The Role of Liquidity and Substitution Effects of Unemployment Insurance in the Great Recession

Kay Dannenmaier
Department of Economics
Stanford University, Stanford, CA 94305
kdanne@stanford.edu

Under the direction of
Luigi Pistaferri

Draft: May 5, 2016

Abstract

In response to the 2007 recession in the United States, the federal and state governments extended the duration of unemployment insurance benefits for millions of people by 13 to 72 weeks. Using data from the 2008 panel of the Survey on Income and Program Participation, this paper proceeds in three steps. I first estimate the effect of these extensions on the likelihood of re-employment for the pooled sample. I compare these estimates to others made using data from the Current Population Survey. I then investigate whether the effect is heterogeneous across individuals with different levels of liquid wealth. I find evidence that the effect is heterogeneous, which implies the presence of a substantial liquidity effect. Finally, I investigate the hypothesis that liquidity effects translate into improved post-unemployment wages. I find no evidence for this hypothesis.

Keywords: Unemployment insurance, moral hazard, liquidity effect, expectations

---

I am indebted to Luigi Pistaferri, my thesis advisor, for his feedback and support, both on this project and throughout my time at Stanford. I thank Marcelo Clerici-Arias for his guidance in the writing process, from the beginning to the end. I thank Caroline Hoxby, Raj Chetty, and Jeremy Majerovitz for their comments and literature suggestions. I thank the Census Bureau data specialists for their quick answers to my queries. I am grateful to Andy Werner for his questions.
1. Introduction

In response to the 2007 recession in the United States, the federal and state governments extended the duration of unemployment insurance benefits for millions of people by 13 to 72 weeks. In general, increasing unemployment insurance (UI) benefits increases unemployment. A key question is the channel by which UI benefits produce this effect. The answer has implications for the welfare effects of unemployment insurance.

From a partial equilibrium perspective, there are two main possible channels: (1) UI benefits increase the opportunity cost of leaving the labor force, incentivizing people who would have otherwise left the labor force to continue to search; (2) UI benefits increase the opportunity cost of working, incentivizing people who would have otherwise searched hard for a job to search less. This second possibility can be further broken down into two sub-channels: (a) a moral hazard effect and (b) a liquidity effect. The moral hazard and liquidity effects can be compared to the substitution and income effects of the change in the price of a good: the moral hazard (substitution) effect is the extent to which an individual substitutes leisure for search, holding the marginal utility of wealth constant. The liquidity effect is the additional extent to which an individual substitutes leisure for search, given that her increased wealth means that the marginal utility of additional wealth has decreased. In this paper, I am interested in the extent to which the effect of UI benefit extensions on recipients’ likelihood of leaving non-employment is due to the presence of liquidity effects versus the extent to which it is due to the presence of substitution effects. While previous papers (Chetty 2008, Landais 2015) have investigated liquidity and substitution effects of changes in UI benefit levels and in UI benefit durations, both use data from before 2000. The contribution of this paper is its investigation of these effects during the 2007 recession.

I exploit the variation in the timing of the rollout of unemployment insurance extensions across states to estimate the effect of extensions on the non-employment hazard rate. To investigate the presence of liquidity effects, I use the asset data collected through the SIPP to construct a liquid wealth proxy for total liquidity and stratify my core sample into two groups: one with liquid wealth above $295 prior to job loss, one with liquid wealth below $295 prior to job loss. I find evidence for the presence of a liquidity effect of UI benefit duration extensions, one which drives around 30 percent of their effect on the job-finding hazard rate.

The paper is organized into the following sections. Section 2 provides a brief overview of unemployment insurance benefits and extensions in the United States during the Great Recession. Section 3 provides the theoretical framework. It explains how a simplified version of the Chetty (2008) model of the effects of UI benefit levels on job search can be applied to the case of UI benefit duration extensions. Section 4 reviews the recent relevant literature. Section 5 describes the empirical methodology. In particular, it explains the use of the Cox (1972) proportional hazards model. Section 6 gives data sources and summary statistics for key variables. Section 7 gives the results. Section 8 describes the robustness checks and outcomes. Section 9 concludes. Please note that all tables and figures can be found in the appendix.

2. Background on U.S. Unemployment Insurance during the 2008 Recession
2.1 Background on the U.S. Unemployment Insurance Program

Unemployment insurance in the United States is administered through the Unemployment Compensation program, a joint federal-state program. State taxes on employers fund regular benefits. The Federal Unemployment Account, supported by federal taxes on employers, funds program administration and fifty percent of Extended Benefit (EB) payments. It also funded the Emergency Unemployment Compensation (EUC08) program through April 1st, 2009 and grants loans to insolvent state unemployment insurance programs. The American Recovery and Reinvestment Act of 2009 (ARRA) increased the federal share of Extended Benefits funding to 100 percent through 2011 (Shelton 2009). The ARRA also changed the funding source of the EUC08 program to general revenues.

Each state determines eligibility rules for its own unemployment insurance system. In general, one must meet three conditions to be eligible. First, an individual must have involuntarily lost her job for reasons unrelated to disability or misconduct. Second, individuals must have earned a certain level of wages or worked a certain number of hours (or both) in a base period of time preceding the unemployment spell. Third, once an individual has been determined eligible, she must maintain her eligibility by actively searching for work.

Two factors determine the duration of an individual’s unemployment insurance. The first factor is the individual’s weekly benefit amount. The formula for the weekly benefit amount varies by state; in general, the weekly benefit replaces some percentage of a worker’s average covered weekly wage up to a maximum determined by the state. The second factor is the total benefit amount. In most states, this is twenty-six times the weekly benefit amount. \(^2\) Together,

---

\(^2\) Before 2011, the only states with different benefit durations were Montana (28 weeks) and Massachusetts (30 weeks) (Isaacs 2013).
these two factors mean that the duration of regular benefits is twenty-six weeks in the majority of states.

Finally, if an individual is determined eligible and she meets the conditions for maintaining eligibility, her claim to unemployment insurance is good for the following year in most states. This does not mean that she can collect unemployment insurance continuously for the year. Instead, the year-long claim means that if an individual has a temporary job that, through no fault of her own, ends within the claim-year, she can continue receiving any remaining benefits through the end of the claim-year.

2.2 Background on Unemployment Insurance Duration Extensions in the U.S. during the Great Recession

Congress established the permanent Extended Benefit (EB) program in 1970 to provide states with extra unemployment compensation during economic downturns. It extends the duration of UI benefits in three scenarios triggered at different levels of a state’s insured unemployment rate (IUR) or total unemployment rate (TUR). One threshold is mandatory – if a state meets it, it must extend benefits. If a state’s IUR for the previous 13 weeks has been at least 5% and is 120% of the average of the rates for the same period in each of the previous years, the state is mandated to pay 13 weeks of extended benefits.

The other two thresholds are optional – upon meeting them, the state can choose whether to trigger benefits or not. The state may provide an additional 13 weeks of benefits if the state’s IUR is at least 6%, regardless of previous averages. The state may also provide an additional 20 weeks of benefits if the state’s TUR is both at least 8% and at least 110% of the state’s TUR for the same 13-weeks in either of the past two years.
When the program is triggered “ON” in a state (and in the two weeks after it has been turned off) individuals who have used all of their weeks of regular unemployment insurance benefits and who remain eligible for unemployment benefits are eligible for extended UI benefits, up to the number of extra weeks specified by the triggered scenario. The Extended Benefit program is a week-by-week program. If an individual has used 5 weeks of her EB benefits and the EB program in her state is triggered off, she will not be able to collect the full 13 or 20 weeks of extended unemployment insurance.

In contrast with the Extended Benefit Program, the Emergency Unemployment Compensation Act enacted in July of 2008 was a temporary program. It was originally authorized for nine months. However, multiple expansions and extensions meant that it instead ended on December 2013.

EUC08 extended possible benefit durations in tiers. If a tier was available in a state, eligible individuals who had either exhausted their unemployment benefits or were at the end of their claim-year were credited with unemployment insurance equal to their weekly benefit amount times the number of weeks specified by the tier. The first tier was nationally available; further tiers became available depending on a state’s level of unemployment. In every state (other than Alaska before February 12, 2012), unemployed individuals went onto EUC08 before they went onto EB.

To understand how the EUC08 tiers work, let’s consider an example. Suppose I live in Wyoming, and I become unemployed on July 1, 2011. Though I search for a job, I do not find one, and in 26 weeks, on December 30, 2011, I exhaust my regular benefits. At this point in time, Tier I of EUC08 is available in Wyoming because Public Law (P.L.) 111-312 is in effect. Tier I of EUC08 gives me another 20 weeks of unemployment compensation. Again, I search for a job
and cannot find one, and so I exhaust my Tier I of EUC08 benefits on April 20, 2012. Tier II of EUC08 benefits is available in Wyoming at this time because P.L. 112-96 is in effect, and so 14 additional weeks of benefits are credited to my account. This means I can continue receiving benefits through July 27, 2012, even though Tier II of EUC08 benefits ceases to be available in Wyoming on June 24, 2012 because under the new law, P.L. 112-96, Tier II is only available in states with unemployment rates above 6%. After July 27, 2012, no further unemployment compensation is available to me (Extended Benefits aren’t available in Wyoming). See Figure 1 for a visual representation of this example.

The example demonstrates that the length of the extension an unemployed person receives depends on what laws are in effect at the time she exhausts her regular or most recent tier of benefits, rather than on what laws are in effect at the time she becomes unemployed. This fact has implications for the analysis. Any effects of the benefit extensions on individuals’ likelihood of exit into employment are caused by eligible individuals’ expectations that the benefit extensions would increase eligible individuals’ income in the future. This leads us to two questions: first, when are individuals’ expectations of the length of their extensions formed? Are they formed when individuals first lose their job or when they start receiving benefits? Second, how are eligible individual’s expectations formed? At the beginning of their unemployment spells, would eligible individuals expect the EUC08 and EB programs to continue to be available at their current levels, regardless of the length of time for which the programs were actually authorized? Or would they expect the EUC08 program to end at the time specified in the authorizing law?

Rothstein (2011) investigates the second question. He finds that if we assume individuals expect the EUC08 and EB programs to continue to be available at their current levels, the effect
of UI benefits on the likelihood of exit from unemployment is much larger than it is if we assume individuals expect no further federal legislative action to continue the EUC08 and EB programs. I follow Farber and Valleta (2011) in assuming that individuals expect the EUC08 and EB programs to continue. I explore the first question – when are individual’s expectations of the length of benefit extension they will receive formed? – in the robustness checks. What else do we get from this background?

First, it drives the question I am interested in answering. Given the permanence of the 1970 law, the United States’ policy reaction to high unemployment will always include some extension of the duration of UI benefits. Therefore, it is important to understand the effects of extensions on the likelihood of finding a job.

Second, because of the on-and-off availability of the extension programs during the recession, unemployment insurance benefit duration varied across states and time. This creates the variations in unemployment insurance policy I will exploit in my regression analysis. See Figure 2 for a visual summary of benefit availability during the 2008 recession.

3. Theory

3.1 Model

We can understand the situation using a simple version of the partial-equilibrium job search model developed in Lentz and Tranaes (2005) and Chetty (2008) on the basis of the model proposed by Baily (1978). We will also consider extensions proposed by Landais (2015).

Consider a discrete time setting where a utility-maximizing individual becomes unemployed at t = 0. Suppose the interest rate and the agent’s own discount rate are 0. Let the agent begin unemployment with assets A0; assume these assets are exogenous to UI benefit
availability prior to job loss – the agent’s savings behavior is not affected by the level or duration of her potential unemployment insurance benefits. The agent can either find a job at the beginning of period \( t \) or fail to do so. She first chooses her search intensity \( s_t \). Normalize \( s_t \) to be the probability that she finds a job. Let \( e(s_t) \) be the cost of the search intensity. Note that \( e(s_t) \) is strictly increasing and convex. If the individual finds a job at the beginning of the period \( t \), she begins immediately. The job will be permanent, and she will receive a fixed wage \( w_t \) (that is, there is not a distribution of wages, so she does not have a reservation wage).\(^3\) She sets her consumption to \( c_t^e \), so that her flow consumption utility in the period is \( v(c_t^e) \). Her subjective discount rate is \( \delta \). In this situation, her value function will be \( V_t \):

\[
V_t(A_t) = \max_{s_t} v(A - A_{t+1} + w_t - \gamma) + \delta V_{t+1}(A_{t+1})
\]  

(1)

where \( L \) is the lower bound on assets and \( \gamma \) is the tax the worker pays on her earnings to finance unemployment benefits. Here \( c_t^e \) is the difference between the sum of her assets and income in the current period and her planned assets going into the next period.

If the worker remains unemployed at the beginning of the period, she receives a benefit \( b_t < w_t \). Note that there are only two states: employed and “unemployed,” where an individual is “unemployed” if she is non-employed. The worker’s consumption is \( c_t^u \) and her flow consumption utility is \( u(c_t^u) \). Her value function is described by \( U_t(A_t) \):

\(^3\) This is a valid assumption because the micro-economic evidence suggests that increases in UI benefits are not correlated with increases in the wages the unemployed accept. See Section 8.3 of this paper. In addition, see Addison and Blackburn (2000) for evidence on the wage effect resulting from increases in UI benefit levels in the United States; see Van Ours and Vodopivec (2008); Card, Chetty and Weber (2006); and Caliendo, Tatsiramos, and Uhlendorff (2013) for evidence on the wage effect resulting from increases in the duration of UI benefits outside of the United States.
Assume that $U$ and $V$ are concave. An unemployed agent chooses $s_t$ to maximize her expected utility, as shown in (3). The first order conditions tell us that she does this when:

$$
\phi'(s_t) = V_t(A_t) - U_t(A_t)
$$

The agent optimizes search intensity at the point at which the marginal cost of the search is equivalent to the difference between the value of the employed and unemployed states.

To use this model to understand how UI benefit extensions change individuals’ search behavior, we first note that extending benefits such that benefits remain at level $b_0$ for $B + y$ periods instead dropping to 0 after $B$ periods ($y > 0$) is equivalent to increasing benefits from 0 to $b_0$ for $y$ periods. Then, we follow Chetty (2008) in considering three effects. We begin by considering the effect of a $1 increase in benefit $b_t$ on search intensity $s_t$ in period $t$:

$$
\frac{\partial s_t}{\partial b_t} = \frac{-u'(c_t^{\text{e}})}{v'w_t^{\text{e}}} 
$$

We then consider the effect of a $1 increase in the assets $A_t$ on $s_t$ in period $t$:

$$
\frac{\partial s_t}{\partial A_t} = \frac{v'(c_t^{\text{e}};w_t^{\text{e}}) - u'(c_t^{\text{u}};w_t^{\text{e}})}{v'w_t^{\text{e}}}, \quad \frac{\partial s_t}{\partial A_t} \leq 0
$$

The effect of an increase in assets (cash grant) on search intensity depends on the difference between the marginal utility of consumption in the employed and unemployed states.

Finally, we consider the effect of a $1 increase in the wage rate $w_t$ on $s_t$ in period $t$:
\[
\frac{\partial s_i}{\partial w_i} = \frac{v'(c_i^w)}{\sigma''(s_i)} \tag{7}
\]

Intuitively, an increase in wage increases the value of being employed, and therefore increases search intensity. Putting this all together, we have the decomposition.

\[
\frac{\partial s_i}{\partial b_i} = \frac{\partial s_i}{\partial A_t} - \frac{\partial s_i}{\partial \nu_i} \tag{8}
\]

In (8), we see two distinct channels through which benefits affect search intensity. \(\frac{\partial s_i}{\partial \nu_i}\) is the moral hazard or substitution effect: benefits temporarily lower the effective net wage rate by increasing the opportunity cost of working, thus decreasing the incentive to work.\(^4\) This generates a clear social welfare loss, since it distorts recipients’ private incentive to work below the social incentive to work – that is, below the true wage rate –holding the marginal utility of consumption in the unemployed state constant. \(\frac{\partial s_i}{\partial A_t}\) is the liquidity effect: benefits increase one’s liquid wealth. This effect also decreases the incentive to return to work since individuals with greater liquidity can sustain the same level of consumption while working (at the same wages) for fewer hours. In and of itself, however, this decrease in the incentive to work is not a distortion, since it does not distort the wage rate. See Figure 3 for a graphical representation of the effect of an increase in benefits on a liquidity-constrained individual’s search effort \(s_i\).

Furthermore, the liquidity effect is associated with the difference between the marginal utilities of consumption in the unemployed and employed states for the recipients of UI.\(^5\) In general, these benefits allow liquidity-constrained individuals to smooth their consumption

\(^4\) Landais (2015) notes that this effect is also known as the Frisch elasticity in other contexts.

\(^5\) Note that Chetty sometimes uses language that suggests the liquidity effect “reduces the need for agents to rush back to work.” “Rush” is a normative term that seems to imply that in the absence of the liquidity effect, individuals would have returned too quickly to work – they would have found worse matched jobs, or been less productive, or something in that vein. However, such effects on productivity or job-matching are not what Chetty intends by effects on welfare.
between employed and unemployed states. Given some common assumptions about individuals’ utility functions, this consumption-smoothing increase individuals’ total consumption utility over their lifetimes (Pistaferri and Japelli, forthcoming). This is true even if the liquidity-constrained individual’s UI benefits are completely funded by the taxes she pays while employed (the balanced-budget case): then benefits serve to move liquid wealth from the individual’s future (where she presumably has greater liquidity) to her present (where she less liquidity), again allowing her to smooth her consumption.

Depending on the way in which the benefits are funded (and the ethical framework we use), the liquidity effect may result in a social welfare increase. In a balanced-budget case, an individual’s UI benefits are completely funded by the taxes she pays while employed – UI is non-redistributive. That means that we can say that increases in utility resulting from the liquidity effect of UI benefits increase societal welfare with only the relatively uncontroversial assumption that an increase in utility increases welfare. In the non-balanced-budget case, in which taxes on other people fund an individual’s benefits, claiming that the increases in utility resulting from the liquidity effect of UI benefits increase societal welfare requires the additional controversial assumption that redistribution can increase welfare. Even in this case, however, we can still say that a UI benefits extension results in a utility gain for individuals since it increases the recipients’ flow consumption utility. Since the benefit extensions at question in this paper are not paid for in the same way, the needed normative claims about welfare are more controversial and so I’ve avoided them.  

---

6 Chetty (2008) expands the model given here with the addition of a social planner who determines the level of UI benefits by optimizing social welfare. Given that Chetty assumes that increases in benefits will be balanced by increases in taxes on the agent receiving the benefits, he needs to make relatively uncontroversial normative claims about welfare to come to his conclusion.
3.2. Implications for Empirical Analysis

The decomposition in (8) only explains a 1-period increase in benefits – a one-week extension. The extensions in question are usually much longer – up to seventy-two weeks. While the intuition remains similar, the model is more complex. Therefore, to determine which parameter we want to estimate, we turn to Landais (2015) for his expansion of the model. He starts with the following Euler equations:

\[ v'(c^e_t) = \begin{cases} \delta u'(c^e_{t-1}) \text{ if } \Lambda_t > L \\ u'(w_t - y) \text{ if } \Lambda_t = L \end{cases} \] (9)

\[ u'(c^n_t) = \begin{cases} \delta s_t u'(c^{e+1}_t) + (1-s_{t+1})u'(c^n_{t+1}) \text{ if } \Lambda_t > L \\ u'(b_t) \text{ if } \Lambda_t = L \end{cases} \] (10)

Variables refer to the same parameters as in previous equations. If the credit constraint is not binding at time \( t \), these imply:

\[ v'(c^e_t) = \delta^t v'(c^e_0) \] (11)

\[ u'(c^n_t) = [\sum_{j=0}^{t-1}(1-s_j)\delta^j u'(c^e_{j+1})] + \delta^t \prod_{i=1}^t (1-s_i)u'(c^n_t) \] (12)

After some algebra, Landais (2015) shows that equations (11) and (12) together with a generalization of equation (5) implies that an increase in benefit duration of \( B \) periods at a constant benefit amount of \( b \) has the effect on search described by:

\[ \frac{\partial \gamma}{\partial \nu} = b \left[ \frac{\partial \gamma}{\partial \omega} - \frac{\partial \gamma}{\partial \nu} + \prod_{i=1}^t (1-s_i) \right] \] (13)

Here \( \prod_{i=1}^t (1-s_i) \) is the survival rate at time \( B \), conditional on being still unemployed at time \( t = 1 \), and the derivatives are the elasticities of search intensity with respect to various parameters at the start of the benefit period. Can we use this model to identify the liquidity and substitution effects? Chetty (2008) points out that for a non-liquidity-constrained agent, the liquidity effect is zero: since such an agent has access to perfect credit and insurance markets, she can smooth her consumption so that \( u'(c^n_t) = v'(c^e_t) \) for all \( t \). Since her marginal...
consumption utilities are the same, a change in benefits has the same effect on her consumption utility for both her future employed and unemployed states. Therefore, for a non-liquidity-constrained agent, the only channel through which UI benefit extensions affect the likelihood of exit from unemployment to work is the substitution or moral hazard effect.

For a liquidity-constrained agent, \( v'(c_t^e) - v'(c_t^u) < 0 \). A change in benefits increases her consumption utility in the unemployed state, but since she cannot move wealth between states, the marginal consumption utilities do not equilibrate. Since her unemployed marginal consumption utility is higher than her employed marginal consumption utility by the concavity of \( u \) and \( v \), this implies that the liquidity effect is negative for her.

Do substitution (moral hazard) effects vary between those who are liquidity-constrained and those who are not? In their literature review, Reichling and Whalen (2012) mention studies of differences in substitution effects between other pairs of social groups (men/women, single/dual-income, children/no children), but do not offer evidence of differences between liquidity groups. In simulations using isoelastic utility functions and realistic parameters, Chetty (2008) finds that there is no difference in the substitution effect between wealth groups. In this paper, I make the identifying assumption that substitution effects do not vary.

This gives me a strategy to identify the liquidity effect of UI benefit duration extensions.

Let the \( L \) subscript denote liquidity constrained individuals; let the \( NL \) subscript denote non-liquidity constrained subjects.

\[
\frac{\partial s_{c,L}}{\partial v} \times \frac{1}{b_L} - \frac{\partial s_{c,NL}}{\partial v} \times \frac{1}{b_{NL}} \times \frac{\prod_{i=1}^P \pi_{i-1}^{1},1-s_{1NL}}{\prod_{i=1}^P \pi_{i-1}^{1},1-s_{1NL}} = \left( \frac{\partial s_{c,L}}{\partial u} - 0 \right) - \left( - \frac{\partial s_{c,L}}{\partial w_{u}} + \frac{\partial s_{c,NL}}{\partial w_{u}} \right)
\]

\[
= \frac{\partial s_{c,L}}{\partial A_{g}}
\]
I can find approximations for $b_L$ and $b_{NL}$ from median benefits for individuals above and below the liquidity median in the SIPP data. Similarly, I can find the cumulative survival rates through $B=6$ weeks, the regular UI benefit period, from the data. In Section 5, I outline my strategy for estimating $\frac{\delta x_{ult}}{\delta u}$ and $\frac{\delta x_{ult,NL}}{\delta u}$. Before I describe my empirical strategy more fully, I briefly review the methods and results of four recent empirical papers that analyze the actual effect of changes in UI benefits on the likelihood of exit from unemployment in the United States.

4. Literature Review

4.1 The Presence of the Liquidity Effect

Chetty (2008) uses a two-pronged approach to estimate the size of liquidity effect of benefit level increases relative to the total effect of UI benefits. First, he uses data from the Survey on Income and Program Participation (SIPP) for the years 1985 to 2000 to estimate both the total effect of increases in UI benefit levels on the likelihood exit from unemployment and the effects of increases in UI benefit levels on individuals stratified by net financial wealth quartile. Next, he uses data from Mathematica on severance payments to estimate the liquidity effect of increases in UI benefit levels on the likelihood of exit from non-employment. He finds that when benefits are 50 percent of the wage, 60 percent of the effect on likelihood of exit to employment is driven by the liquidity effect.

While these results are suggestive for the effects UI benefit extensions in the Great Recession, there are three worries about their exact applicability. First, Chetty estimates the liquidity effects of UI benefit levels based on the liquidity effects of severance payments. While severance payments are similar to UI benefits, severance payments are often given out as lump
sums rather than short-duration weekly grants, in the way that UI benefit levels are. Indeed, when studying the liquidity effects of tax refunds – another large grant – LaLumia (2013) finds effect sizes similar to Chetty’s. At the very least, this means that Chetty is estimating the liquidity effects of much larger grants than the UI weekly grants, a concern if liquidity effect size changes depending on grant size.

Second, there is reason to think that the average liquidity effects were larger in the 2008 recession than in previous periods. Studying recent administrative data from Missouri, Card et al. (2015) find that unemployment durations were more responsive to unemployment benefit levels during the recession. They suggest that a possible explanation is a composition effect: more people are liquidity-constrained, so the average liquidity effect is greater (because their data set does not include information on individuals’ access to savings or credit, they are not able to investigate their hypothesis). This suggests that the Chetty (2008) results may under-estimate the effect of UI benefit extensions during the 2008 recession.

Landais (2015) goes some way towards solving the first problem, but doesn’t solve the second. Rather than using data on severance payments, he uses administrative data on the U.S. UI benefit system and a regression kink design around the maximum weekly benefit amount and maximum duration of UI benefits. He estimates the size of the moral hazard and liquidity effects on the duration of non-employment. In contrast with Chetty (2008), Landais (2015) finds that liquidity effects drive 46 percent of the effect on likelihood of exit to employment. His data from the Continuous Wage and Benefit History Project only covers unemployment spells between the late 1970s and 1984, however, so his results may not hold for UI benefit effects occurring in more recent years.

4.2 Estimates of the Effect of UI Benefits in the Recent Recession
Hagedorn, Karahan, Manovski and Mitman (2013) investigate the effect of extending unemployment benefits in the United States during the Great Recession using Pissarides’ (2000) general equilibrium model. They come to the conclusion that a reservation wage increase resulting from UI benefit duration extensions explains much of the persistence of high unemployment in the US during the time.

In order to identify the effects of unemployment benefit extensions, Hagedorn et al. exploit the policy discontinuity at state borders, estimating the effect of the quasi-difference of benefit policies on the quasi-difference of variables such as vacancies and unemployment for counties that neighbor each other, but are across state lines. They find that extending the duration of benefits is associated with a large increase in unemployment – every additional week of unemployment insurance increases the unemployment rate by about .06 log points. Furthermore, they find evidence that UI benefits cause this effect by causing a shift along the labor demand curve: extending benefit durations increases the equilibrium wage, and decreases the number of vacancies, consistent with the labor demand mechanism.

Their results have been questioned, notably by Hall, who suggests that their identification strategy is relatively weak, given the low correlation between reported unemployment in neighboring counties (Hall 2013). In addition, Landais et al. (2015) criticize the Pissarides (2000) model on which Hagedorn et al. (2013) base their analysis. Landais et al. (2015) note that it does not account for UI benefit extensions’ effect on search effort apart from their effect on the reservation wage.

A completely different approach to identifying the effect of UI benefit extensions is taken by Rothstein (2011). Controlling for macro-economic conditions, he investigates the extent to which unemployment insurance extensions affect unemployment. Working with data from the
Current Population Survey (CPS), Rothstein suggests a logit model where the dependent variable is the likelihood ratio of an individual exiting unemployment in the next month she is observed. The independent variable of interest is the total weeks of unemployment insurance available to the individual. As controls, Rothstein includes, the length of the individual’s unemployment to date, and state and month fixed effects. To account for labor demand conditions, Rothstein runs different specifications. He first attempts to absorb the labor demand conditions by controlling for measures of economic conditions such as the unemployment rate. He then focuses on variation in state decisions about which EB triggers to adopt by adding controls for the number of EUC weeks available and status for each of the four EB triggers. Rothstein (2011) finds a much smaller effect on unemployment than do Hagedorn et al (2013). At most, he estimates, all of the extensions together increased the unemployment rate by 0.5 percentage points. Farber and Valletta (2011) undertake a similar study of CPS data and find similar results.

One problem Rothstein faces is that, since he is using CPS data, he does not have good information on whether individuals are actually eligible for unemployment benefits. In addition, since he is working in 2011, he does not have information on the entire cycle of the benefits. Finally, since the CPS data has very limited information about assets, he does not look for the presence of liquidity effects.

4.3 Contribution

While there have been papers that look for the size of the liquidity effect of UI benefits on likelihood of exit from employment in the years before 2000, and there have been papers that look for the size of the effect of UI benefits on likelihood of exit from employment during the 2008 recession, there is no research on the presence or size of the liquidity effect of UI benefits
on likelihood of exit from employment during the recession. My goal is to take a small step towards closing this research gap by estimating the average size of the liquidity effect among low-liquidity individuals on the likelihood of exit to employment that results from an additional month of UI benefits in comparison with the total effect that results from an additional month of UI benefits. I do this by comparing the effect of UI benefit extensions on the non-employment hazards of individuals with greater and lesser liquidity.

5. Estimation Strategy

Following Meyer (1990) and Chetty (2008), I estimate the effect of UI benefit duration extensions on the hazard rate from non-employment. Let $T_i$ be the length of an individual’s non-employment spell. Then the hazard rate – the likelihood that the individual will exit non-employment – for the individual is:

$$\lambda(t) = \lim_{dt \to 0} \frac{\Pr[T_i \leq dt | T_i > t]}{dt}$$

(15)

Now we have the basic Cox (1972) proportional hazard model:

$$\lambda_g = \lambda_0 \cdot e^{\mu X}$$

(16)

where $h_g$ is the hazard of an individual with characteristics $g$, $h_0$ is the (unknown) baseline hazard, and $X$ is a vector of the group characteristics that affect the hazard rate. I use the Cox (1972) model here because it does not make any assumptions about the baseline hazard function and it has the advantage of being able to account for censored data. Taking the natural log of both sides and filling in $X$ with the characteristics in this case, I first estimate:

$$\ln (\lambda_{ist} / \lambda_0) = \gamma_{d_{ist}} + \beta_{Z} (Z_{ist}; \delta) + \beta X_{ist} + \alpha_{s} + \tau_{t}.$$  

(17)

Here, the dependent variable of interest $\lambda_{ist}$ is the probability that the individual $i$ exits non-employment by month $t + 1$, given that he has remained unemployed until month $t$. I focus
on non-employment rather than unemployment (which doesn’t include those who have left the labor force) for two reasons. First, it makes more sense given the job search model explained above – only two states are considered, employment and non-employment. Second, there are some individuals who both report that they’re not searching for work and that they are receiving unemployment benefits. This fact suggests that the search effort condition of UI benefits is imperfectly enforced, so that receipt of UI benefits is conditioned on not being employed, rather than on being employed.

\( d_{ist} \) is the length of unemployment benefits in statute at the time the individual first started benefits. In the robustness checks, I consider the effects of defining \( d_{ist} \) in different ways. \( Z_{st} \) is a vector of controls for macro-economic conditions at the time of the individual’s job separation. It includes state unemployment rate, national job openings rate, and season in which benefits began. \( P_Z \) is its polynomial. \( X_{ist} \) is a vector of controls for personal characteristics. It includes financial net wealth, marital status, age, the indexed maximum of weekly benefits. Finally, I include a control for being on the survey seam (that is, the month when the survey was collected) in order to correct for the seam effect. \( \alpha_s \) and \( \tau_t \) are fixed effects for states and years, and control for fixed characteristics of an individual’s state and year of unemployment.

Next, I estimate a regression to investigate the question of interest: how does the effect of extensions of unemployment benefit duration on the likelihood of exit from non-employment vary between individuals with liquidity constraints and individuals without?

I estimate a Cox proportional hazard model of the form:

\[
\ln \left( \frac{\lambda_{ist}}{\lambda_0} \right) = \gamma d_{ist} Q_j + \alpha l + P_Z (Z_{st} ; \delta) + \beta X_{is} t + \alpha_s + \tau_t \tag{17}
\]

The variable definitions are the same as above. The additional variable \( l \) is liquid wealth.
I use an individual’s liquid wealth as a proxy for being liquidity constrained. Liquid wealth is defined as the sum of an individual’s interest-earning assets, non-interest-earning assets, bonds and securities, and stocks. In this definition, I diverge from the literature in two ways (Browning and Crossley 2001; Bloemen and Stancanelli 2005; Sullivan 2007; and Chetty 2008). First, I do not subtract unsecured (credit card) debt from liquid wealth. It is not clear that higher credit card debt indicates lower liquidity, since the absence of credit card debt may indicate either that an individual has credit available but does not need to use it or that an individual does not have credit available. Indeed, Sullivan (2007) finds that individuals in the lowest decile of assets do not borrow to smooth consumption during unemployment spells, while individuals in the second and third decile of assets do borrow to smooth consumption, perhaps because they have access to more credit. This suggests that individuals in the lowest decile of assets without credit card debt are in fact more liquidity constrained than individuals in higher deciles who have credit card, which argues against subtracting unsecured debt from any measure meant to proxy for liquidity. Second, I do not count financial wealth in retirement savings accounts such IRAs or 401ks. This is because one’s retirement savings may be subject to a penalty if one withdraws from them at early ages, making them imperfectly liquid.

The additional variable $Q_i$ is a dummy variable for whether the individual has liquid wealth greater or less than the median value of the sample. In the absence of a theory of how much wealth one needs to be free of liquidity constraints and in the interest of using as many observations as possible, I split the sample at the median and use the upper half as my non-liquidity-constrained group and the lower half as the liquidity-constrained group. In doing this, I make the assumption that the upper half of my data is not liquidity constrained; if it has members who are liquidity constrained, then my estimates of a liquidity effect will be biased towards zero.
I estimate coefficients both level of liquidity and on the interactions between the liquidity dummies and benefit months. This allows me to isolate the liquidity effect of the UI benefit duration extensions from the more general effect of having greater liquidity. Following Chetty (2008), I focus on the interaction effect since whether an individual’s benefits are extended is exogenous to his preferences, while differences in liquidity may arise from differences in tastes for savings.

The Cox model does not make any assumptions about the form of the baseline hazard. However, two conditions must be met in order for its use to be valid. First, censoring must be non-informative. That is, the characteristics that cause an individual to be censored should not be related to the probability that he will leave unemployment. This condition is met since whether an individual is censored depends on when he enters and exits the SIPP survey, which is unrelated to his likelihood of leaving unemployment.

The second condition is that of proportional hazards. This means that the survival curves must have hazard functions that are proportional over time, so that the relative hazard out of non-employment is constant. Meyer and Katz (1990) find that the hazard rate from non-employment for UI recipients has a steep downward slope at the very beginning of unemployment, a slight downward slope for the duration of UI benefits, and a sharp upward slope at the end of UI benefits. Since the majority of my sample has a non-employment length between 12 and 42 weeks, well within the boundaries of UI benefit availability at this time, the evolution of their hazard rate can be described as a slight downward slope – close to a constant relative hazard. Looking at Figure 2, we see that over 30 months (about 120 weeks), the average hazard rate declines by only 4 percentage points. Therefore, we can begin with the assumption that the hazard condition is met. In section 8.2 of the robustness checks, we will test this condition and
find that there is non-linear trend in the effect of UI benefit duration on hazard rate from non-employment, depending on the length the individual has already been unemployed.

If benefit extensions have the same effect (or a smaller effect) on the non-employment durations (likelihood of exit from non-employment) of individuals with less liquid wealth as they do on the non-employment durations of individuals with greater liquid wealth, this is evidence that the liquidity effect is small or not present. If benefit extensions have a greater effect on the non-employment durations of individuals with less liquid wealth than they do on the non-employment durations of individuals with greater liquid wealth, then we can take this as evidence for the presence of a liquidity effect, and an indication of its size.

6. Data

6.1 Data sources

I use data from the 2008 panel of the Survey of Income and Program and Participation (SIPP), a national survey of 131,892 individuals. I do not use the survey weights. The survey collected information on participants’ employment status every week. For the first three years of the survey, information on participants’ assets was collected every year. Participants were surveyed every four months between September 2008 and December 2013. The length of this survey combined with its information on participants’ assets means that it is uniquely suited for the analysis in which I am interested. The data from this survey is preferable to that from surveys that follow participants over longer periods of time, such as the National Longitudinal Study of Youth (NLSY97) or the Panel Study of Income Dynamics (PSID) because it interviews panel members more frequently, and so is more detailed. One way, in fact, in which the SIPP data
could have been improved for the purposes of this project would have been more frequent collection of asset information.

I construct the .dta files using SIPP files and instructions provided by the National Bureau of Economic Research (NBER). I match this data with information from the United States Department of Labor on the week-by-week availability of unemployment benefit extensions between 2008 and 2013, UI weekly benefit levels, median annual wages, and week-by-week macro-economic indicators such as the unemployment rate and job openings rate between 2008 and 2013. I also merge in information from the United States Congressional Reporting Service on the duration of the authorization of each unemployment benefit extension.

6.2 Construction of the sub-sample

I begin by focusing my analysis on non-employed males between the ages of 18 and 65 who participated in the survey for at least 3 months. This narrows my survey population to 9,403 (19,062 including women). To be counted as non-employed in a month, an individual had to be without a job for at least part of the month and/or looking for work. The basic justification for focusing on this section of the non-employed population that this is the standard working population: neither so young as to be in school nor so old as to be retired, and expected to work rather than to stay home to take care of children or parents.

I further focus my analysis on the part of the sub-sample that meets four additional conditions. The first two conditions restrict the sample to individuals (a) on whom I have pre-non-employment asset data, and (b) on whom I have an employment history (job start date, wage) within the 6 months prior to non-employment. They also narrow my sample to 6,031

---

7i.e., I counted a person as non-employed in a month if RMESR = 4, 5, 6, 7, or 8 for that person-month using the SIPP variables.
(12,429 including women). These restrictions ensure that I have the necessary data on each individual in my sample.

Next, I only look at individuals who collected unemployment insurance benefits within two months of becoming unemployed. I find that 1,210 men collected benefits out of the 6,031 men who I counted as non-employed. This number is somewhat below the range of the estimates of unemployment insurance take-up found by Card and Blank (1991) – they find that 27% to 30% of the unemployed population receives unemployment insurance; my figure is 20%. The gap can be explained by the fact that the original 6,031 included 1,569 non-employed men who reported not searching for work. This part of the population received benefits at a much lower rate than other non-employed groups.

Finally, among those who received benefits within the first two months of one of their unemployment spells, I focus on those who received benefits during their first unemployment spell. This further reduces my target population to 728 spells (1,346 with the addition of women). This restriction ensures that everyone who receives benefits in the sample receives roughly the same replacement rate of benefits to prior wages – in many states, one’s regular benefits are capped at twenty-six times a fraction close to half of one’s highest wage in the previous quarter, so if an individual delays in filing and receiving a benefit claim, he may receive lower benefits. There is a worry that restricting the sample in this way could lead to bias because the decision to file for benefits or to take them up may be endogenous to the benefit level or length of benefits available or to one’s own preference for leisure. However, Chetty (2008) finds that the elasticity of take-up with respect to benefit levels is similar across liquidity-constrained

---

8 1,055 women collected benefits out of the 5232 women I counted as unemployed (20%).
and unconstrained groups. This suggests that endogeneity to availability of benefits at take-up

does not explain heterogeneous responses to benefit length.

### 6.3 Summary statistics

Summary statistics for the core sample are shown in the first column of Table 1. We see
that the median UI benefit recipient is a high school graduate with a pre-unemployment nominal
monthly wage of $2,877. I split the sample along the median of liquid wealth, so that everyone in
the lower half has between $0 and $295 in liquid wealth. All asset values are in 2009 dollars.

Turn to the first and second columns of Table 1 to see the comparison between the high-
liquid-wealth and low-liquid-wealth individuals. We see that they are roughly similar in terms of
personal characteristics and macro-economic conditions. The average person in each group has
graduated high school, but not college, and is married. At the time of his unemployment, he faces
a state unemployment rate of between 9.2 and 9.5 percent and a national job openings level
within 0.03 standard deviations of the overall average.

However, the groups are strikingly different in terms of their assets. Only 30\% of those
in the low liquid wealth group have a retirement account while 56\% of those in the high liquid
wealth group have one. The median individual in the low liquid wealth group has 66\% as much
unsecured debt as the median individual in the high liquid wealth group. While this suggests that
he has access to credit markets, the difference in credit usage does not match the difference in
access to savings; remarkably, the median individual in the low liquid wealth group has real
financial wealth of $360, while the median individual in the high liquid wealth group has real
financial wealth of $26,000. This difference suggests that financial wealth may act as a good
proxy for the liquidity constraint: those in the low liquid wealth group are not in a position to
smooth consumption after becoming unemployed, while those in the high liquid wealth group are. One worry is that there are large differences between the mean and medians for the asset variables – this suggests that there are some significant outliers in the data who may affect the results.

7. Results and Discussion

7.1 Results from the non-stratified sample

I first estimate equation (16) on my sub-sample to identify the effect of extending unemployment benefits by one month on the likelihood of an individual’s exit from non-employment. The results are specified in the first column of Table 2. In this specification, as in the others in this table, I assume that an individual’s expectation of the benefit months he will receive is the length of unemployment insurance benefits in statute at the time the individual first started receiving benefits. I use linear controls for liquid wealth, the indexed weekly benefit amount, the seam effect, whether the individual left employment in the winter, and individual covariates like marital status, education and age. I use a cubic control for the state unemployment rate and a logarithmic control for the number of job openings available nationally. I control for state and year fixed effects. Standard errors in this and all other specifications are clustered at the state level. The coefficient on the months of expected unemployment benefits is -.033; it is significant at the 10% level, but not at the 5% level. This coefficient implies that the hazard ratio (ratio of the likelihood of leaving non-employment) between an individual with X+1 months of additional expected benefits and an individual with X months of additional expected benefits is $e^{(-.033)}=.967$ – each month of additional expected benefits decreases the hazard ratio by 3.3 percent. This estimate is about 2 times as large as that found by Rothstein (2011) for similar
definitions of expectations of benefit months. One difference between my sub-sample and Rothstein’s is that Rothstein includes women, whereas I do not; however, the inclusion of women in the sub-sample only decreases the ratio of the estimates to 1.85. The remaining differences may be explained by Rothstein’s assumption of a logistic hazard, which might cause him to underestimate the strength of the relationship between UI benefit extensions and the hazard; his focus on unemployment hazard, rather than non-employment hazard; or the fact that he is calculating the hazard rate by looking at the effect of individuals’ expectations of benefit duration on the probability of their next-month exit from non/unemployment, while I am calculating the hazard rate by looking at the effect of individuals’ expectations of benefit duration on the total duration of their non/unemployment.

7.2 Results from the stratified sample.

I now estimate a series of regression to investigate the question of interest: how does the effect of extensions of unemployment benefit duration on the likelihood of exit into employment vary between individuals with liquidity constraints and individuals without? Looking at graphs of the difference in hazard rates between individuals facing longer potential benefits and shorter benefits (Figures 5a and 5b), we find visual evidence that individuals with lower liquidity are more affected by UI benefit extensions.

To investigate the heterogeneity of the effect of UI extensions, I estimate separate coefficients for the upper and lower half of the liquid wealth distribution, as I do in equation (17). The results are again in Table 2. The second column reports the results of the regression

---

9 Rothstein (2011) estimates that UI extensions decreased the exit rate from unemployment by 10.2% in his main specification (Table 3, Column 5). In this specification, he assumes that individuals do not expect EUC08 or EB benefits to be renewed. Under the assumption that recipients do expect EUC08 to be renewed, he finds that the estimated effect of UI extensions on the monthly exit rate “more than doubles.” My calculations of the total effect of UI extensions on the exit rate from unemployment can be found at the end of section 7.2.
done without controls. Each month of additional expected benefits decreases the likelihood that the individual will leave unemployment by 3.1% for those with less wealth and by 1.9% for those with greater wealth. The coefficient for those with low liquidity is estimated with enough precision to reject the null hypothesis that the coefficients are 0 at the 1% level; a chi-squared test also tells us that we can reject the null hypothesis that the coefficients are the same at the 1% level. The third and fourth columns report the results for the same regression with additional controls. The coefficient for the effect of UI benefit durations on those with low liquidity increases – to 3.6% with just individual characteristic controls and then to 4.1% with additional macro controls – as does the difference between the low-liquidity UI benefit duration coefficient and the high-liquidity UI benefit duration coefficient. We can continue to reject the null hypothesis that the coefficients are the same at the 1% level with individual characteristic controls, and at the 5% level with the additional controls for economic conditions. In the fifth column, I estimate the following equation to highlight the difference between the low liquidity and the high liquidity coefficients:

$$\ln \left( \frac{\lambda_{is}}{1 - \lambda_{is}} \right) = \gamma D_{is} Q_1 + \alpha_s + \theta D_{is} P_Z (Z_s; \delta) + \beta X_{is} + \alpha_s + \tau_t$$  \hspace{1cm} (17)$$

I get the same results found in the fourth column, but in a different form.

One worry about these results is that they imply that extensions have a very large effect on the job finding rate. The average member of the sample expected to have 21 months of UI benefits, including both regular and extended benefits. Regular benefits are 6 months long, so the average member of the sample expected to have 15 months of UI benefit extensions. This implies that the ratio between the job-finding hazard for a person who only expects regular benefits and the hazard for one who expected both regular benefits and the average UI benefit extension is: $e^{(-0.033 \times 15)} = 0.60$ for the full sample – the extension lowers the job finding rate by
40 percent. Using similar calculations, we find that extensions lowered the job finding rate by up to 46 percent for the low-liquidity sample. These numbers seem large. In addition, a visual comparison of the non-employment hazard rate we see in Figure 2 with the (now) historical non-employment hazard rates for UI populations found by Katz and Meyer (1990) and Chetty (2008), we see that hazard rates during the 2008 recession were more than 50 percent of hazard rates during non-recessions.

Given this concern, I perform a series of robustness checks in Section 8.

7.3 Estimation of the Liquidity Effects

Together, these results suggest the presence of a liquidity effect. We now turn to estimate the average liquidity effect of a benefit extension of a single month for the low-liquidity half of the population on the hazard rate out of non-employment using equation (14). I use the coefficients reported in column four of Table 2 as the elasticities of search intensity with respect to benefit duration for the high and low liquidity groups. I use the median weekly benefit amounts reported in Table 2 to find the monthly benefit levels available to high and low liquidity groups. Finally, I use the observed survival rates through \( B = 6 \) months for the high and low as my basic hazard rate:

\[
\frac{\partial s_{NL}}{\partial w} \times \frac{1}{w_L} = \frac{\partial s_{NL}}{\partial \tau} \times \frac{1}{w_{NL}} \times \frac{\prod_{b=1}^{B} (1 - s_{NL}^b)}{\prod_{b=1}^{B-1} (1 - s_{NL}^b)} = \left( \frac{\partial s_{NL}}{\partial \lambda} - 0 \right) - \left( - \frac{\partial s_{NL}}{\partial \lambda} + \frac{\partial s_{NL}}{\partial w} \right) \\
= \frac{1}{s_{000}} \times (\exp(-.041) - 1) - \frac{1}{s_{000}} \times (\exp(-.026) - 1) \times \frac{.1241}{.019} \\
\frac{\partial s_{NL}}{\partial \lambda} = -0.000003305
\]

If we multiply this number by the median monthly benefit amount for the low-liquidity half of the population, and divide that number by the elasticity of search intensity with respect to benefit
duration, we can find the degree to which the effect of UI benefit durations on the job-finding rate is driven by liquidity effect for the lower-liquidity half of the population:

\[
\frac{\partial s_{u,l}}{\partial A_0} \times b_c \cdot \frac{\partial s_{u,l}}{\partial B} = \frac{-3.305 \times 10^{-6} \times 3600}{\exp(-0.41) - 1} = -0.296
\]

This calculation implies that 29.6 percent of the effect of UI benefit durations is due to the liquidity effect and 70.4 percent is due to the substitution effect for the lower-liquidity half of the population. This estimate is much lower than the one found by Chetty (2008), who finds that 60 percent of the total UI benefit level effect is due to the liquidity effect; it is closer to – though still lower than – Landais (2015)’s estimate that 46 percent of the total UI benefit/duration effect is due to the liquidity effect.

8. Robustness Checks

8.1 Definition Changes

In the first set of robustness checks, I check the impacts of three definitions. In all of these specifications, I use the full controls used in the last two columns of Table 2. First, I consider the impact of my definition of liquidity. In the first column of Table 3, I replace the main specification’s definition of liquidity with household liquidity: the sum of the household’s interest-earning assets in banks, interest-earning assets at other institutions, and stocks and bonds. Note that while there is a statistically significant difference between the liquidity x benefit duration interaction coefficients, the gap is narrower. This has the effect of decreasing the estimated liquidity effect to 19.3 percent for low-liquidity individuals. In the second column of Table 3, I replace the main specification’s definition of liquidity with the definition of financial wealth used in Chetty (2008): total household wealth minus unsecured debt minus housing stock equity minus vehicle equity. Again, the coefficient for high liquidity individuals is more negative.
than in the main specification, but it remains statistically different than the coefficient for low liquidity individuals at the 5 percent level.

Next, I investigate the robustness of the results to changes in assumptions about when individuals form their expectations of benefit duration. The third column of Table 3 considers the coefficients that result if the expected length of benefits is defined as the maximum number of months of benefits in statute throughout the time of an individual’s unemployment. The coefficients are more than three times as large as the original coefficients and significant at the 1 percent level. The fourth column of Table 3 considers the change in the coefficients that occurs if the expected length of benefits is defined as the months of benefits in statute when the individual began unemployment. Given that the sample is such that individuals began receiving benefits within two months of becoming unemployed, it is interesting that the coefficients for the interactions using the redefined expectation become so insignificant. This provides some evidence that individuals do not take UI benefits into account before they start to receive them, perhaps out of uncertainty that they will receive the benefits.

8.2 Controlling for Spell Duration

Next, I investigate whether the effect of UI benefit extensions differs depending on the length of one’s spell. The investigation is partially motivated by the fact that the hazard rate varies over the duration of non-employment (see Figure 4); the worry is that the coefficients on the UI benefit duration interactions are absorbing this variation, and so not accurately reflecting the true effect of the UI benefit duration on the hazard rate at the beginning of non-employment. An alternative explanation for why the coefficients on the UI benefit duration interactions would not reflect the true effect of the UI benefit duration on the hazard rate at the beginning of
unemployment is that the effect of UI benefit extensions might change over the course of non-employment and the coefficients may reflect the average of the effects, rather than the particular effect in which we’re interested. Identifying and controlling for the duration of unemployment, however, is tricky because given the structure of the SIPP dataset, it is perfectly correlated with the dependent variable.

In the first column of Table 4, I control for the duration of unemployment by adding another interaction term to the specification: liquidity x expected benefit duration x spell duration. I interact the duration variable in order to avoid the identification problem discussed above. We see that the coefficients on the expected benefit duration x liquidity dummy interactions separate further, so that the difference between the effects of UI benefit duration for high- and low-liquidity individuals is significant at the 1 percent level. This difference is sufficient to increase the liquidity to total effect ratio to 38.94 percent.

In the second column of Table 4, I interact the liquidity x expected benefit duration dummy with dummies for being more and less than 6 months into unemployment. This test is inspired by Rothstein’s finding that the job finding rates of individuals who are more than 6 months into unemployment – that is, at a point when they would start receiving extended benefits – are far more affected by extended benefits than are people less than 6 weeks into their spells. The results are striking.

The job finding rates of individuals who are unemployed for more than 26 weeks do decrease for every month of UI benefits that they expected to receive when they first started benefits. The magnitude of the effects is slightly greater than that found in earlier specifications: -.044 for low liquidity individuals and -.027 for high liquidity individuals. The magnitude of the effects is significant at the 1 percent level, and the coefficients for high and low liquidity
individuals are significantly different at the 6 percent level. However, the job finding rates of individuals who are unemployed for fewer than 26 weeks actually increase for every month of UI benefits that they expected to receive when they first started benefits – again, this result is significant at the 1 percent level. At this point, however, there is no significant difference between the coefficients for the interactions with the liquidity dummies. That the negative effect is concentrated after the 26\textsuperscript{th} week of unemployment confirms Rothstein’s (2011) findings. Rothstein (2011), however, does not find any significant effect in the first 6 months, much less a large significant effect. The best explanation for the difference is that, because of the interview structure of the CPS, Rothstein can identify and control for the duration of individuals’ unemployment prior to their random entry into his dataset in a way that’s not possible in the SIPP data.

8.3 Sample Changes

In Table 5, I investigate the robustness of the results to sample changes. I do not estimate liquidity to total effect ratios for these estimates because the change in the sample means that the median benefit amount and the average survival rates have also changed. In the first column, I estimate equation (17) with a smaller sub-sample: males above the age of 50. The idea here is that since men above the age of 50 are closer to retirement, the extension of benefits could have two very different effects for them. One the one hand, since they are looking forward to retiring, they may already have a relatively low labor force attachment, and so UI benefit extensions may be the extra push they need to not return to work. On the other hand, they may want to save as much as possible before retirement, and so UI benefit extensions may not prevent them from working more. Although the estimates of the effect of UI benefit duration extensions are
imprecise, the second effect appears to dominate: the point estimates are closer to zero than they are in other specifications. In addition, there is no significant difference between high and low liquidity individuals, which may be caused by the smaller sample size generating higher standard errors.

In the second column, I estimate equation (17) for a sample that includes women who otherwise meet the conditions specified above. Looking at the second column of Table 5, I find that each month of additional expected benefits decreases the likelihood that the individual will leave unemployment by 3.3 percent for low liquidity individuals and by 2.8 percent for high liquidity individuals. These coefficients are significant at the 5 percent level, though they are not significantly different from each other. Interestingly, the coefficient for low liquidity individuals is lower than that in the main sample, though the coefficient for high liquidity individuals on the low end of the range in the main sample. The fact there is a less of a difference in the effects of UI benefit extensions for low- and high-liquidity women could be explained by the fact that women are more dependent on the income and assets of their male partners than men are on the income and assets of their female partners.

If this were the case, then a measure of household liquidity measure would more accurately express women’s true liquidity level than a measure of individual liquidity. Indeed we see some evidence that the household liquidity measure is better in this case: in the third column of Table 4, I estimate equation (17) for a sample that includes women and uses household liquidity as the definition of liquidity. Whereas the use of the household liquidity measure brings the coefficients on the high- and low-liquidity interaction terms closer together for the male-only sample (Table 3, Column 1), it increases the distance between the coefficients on the interaction
terms for the combination female and male sample. In fact, the coefficients become significantly different at the 5 percent level.

8.4 Effects on Wages

Finally, there is the question of whether the liquidity effect of UI benefit extensions has positive effects beyond increasing the consumption utility of recipients. One potential story might run like this: In the absence of UI benefits, the unemployed who are without access to credit or savings are incentivized to rush to employment. Consequently, the jobs they take may be poorly matched to their abilities. To the degree UI benefit extensions decrease the incentive to rush – to the degree that a liquidity effect exists – they increase productivity by improving job matches (see Acemoglu and Shimer (2000) for one version of this story).

To test this theory, Table 6, I estimate the effect of UI benefit extensions on wages after re-employment. Following Addison and Blackburn (2000), I estimate the following equation:

\[
\ln \left( \frac{W_{\text{post}}}{W_{\text{pre}}} \right) = \alpha + \eta D_i + \gamma \ln (W_{\text{pre}}) + \theta b_i + \beta X_i + v
\]

where \( W \) is wage, \( \alpha \) is the constant term, \( D_i \) is the expected length of benefits, \( b_i \) is the indexed weekly maximum benefit amount, \( X_i \) is a vector of personal and macro-economic controls, and \( v \) is the error term. Consistent with other studies, I find that the UI benefit duration extensions do not have any effect on post-unemployment wages (Van Ours and Vodopivec 2008; Card, Chetty, and Weber 2007; Caliendo, Tatsiramos, and Uhlendorff 2013).

9. Conclusion

In this paper, I start by using data from the Survey of Income and Program Participation to estimate the effect of UI benefit extensions on the non-employment hazard rate during the
Great Recession. With this new data, I find effects similar in direction to previous estimates of the effect of UI benefit extensions on the UI non-employment hazard rate, and confirm that the effect is concentrated among individuals who are not employed for more than 6 months.

I then exploit the asset data provided by SIPP to look for evidence of heterogeneity in the effect of benefit duration extensions across groups with different levels of liquidity. I do find evidence of the presence of a substantial liquidity effect. In my main specification, I estimate that 30 percent of the effect of UI benefit duration extensions is due to the liquidity effect for low-liquidity individuals. The presence of a liquidity effect is robust to different definitions of liquidity, the inclusion of women in the sample, and some different definitions of expected benefit duration. The ratio of liquidity to total effect, however, varies with the specification from a low of 20 percent to a high of 40 percent. There is no evidence that the liquidity effect translates into increased wages for those who regain employment.

While it is reassuring to find substantial evidence of a liquidity effect, the variability of the effect is surprising. In addition, my estimate of the ratio of the liquidity to total effect is smaller than previous ones. The discrepancy may be due to the fact that my identification strategy assumes that individuals in the upper half of the liquid wealth distribution do not experience a liquidity effect, an assumption that may have been violated given the strength of the 2008 recession.
10. Appendix

A. Figures and Tables

Figure 1: The path of UI benefits for an individual who resides in Wyoming\(^2\) and who became unemployed on July 1, 2011.

\(^1\) If a tier is indicated as available, this means that it is available for all states. Additional tiers may have been available for specific states depending on their economic conditions.

\(^2\) Benefits from the Extended Benefit (EB) program were not available in Wyoming at this time.
Figure 2

Figure 3: Effect of a 1 period increase in benefits, $b_1 > b_0$, on search intensity $s_t$ for a liquidity-constrained individual

Source: Author’s approximation, where

$$e'(s_t) = 0.5 \times e^{s/2} - 0.5$$

$$V_t(s_t) - U_t(s_t) = 5 - 5s + e^{s/2}$$

The optimal search intensity occurs at the points where the marginal cost of search effort $e'(s_t)$ is equivalent to the difference between the value of being employed and the value of being unemployed $V_t(s_t) - U_t(s_t)$. 
Table 1
Summary Statistics by Net Financial Wealth for SIPP sample

<table>
<thead>
<tr>
<th>Net Liquid Wealth Group</th>
<th>Pooled</th>
<th>Lower (&lt; $295)</th>
<th>Upper (&gt; $295)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sample Size</td>
<td>728</td>
<td>361</td>
<td>367</td>
</tr>
<tr>
<td>Years of Education</td>
<td>13.022</td>
<td>12.493</td>
<td>13.542</td>
</tr>
<tr>
<td>Percent Married</td>
<td>58%</td>
<td>52%</td>
<td>63%</td>
</tr>
<tr>
<td>Age</td>
<td>42.5</td>
<td>40.8</td>
<td>44.3</td>
</tr>
<tr>
<td>Mean Nominal Monthly Wage, pre-unemployment</td>
<td>$3,588.133</td>
<td>$2,954.94</td>
<td>$4,210.96</td>
</tr>
<tr>
<td>Median Nominal Monthly Wage, pre-unemployment</td>
<td>$2,877.80</td>
<td>$2,282.55</td>
<td>$3,500.00</td>
</tr>
<tr>
<td>Mean Non-employment Duration (months)</td>
<td>10.13</td>
<td>10.79</td>
<td>9.487</td>
</tr>
<tr>
<td>Median Non-employment Duration (months)</td>
<td>6</td>
<td>7.0</td>
<td>6.0</td>
</tr>
<tr>
<td>Max weekly UI benefit amount, indexed by median wage</td>
<td>0.63</td>
<td>0.621</td>
<td>0.641</td>
</tr>
<tr>
<td>Mean Weekly UI benefit amount</td>
<td>$1040.40</td>
<td>$974.01</td>
<td>$1105.70</td>
</tr>
<tr>
<td>Median Weekly UI benefit amount</td>
<td>$953.5</td>
<td>$900</td>
<td>$995</td>
</tr>
<tr>
<td>UI benefit months available (months)</td>
<td>21.12</td>
<td>21.22</td>
<td>21.02</td>
</tr>
<tr>
<td>State unemployment rate at time of unemployment</td>
<td>9.378</td>
<td>9.507</td>
<td>9.251</td>
</tr>
<tr>
<td>National job openings (thousands) at time of unemployment</td>
<td>3039.099</td>
<td>3032.548</td>
<td>3045.542</td>
</tr>
<tr>
<td>Percent with retirement account</td>
<td>43%</td>
<td>30%</td>
<td>56%</td>
</tr>
<tr>
<td>Percent renters</td>
<td>34%</td>
<td>40%</td>
<td>27%</td>
</tr>
<tr>
<td>Mean real household financial wealth</td>
<td>$66,020.66</td>
<td>$36,161.14</td>
<td>$95,392.02</td>
</tr>
</tbody>
</table>

1 Data source is the SIPP 2008 panel unless otherwise noted. Table entries are means unless otherwise noted. Asset data was collected three times during the panel; for each individual, I use the most recent asset data collected prior to their unemployment. Asset data is in 2009 dollars.

2 Net financial wealth is defined as liquid wealth. See text.

3 This calculated as the maximum weekly benefit available in the state divided by the median weekly wage in the state. Information is from the Employment & Training Administration at the Department of Labor and from the Bureau of Labor Statistics.

4 Information from the Employment & Training Administration at the Department of Labor

5 Information from the Job Openings and Labor Turnover Survey at the Bureau of Labor Statistics
### Median Real Household Financial Wealth

<table>
<thead>
<tr>
<th>Financial Wealth</th>
<th>Median</th>
<th>Mean</th>
<th>Unsecured Debt</th>
</tr>
</thead>
<tbody>
<tr>
<td>$8,852.96</td>
<td>$14,396.46</td>
<td>$11,989.04</td>
<td>$16,764.53</td>
</tr>
</tbody>
</table>

\[6\] Defined as the sum of a household’s interest earning assets held at banks and other institutions, equity in stock and mutual fund shares, IRA and KEOGH accounts and equity in 401k and Thrift savings accounts.

Figure 4

Hazard Rate Over Time (Smoothed)

Data Source: 2008 Survey of Income and Program Participation
<table>
<thead>
<tr>
<th></th>
<th>(1) Pooled</th>
<th>(2) Stratified, no controls</th>
<th>(3) Stratified, individual controls</th>
<th>(4) Stratified, individual and macro controls</th>
<th>(5) Eq (18)</th>
</tr>
</thead>
<tbody>
<tr>
<td># benefit months expected</td>
<td>-0.033</td>
<td>-0.031</td>
<td>-0.036</td>
<td>-0.041</td>
<td>-0.026</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.015)**</td>
<td>(0.018)*</td>
<td>(0.019)*</td>
<td>(0.017)</td>
</tr>
<tr>
<td># benefit months expected* less liquid wealth</td>
<td>-0.019</td>
<td>-0.021</td>
<td>-0.026</td>
<td>-0.015</td>
<td>-0.015</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.015)</td>
<td>(0.017)</td>
<td>(0.006)*</td>
<td>(0.006)*</td>
</tr>
<tr>
<td># benefit months expected* greater net financial wealth</td>
<td>-0.019</td>
<td>-0.021</td>
<td>-0.026</td>
<td>-0.019</td>
<td>-0.019</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.015)</td>
<td>(0.017)</td>
<td>(0.007)**</td>
<td>(0.007)**</td>
</tr>
<tr>
<td>Liquid wealth</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Indexed weekly benefit amount</td>
<td>0.095</td>
<td>1.321</td>
<td>-1.397</td>
<td>-1.397</td>
<td>-1.397</td>
</tr>
<tr>
<td></td>
<td>(2.833)</td>
<td>(1.487)</td>
<td>(2.708)</td>
<td>(2.708)</td>
<td>(2.708)</td>
</tr>
<tr>
<td>Pre-unemployment monthly wage spline</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>State unemployment rate</td>
<td>cubic</td>
<td>cubic</td>
<td>cubic</td>
<td>cubic</td>
<td>cubic</td>
</tr>
<tr>
<td>National job openings (thousands)</td>
<td>logarithmic</td>
<td>logarithmic</td>
<td>logarithmic</td>
<td>logarithmic</td>
<td>logarithmic</td>
</tr>
<tr>
<td>Age</td>
<td>-0.029</td>
<td>-0.030</td>
<td>-0.029</td>
<td>-0.029</td>
<td>-0.029</td>
</tr>
<tr>
<td></td>
<td>(0.004)**</td>
<td>(0.004)**</td>
<td>(0.005)**</td>
<td>(0.005)**</td>
<td>(0.005)**</td>
</tr>
<tr>
<td>Married</td>
<td>0.268</td>
<td>0.276</td>
<td>0.257</td>
<td>0.257</td>
<td>0.257</td>
</tr>
<tr>
<td></td>
<td>(0.127)*</td>
<td>(0.124)*</td>
<td>(0.128)*</td>
<td>(0.128)*</td>
<td>(0.128)*</td>
</tr>
<tr>
<td>Years of Education</td>
<td>-0.037</td>
<td>-0.044</td>
<td>-0.047</td>
<td>-0.047</td>
<td>-0.047</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.028)</td>
<td>(0.027)</td>
<td>(0.027)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Seam adjustment</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Seasonal adjustment</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

1 Coefficients reported are elasticities of the hazard rate with respect to the number of benefit months.
2 Expectation of benefit months is assumed to be the length of unemployment insurance benefits in statute at the time the individual first started receiving benefits.
Year fixed effects | X | X | X | X | X
State fixed effects | X | X | X | X | X
Interaction terms equality p-val | 0.0017 | .0084 | .0126

| N | 727 | 728 | 727 | 727 | 727 |

* p<0.05; ** p<0.01; standard errors shown; standard errors clustered at the state level

Figure 5.a.

Survivor functions for core sample

Figure 5.b.

Survivor functions for low liquidity individuals

Length of non-employment (months)

Below median expected benefit duration
Above median expected benefit duration

Figure 5.c.

Table 3. Robustness Checks I – Definition Changes
Dependent variable: Non-employment Exit Hazard
See footnotes for description of definitions used.

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td># benefit months expected*</td>
<td>1.040</td>
<td>0.041</td>
<td>0.125</td>
<td>0.023</td>
</tr>
<tr>
<td>less liquid wealth</td>
<td>(0.018)*</td>
<td>(0.018)*</td>
<td>(0.037)**</td>
<td>(0.019)</td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>0.029</td>
<td>0.029</td>
<td>0.112</td>
<td>0.009</td>
</tr>
<tr>
<td>more liquid wealth</td>
<td>(0.018)</td>
<td>(0.017)</td>
<td>(0.036)**</td>
<td>(0.018)</td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>0.029</td>
<td>0.029</td>
<td>0.112</td>
<td>0.009</td>
</tr>
<tr>
<td>less liquid wealth* &gt;26weeks</td>
<td>(0.018)</td>
<td>(0.017)</td>
<td>(0.036)**</td>
<td>(0.018)</td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>0.029</td>
<td>0.029</td>
<td>0.112</td>
<td>0.009</td>
</tr>
<tr>
<td>more liquid wealth* &gt;26weeks</td>
<td>(0.018)</td>
<td>(0.017)</td>
<td>(0.036)**</td>
<td>(0.018)</td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>0.029</td>
<td>0.029</td>
<td>0.112</td>
<td>0.009</td>
</tr>
<tr>
<td>less liquid wealth* &lt;26weeks</td>
<td>(0.018)</td>
<td>(0.017)</td>
<td>(0.036)**</td>
<td>(0.018)</td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>0.029</td>
<td>0.029</td>
<td>0.112</td>
<td>0.009</td>
</tr>
<tr>
<td>More liquid wealth* &lt;26weeks</td>
<td>(0.018)</td>
<td>(0.017)</td>
<td>(0.036)**</td>
<td>(0.018)</td>
</tr>
</tbody>
</table>

Table 2, Column 4 Controls
X X X X

Interaction terms equality p-val
0.0432 0.0105 0.0228 0.0154

N 727 727 727 727


1 Liquidity variable is household liquid wealth. See text for details. Expected benefit duration variable is months of benefits in statute when the agent started benefits.

2 Liquidity variable is household financial wealth. See text for details. Expected benefit duration variable is months of benefits in statute when the agent started benefits.

3 Liquidity variable is individual liquid wealth (original definition). Expected benefit duration variable is the maximum number of months of benefits in statute during the agent’s unemployment.

4 Liquidity variable is individual liquid wealth (original definition). Expected benefit duration variable is benefit months in statute when the agent began unemployment.
Table 4. Robustness Checks II – Controlling for Spell Duration  
Dependent variable: Non-employment Exit Hazard

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
</tr>
</thead>
<tbody>
<tr>
<td># benefit months expected*</td>
<td>-0.043</td>
<td></td>
</tr>
<tr>
<td>less liquid wealth</td>
<td>(0.019)**</td>
<td></td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>-0.024</td>
<td></td>
</tr>
<tr>
<td>more liquid wealth</td>
<td>(0.017)</td>
<td></td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>-0.044</td>
<td></td>
</tr>
<tr>
<td>less liquid wealth* &gt;26weeks</td>
<td>(0.008)**</td>
<td></td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>-0.027</td>
<td></td>
</tr>
<tr>
<td>more liquid wealth* &gt;26weeks</td>
<td>(0.008)**</td>
<td></td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>0.100</td>
<td></td>
</tr>
<tr>
<td>less liquid wealth* &lt;26weeks</td>
<td>(0.008)**</td>
<td></td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>0.095</td>
<td></td>
</tr>
<tr>
<td>More liquid wealth* &lt;26weeks</td>
<td>(0.010)**</td>
<td></td>
</tr>
</tbody>
</table>

Liquid wealth

Liquid wealth * # benefit months expected * spell duration

Table 2, Column 4 Controls   X       X
Interaction terms equality p-val 0.0037 0.058, 0.601

N 727 727

Table 5. Robustness Checks II – Sample Changes
Dependent variable: Non-employment Exit Hazard
See footnotes for description of sample changes

<table>
<thead>
<tr>
<th></th>
<th>1(^1)</th>
<th>2(^2)</th>
<th>3(^3)</th>
</tr>
</thead>
<tbody>
<tr>
<td># benefit months expected* less liquid wealth</td>
<td>-0.016</td>
<td>-0.033</td>
<td>-0.035</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.015)*</td>
<td>(0.015)*</td>
</tr>
<tr>
<td># benefit months expected* more liquid wealth</td>
<td>-0.001</td>
<td>-0.029</td>
<td>-0.026</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.014)*</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Table 2, Column 4 Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Interaction terms equality p-value</td>
<td>0.2202</td>
<td>0.3888</td>
<td>0.0500</td>
</tr>
<tr>
<td>N</td>
<td>222</td>
<td>1,345</td>
<td>1,345</td>
</tr>
</tbody>
</table>


---

1 Sample only includes men above the age of 50. Definitions of expected benefit duration and liquidity are the same as in the main sample.

2 Sample includes both men and women. Definitions of expected benefit duration and liquidity are the same as in the main sample.

3 Sample includes both men and women. Definition of liquidity is household liquidity. Definition of expected benefit duration is the same as in the main sample.
Table 6. Robustness Checks III – Effects on Wage
Dependent variable: Change in Log Wage (estimated with OLS)

<table>
<thead>
<tr>
<th></th>
<th>( \ln \left( \frac{W_{\text{TOTAL},i}}{W_{\text{PRE},i}} \right) )</th>
<th>( \ln \left( \frac{W_{\text{TOTAL},i}}{W_{\text{PRE},i}} \right) )</th>
</tr>
</thead>
<tbody>
<tr>
<td># benefit months expected(^1)</td>
<td>0.008 (0.011)</td>
<td>0.008 (0.011)</td>
</tr>
<tr>
<td># benefit months expected*</td>
<td>0.007 (0.011)</td>
<td>0.007 (0.011)</td>
</tr>
<tr>
<td>less liquid wealth</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>greater net financial wealth</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Liquid wealth</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Indexed weekly benefit amount</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Pre-unemployment</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>monthly log wage spline</td>
<td></td>
<td></td>
</tr>
<tr>
<td>State unemployment rate</td>
<td>cubic</td>
<td>cubic</td>
</tr>
<tr>
<td>National job openings (thousands)</td>
<td>logarithmic</td>
<td>logarithmic</td>
</tr>
<tr>
<td>Age</td>
<td>-0.002 (0.003)</td>
<td>-0.002 (0.003)</td>
</tr>
<tr>
<td>Married</td>
<td>0.023 (0.069)</td>
<td>0.024 (0.069)</td>
</tr>
<tr>
<td>Years of Education</td>
<td>0.022 (0.017)</td>
<td>0.023 (0.017)</td>
</tr>
<tr>
<td>Seam and seasonal adjustment</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>State and Year fixed effects</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Interaction terms equality p-val</td>
<td>N/A</td>
<td>0.7887</td>
</tr>
<tr>
<td>R(^2)</td>
<td>0.31</td>
<td>0.31</td>
</tr>
<tr>
<td>N</td>
<td>453</td>
<td>453</td>
</tr>
</tbody>
</table>

\(^1\) Expectation of benefit months is assumed to be the length of unemployment insurance benefits in statute at the time the individual first started receiving benefits.
References


