837	101,730	156,130
Bandwidth	1065	1527	2131	3529
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

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)	-0.00486	-0.0819	0.0351	-0.0757
	(0.0648)	(0.104)	(0.0263)	(0.0914)
N	39,709	77,756	79,086	152,079
Bandwidth	868	1706	1736	3698
Reserve ratio	-0.0270	-0.101	-0.0372	-0.0553
	(0.0442)	(0.0552)	(0.0172)	(0.0451)
N	37,979	81,752	75,547	165,715
Bandwidth	835	1809	1670	4230
Switching occupation	-0.0389	-0.111	0.0322	0.0917
	(0.0752)	(0.140)	(0.0267)	(0.0932)
N	45,122	78,422	89,433	172,246
Bandwidth	968	1690	1935	4309
Contacts	0.181	0.781	0.105	1.035
	(0.201)	(0.558)	(0.0809)	(0.571)
N	44,869	59,563	85,210	104,159
Bandwidth	1359	1813	2717	3544
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

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

	(1)	(2)	(3)	(4)	(5)
Log Reservation Wage	0.961	0.598	0.545	0.542	
	(0.0503)	(0.103)	(0.103)	(0.109)	
Log Previous Wage		0.405	0.152	0.112	0.541
		(0.102)	(0.111)	(0.120)	(0.0830)
Expecting recall			0.202	0.203	0.225
			(0.0511)	(0.0525)	(0.0530)
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 Occupation	No	No	No	Yes	Yes
R-squared	0.269	0.278	0.318	0.373	0.360
N	1,650	1,650	1,650	1,650	1,650

Standard errors in parentheses

Notes: This table shows regression results from the matched sample of Missouri UI claimants appearing in the audit data with logged quarterly reemployment wage as the dependent variable. Month FEs are indicators for month of the first UI payment. Demographics include age, age squared, ethnicity, and education. Last occupation is an indicator for the 3-digit SOC code and industry is two-digit NAICS. Robust standard errors.

Table 5: Duration dependence and cyclicality of search behaviors

	(1) Reserve ratio	(2) Ln(Res wage)	(3) Contacts	(4) Switching occ
Panel A: Duration dependence				
	No controls			
Weeks claimed	-0.0169	-0.0177	0.0244	0.0218
	(0.000264)	(0.000657)	(0.00240)	(0.000595)
R-squared	0.071	0.358	0.341	0.070
N	861,020	872,266	612,422	867,925
	Full controls			
Weeks claimed	-0.0141	-0.0207	-0.0139	0.0134
	(0.000254)	(0.000350)	(0.00246)	(0.000620)
R-squared	0.228	0.850	0.404	0.119
N	786,934	791,747	547,832	784,537
	Restricted sample with multiple audits			
Weeks claimed	-0.00785	-0.0101	-0.0229	0.00311
	(0.00341)	(0.00412)	(0.0410)	(0.00947)
R-squared	0.902	0.985	0.836	0.834
N	7,930	8,037	6,124	7,969
Panel B: Cyclicality				
	All claimants			
State UE rate	-0.005	-0.007	0.011	-0.001
	(0.001)	(0.002)	(0.020)	(0.003)
R-squared	0.364	0.874	0.448	0.282
N	625,852	630,121	414,797	624,498
	Just first week			
State UE rate	-0.007	-0.010	0.017	-0.002
	(0.001)	(0.002)	(0.030)	(0.003)
R-squared	0.439	0.899	0.591	0.407
N	35,289	35,531	18,925	34,936

Notes: **Panel A:** Coefficients are scaled to be interpretable as the change in the column variable for every 10 weeks claimed. The first two rows use state-by-month fixed effects and robust standard errors. The multiple audits row regresses the column variable on weeks claimed and claimant fixed effects, with standard errors clustered at the claimant level. **Panel B:** Standard errors clustered at the state level. The "All claimants" row uses the full controls set from Panel A along with dummies for weeks claimed at audit. The "Just first week" sample uses only claimants in their first week.

Table 6: Search behaviors and the reservation ratio

	Contacts		Switch Occs	
	All claimants	Just first week	All claimants	Just first week
Reserve ratio	-0.019	-0.034	-0.021	-0.016
	(0.001)	(0.006)	(0.000)	(0.001)
R-squared	0.409	0.598	0.126	0.352
N	532,548	44,461	768,733	68,479

Notes: This table shows regressions of contacts and an indicator for job switch against the reserve ratio and the full set of controls from Table 5 along with state-by-month fixed effects. Coefficients are scaled to represent the change in the search variable in response to a 10 percentage point increase in the reservation wage. Robust standard errors in parentheses.

Table 7: Search behaviors and holidays

	(1)	(2)	(3)	(4)
	Contacts	Ln(res wage)	Reserve ratio	Switch occs
Holiday	-0.018***	0.001	0.001	-0.001
	(0.005)	(0.001)	(0.000)	(0.001)
Controls	X	X	X	X
State FEs	X	X	X	X
Month x year FEs	X	X	X	X
R-squared	0.220	0.826	0.164	0.074
N	573,100	819,379	814,052	811,139

Standard errors in parentheses

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

Notes: This table shows the coefficient on Holiday, an indicator for whether the key week included a federal holiday, in regressions with each of the four search outcomes as the dependent variables and as controls all controls from Table 5 with the addition of indicators for the number of weeks claimed and a control for the unemployment rate. Robust standard errors in parentheses.

A Online Appendix (not for publication)

Figure A.1: Histogram of percent spell completed at audit, Matched Missouri sample

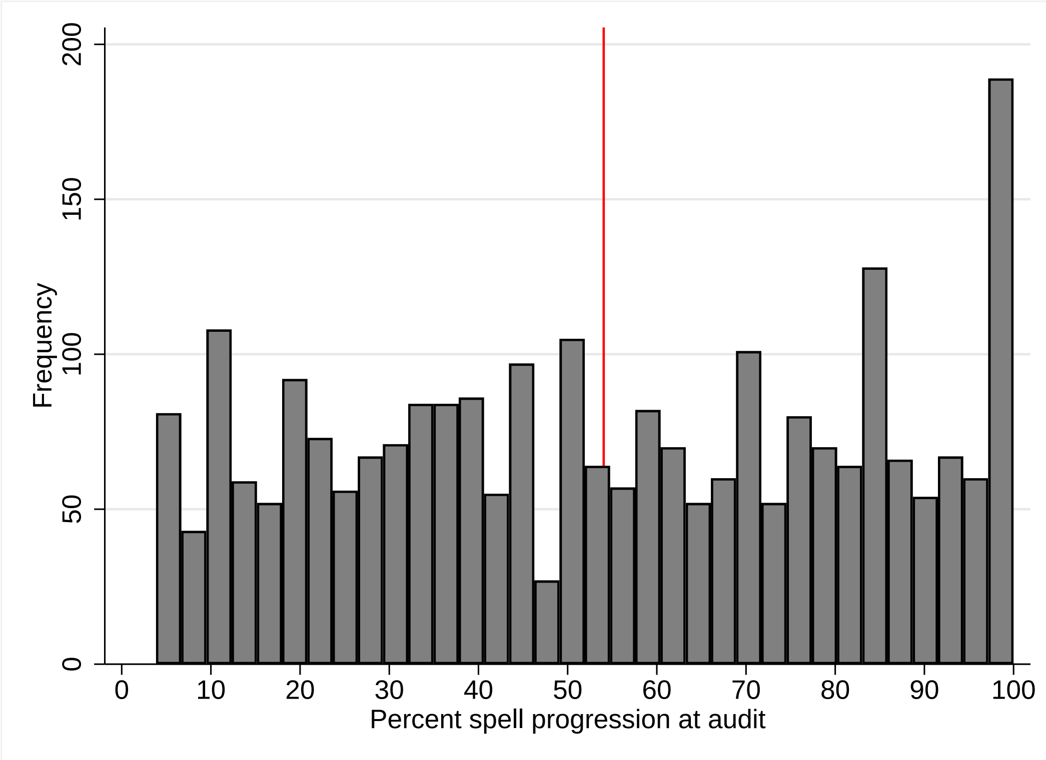
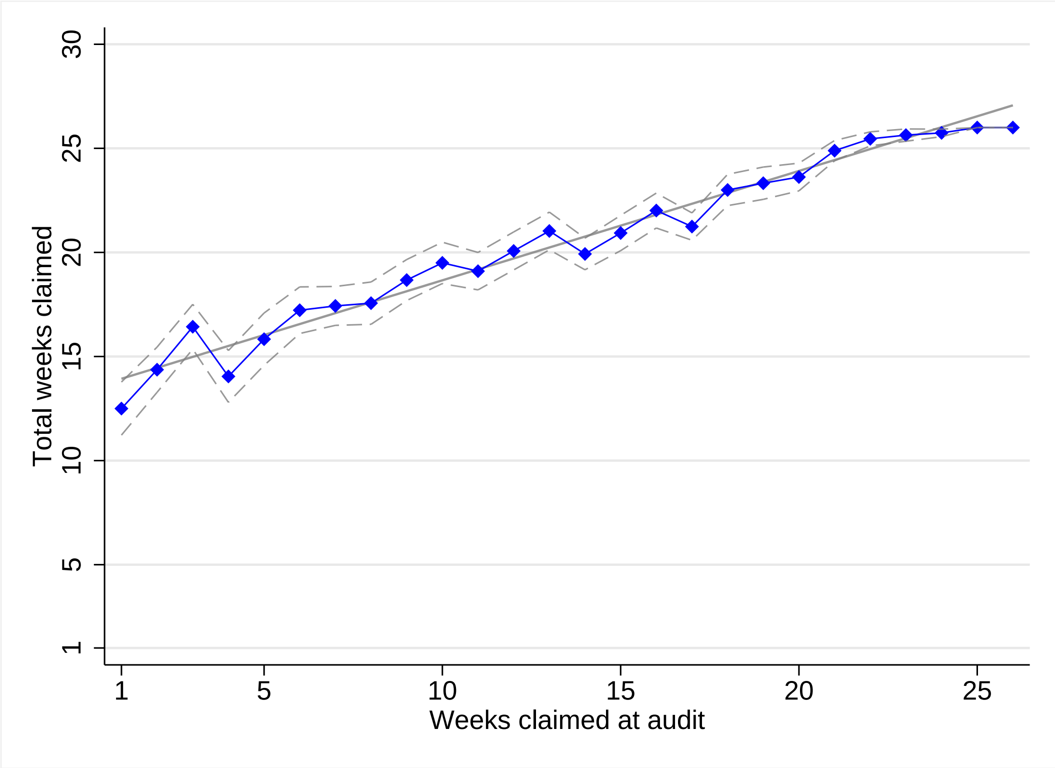
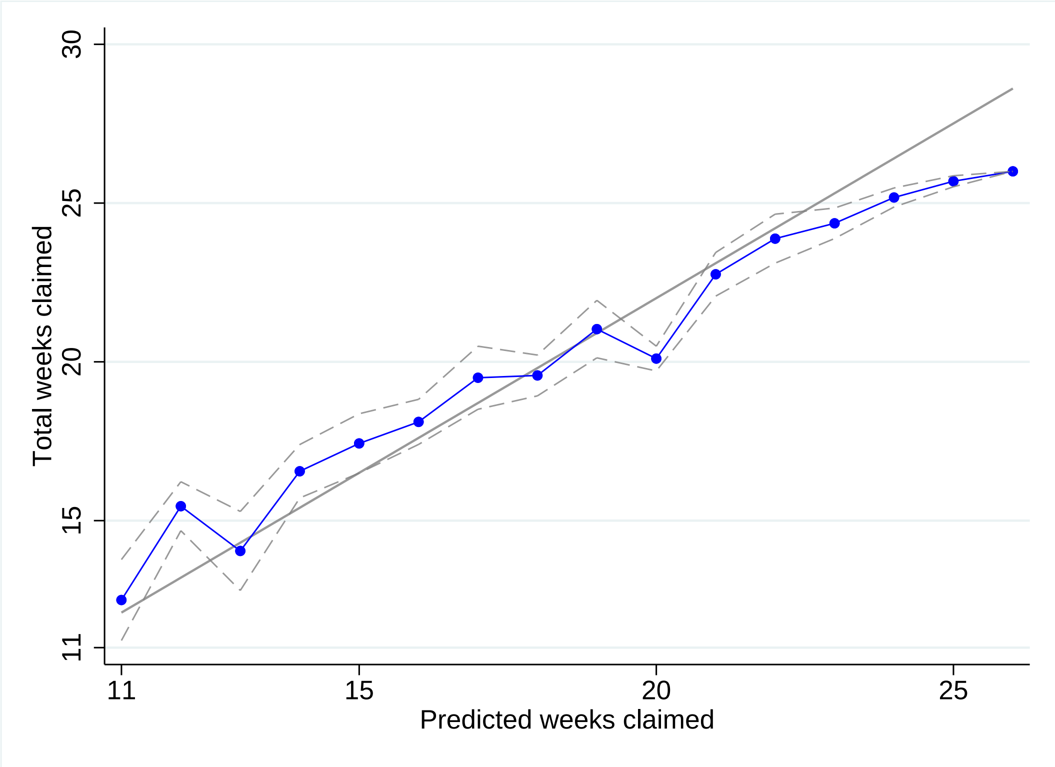


Figure A.2: Calibration using Missouri sample

(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 grey lines show linear fits.

Figure A.3: State sample sizes in RKD

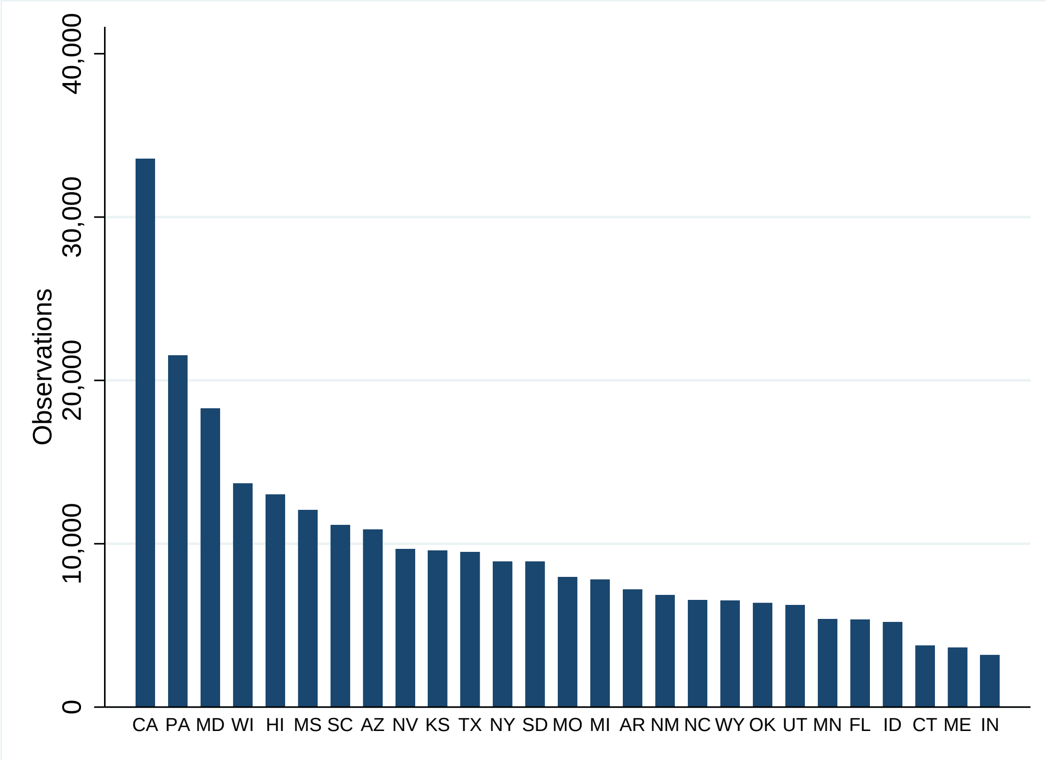
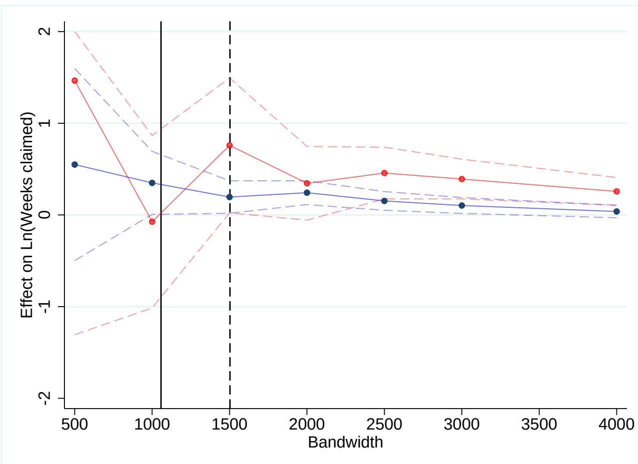
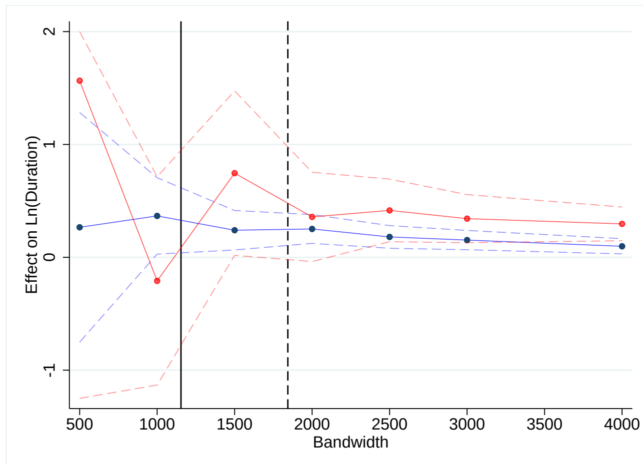


Figure A.4: RKD estimates varying bandwidth

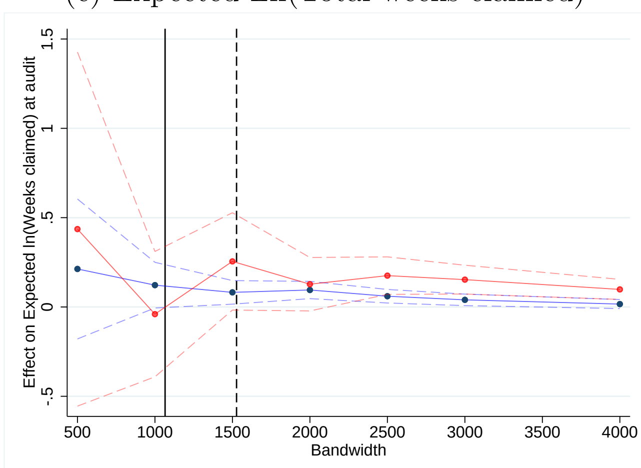
(a) Ln(Weeks claimed at audit)



(b) Ln(unemployment duration at audit)

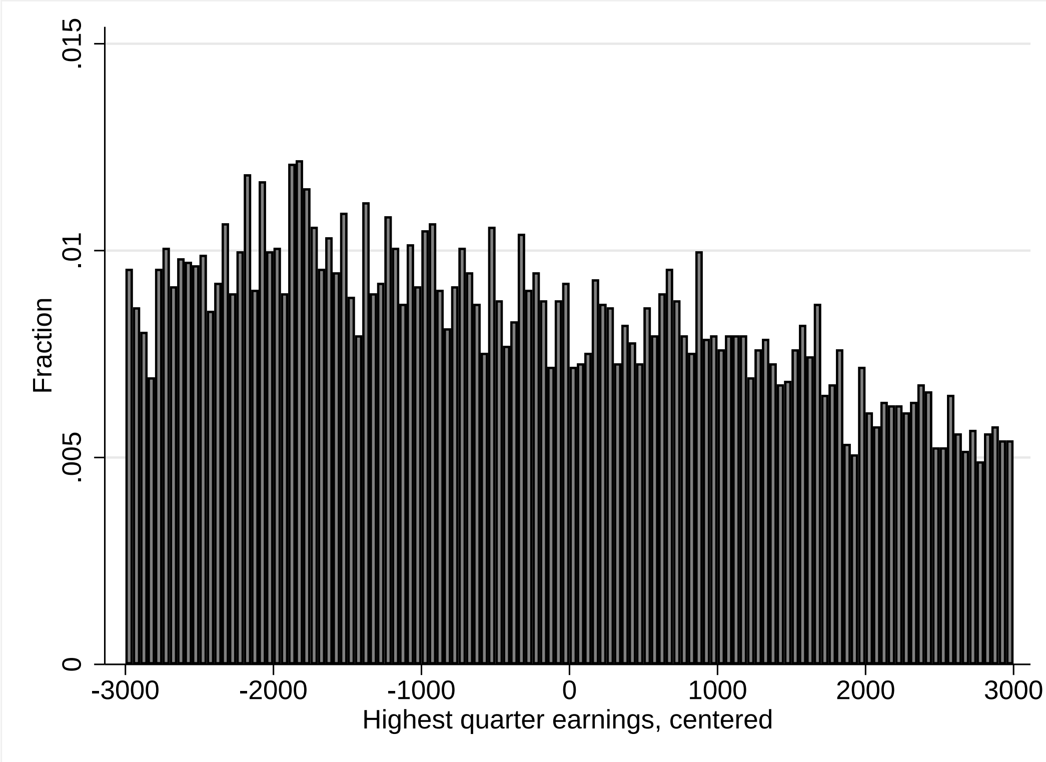


(c) Expected Ln(Total weeks claimed)



Note: The blue series gives point estimates and 95% confidence intervals for the linear RKD specification, with the solid verticle line indicating the optimal Calonico et al. (2014) bandwidth. The orange series and dotted vertical line show the analogous quantities for the quadratic specification.

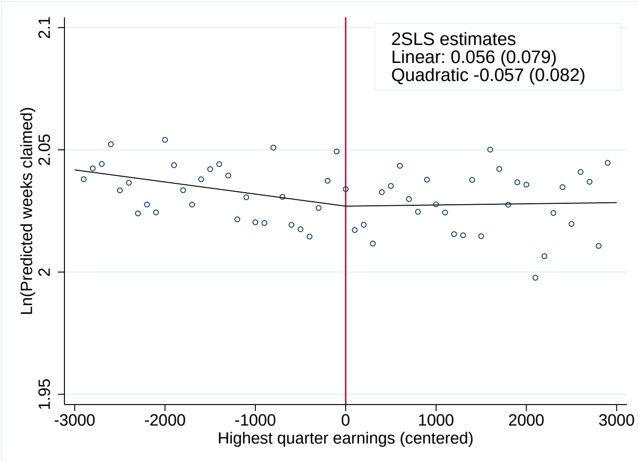
Figure A.5: Density check around kink point



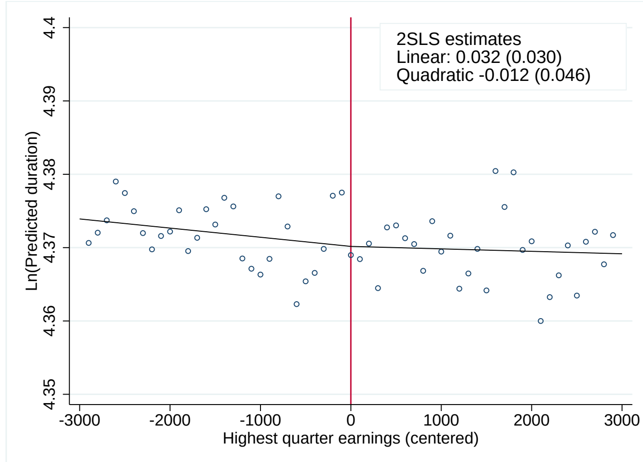
Note: Histogram of all claimants in the regression kink sample

Figure A.6: RKDs showing covariate smoothness

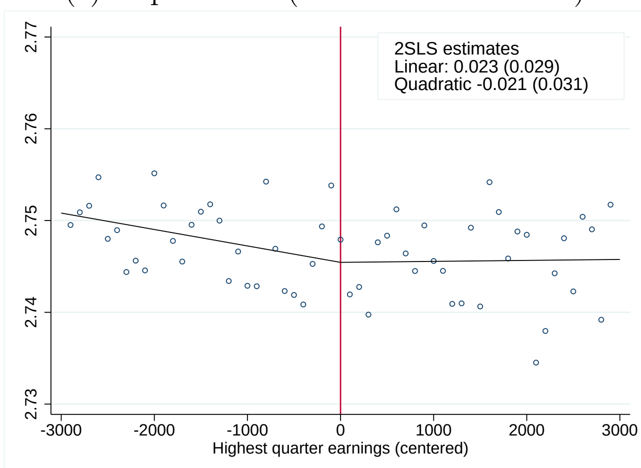
(a) Ln(weeks claimed at audit)



(b) Ln(unemployment duration at audit)

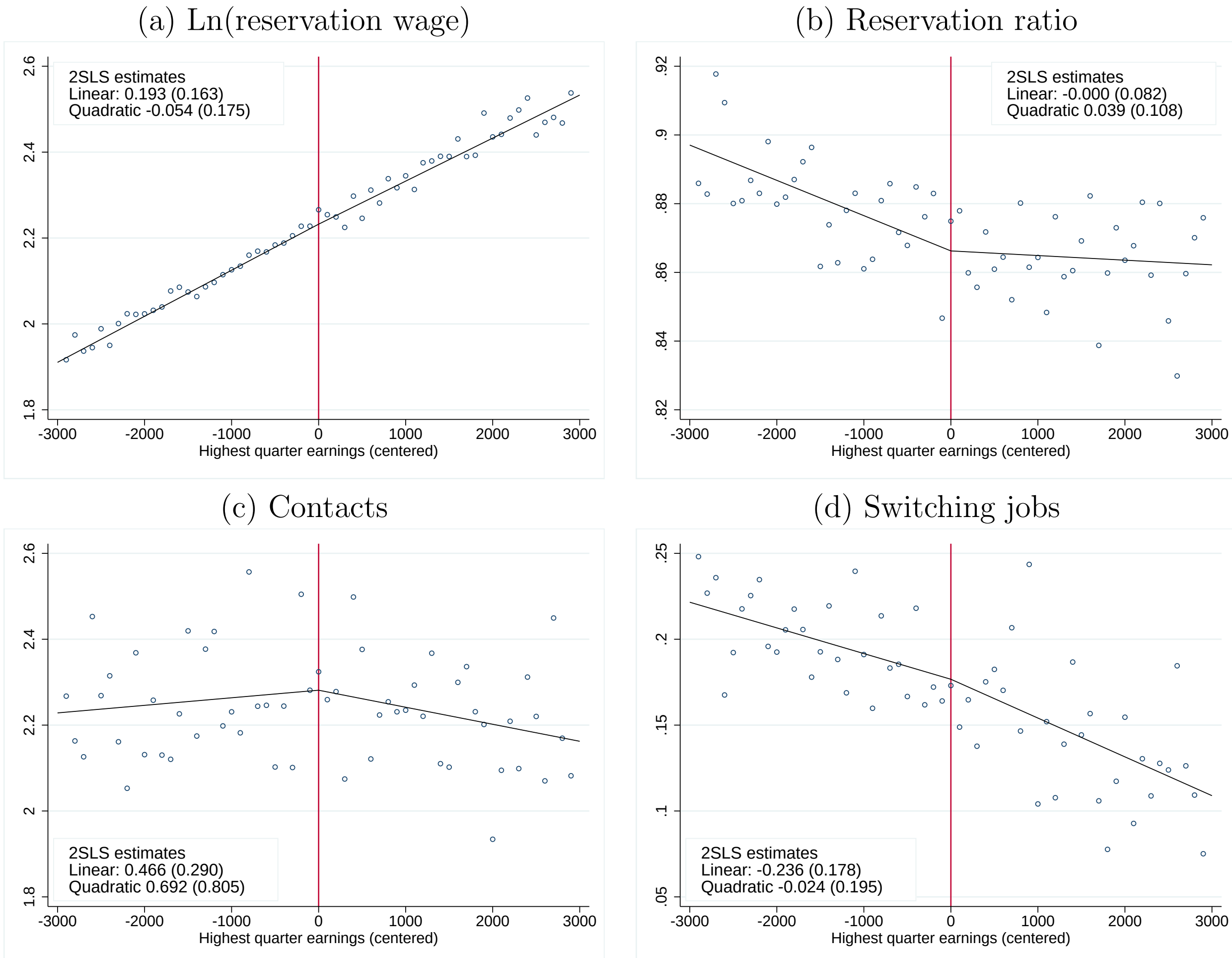


(c) Expected Ln(Total weeks claimed)



Notes: Each plot shows the same RKD figures as Figure 3, except using fitted values from a regression specification that uses covariates to predict the outcomes. The full list of covariates is ethnicity, sex, age, education, reason for being laid off, vocational certification, and fixed effects for occupation-by-industry, week of audit, and benefit schedule cell.

Figure A.7: Graphical evidence on the effect of UI benefits on search, First week claimants



Note: These show limited evidence of a kink for the four search outcomes, restricting to claimants in their first week of UI. The textbox in each plot reports the 2SLS results from the regression specification outlined in Section 3.1 using optimal bandwidths reported in Table A.2. In each case, the outcome on the y-axis is the residual from the reduced form equation plus the global mean, the main effect of highest quarter earnings, and the effect of highest quarter earnings interacted with an indicator for being above the kink. Each dot represents a binned average, with a bin size of $100.

Table A.1: Weeks claimed vs. recall status

	(1)	(2)	(3)
Expecting recall	-1.942	-1.991	-1.719
	(0.313)	(0.310)	(0.327)
Definite recall date	-5.269	-5.333	-5.321
	(0.422)	(0.428)	(0.462)
Month FEs	Yes	Yes	Yes
Demographics	No	Yes	Yes
Industry	No	No	Yes
Last Occupation	No	No	Yes
R-squared	0.266	0.285	0.324
N	2,453	2,453	2,453

Standard errors in parentheses

Note: This table provides validation for the match between BAM and the Missouri claims data. The dependent variable is total weeks of UI received as recorded in the claims data. The strong negative coefficients on indicators for the BAM-derived recall variables suggests that the matching technique is connecting the correct entries.

Table A.2: RKD results for search outcomes, only first week claimants

	(1)	(2)	(3)	(4)
Ln(reservation wage)	0.193	-0.0544	-0.0138	0.0364
	(0.163)	(0.175)	(0.0418)	(0.304)
N	4,594	10,104	9,122	14,150
Bandwidth	1195	2679	2390	4090
Reserve ratio	-0.000117	0.0388	-0.0375	0.0960
	(0.0822)	(0.108)	(0.0357)	(0.195)
N	4,297	10,122	8,564	13,042
Bandwidth	1123	2703	2246	3694
Switching occupation	-0.236	-0.0236	0.0717	-0.203
	(0.178)	(0.195)	(0.0602)	(0.341)
N	4,435	10,375	8,856	13,997
Bandwidth	1127	2684	2253	3904
Contacts	0.466	0.692	0.263	1.574
	(0.290)	(0.805)	(0.148)	(2.127)
N	5,445	8,139	9,373	8,358
Bandwidth	2130	3432	4260	3552
Bandwidth times optimal	1	1	2	1
Polynomial order	1	2	1	3
Kernel	Uniform	Uniform	Uniform	Uniform

Standard errors in parentheses

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).