Job Search and Unemployment Insurance

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Abstract

In models of job search, unemployment benefits lengthen unemployment duration by decreasing search effort and increasing job selectivity or the reservation wage, but few studies can shed light on these mechanisms. I provide new evidence on job search behaviors using audits of unemployment insurance claimants surveyed as part of the US Department of Labor's Benefit Accuracy Management program. When state unemployment is high, claimants have lower reservation wages and search for lower-paying occupations. Reservation wages are strongly predictive of reemployment earnings and, in a regression kink design, I find that they positively respond to unemployment benefits. Search effort (measured by the number of weekly work contacts) and occupational choice show no response to benefits.

1 Introduction

Unemployment insurance (UI) tends to increase unemployment duration. Understanding the mechanisms underlying this is pivotal to inferring optimal benefit levels. If UI increases duration primarily through a loosening of liquidity constraints, then it may improve match quality and efficiency. If, on the other hand, the duration response stems from a decrease in search behavior through a substitution effect, efficiency losses loom larger (Chetty, 2008; Baily, 1978).

Despite the centrality of reservation wages and search behavior to many analyses of unemployment, most existing evidence on these mechanisms is limited or indirect. Further, little is known about the determinants of search behavior and the reservation wage more broadly.

This paper attempts to fill that gap using information from audits performed by the Department of Labor as part of its Benefit Accuracy Management (BAM) program. The audits, which span 1987 to present day, are meant to determine whether randomly sampled benefit payments were valid according to state eligibility criteria. Importantly, claimants are questioned on their work contacts and reservation wage. In both cases, claimants face potential monetary penalties if their answers are found by examiners to be inaccurate.

I first use this data to ask some basic questions about reservation wages and the job search measures. As a validation exercise, I show that reservation wages are highly predictive of

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reemployment wages in a sample of Missouri claimants matched to the audit data. In this sample, reservation wages negatively predict duration, suggesting that cross-sectional variation in the reservation wage largely reflects positive selection (Le Barbanchon et al., 2017).

Returning to the full BAM data, I find that reservation wages are negatively correlated with the state unemployment rate, and lower when a claimant is seeking to switch occupations. Echoing Krueger and Mueller (2010), job search activity, as measured by job contacts, is lower for claimants expecting recall. However, the number of contacts is not related to state unemployment rates.

Claimants appear more likely to switch occupations during downturns. Further, they report searching for lower paying occupations when state unemployment is higher, even conditional on their reservation wage. This raises the possibility that jobseekers respond to labor market conditions through two distinct channels—the reservation wage and a broader selectivity around occupations. (If reservation wages are measured noisily, however, the observed effects on occupation choice might still operate through a latent reservation wage.)

Next, I use a regression kink design (RKD) across multiple states to identify the causal effect of benefits on reservation wage, number of work contacts, and stated desire to switch occupations. I find effects indistinguishable from zero for contacts and occupational choice. However, reservation wages do appear to respond to weekly benefit amounts, suggesting an elasticity of around .2. The results contrast with Le Barbanchon et al. (2017), who, using quasi-experimental variation and administrative data from France, estimate a precise zero effect of potential benefit duration on reservation wages.

This paper is complementary to several studies on the effects of unemployment benefits on search. Notably, Shimer and Werning (2007) argue that the reservation wage response to UI is key to determining the optimal level of benefits, and point out the limitations of existing evidence on that question. 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. 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. Finally, Marinescu (2017), using data from CareerBuilder.com, finds that a one percent increase in potential benefit duration decreases job applications by 0.4 percent.

More broadly, several papers have studied the effect of UI benefit generosity on reemployment wages. Depending on how closely reported reservation wages map to accepted offers, these studies carry implications for what kind of results to expect in the present context. Using quasi-experimental designs in large administrative data, previous papers find effects indistingushable from zero (Card et al., 2007; Lalive, 2007; Van Ours and Vodopivec, 2008; Johnston and Mas, 2018) and even small negative effects (Schmieder et al., 2016). An important exception is Nekoei and Weber (2017), which finds an increase in reemployment wages of 0.5 percent in response to a 30 percent increase in potential benefit duration (from 30 to 39 weeks).

The audit data provides the largest sample of reservation wages in the US to my knowledge, and this study uses quasi-experimental evidence brought about by the UI schedule in most states. Also, in addition to the reservation wage, it adds two explicit measures of search: the number of job contacts and the targeted occupation.

2 Data

The data covers 861,494 job search audits from 1987 to 2017 from the Department of Labor's Benefit Accuracy Management (BAM) program. The BAM program seeks to measure the share of unemployment benefit payments that are made in error so that states can take corrective actions.[1] Top reasons for overpayment concern work search, benefit calculations, separation issues, and availability for work. The interviews are 55 percent in-person; 23 percent by telephone; and 22 percent by e-mail, mail, or fax.

Audits are based on random samples of benefit payments: Each week, auditees are randomly chosen from a list a claimants with positive benefit amounts in that state. The target number of auditees scales loosely with state population, ranging from 6 per week in Delaware to 15 per week in California.[2] They are not followed after the audit, so it's not possible to know the length of their ultimate duration.

Because of this sampling procedure, any unadjusted estimates based on the audit data will be representative of a sample with more UI usage than the average claimant.[3] This is as the DOL intended, but results for typical claimants may be more relevant for other questions around UI policy. For the analyses below, I confirm that the same results hold using only claimants audited in their first week of unemployment.

Importantly, auditees are asked about aspects of their 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 2, 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.[4]

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.

3 Empirics of reservation wages and search

3.1 Matched validation sample

In order to partially validate the reservation wage measure, I match 2,475 BAM auditees to UI claims data and quarterly wages from Missouri using the claimants in that state with

[1] Here is a link to the most recent BAM annual report.

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

[3] See Krueger and Mueller (2008) for a discussion of length-biased sampling.

[4] 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.

unique combinations of highest quarter earnings, base period earnings, and week of claim.[5] This matched sample is useful because it allows me to observe the total number of weeks claimed and eventual reemployment wages. As evidence that the match worked, 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)

I use this sample to study whether reservation wages are predictive of reemployment wages.[6] I regress log reemployment earnings—that is, the log of the claimant's first positive quarterly earnings following the claim—on the log reservation wage, with fixed effects for the month the claim was started. The results are in Table 1. Across the columns, I incrementally include person-level covariates.

The reservation wage is highly predictive of the claimant's ultimate quarterly reemployment wages. The coefficient drops by roughly half when I control for the log of their 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), where reservation wages are predictive of accepted job offers. It also provides evidence that answers to the reservation wage question in this context are not meaningless, containing information about the claimant's eventual employment.

Holding all else constant, a claimant with a higher reservation wage should have a longer unemployment spell. However, even conditional on the strictest set of controls, cross-sectional variation in the reservation wage seems to reflect positive selection as it is consistently associated with lower durations—claimants with higher reservation wages may be more likely to have incoming offers or better job prospects. Table A.2 shows results from the same specifications as in Table 1 with the total weeks of UI benefits as the outcome. This is notably different from Le Barbanchon et al. (2017), who find a positive association between reservation wages and duration, possibly because their data allows for individual fixed effects.

3.2 Search behavior

In Tables 2-4 I study the inter-relatedness and cyclicality of my three main outcome measures: reservation wage, work contacts, and occupation switching. In all cases, I estimate a linear regression with fixed effects for state and month of claim initiation. Importantly, 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 this, but of course unobserved heterogeneity across cycles could still drive some of the measured effects. I include only auditees in the first week of their claim.

Table 2 shows that the reservation wage decreases with the state unemployment rate, by about 1 percent for every 1 percentage point increase in unemployment.[7] The coefficient decreases by half with the controls for demographics, base period earnings, and time since layoff, and remains stable when I add further controls for 4-digit industry and 3-digit O*NET

[5] This analysis was facilitated by Andrew Johnston.

[6] I drop 418 observations in the matched sample that do not appear again in the quarterly wage records.

[7] State unemployment it used to better capture local labor market conditions. For recent papers studying cyclicality using state-level variation, see Mukoyama et al. (2018) and Aguiar et al. (2013). Extant studies of the cyclicality of reservation wages have somewhat conflicting results. For a semi-recent review, see Haurin and Sridhar (2003)

occupation.

Next I study work contacts in a similar regression framework reported in Table 3. I restrict the sample to claimants who are required to search for a job based on an indicator in the data. Even with this adjustment, claimants who are expecting to be recalled report significantly fewer work contacts. This is in line with findings from Krueger and Mueller (2010), where unemployed people expecting recall were found to spend significantly less time searching for a job.

These results show no evidence of cyclicality in search activity as measured by work contacts. 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.

Claimants are asked both their previous job and the job they are seeking. Around 20 percent of the sample is reportedly switching occupations. In Table 4, I examine occupation switching, adding the reservation wage to the specifications in columns (2) and (3). Older claimants and those expecting recall are less likely to be willing to switch occupations. And the negative coefficient on log reservation wage suggests that, in general, claimants willing to switch occupations are also willing to accept significantly lower wages.

What kind of occupations do people look for when state unemployment is higher? In Table 5, I how study log earnings at the "seeking occupation"—the job that the claimant reports looking for—vary with the unemployment rate. State-by-occupation level earnings for each O*NET occupation are calculated using the American Community Survey (Ruggles et al., 2018). I find that otherwise similar claimants facing unemployment rates 1 percentage point higher report looking for occupations that pay 1-3 percent less, even conditional on their reservation wage. The results are robust to stringent fixed effects for the claimant's original state-industry-occupation (column (3)), and identical when I use the state-year income percentile of each occupation as the outcome (not shown).

4 Effect of UI benefits on search

I next turn to assessing the impact of unemployment insurance benefits on search behavior. In most states, the benefit amount is a fixed fraction τ of highest-quarter wages (hqw), up to a maximum weekly amount. Thus a natural way to investigate the causal effects of benefits is through a regression kink design (RKD), which tests for a significant change in the slope of an outcome variable at the point in highest quarter earnings where weekly benefits max out (Card et al., 2015; Landais, 2015).[8] (Other states base benefits on multiple quarters in the base period or average weekly wage; unfortunately BAM does not collect these measures so those states are omitted from this analysis.)

While the data is large, it's not as well-suited to an RKD as administrative data on a large state, since the claimants in my sample face almost 500 different benefit schedules (i.e., unique combinations of τ and b_{max}).

The largest formula cell in my sample California from 1992 to 2000, during which period its benefit formula was fixed at $\tau = 1/33$ of hqw with a maximum benefit amount of \$450.[9] I illustrate my approach using these claimants in Figure 3.

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

[9] People with lower hqw actually faced τ's of up to 1/23, but they are not affected by the kink given how eligibility for the higher τ was determined.

The top left plot in Figure 3 shows the weekly benefit amount against the highest quarter earnings (centered to equal one where benefits max out) demonstrating strong correspondence to the UI benefit schedule. The top right quadrant suggests that the density of the highest quarter earnings variable is smooth around the kink point, a key assumption of the RKD. Next, the reduced form picture in the bottom right suggests that there is little or no kink in the log reservation wage, which implies that the reservation wage is not strongly affected by weekly benefits. In the Appendix, I show these same four pictures for the four other biggest formula cells in the audit data.

In practice, it would require improbable foresight to manipulate hqw, which is based on administrative wage records (Landais, 2015). However, UI claimants are a nonrandom sample of the unemployed; it's plausible that people select into claiming based on the generosity of benefits, which could bias the estimates (in fact, this is found in (Anderson and Meyer, 1997)). In order to test for this, I estimate the same RKD specifications with a prediction of the outcome using all available predetermined covariates (Card et al., 2016). A visual test of this relationship is shown in the bottom right plot of Figure 3, suggesting no sorting based on covariates.

In Table 6, I give regression estimates of the causal effect of an additional benefits on three search outcomes in the five samples. In all cases, I use a quadratic polynomial and MSE-optimal bandwidths calculated following Calonico et al. (2014). For ease of exposition, the coefficients are scaled to represent the effect of a \$10 increase in the weekly benefit amount (5% of average weekly benefit amount in these samples and close to the average allowance for an additional dependent). These analyses are suggestive of a positive effect on reservation wages and no impact on contacts or occupation switching, but in these individual samples this null finding cannot reject fairly large responses.

In order to gain precision, I perform an RKD that aggregates the cells in this sample. First, I calculate the optimal bandwidth under a uniform kernel for each of the 5 cells using the Calonico et al. (2014) procedure. Next, using 2SLS, I estimate an interacted fuzzy RKD specification that allows each parameter to vary by cell except for the treatment effect β. Let ω_c be a dummy indicating that claimant i is in cell c, and k_c the kink point of cell c. The first stage and second stage equations are as follows:

$$\begin{aligned} b_{ic} &= \delta_c + \sum_{p=1}^{\overline{p}} \sum_{c} \omega_c (\gamma^1_{c,p}(hqe_i - k_c)^p + \gamma^2_{c,p}(hqe - k_c)^p \cdot D_i) + e_{ic} \\ y_{ic} &= \alpha_c + \sum_{p=1}^{\overline{p}} \sum_{c} \beta_{c,p}(hqe_i - k_c) \cdot \omega_c + \beta \cdot b_i + \epsilon_{ic} \end{aligned} \tag{1}$$

where δ_c and α_c are cell fixed effects.

The results of this exercise are in Table 7. In order to probe the robustness of the results, across columns I vary the bandwidth (as a multiple of each cell's optimal bandwidth), polynomial order $\overline{p}$, and sample used (in order to address the length-sampling bias in the audit selection procedure). The estimates do not differ substantially across specifications. They suggest a positive effect on the reservation wage corresponding to an elasticity of around .2 (using the estimates for log reservation wage in the first column). The estimates for contacts and occupation switching are inconsistent with large effects, but still cannot rule out reasonable responses. Most of the contacts specifications cannot rule out effects of -.05, or 2 percent of the average, which would represent an elasticity of around -.4.

In Table 8, I show checks for covariate smoothness, discussed above, in which I use all available predetermined covariates in the data to predict the outcomes and check for kinks in those fitted values using the same specifications from the previous table. These results are usually much smaller than the estimates in Table 7 but are more suggestive of selection around the kink compared to past studies. This could be due to the richer set of covariates available in the audit data, or the process through which audited claimants are selected (around 8 percent of selected claimants do not respond).

5 Discussion

The preceding analysis finds that reported reservation wages are highly predictive of reemployment wages and negatively correlated with local unemployment. Further, people target lower-paying jobs in depressed labor markets even conditional on their reservation wage. Reservation wages do appear to respond to benefits, contrary to the results in Le Barbanchon et al. (2017) and with an elasticity similar in magnitude of Feldstein and Poterba (1984). Finally, search intensity and selectivity, measured by work contacts and occupational preferences, do not seem to depend on benefits.

Caveats are in order. While the aggregated RKD effects on the reservation wage are fairly consistent, the data shows some potentially worrisome sorting around the kink, which would challenge the validity of the design. Further analysis could address potential confounds and subject the design to more scrutiny using multi-sample analogs of the conventional procedures.

Additionally, it is possible that the survey measures are quite different from the underlying concepts. For instance, while reported contacts exhibited reasonable correlations in Section 3.2, this measure might only weakly correspond to actual job search effort. While increased mandatory contacts tends to decrease time spent on UI, a prominent study finds this acts through an increased the cost of remaining on UI as opposed to enhanced job-finding prospects (Klepinger et al., 1997). However, more recent work finds that increased stringency in the work test does affect long term economic outcomes for the permanently separated (Lachowska et al., 2016).

The evidence that UI benefits increase the reservation wage while leaving work contacts unchanged is interesting because, in most models, these two components of search are not separated. If the reservation wage and contacts measures can be taken at face value, an important next step is to model the inter-relatedness of these factors and study the efficiency implications of these results. Finally, while occupational choice was not found to depend on benefits, it at least responds to local labor market conditions. The occupational choice in the BAM data can be used to study how unemployed people choose occupations, and whether certain states are better at directing their claimants toward productive employment.

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6 Figures

Figure 1: Reservation wage ratio distribution

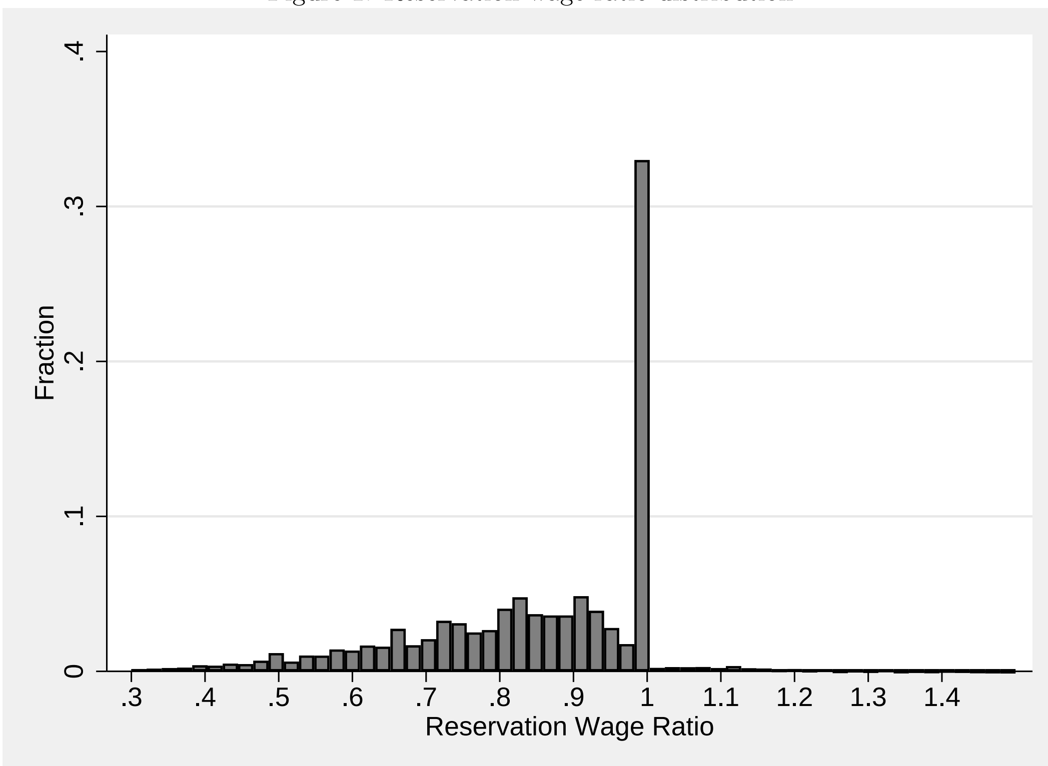


Figure 2: Work contacts distribution

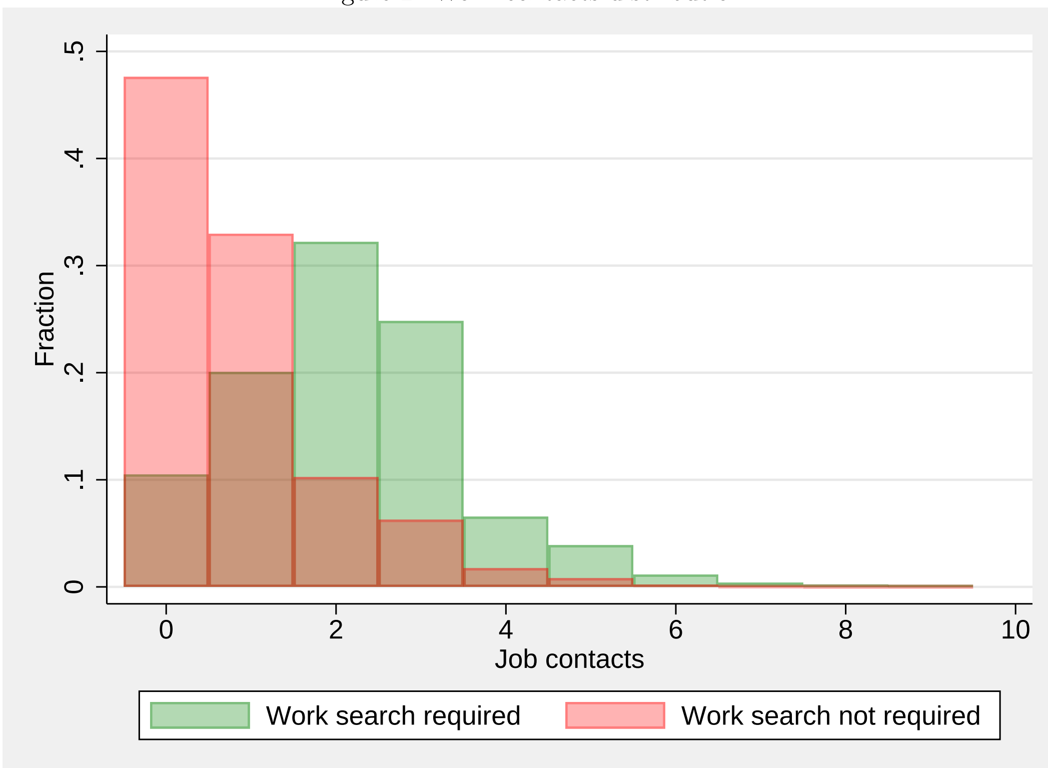
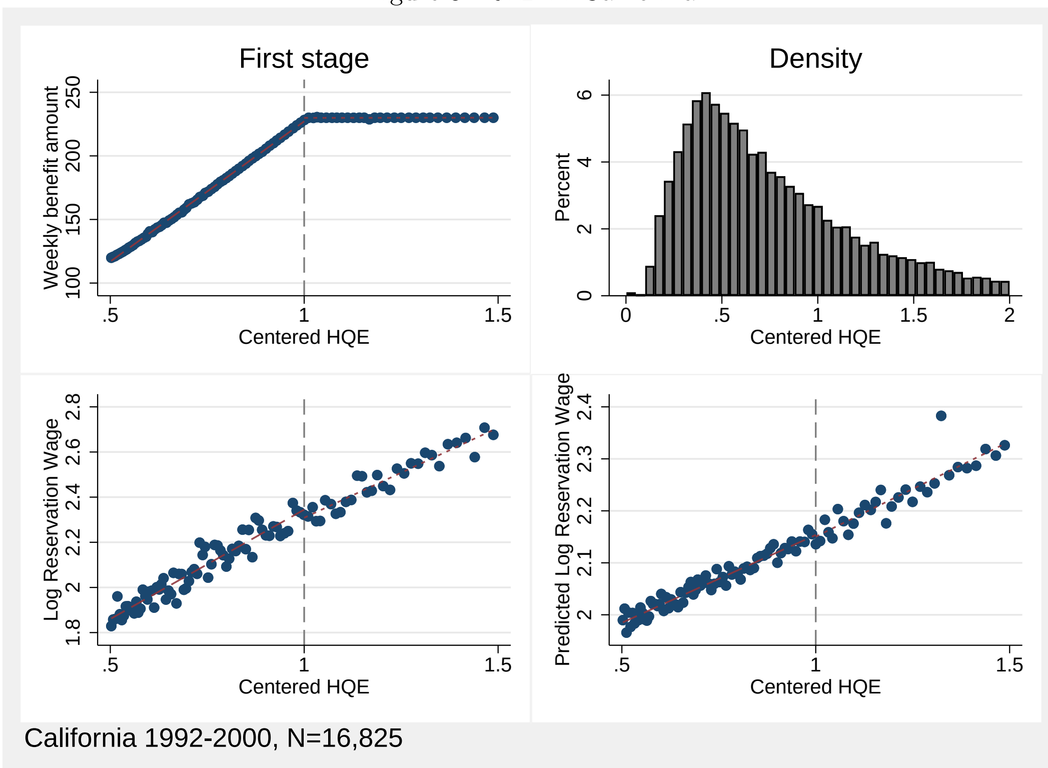


Figure 3: RKD in California



7 Tables

Table 1: Log reemployment wage vs. reservation wage in the matched sample

	(1)	(2)	(3)	(4)	(5)
Log Reservation Wage	0.963***	0.659***	0.510***	0.467***	0.436***
	(0.0490)	(0.0880)	(0.0881)	(0.0899)	(0.0946)
Log Previous Wage		0.340***	0.0737	0.0328	0.0328
		(0.0819)	(0.0942)	(0.0965)	(0.102)
Expecting recall			0.138**	0.135**	0.151**
			(0.0511)	(0.0519)	(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	No	Yes	Yes
Last Occupation	No	No	No	No	Yes
Observations	1673	1673	1673	1673	1673

Standard errors in parentheses

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

Table 2: Outcome variable: Log Reservation wage

	(1)	(2)	(3)
State UE rate	-0.009***	-0.006***	-0.006***
	(0.002)	(0.001)	(0.001)
Log previous wage		0.755***	0.699***
		(0.003)	(0.004)
Log base period earnings		0.048***	0.060***
		(0.002)	(0.002)
Male		0.036***	0.024***
		(0.002)	(0.002)
Age		0.003***	0.003***
		(0.000)	(0.000)
Age^2		-0.000***	-0.000**
		(0.000)	(0.000)
Education (years)		0.005***	0.006***
		(0.000)	(0.000)
Expecting recall		0.035***	0.031***
		(0.002)	(0.002)
Log days since separation		-0.013***	-0.011***
		(0.001)	(0.001)
Month x Year	Yes	Yes	Yes
Ethnicity	No	Yes	Yes
Occupation	No	No	Yes
State	Yes	Yes	No
State x Industry	No	No	Yes
Observations	53047	53047	53047

Standard errors in parentheses

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

Table 3: Outcome variable: Contacts

	(1)	(2)	(3)
Expecting recall	-0.337***	-0.291***	-0.240***
	(0.025)	(0.025)	(0.029)
State UE rate	0.004	0.006	0.010
	(0.010)	(0.010)	(0.011)
Log previous wage		0.038	0.005
		(0.028)	(0.034)
Log base period earnings		-0.000	-0.018
		(0.017)	(0.020)
Male		-0.094***	-0.036
		(0.018)	(0.023)
Age		0.027***	0.026***
		(0.004)	(0.005)
Age^2		-0.000***	-0.000***
		(0.000)	(0.000)
Education (years)		0.033***	0.026***
		(0.004)	(0.004)
Log days since separation		0.036***	0.036***
		(0.009)	(0.010)
Month FE	Yes	Yes	Yes
Ethnicity	No	Yes	Yes
Occupation	No	No	Yes
State	Yes	Yes	No
State x Ind	No	No	Yes
Observations	28116	28116	28116

Standard errors in parentheses

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

Table 4: Outcome variable: Switching occupations

	(1)	(2)	(3)
State UE rate	-0.003	-0.006**	-0.006**
	(0.002)	(0.002)	(0.002)
Log reservation wage		-0.182***	-0.146***
		(0.008)	(0.008)
Log previous wage		0.018*	0.009
		(0.008)	(0.009)
Log base period earnings		-0.002	-0.016***
		(0.003)	(0.004)
Male		0.004	0.006
		(0.004)	(0.004)
Age		-0.003***	-0.001
		(0.001)	(0.001)
Age^2		0.000	0.000
		(0.000)	(0.000)
Education (years)		0.009***	0.007***
		(0.001)	(0.001)
Expecting recall		-0.080***	-0.068***
		(0.004)	(0.004)
Log days since separation		0.022***	0.017***
		(0.002)	(0.002)
Month x Year	Yes	Yes	Yes
Ethnicity	No	Yes	Yes
Occupation	No	No	Yes
State	Yes	Yes	No
State x Industry	No	No	Yes
Observations	51052	51052	51052

Standard errors in parentheses

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

Table 5: Outcome variable: Log average income at target occupation

	(1)	(2)	(3)
State UE rate	-0.033***	-0.025***	-0.020**
	(0.006)	(0.006)	(0.006)
Log previous wage		0.278***	0.001
		(0.029)	(0.014)
Log reservation wage		0.092**	0.062***
		(0.027)	(0.013)
Log base period earnings		0.026	-0.007
		(0.017)	(0.007)
Male		0.049**	-0.005
		(0.015)	(0.009)
Age		-0.003	-0.004
		(0.003)	(0.003)
Age^2		0.000	0.000
		(0.000)	(0.000)
Education (years)		0.028***	0.006**
		(0.003)	(0.002)
Log days since separation		0.008	0.006
		(0.005)	(0.003)
Month FE	Yes	Yes	Yes
Ethnicity	No	Yes	Yes
State	Yes	Yes	No
State x Industry x Occupation	No	No	Yes
Observations	6132	6132	6117

Standard errors in parentheses

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

Table 6: RKD estimates in five biggest samples

	(1) Log Reservation Wage	(2) Reservation wage	(3) Contacts	(4) Switching Occs
California 1992-2000	0.204	1.633	-0.077	0.273
	(0.120)	(1.353)	(0.641)	(0.147)
Observations	15244	15244	5498	15345
California 2005-2016	0.103	2.499	0.377	0.034
	(0.049)	(1.148)	(0.292)	(0.025)
Observations	9216	9216	3585	9228
Florida 1999-2005	-0.042	-0.550	0.150	-0.023
	(0.047)	(0.463)	(0.360)	(0.065)
Observations	7239	7239	6499	7531
Michigan 2002-2017	0.109	0.781	-0.020	-0.015
	(0.055)	(0.527)	(0.115)	(0.042)
Observations	6777	6777	4790	6759
New York 2000-2014	0.055	0.930	-0.391	0.037
	(0.042)	(0.730)	(0.292)	(0.029)
Observations	5995	5995	2096	6092

Standard errors in parentheses.

Table 7: Aggregated RKD estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Log Reservation Wage	0.0100***	0.0285*	0.0123***	0.0336***	0.0198*	0.0057
	(0.0031)	(0.0127)	(0.0032)	(0.0100)	(0.0089)	(0.0045)
Reservation Wage	0.0718	0.5527**	0.0122	0.3333**	0.3445*	0.1082
	(0.0540)	(0.2216)	(0.0497)	(0.1305)	(0.1533)	(0.0760)
Contacts	0.0008	0.0181	0.0180	-0.0210	0.0545	-0.0319
	(0.0177)	(0.0704)	(0.0111)	(0.0327)	(0.0466)	(0.0268)
Switch Occs	0.0042	0.0076	-0.0121**	-0.0113	0.0010	0.0034
	(0.0035)	(0.0137)	(0.0052)	(0.0184)	(0.0098)	(0.0050)
Sample	Full	Full	1st Week	1st Week	Full	Full
Polynomial Order	1	2	1	2	1	2
Bandwidth (times optimal)	1	1	1	1	.5	2
Total Sample	47790	47790	3091	3091	47790	47790

Standard errors in parentheses

Table 8: RKD tests of covariate smoothness

	(1)	(2)	(3)	(4)	(5)	(6)
Log Reservation Wage	0.0034*	0.0008	0.0056	-0.0181	0.0003	0.0061***
	(0.0016)	(0.0063)	(0.0042)	(0.0143)	(0.0043)	(0.0023)
Reservation Wage	0.0404*	0.0197	0.0707	-0.2784	0.0091	0.0778***
	(0.0200)	(0.0804)	(0.0523)	(0.1834)	(0.0559)	(0.0301)
Contacts	0.0019	0.0357*	-0.0203*	-0.0365	0.0282*	0.0014
	(0.0046)	(0.0182)	(0.0100)	(0.0334)	(0.0127)	(0.0069)
Switch Occs	0.0002	0.0028	-0.0029*	-0.0026	0.0033*	-0.0001
	(0.0006)	(0.0023)	(0.0014)	(0.0041)	(0.0016)	(0.0008)
Sample	Full	Full	1st Week	1st Week	Full	Full
Polynomial Order	1	2	1	2	1	2
Bandwidth (times optimal)	1	1	1	1	.5	2
Total Sample	47790	47790	3091	3091	47790	47790

Standard errors in parentheses

A Additional tables and figures

Table A.1: Validation of Missouri match

	(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	2475

Standard errors in parentheses

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

Table A.2: Outcome variable: number of weeks received

	(1)	(2)	(3)	(4)	(5)
Log Reservation Wage	-1.469***	-4.471***	-3.592***	-3.633***	-3.592***
	(0.313)	(0.570)	(0.540)	(0.548)	(0.577)
Log Prev Wage		3.317***	0.179	0.251	0.0474
		(0.528)	(0.595)	(0.612)	(0.646)
Expecting recall			-1.745***	-1.734***	-1.631***
			(0.312)	(0.317)	(0.325)
Month FEs	Yes	Yes	Yes	Yes	Yes
Weekly benefits	No	No	Yes	Yes	Yes
Demographics	No	No	Yes	Yes	Yes
Industry	No	No	No	Yes	Yes
Last Occ	No	No	No	No	Yes
Observations	2475	2475	2475	2475	2475

Standard errors in parentheses

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

Figure A.1: California

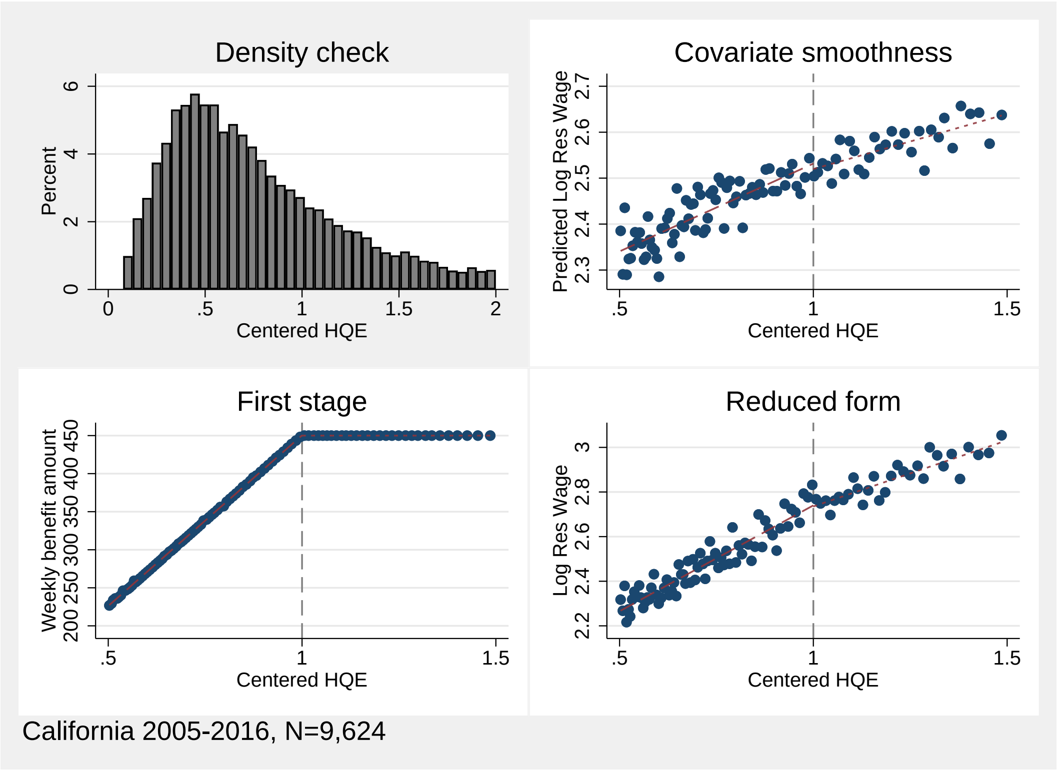


Figure A.2: Florida

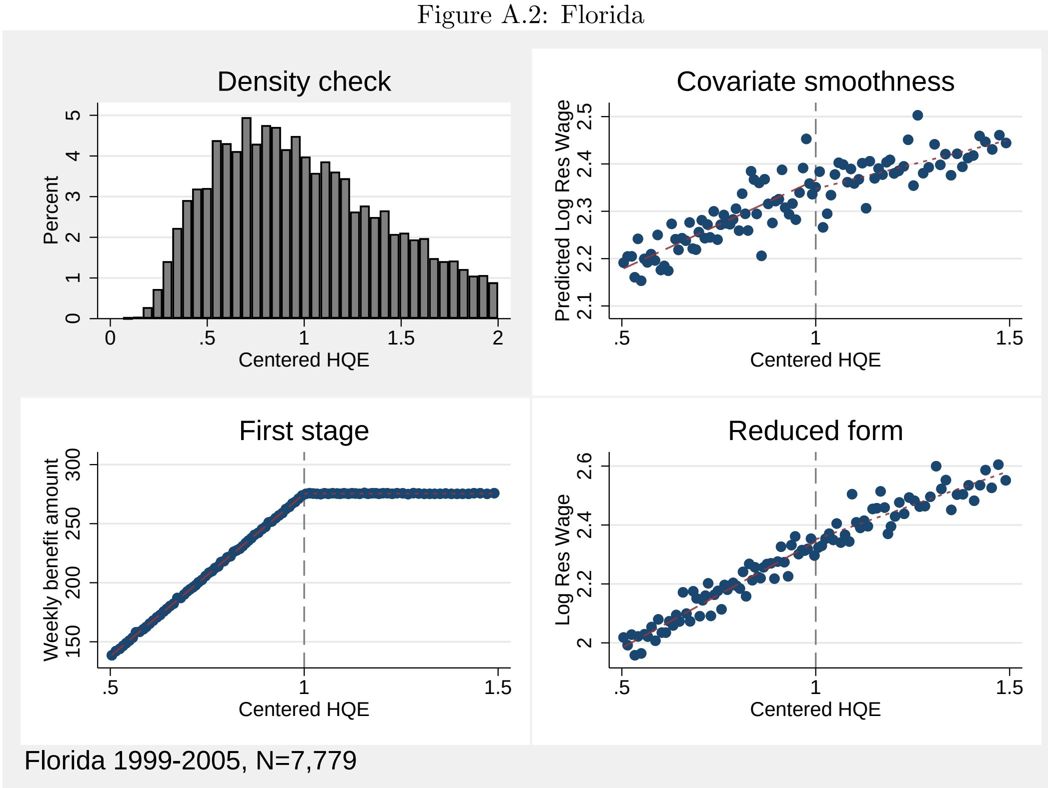


Figure A.3: Michigan

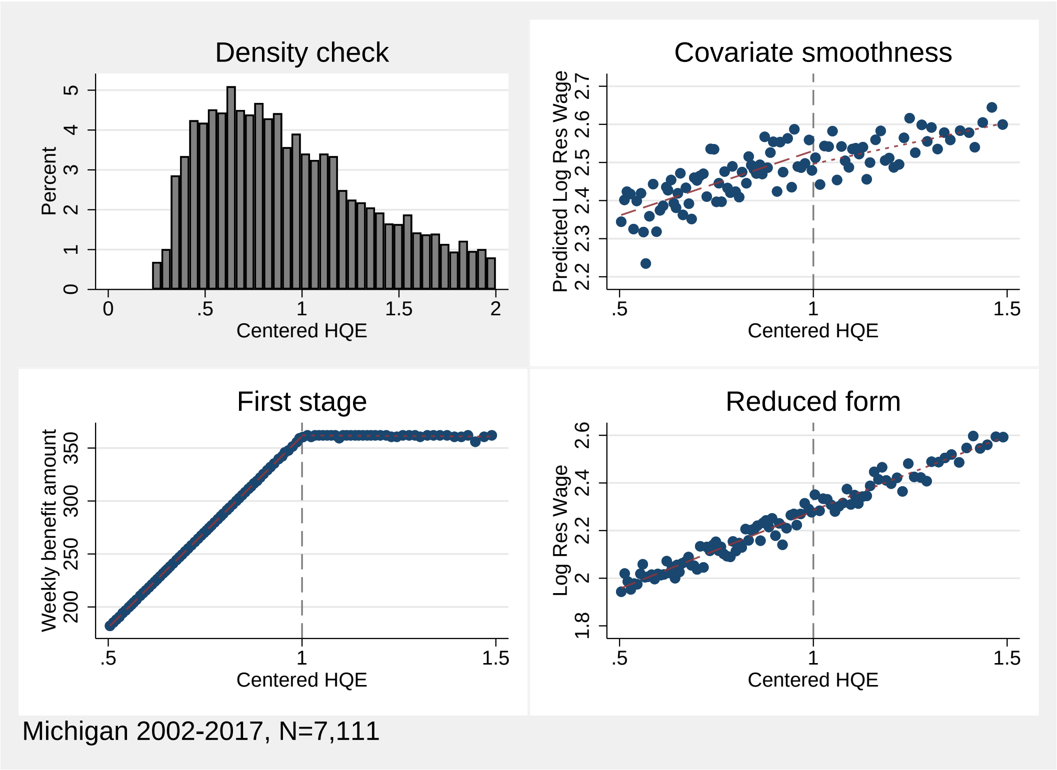


Figure A.4: New York