Why do Unemployment Benefits Raise Unemployment Durations? The Role of Borrowing Constraints and Income Effects

PRELIMINARY

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Abstract

It is well known that unemployment insurance (UI) benefits raise unemployment durations. This result has traditionally been interpreted as a substitution effect caused by a reduction in the price of leisure relative to consumption, generating a deadweight burden. This paper questions the validity of this interpretation by showing that UI benefits can also affect durations through a non-distortionary income effect for agents who face borrowing constraints. UI benefits have a pure substitution effect only for those who have sufficient resources to smooth consumption while unemployed. The empirical relevance of borrowing constraints and income effects is evaluated in two ways. First, I classify households into groups that are likely to be constrained and unconstrained based on their asset holdings, mortgage payments, and spouse's labor force status. Non-parametric and semi-parametric tests reveal that unemployed benefits raise durations much more sharply in the constrained groups. Second, I find that lump-sum severance payments granted at the time of job loss significantly increase durations. These results suggest that transitory benefits affect search behavior primarily through an income effect, challenging the prevailing view that social safety nets create large efficiency costs.

Keywords: liquidity constraints, life cycle model, consumption smoothing, insurance

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1 Introduction

One of the classic empirical results in public finance is that social insurance programs such as unemployment insurance (UI) cause significant reductions in labor supply. Moffitt (1985), Meyer (1990), and others find that a 10% increase in unemployment benefits raises average unemployment durations by 4-8% in the U.S. Other studies have documented a surge in the unemployment exit rate around the time of UI benefit exhaustion (e.g., Moffitt 1985, Katz and Meyer, 1990) and higher reported reservation wages when UI benefits are high (Feldstein and Poterba, 1984).[1] The traditional interpretation of these findings is that UI induces substitution toward leisure by distorting the relative price of leisure and consumption, generating a moral hazard efficiency cost. In their recent handbook chapter on social insurance, Krueger and Meyer (2002) observe that behavioral responses to UI and other social insurance programs are probably so large because they "lead to short-run variation in wages with mostly a substitution effect." This is also the standard textbook interpretation of the evidence: Gruber (2005) remarks that "UI has a significant moral hazard cost in terms of subsidizing unproductive leisure." The logic underlying these interpretations is presumably that transitory UI benefits are a minor part of lifetime wealth, and UI therefore generates only substitution effects (with negligible income effects).

An important assumption in this logic is that households are able to access lifetime wealth while unemployed, which requires that they do not face borrowing constraints. However, recent studies of consumption smoothing have provided strong evidence that many unemployed agents do face binding borrowing constraints. Gruber (1997) finds that increases in UI benefits reduce the consumption drop during unemployment, indicating that agents are unable to smooth consumption perfectly as they would if they faced no borrowing constraints. Browning and Crossley (2001) and Bloemen and Stancanelli (2003) provide more direct evidence for the borrowing constraint mechanism by showing that the UI-consumption

[1] Atkinson and Micklewright (1990) and Krueger and Meyer (2002) give excellent reviews of this literature.

link identified by Gruber holds only for the subset of individuals who report holding few assets at the time of job loss. Nearly half of job losers in the United States report zero liquid wealth at the time of job loss, suggesting that borrowing constraints are potentially relevant for a large fraction of the unemployed.

In this paper, I analyze a model where unemployed agents face borrowing constraints, and show that the effect of UI benefits on durations may largely be due to a non-distortionary *income* effect in this environment. To see the intuition, first note that the wealth effects of UI are indeed trivial for agents who are able to smooth consumption during unemployment spells, since UI benefits do not change permanent income much. But when agents are borrowing constrained, their behavior while unemployed is determined by cash on hand rather than lifetime resources. UI benefits have a 1-1 effect on relaxing the constraint for such individuals, raising their level of consumption while unemployed and potentially making their optimal reservation wage higher or search effort lower. Consequently, durations can rise simply because agents have more cash on hand, independent of changes in the relative price of consumption and leisure. Importantly, the "cash-on-hand" or income effect is non-distortionary, in the sense that it does not reflect a moral-hazard problem and therefore does not generate a deadweight burden. A benevolent social planner would *not* choose to undo behavioral responses to lump-sum income grants. Hence, once one admits the possibility of borrowing constraints, the efficiency costs of social insurance depend on the relative magnitudes of income and substitution effects caused by UI benefits.

I use two independent methods to evaluate the empirical relevance of borrowing constraints and income effects. The first explores the role of liquidity constraints in the UI-duration link documented in the existing literature, while the second provides more direct evidence of income effects using variation in lump-sum severance payments. I examine the effects of borrowing constraints using a strategy similar to Zeldes' (1989) method of studying consumption patterns. For "unconstrained" individuals, for whom the borrowing constraint does not bind, UI benefits necessarily act only through a substitution effect. In contrast, for "constrained" individuals, whose consumption while unemployed is determined by UI benefits, the UI benefit effect is the sum of the income and substitution effects. We can therefore obtain an estimate of the pure substitution effect of UI for the unconstrained group by simply estimating the UI benefit elasticity for that group using cross-state and time differences in benefit levels. Provided that the substitution elasticity is the same in the constrained and unconstrained groups – an issue I return to below – we can then estimate the income elasticity from the difference in the UI benefit effect between the constrained and unconstrained groups.

Of course, we cannot directly observe which households are constrained in the data. To overcome this latent variable problem, I use several intuitive proxies that have also been shown to predict constraints in studies of consumption (e.g. Browning and Crossley 2001). The first is simply a household's liquid asset holdings, net of unsecured debt. Households with higher levels of assets relative to their permanent income level at the time of unemployment are less likely to be constrained than those who have a smaller buffer stock. The second proxy is whether the individual has a working spouse. Dual-earner households are more likely to have the resources necessary to smooth the most important components of consumption when one of them loses a job, making them less likely to be constrained. The third proxy is whether the individual has to make a home mortgage payment, which is a fixed commitment that effectively reduces liquid wealth as well.

To implement this strategy, I first plot simple Kaplan-Meier survival curves and conduct a series of non-parametric tests for differences in the unemployment exit hazard across high-UI and low-UI (state,year) pairs in each of the constrained and unconstrained groups. These tests uniformly find little correlation between UI benefits and hazard rates among the unconstrained groups (those with more than \$1000 in liquid wealth, those without a mortgage, and those with a working spouse). However, there is a very strong link, both economically and statistically, between the level of benefits and unemployment exit rates in all the constrained groups.

To evaluate the robustness of these results to controls, I estimate a set of semiparametric Cox hazard models. The point estimates indicate that a 10% increase in benefits raises unemployment durations by around 6-8% in the constrained groups, but have little or no effect in the unconstrained groups. Moreover, the effect of UI on durations becomes monotonically larger as we examine groups of households that are progressively more likely to be constrained (e.g., as liquid wealth holdings fall). These results are fully robust to the inclusion of rich controls and other specification checks such as the permission of unobserved heterogeneity in baseline hazards. Moreover, there is no association between UI benefits and durations in the "control group" of UI-ineligible and non-claiming individuals, supporting the causal interpretation of the results.

In sum, there is strong evidence that UI induces no substitution effect for unconstrained individuals and that the benefit-duration link documented in prior studies is driven by a subset of the population that is constrained. This evidence suggests that the UI affects duration primarily through an income effect, assuming that the substitution effect is similar in the constrained and unconstrained groups. To establish the existence of an income effect more directly without relying on this assumption, I implement a second set of tests using variation in severance payments coupled with administrative data on unemployment durations. Non-parametric tests show that individuals who received a lump-sum severance payment (worth about $1000 on average) at the time of job loss have significantly lower unemployment exit hazards. This conclusion is robust to the inclusion of a large set of controls for demographics, income, job tenure, and industry and occupation in a Cox regression. Insofar as the remaining variation in severance pay is not correlated with an omitted variable that influences durations, these results provide direct evidence for the presence of income effects. The point estimates imply that nearly two-thirds of the duration elasticity of UI benefits may be due to an income effect.

The results of this paper have several positive and normative implications. First, they provide new evidence that borrowing constraints matter for the behavior of many households. Second, the evidence challenges the prevailing view that social safety nets create large deadweight costs, at least on the unemployment duration dimension.[2] Finally, the results shed some light on optimal UI policies. Each of these points are discussed in detail in the conclusion.

The remainder of the paper proceeds as follows. The next section demonstrates the connection between borrowing constraints and income effects of UI formally in a lifecycle model. Section 3 describes the estimation strategy, data, and results for the borrowing constraint tests. Section 4 examines the link between severance payments and durations. Section 5 discusses the implications of the results.

2 Theory

I formalize the connection between borrowing constraints and income effects using a stylized continuous time, finite horizon lifecycle model of labor supply. The model is similar to that used by Zeldes (1989) to analyze the effect of borrowing constraints on consumption dynamics. The only differences are that I (1) ignore portfolio choice and (2) introduce endogenous labor supply to study unemployment durations.

I model the borrowing constraint by assuming that agents must maintain a liquid wealth balance above some threshold $-B$.[3] Let c_{is} = consumption by household i in period s, Θ_{is} = the household's tastes in period s, $\widetilde{w}_{is}$ = wage in period s. Normalize the interest and discount rates at zero. Assume that the agent lives for T years.

Suppose the agent loses his job at time t. I abstract from the dynamics of the job search process, and assume instead that agents can control their unemployment duration, d, deterministically by varying search effort appropriately. Search costs, the leisure value

[2] Of course, it does not follow that direct *government* provision of these safety nets is optimal; other methods of insurance provision may improve welfare further.

[3] As Zeldes (1990) notes, this is not the only plausible model of borrowing constraints, but other more general formulations deliver similar results.

of unemployment, and the benefits of additional search via improved job matches are all incorporated in a reduced-form manner through a concave, increasing function $\varphi(d)$.

The agent is eligible for unemployment benefits of b while he is not working. The government finances the benefits by taxing the worker at a rate τ while employed, so his net-of-tax wage is $w_{it} = \widetilde{w}_{it}(1-\tau)$. To focus on the duration margin, I assume that the probability of job loss does not vary with b.[4]

For simplicity, assume that the agent never loses his job again after he finds a new job, and supplies one unit of labor permanently after that point. Assuming the usual Inada condition $u_c(c=0)=\infty$, the technological constraints $c_{i,s} \geq 0$ will never bind and can be ignored in the maximization. Therefore, household i chooses the path of c_{is} and d to

$$\begin{aligned} &\max \int_t^T u(c_{i,s}, \Theta_{i,s})ds + \psi(d) \\ &\text{s.t. } A_{i,T} = A_{i,t} + bd + w(T-d) - \int_t^T c_{i,s}ds = 0 \\ &\qquad A_{i,s} \geq 0 \forall s \in [t,T) \end{aligned}$$

where $A_{i,t}$ denotes asset holdings at time t.

Since there is no uncertainty or discounting and no income growth both when unemployed and employed, the optimal consumption path is flat in both states. Therefore, let c_u denote consumption while unemployed and c_e consumption while employed. The agent's problem can be rewritten as

$$\max du(c_u) + (T-d)u(c_e) + \psi(d)$$

$$\text{s.t. } [\lambda_t]\ A_T = A_t + bd + w(T-d) - dc_u - (T-d)c_e = 0 \quad (1)$$

$$[\mu_t]\ A_d = A_t + bd - dc_u \geq 0 \quad (2)$$

[4]This is not because these concerns are unimportant. Feldstein (1978) and Topel (1983) present evidence showing a strong relationship between the rate of temporary layoffs, UI benefits, and lack of experience rating.

Let λ_t denote the multiplier associated with the intertemporal budget constraint (1) and μ_t the multiplier associated with the borrowing constraint (2) in period t. These multipliers represent the marginal value of relaxing each of the constraints at the optimum in period t. Taking the Kuhn-Tucker conditions for this maximization problem, the following must be true at the optimum:

$$u'(c_u) = \lambda_{it} + \mu_{it} \tag{3}$$

$$u'(c_e) = \lambda_{it} \tag{4}$$

$$\varphi'(d) = (\lambda_{it} + \mu_{it})(w - b) + (\lambda_{it} + \mu_{it})(c_e - c_u) + \Delta u(c, \Theta_{i,t}) \tag{5}$$

The intuition for these optimality conditions can be seen with standard perturbation arguments. First consider the case where (2) does not bind. In this case, $\mu_t = 0$: If the constraint is slack at the optimum, there cannot be any marginal value in loosening it further. Hence, $u'(c_u) = \lambda_{it} = u'(c_e)$. Intuitively, the marginal utility of a dollar of wealth must equal the marginal utility of consuming that dollar immediately or consuming it in the future. The optimality condition for the duration choice simplifies to $\varphi'(d) = \lambda_{it}(w - b)$. Intuitively, the marginal benefit of remaining unemployed one week longer should offset the marginal consumption utility loss of losing $w - b$ in income.

Now consider the case where the borrowing constraint (2) binds. In this case, the provision of an extra dollar of wealth at time t relaxes both the borrowing constraint and the intertemporal budget constraint, raising utility by $\lambda_{it} + \mu_{it}$. Since it is strictly optimal to consume that dollar immediately if the borrowing constraint is binding, the marginal utility of consumption while unemployed must equal the sum of these two multipliers. But additional wealth when employed does not relax the borrowing constraint, so $u'(c_e) = \lambda_{it}$. Finally, when the agent is constrained, the optimality condition for duration has additional terms which arise from the fact that consumption is not smooth across the employed and unemployed periods.

Equation (5) can be solved for the duration in terms of $w-b$, the current marginal utility of wealth, $\lambda_{it}+\mu_{it}$, the change in consumption between the unemployed and employed period, $\Delta c = c_e - c_u$, and the taste shift variable, $\Theta_{i,t}$:

$$d = g(w-b, \lambda_{it}+\mu_{it}, \Delta c, \Theta_{i,t})$$

Transforming the taste shift variable $\Theta_{i,t+k}$ appropriately, it is convenient write a log-linear approximation to this equation as follows.[5]

$$\log d_{it} = \Theta_{i,t} + \eta \log(w-b) - \delta \log(\lambda_{it}+\mu_{it}) - \sigma \log(\Delta u) \tag{6}$$

In this equation, the coefficients η, δ, and σ are positive numbers. An important property of (6) for what follows is that relative price changes (which do not affect total wealth or the marginal utility of income) affect durations only through the second term. Conversely, exogenous changes in wealth or income that do not distort prices affect durations only to the extent that they change the multipliers λ and μ and change the value of Δc. Hence, all substitution effects occur through the second term, and all income or wealth effects occur through the third and fourth terms. Note that this equation is very similar to the conventional Frisch labor supply equation derived from intertemporal labor supply models (see MaCurdy 1983; Blundell and MaCurdy 1999). The only difference is the additional multiplier μ_{it}, which enters because the present model allows for a borrowing constraint.

2.1 Effect of UI Benefits on Durations

Unconstrained Case. Consider an individual for whom (2) does not bind at his optimal allocation at the time of unemployment ($\mu = 0$). To reduce notation, the i subscript is

[5] The log linearization simplifies the algebra that follows but is not necessary for the main result that only unconstrained households experience a pure substitution effect.

omitted below. First examine the effect of raising total expected wealth at time t,

$$W = A_t + bd + w(T - d)$$

on his unemployment duration. Since $\Delta u = 0$ when $\mu = 0$, the elasticity of d with respect to W is

$$\varepsilon_{d,W} = \frac{\partial \log d}{\partial \log W} = \delta \frac{\partial \log \lambda}{\partial \log W}$$

Now turn to the effect of raising the benefit level, b, on d. The elasticity of durations with respect to the UI benefit rate for this individual is

$$\begin{aligned} \varepsilon_{d,b} &= \frac{\partial \log d}{\partial \log b} = -\eta \frac{b}{w-b} + \delta \frac{\partial \log \lambda}{\partial \log W} \frac{\partial \log W}{\partial \log b} \\ &= -\eta \frac{b}{w-b} + \varepsilon_{d,W} \varepsilon_{W,b} \end{aligned}$$

The first term in this expression is the substitution effect of UI benefits on unemployment durations. It reflects the fact that higher UI benefits distort the price of leisure in period t, creating an incentive for the agent to work less and enjoy a partially paid vacation at the expense of other taxpayers. The second term is the wealth effect associated with a change in UI benefits, which affects labor supply decisions by changing λ, the marginal utility of wealth. The magnitude of the wealth effect is negligible because $\varepsilon_{W,b}$ is very small in practice. There are two reasons for this: (1) UI benefits are a small fraction of total lifetime wealth and (2) most if not all of the increase in benefits will be offset by corresponding increases in the UI tax, τ, levied on the same individual.[6] Given that the wealth effect can be ignored,

[6] To see this formally, note that $\varepsilon_{W,b} \leq \alpha$ where α is the fraction of wealth earned through UI benefits. A 1% increase in b increases total wealth W by at most α because behavioral responses to higher UI payments will, if anything, lead to labor supply reductions. The fraction of income earned through UI benefits can be approximated by the share of UI in the labor share of GDP, which is approximately $\frac{\$50bil}{2/3*10tril} < 0.01$. Even if the wealth elasticity of unemployment durations were a very large $\varepsilon_{d,W} = 1$, the wealth effect of the change in benefits on durations is at most 0.01. Moreover, this calculation overstates the wealth effect by assuming that none of the rise in benefits is paid for by increased tax payments from this individual.

we can write

$$\varepsilon_{d,b}^{\mu=0} = -\eta \frac{b}{w-b} \tag{7}$$

for any individual for whom the borrowing constraint is slack.

Constrained Case. Now consider an individual for whom (2) binds, perhaps because he faced a series of bad wealth realizations or income shocks before period t, or because he has a low discount factor β and chose not to build up a sufficiently large buffer stock to fully smooth consumption during his current unemployment spell. For this individual,

$$\varepsilon_{d,b} = \frac{\partial \log d}{\partial \log b} = -\eta \frac{b}{w-b} + \delta \frac{\partial \log(\lambda+\mu)}{\partial \log b} + \sigma \frac{\partial \log(\Delta c)}{\partial \log b} \tag{8}$$

Now the second term is potentially non-trivial in magnitude. To see this, note that the second term in (8) is

$$\frac{\partial \log(\lambda+\mu)}{\partial \log b} = \frac{\partial \log u_c(c_u)}{\partial \log c_u} \frac{\partial \log x}{\partial \log b} = \gamma \varepsilon_{c_u,b}$$

where γ denotes the curvature of utility over consumption and $\varepsilon_{c_u,b}$ is the elasticity of consumption while unemployed with respect to benefits. Note that the provision of UI benefits will not affect c_e when $\mu > 0$; therefore

$$\varepsilon_{c_u,b} = \varepsilon_{\Delta c,b} \frac{\Delta c}{c_u}$$

We can thus write

$$\varepsilon_{d,b}^{\mu>0} = -\eta \frac{b}{w-b} + (\delta\gamma \frac{\Delta c}{c_u} + \sigma)\varepsilon_{\Delta c,b} \tag{9}$$

This equation shows that UI benefits can have a large *income effect* on durations, in addition to the usual substitution effect, when $\mu > 0$ and $\varepsilon_{\Delta c,b}$ is large. Intuitively, when the agent relies primarily on UI income for consumption while unemployed, the provision of an extra dollar of benefits has a large effect on consumption while unemployed. It thereby induces large changes in the multiplier on the borrowing constraint, generating a potentially non-trivial income effect. In contrast, when agents are unconstrained, the income effect channel is shut down because UI benefits are a trivial fraction of lifetime wealth.

Deadweight cost of UI benefits. In analogy with the deadweight cost of taxation, the efficiency cost of unemployment insurance can be defined as the loss in welfare from having a benefit proportional to duration instead of a lump-sum grant given at the time of job loss of an equivalent amount ($B_i = b \times d_i$) for each individual. As in the case of taxes, the deadweight cost of UI is determined strictly by the size of the substitution elasticity. To see this, suppose UI affects search behavior only through an income effect. In this case, having a lump-sum benefit equal in size to the total original UI payment would leave behavior unchanged, and therefore would not generate any efficiency cost. In contrast, if UI affects search behavior through a substitution effect, provision of a lump-sum benefit would make agents voluntarily reduce unemployment durations while keeping income in the unemployed state constant, raising welfare. Put differently, only behavioral responses to price distortions generate distortionary costs, so only the substitution elasticity matters in the efficiency calculation.

3 Empirical Evidence I: The Role of Constraints

3.1 Estimation Strategy

The model suggests a natural method of assessing the empirical importance of borrowing constraints in explaining the UI-duration link documented in the literature: Compare the effect of UI benefits on durations for constrained individuals ($\mu > 0$) vs unconstrained individuals ($\mu = 0$). To the extent that UI benefits have stronger effects on durations in constrained groups, borrowing constraints and income effects could play a substantial role in the UI-duration link. To implement this idea formally, I divide individuals into unconstrained and constrained groups and estimate equations of the following form:

$$\log d_{it} = \beta_0 + \beta_1 \log b + \beta_2 X_{i,t} + \theta_{i,t} \tag{10}$$

where $X_{i,t}$ denotes the observable component of the taste shift variable for household i and $\theta_{i,t} = \Theta_{i,t} - \beta_2 X_{i,t}$ denotes the component of that variable that cannot be observed in the data. A key identifying assumption for the empirical analysis is that the UI benefit rate is orthogonal to unobserved variation in tastes:

$$Eb \times \theta_{i,t} = 0 \tag{11}$$

Tests of this assumption are discussed in the next section. Assuming for now that the orthogonality condition holds, observe that when (10) is estimated for the unconstrained ($\mu = 0$) group, the coefficient

$$\beta_1^{\mu=0} = \varepsilon_{d,b}^{\mu=0} = \varepsilon_{d,b}^{s,\mu=0}$$

gives the pure substitution effect of UI benefits on unemployment durations. This substitution effect directly reveals the extent to which UI generates a deadweight cost among the unconstrained group.

When (10) is estimated for a group of constrained individuals ($\mu > 0$), we obtain

$$\beta_1^{\mu>0} = \varepsilon_{d,b}^{\mu>0} = \varepsilon_{d,b}^{s+I,\mu>0}$$

which is an estimate of the composite effect of UI on durations for this group, including both substitution and income effects. If one assumes that the substitution elasticity does not vary with μ conditional on the covariates, i.e.

$$\varepsilon_{d,b}^{s,\mu>0} = \varepsilon_{d,b}^{s,\mu=0} \tag{12}$$

we can obtain an estimate of the income effect for the constrained group by subtracting the UI benefit elasticity for the unconstrained group. The cross-group comparison is necessary because the substitution elasticity for the constrained group cannot be directly observed. Of course, this method of estimating the income elasticity raises the question of whether the constrained and unconstrained groups are sufficiently similar that (12) actually holds. I provide some evidence supporting the claim that the unobservable differences between the groups are not driving the results in the empirical analysis below. In addition, section 4 provides an independent estimate of the income elasticity that does not rely on (12) using variation in severance payments.

One might wonder why I focus on UI benefits to test whether cash-on-hand affects unemployment durations, rather than using variation in wealth holdings more generally. The main reason is that the variation in unemployment benefits is credibly exogenous, insofar as it comes from differences across states and time in laws. In contrast, variation in wealth holdings at the time of unemployment are endogenous and highly likely to be correlated with other unobservable factors that could influence durations such as skills. Indeed, Gruber (2001) argues that agents with low levels of wealth also tend to have short job tenures and limited labor force experience, therefore inducing a negative correlation between wealth and duration.[7]

Defining the constrained group. The main difficulty in implementing (10) is that μ is a latent variable, making it impossible to classify households into groups directly based on whether they face a binding borrowing constraint. Therefore, following the approach of Zeldes (1989), I use a set of proxies that are likely to predict whether a household is constrained. The primary proxy is liquid wealth net of unsecured debt, which I term "net wealth." Households that have higher levels of net wealth relative to their level of permanent income (measured e.g. by their pre-unemployment wage) are less likely to be constrained.

[7] This type of endogeneity problem could explain why Lentz (2003) and others generally find little association between wealth holdings and unemployment durations in the cross-section.

In the model, these households have high $A_{i,t}$ at the time of unemployment, allowing them to smooth consumption provided that the spell is not too long. In contrast, households with low assets, especially $A_{i,t} < 0$, are likely to be completely reliant on UI income for consumption while unemployed, making $\mu > 0$ for many of them.

The second proxy is whether the individual has a spouse who is also working. Households that rely on a single income are more likely to be constrained when that individual loses his job; those with an alternate source of income may have additional sources of liquidity, including a better access to credit because at least one person has a stable job.[8] The third proxy for μ is the household's mortgage payments. Gruber (1998) finds that fewer than 5% of the unemployed move during a spell. Consequently, if an individual must make large mortgage payments, he effectively has less liquid wealth, and is more likely to be constrained.

The validity of each of these variables as proxies for being constrained by UI income is substantiated by the results of Browning and Crossley (2001), who find a positive association between UI benefits and consumption precisely when households have low-assets or only one earner.[9] Nonetheless, these markers are imperfect predictors of who is constrained. Some households with $\mu = 0$ are presumably misallocated to the $\mu > 0$ group and vice-versa. Such classification error will pull the estimated elasticities for the two groups closer together, thereby causing us to *underestimate* the magnitude of the income elasticity, which is based on the difference between the two.

An additional concern in implementing (10) is that households may move across groups as an unemployment spell elapses. Although the simple model above assumes that households can anticipate their unemployment durations perfectly at the time of job loss, in practice search is a dynamic process in practice and households update their expectations over time while depleting their buffer stocks. In this setting, the probability that a household faces a

[8] There is no data that directly measures access to credit in the SIPP.

[9] Unfortunately, the SIPP lacks consumption data, so their findings cannot be directly corroborated for this sample.

constraint will rise as the spell elapses. Since the SIPP contains full asset data only at one point, one way to account for this effect is by estimating models that allow UI benefits to have a time-varying effect on unemployment exit rates. This concern, and more importantly the fact that many unemployment spells are censored in the data, motivates estimation of a hazard model with time-varying covariates rather than estimation of (10) using OLS. Letting $h_{i,s}$ denote the unemployment exit hazard rate for household i in week s of an unemployment spell and $X_{i,s}$ denote a set of controls, the primary estimating equation for the constraint tests is thus

$$h_{i,s} = f(b_i, s \times b_i, X_{i,s}) \tag{13}$$

By estimating (13) for each of the groups defined by the proxies of μ described above, we can recover the income and substitution elasticities of interest. While this reduced-form approach does not permit complete recovery of the structural parameters needed to make welfare calculations and analyze optimal UI policy numerically, is provides a transparent means of illustrating the main features of the data.

3.2 Data

The data used to implement the constraint tests are from the 1985-1987 and 1990-1996 panels of the Survey of Income and Program Participation (SIPP). The SIPP collects information from a sample of approximately 30,000 households every four months for a period of two to three years. The interviews I use span the period from the beginning of 1985 to the middle of 2000. At each interview, households are asked questions about their activities during the past four months, including weekly labor force status. Unemployed individuals are asked whether they received unemployment benefits in each month.[10] Other data about the demographic and economic characteristics of each household member are also collected.

[10] The ability to identify UI takeup is one advantage of using the SIPP rather than the Current Population Survey. Another advantage is that the SIPP is a panel dataset, making it more suitable to measure unemployment durations. The CPS only gives a cross-section of ongoing spells.

I make five exclusions on the original sample of job leavers to arrive at my core sample. First, following previous studies of UI, I restrict attention to prime-age males (over 18 and under 65). Second, I include only the set of individuals who report searching for a job at some point after losing their job, in order to eliminate from the analysis individuals who have dropped out of the labor force. Third, I exclude individuals who report that they were on temporary layoff at any point during their spells, since they might not have been actively searching for a job.[11] Fourth, I exclude individuals who have less than three months work history within the survey because there is insufficient information to estimate pre-unemployment wages for this group. Finally, I focus primarily on individuals who take up UI within one month after losing their job because it is unclear how UI should affect hazards for individuals who delay takeup. The potential sample selection bias related to UI takeup that arises from this exclusion is addressed below.

These exclusions leave 4,560 individuals in the core sample. Note that asset data is generally collected only once in each panel, so pre-unemployment asset data is available for approximately half of these observations. The first column of Table 1 gives summary statistics for the core sample, which looks reasonably representative of the general population. The median UI recipient is a high school graduate and has pre-UI gross annual earnings of $20,726 in 1990 dollars. The most germane statistic for the present analysis is pre-unemployment wealth: median liquid wealth net of unsecured debt is only $128.

The raw data on UI laws were obtained from the Employment and Training Administration (various years), and supplemented with information directly from individual states.[12] The computation of weekly benefit amounts deserves special mention. Measurement er-

[11] Katz and Meyer (1990) show that whether an individual considers himself to be on temporary layoff is endogenous to the duration of the spell; recall may be expected early in a spell but not after some time has elapsed since a layoff. Excluding temporary layoffs can therefore potentially bias the estimates. To check that this is not the case, I include temporary layoffs in some specifications of the model.

[12] I am grateful to Julie Cullen and Jon Gruber for sharing their simulation programs, and to Suzanne Simonetta and Loryn Lancaster in the Department of Labor for providing detailed information about state UI laws from 1984-2000.

ror and inadequate information about pre-unemployment wages for many claimants make it difficult to simulate the potential UI benefit level for each agent precisely. I use three independent approaches to proxy for each claimant's (unobserved) actual UI benefits, all of which yield similar results. First, I use published state average benefits in lieu of each individual's actual UI benefit amount. Second, I proxy for the actual benefit using published maximum weekly benefit amounts, which are the primary source of variation in benefit levels across states. Most states replace 50% of a claimant's wages up to a maximum benefit level. The third method involves simulation of each individual's weekly UI benefit using a two-stage procedure. In the first stage, I predict the claimant's pre-unemployment annual income using information on education, age, tenure, occupation, industry, and other demographics. The prediction equation for pre-UI annual earnings is estimated on the full sample of individuals who report a job loss at some point during the sample period.[13] In the second stage, I use the predicted wage as a proxy for the true wage, and assign the claimant unemployment benefits using the simulation program.

The mean weekly benefit amount (based on the simulation method) is $166. Importantly for the identification strategy, there is considerable cross-state and time variation in unemployment benefits, from an average weekly benefit amount of $102 in Louisiana to $217 in Massachusetts in 1990.

Figure 1 shows the mean unemployment exit hazards for the core sample. The hazard rate is typically around 5-7%, but there are sharp spikes at $t = 17$ and $t = 35$. These spikes reflect a reporting artefact known as the "seam effect," which is common in longitudinal panels such as the SIPP. To see how the seam effect arises, recall that the SIPP data is collected by interviewing individuals every four months about their activities during the past four months, which is termed the "reference period." Individuals tend to repeat

[13] Since many individuals in the sample do not have a full year's earning's history before a job separation, I define the annual income of these individuals by assuming that they earned the average wage they report before they began participating in the SIPP. For example, individuals with one quarter of wage history are assumed to have an annual income of four times that quarter's income.

answers about weekly job status. As a result, they under-report transitions in labor force status within reference periods and overreport transitions on the "seam" between reference periods. Hence, many spells of unemployment appear to last for exactly the length of one or two reference periods, which correspond to lengths of 17 and 35 weeks. The dashed line in Figure 1 shows the empirical hazards for spells that did not begin on a seam, and as one would expect, the two spikes no longer exist.[14] The empirical analysis below adjusts for the seam effect to produce smooth curves without artificial spikes.

3.3 Results

3.3.1 Graphical Evidence and Non-Parametric Tests

I begin by providing graphical evidence on the benefit elasticity of durations in constrained and unconstrained groups, and then show the robustness of these results to controls, sample selection, and other specification concerns. First consider the asset proxy for constraints. I divide households into four quartiles based on their net liquid wealth (liquid wealth minus unsecured debt) in the period prior to job loss (households for whom asset data is available only after job loss are excluded). Table 1a shows summary statistics for each of the four quartiles. Although the households in the lower net liquid wealth quartiles are slightly poorer and less educated, the differences between the four groups are not extremely large. Notably, quartiles 1 and 3 are very similar in terms of income, education, and other demographics. Hence, UI benefits are similar both in levels and as a fraction of permanent income for all the groups. The fact that the variations between quartiles are not drastic suggests that comparisons of behavioral responses to UI benefits across these quartiles can be reasonably

[14] The remaining fluctuations in the hazard rate are generally consistent with the findings of Meyer (1990), who uses an administrative dataset of UI recipients. One exception is that we do not see a spike in the hazard rate around the time of benefit exhaustion (26 weeks), as documented e.g. in Katz and Meyer (1990). The main reasons for this difference are the definition of duration used here (weeks searching, not weeks of UI collected) and the exclusion of temporary layoffs, which drive most of the spike in the data used by Katz and Meyer.

informative about the effect of net liquid wealth itself.

Figures 2a-d show the effect of UI benefits on unemployment exit rates for households in the each of the four quartiles of the net wealth distribution. To construct these figures, I first divide the (state, year) pairs in the data into two categories: Those that have average weekly benefit amounts above the sample mean and those below the mean. Kaplan-Meier survival curves are then plotted using the observations in these two groups that fall into each quartile of the net wealth distribution. The seam effect is smoothed out by first fitting a Cox model with a time-varying indicator variable for being on a seam between interviews, and then recovering the (nonparametric) baseline hazards to construct a seam-adjusted Kaplan-Meier curve. The resulting survival curves give the probability of remaining unemployed after t weeks for an individual who never crosses an interview seam.

Figure 2a shows that higher UI benefits are associated with much lower unemployment exit rates for individuals in the lowest wealth quartile, who are most likely to be in the constrained group ($\mu > 0$). For example, after 15 weeks, 55% of individuals in low-benefit state/years are still unemployed, compared with 68% of individuals in high-benefit state/years. A nonparametric Wilcoxon test rejects the null hypothesis that the two survival curves are identical with $p < 0.01$. Figure 2b constructs the same survival curves for the second wealth quartile. UI benefits appear to have a smaller, but still powerful effect on durations in this group. At 15 weeks, 63% of individuals in the low-benefit group are still unemployed, vs. 70% in the high benefit group. The Wilcoxon test again rejects equality of the survival curves in this group, with $p = 0.04$. Figure 2c shows that effect of UI on durations virtually disappears in the third quartile of the wealth distribution. At 15 weeks, 57% of those in low-benefit states have found a job, vs. 59% in high-benefit states. Not surprisingly, the Wilcoxon test does not reject equality of these survival curves ($p = 0.74$). Finally, Figure 2d shows that UI appears to have no effect on durations for the richest group of households, who are least likely to be constrained ($\mu = 0$). In both the high-benefit and low-benefit groups, 64% of the job losers remain unemployed after 15 weeks, and the

Wilcoxon test does not reject equality ($p = 0.43$). Hence, UI benefits have much stronger effects on durations in low-wealth households that are more likely to be liquidity constrained while unemployed.

As noted above, an important assumption in this analysis is that the variation in UI benefits is orthogonal to other unobservable determinants of durations, i.e. that (11) holds. To test this identification assumption, Figure 5 shows the effect of UI benefits on durations for a "control group" of below-median net wealth individuals who do not receive UI benefits, either because of ineligibility or because they chose not to take up.[15] The durations of these individuals are insensitive to the level of benefits, supporting the claim that UI benefits play a causal role in increasing durations among low-wealth households who receive benefits.

I now replicate these graphs and nonparametric tests for the other two proxies of constraints. Table 1b shows summary statistics for the constrained and unconstrained groups based on spousal work and mortgage status. As with the asset cuts, there are differences across the constrained and unconstrained groups in income and education, but these are not extremely large. Figures 3a-b compare the effect of UI on unemployment exit rates for households with a single vs.dual earners. Figure 3a shows that UI benefits significantly reduce exit rates for households who are more likely to be constrained at the time of job loss because they were relying on a single source of income. The Wilcoxon test rejects equality of the survival curves with $p < 0.01$. In contrast, UI benefits appear to have no effect on exit hazards for households with two earners (Figure 3b). Control group tests using non-UI recipients (not reported) support the causality of UI benefits here as well.

The results for the mortgage cut are similar. Figure 4a shows that UI benefits have a sharp effect on durations among households that have a mortgage to pay off at the time of job loss, and equality of the two survival curves is again rejected with $p < 0.01$. But among

[15] Results are similar for the set of job losers who are ineligible for UI, who arguably are a better "control" because takeup of UI is endogenous. However, the UI-ineligible group consists of part-time workers who have very low levels of earned income before unemployment and may therefore not be similar to the average UI claimant.

households without a mortgage, the difference between the survival curves in the high-benefit and low-benefit groups is much smaller and statistically indistinguishable. This result is particularly supportive of the causal role of constraints because the constrained types in this cut, who are homeowners with mortgages, have *higher* income, education, and wealth than the unconstrained types, who are primarily renters (see Table 1b). This is in contrast with the asset and spousal work proxies, where the constrained group included the lower types in economic characteristics. Hence, it is unlikely that the differences in the benefit elasticity of duration across constrained and unconstrained groups is spuriously driven by other differences across the groups such as income or education.

3.3.2 Semi-Parametric Estimates

I evaluate the robustness of the graphical results by estimating (13) using a Cox specification for the hazard function. The Cox model assumes a proportional-hazards form for the hazard rate s weeks after unemployment:

$$h_{i,s} = \alpha_s \exp(\beta_1 \log b_i + \beta_2 s \times \log b_i + \beta_3 X_{i,s}) \tag{14}$$

where $X_{i,s}$ denotes a set of covariates and $\{\alpha_s\}$ are the set of baseline hazards. The key coefficient of interest is β_1, which captures the effect of raising the log of the UI benefit assigned to individual i. To control for the fact that the relationship between UI benefits and the hazard rate may vary over time, the model also includes an interaction of $\log(b_i)$ with s, the weeks elapsed since job loss. Note that this specification does not impose any functional form on the baseline unemployment exit rates in each week, so the coefficients on the key covariates are identified purely from cross-state and time variation in UI laws, as in the graphical analysis. Tables 2 and 3 presents estimates of (14) and variants of this basic specification described below. The coefficients reported are hazard ratios (e^{β^j}), which can be interpreted as the ratio of the hazard when covariate j equals $X^j + 1$ to the hazard when the covariate is X^j.

I first estimate (14) on the full sample to identify the unconditional effect of UI on the hazard rate. In this specification, as in most others, I use the average UI benefit level in the individual's (state,year) pair to proxy for b_i for the measurement-error reasons discussed in the data section. This specification includes a full set of controls: Industry, occupation, and year dummies; a 10 piece log-linear spline for the claimant's pre-unemployment wage; linear controls for total (illiquid+liquid) wealth, age, education; and dummies for marital status, pre-unemployment spousal work status, and being on the seam between interviews to adjust for the seam effect. Standard errors in this and all subsequent specifications are clustered by state. The estimates in column 1 of Table 2a show that a 10% increase in the UI benefit level lowers the unemployment exit hazard rate by $(0.671)^{1/10} = 4\%$ in the pooled sample. Reassuringly, this unconditional estimate is in the range found by Moffitt (1985), Meyer (1990), and Katz and Meyer (1990).

Heterogeneity by Net Liquid Wealth Quartiles. I now examine the heterogeneity of the UI effect by estimating separate coefficients for constrained and unconstrained groups as in the graphical analysis. Table 2 considers the asset proxy for constraints by dividing the data into four quartiles of the net wealth distribution as in the graphical analysis. Let $Q_{i,j}$ denote an indicator variable that is 1 if agent i belongs to quartile j of the wealth distribution. In addition, let $\alpha_{s,j}$ denote the baseline exit hazard for individuals in quartile j in week s of the unemployment spell. To avoid parametric restrictions, the baseline hazards are allowed to vary arbitrarily across the constrained and unconstrained groups. Several estimates of the following stratified Cox regression are reported in Table 2:

$$h_{i,s} = \alpha_{s,j} \exp(\beta_1^j Q_{i,j} \log b_i + \beta_2^j Q_{i,j}(s \times \log b_i) + \beta_3 X) \qquad (15)$$

Specification (2) of Table 2a estimates this equation with no controls (no X). The key coefficients of interest are $\{\beta_1^j\}_{j=1,2,3,4}$, which tell us the effect of raising UI benefits on the hazard rate for each quartile of the net wealth distribution. The estimates indicate that β_1^j is rising in j, i.e. the effect of UI benefits monotonically declines as one moves up in the net liquid wealth distribution. Among households in the lowest quartile of net wealth, a 10% increase in UI benefits reduces the hazard rate by 7.7%, an estimate that is statistically significant at the 5% level. In contrast, there is a small, statistically insignificant association between the level of UI benefits and the hazard among households in the third and fourth quartiles of net wealth. The null hypothesis that UI benefits have the same effect on hazard rates in the first and fourth quartiles is rejected with $p < 0.05$, as is the null hypothesis that the mean UI effect for below-median wealth households is the same as that for above-median wealth households. These findings support the conclusion drawn from the graphical analysis that UI benefits have much stronger effects on durations for households constrained by their low net liquid wealth.

Specification (3) replicates the preceding specification with the full set of controls used in column (1). The key coefficients of interest are virtually unchanged when this rich set of covariates is introduced. The fact that controlling for observed heterogeneity does not affect the results suggests that the estimates are unlikely to be very sensitive to unobservable heterogeneity as well.

The preceding specifications maximize sample size by using data on post-unemployment assets for households where pre-unemployment asset data are unavailable. Since post-unemployment assets may be endogenous to the agent's spell length, this form of sample selection could yield biased results. Specification (4) addresses this concern by estimating (3) on the subsample of households with pre-unemployment asset data. Since the sample size is reduced by more than 50%, the standard errors in this specification are larger. However, the pattern of the coefficients remains very similar to that in specification (3). The hypothesis that the effect of UI on exit rates of below-median wealth and above-median wealth households is the same can be rejected with $p = 0.05$.

Specification (5) introduces state fixed effects in addition to the full set of controls. In this model, the variation in the UI benefit level comes purely from within-state law changes. Results remain similar, with monotonically increasing β_1^j coefficients as wealth rises.

Table 2b reports a series of additional robustness checks for the asset heterogeneity tests. All of these specifications include the full control set. Specification (1) restricts the sample to low-wage households, dropping individuals who report pre-unemployment annual wages above \$24,720, the 75th percentile of the wage distribution. The goal of this specification is to address the concern that UI benefits may be a trivial fraction of income for high-income households and may therefore have no effect on their search behavior. Since high income households tend to be somewhat over-represented in the high asset quartiles, this alternative hypothesis could be responsible for the results. The estimates in column 1 reject this alternative explanation, since UI benefits continue to have much stronger effects among low-asset households when high income households are excluded.

Columns (2) and (3) examine robustness to changes in the definition of b_i. Column (2) uses the maximum UI benefit level in individual i's state/year and column (3) uses the simulated benefit for each individual i using the two-stage procedure described above. Both specifications give similar results to the baseline case. Column (4) shows that includes individuals who report being on temporary layoff does not affect the results.

Finally, column (5) replicates the baseline specification but defines the quartiles of wealth in terms of home equity rather than net liquid wealth and restricts the sample to homeowners. Home equity is much less accessible than liquid wealth during an unemployment spell, since borrowing even against secured assets may be difficult when one is unemployed. Home equity should therefore be a poorer predictor of liquidity constraints than liquid wealth. If constraints play a causal role in the UI-duration link, the differences in the effect of UI benefits across quartiles of home equity should be weak. In contrast, if the preceding results are due to spurious correlations between the benefit elasticity of durations and income or wealth, the home equity cut should produce the same results as the liquid wealth cut. The evidence supports the causality of borrowing constraints, as there is no strong pattern in the coefficients on the UI benefit variable across the quartiles in column 5.

Spousal Work Status. Table 3a reports estimates of specifications analogous to (15) for the spousal work proxy. Instead of quartiles of liquid wealth, the UI benefit coefficient is interacted with a dummy for whether the agent lived in a single-earner or dual-earner household prior to job loss, and the baseline hazards are stratified by this dummy. The first specification includes all observations in the core sample without any controls. In this group, there is a moderate but statistically insignificant difference in the UI benefit coefficient for the single-earner and dual-earner groups.

To explore this result in greater detail, observe that households with very low net wealth (who typically have substantial debt) are likely to be constrained irrespective of whether they have two earners or not, and households with very high net wealth are likely to be unconstrained regardless of spousal work status. Specification (2) therefore focuses on households in the middle two quartiles of the net wealth distribution, who are most likely to be on the margin of being liquidity constrained. In this subgroup of households, the effect of spousal work status emerges much more clearly. A 10% increase in the UI benefit reduces the mean unemployment exit hazard by 5.4% for single-earners but has a small, statistically insignificant effect for dual earners. The null hypothesis of identical effect in the two groups is rejected with $p = 0.06$. The third column shows that this result is robust to including the full set of controls described above. The fourth column adds state fixed effects, and shows that the general pattern is preserved although standard errors rise in this specification. Column 5 restricts attention to the households in the lowest quartile of net wealth. Consistent with the hypothesis that these households are constrained regardless of spousal work status, UI benefits have a strong effect on durations in both single-earner and dual-earner families in this category.

Mortgage Status. Table 3b shows results for the mortgage proxy. The first specification supports the graphical evidence in Figure 4, indicating that UI benefits have a much larger effect on durations among households that have mortgages. Equality of coefficients on the

UI benefit variable among mortgage-holders and non-holders is rejected with $p < 0.01$. The second and third specifications confirm that this result is robust to the full set of controls and state fixed effects. The fourth specification includes only households with net liquid wealth below the sample median. The estimates indicate that low-wealth households who have to pay a mortgage – who are perhaps especially constrained – are extremely sensitive to unemployment benefits in their search behavior.

Sample Selection Concerns. One might worry that endogeneity of takeup with respect to the level of benefits biases the estimate of the UI benefit elasticity. In my sample, a 10% increase in the benefit rate is associated with approximately 1% increase in the probability of UI takeup in the first month of unemployment.[16] If the marginal individuals who decide to take up UI when benefits rise tend to have shorter unemployment spells on average, estimates of the UI benefit elasticity will be biased toward zero.

There are two reasons that this issue is unlikely to be important in practice. First, the takeup elasticity is similar across all the constrained and unconstrained subgroups. Hence, there is no reason that it should artificially bias down the estimate only in the unconstrained group. Second, even if there were differential biases across groups, the effects on the estimated UI benefit elasticity would be quite small. The magnitude of the bias can be gauged by assuming that the individuals who are added to the sample through this selection effect are drawn randomly from the group who do not takeup UI. The empirical hazards for the non-UI group are on average 1.1 times as large as those of the UI recipients. In practice, the marginal individual who takes up UI is likely to anticipate a longer UI spell than the average agent who does not take up UI, so the 1.1 ratio provides an upper bound for the size of the selection bias. Starting from an initial takeup rate of 50%, a 10% increase in benefits will cause the average hazard rate to rise through this selection effect by approxi-

[16]The probability of taking up UI at any point during the spell rises by 2% for a 10% increase in UI benefits. This is exactly equal to the estimate reported by Anderson and Meyer (1997), who use a much larger dataset on benefit takeup.

mately $\frac{1\%}{50\%} * (1.1 - 1) = 0.2\%$. But the difference in the hazard rates across constrained and unconstrained groups was an order of magnitude larger (approximately 5%), suggesting that this selection effect is negligible.

In summary, all three proxies for constraints show that the effect of UI on durations identified in earlier studies is driven by a subset of households who are likely to be borrowing constrained at the time of job loss. These results are quite robust to various specification checks and controls. The fact that UI has little effect on durations in the unconstrained groups suggests that it induces little or no substitution effect among households with sufficient resources to smooth consumption while unemployed. Provided that substitution effects are similar in the constrained and unconstrained groups, it follows that non-distortionary income effects generated by borrowing constraints play a large role in the UI-duration link.

4 Empirical Evidence II: Severance Pay and Durations

4.1 Estimation Strategy

While the preceding evidence illuminates the mechanism through which UI benefits affect durations, it does not provide a precise estimate of the income elasticity of unemployment durations. In this section, I estimate the income elasticity directly using variation in lump-sum severance payments made to job losers, without relying on comparisons between constrained and unconstrained households.

Many firms in the United States have severance packages that compensate employees who are laid off. According to a recent survey of Fortune 1000 firms (Lee Hecht Harrisson 1999), the most common policy for regular (non-executive) full-time workers is to make a severance payment of one week of pay for each year of service at the firm. However, some companies have flatter or steeper severance pay profiles with respect to job tenure. Some companies have minimum job tenure thresholds to be eligible for severance pay (e.g. 3 years or 5 years).

Consequently, there is considerable variation at the firm level in whether individuals who are laid off receive a severance payment.[17]

The key characteristic of severance payments for the present analysis is that they are lump-sum, i.e. they are not proportional to the length of unemployment spells. These payments therefore have pure income effects and do not distort the relative price of consumption and leisure for unemployed agents. Therefore, by examining the effect of severance payments on subsequent unemployment exit rates, one can obtain a direct measure of the income elasticity of unemployment durations. To formalize this idea, I estimate models similar to those above, changing the key independent variable from the UI benefit to sev_i, a dummy for receipt of severance pay:

$$h_{i,s} = \alpha_s \exp(\theta_1 \log \text{sev}_i + \theta_2 X_{i,s}) \tag{16}$$

The coefficient θ reveals the causal effect of severance pay on unemployment exit hazards if receipt of severance pay is orthogonal to other determinants of durations. Since severance pay itself is endogenous (and is not randomly allocated across individuals), there is a concern that the estimate of θ_1 may suffer from omitted variable bias. I attempt to address this issue by showing that including very rich controls does not affect the estimate of θ_1 significantly. In addition, I provide additional tests of the identification assumption by investigating whether the estimated θ_1 differs across constrained and unconstrained groups as one would expect.

4.2 Data

The data for this portion of the study come from two surveys conducted by Mathematica on behalf of the Department of Labor, coupled with administrative data from state UI records. The first dataset is the "Study of Unemployment Insurance Exhaustees," which

[17] There is also variation in the amounts of severance payments, but the relatively small sample of job losers who received severance pay makes it difficult to draw statistically precise inferences within this subgroup.

contains data on the unemployment durations of 3,907 individuals who claimed UI benefits in 1998. This dataset is a representative sample of unemployment durations in 25 states of the United States, with oversampling of individuals who exhausted UI benefits. In addition to administrative data on prior wages and weeks of UI paid, there are a large set of survey variables that give information on demographic characteristics, household income, job characteristics (tenure, occupation, industry), and most importantly for this study, receipt of severance pay.

The second dataset is the "Pennsylvania Reemployment Bonus Demonstration." This data was collected as part of an experiment to evaluate the effect of job reemployment bonuses on search behavior. It contains information on 5,678 durations for a representative sample of job losers in Pennsylvania in 1991. The information in the dataset is similar to that in the exhaustees study.

For comparability to the preceding results, I make the same exclusions after pooling the two datasets to arrive at the final sample used in the analysis. First, I include only prime-age males. Second, In the baseline specifications, I exclude temporary layoffs by discarding all individuals who expected a recall at the time of layoff, but show that including these observations do not change the results. These exclusions leave 2,900 individuals in the sample, of whom 531 (18.3%) report receiving a severance payment at job loss.

Two measures of "unemployment duration" are available in this data. The first is the number of weeks for which UI benefits were paid in the base year. This definition has the advantage of accuracy since it comes from administrative records. It also has two disadvantages: it is censored at the time of benefit exhaustion, and it captures total weeks unemployed in a given year rather than the length of a particular spell (which could be different for individuals with multiple short spells). The second measure is the survey measure, constructed from individual's recollection (typically one-two years after the job loss event) of when they lost their initial job and when they found a new one. I focus primarily on the administrative measure here given its significant advantage in terms of accuracy. However, results are quite similar (with larger standard errors) for the survey duration measure.

4.3 Results

I begin again by showing graphical evidence to illustrate the main features of the data. Figure 6 shows Kaplan-Meier survival curves for two groups of individuals: those who received severance pay and those who did not. Severance pay recipients have significantly lower unemployment exit rates in the beginning and middle of the spell. As a result, 66% of individuals who received severance pay claimed more than 10 weeks of UI benefits in their base year, compared with 59% among those who received no severance payment. The convergence of the two survival curves over time is consistent with the hypothesis that severance pay causes a transitory income effect. Since the severance payment is a relatively small lump-sum amount (equal to about three weeks of pay for the typical claimant), one would expect that it should have small effects of search behavior after several weeks have elapsed. Overall equality of the two survival curves is rejected by a nonparametric test with $p < 0.01$. Barring other differences between these two groups, these findings suggest that the transitory income elasticity of unemployment durations is substantial.

To be written up: robustness checks of this result and a calculation to estimate what fraction of the UI benefit elasticity is an income effect.

5 Conclusion

This paper has shown that unemployment benefits raise durations primarily through non-distortionary *income* effects rather than the substitution effects emphasized in the existing literature. In layman's terms, the standard view has been that people take longer to find a job when receiving high UI benefits because it pays less to go back to work. The evidence here suggests instead that people take longer to find a job mainly because borrowing-constrained households have more cash on hand while unemployed and therefore are less pressured to find work quickly. This point has several important implications.

1. *Liquidity Constraints.* Several studies of consumption have examined the extent to which deviations from the permanent income hypothesis can be explained by borrowing constraints. These studies have found mixed results, perhaps because data on consumption is limited and estimates of intertemporal Euler equations are often plagued by econometric problems. This paper provides new evidence that borrowing constraints matter. It circumvents many of the problems faced in prior studies by studying the effects of exogenous transitory income variation on a within-period labor-leisure choice. The empirical results strongly support the view that only agents who face constraints respond to fluctuations in transitory income rather than treating it as a small part of total lifetime wealth.

2. *Efficiency Costs of Social Insurance.* The strong link between unemployment benefits and unemployment durations documented in previous studies has been interpreted as evidence that UI generates a substantial deadweight cost by reducing labor supply. The results of this paper challenge this view. To see this concretely, consider a policy that forces agents to raise search intensity slightly, lowering unemployment durations and reducing the cost of total UI payments. Suppose these UI payments are returned in lump sum form back to the unemployed individuals. If UI generated a deadweight burden by distorting durations, such a policy should raise total welfare by making the economic pie larger. However, the finding that agents respond to UI primarily because of an income effect suggests that such a policy could actually *reduce* welfare because it would pressure individuals to search too intensely relative to the optimal level given the true (social) shadow cost of search. Hence, the moral hazard cost generated by incentives to game the system may be smaller than previously thought.

Some important caveats to this conclusion deserve mention. First, this point applies only to the duration margin As emphasized by Feldstein (1978), Topel (1983), and others, UI benefits could potentially distort other margins of behavior such as the incidence of layoffs, especially in an imperfectly experience rated system. UI could generate substantial deadweight burdens because of substitution effects on such margins. Second, the results apply only locally at the present level of benefits (approximately 50% of pre-unemployment wages) in the U.S. If benefits were closer to full wage replacement, it is certainly plausible that substitution effects could become much more important.

3. *Optimal UI Policy.* Finally, although a formal analysis of optimal UI policy is outside the scope of this paper, there are some qualitative insights worth mentioning.

First, if the deadweight cost of UI is lower than previously thought, it follows naturally that the optimal benefit level should be higher as well. The formal mechanism through which this occurs is best captured by the recent theoretical analysis of Crossley and Low (2004), who analyze how saving and borrowing constraints affect the optimal level of UI benefits. They show, quite intuitively, that tighter constraints make self-insurance a poorer substitute for a pooled UI system, and therefore raise the optimal benefit level. This paper essentially provides evidence that the Crossley-Low model should be calibrated with fairly tight constraints to assess the optimal level of UI.

Second, the results also shed some light on the optimal path of UI benefits. As Karni (1999) points out in his review of this recent and rapidly growing literature, a central incentive effect in these models is that the provision of benefits late in an unemployment spell induces agents to hold out and take advantage of the distorted price of leisure. The findings of this paper suggest that these substitution effects are small, at least for unconstrained individuals, and that incorporating binding borrowing constraints into the analysis could be a fruitful direction for further work in this area.

Third, the results inform the controversial debate on whether temporary income assistance programs should be means-tested (e.g., as in the United Kingdom). Browning and Crossley (2001) and Bloemen and Stancanelli (2003) find that UI does not smooth consumption for those who have high levels of pre-unemployment assets, a point in favor of asset-testing. However, UI does not appear to affect unemployment durations for this group either. Given that means-testing can generate additional distortions in saving behavior, a universal benefit could remain the best option.

Most importantly, the results of this paper call for more research on distinguishing income and substitution effects in the array of behavioral responses to social insurance programs that have been documented. This agenda is especially relevant given the rapid and continuing growth in social safety nets around the world.

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A Measurement of unemployment durations

The measurement of unemployment durations in the SIPP differs from that in the CPS because it requires tabulation of responses to questions about employment at the weekly level. This appendix describes the method used to compute durations, which follows Cullen and Gruber (2000).

The SIPP reports the employment status of every individual over 15 years old for every week that they are in the sample. Weekly employment status (ES) can take the following values:

1 .With a job this week
2 .With a job, absent without pay, no time on layoff this week
3 .With a job, absent without pay, spent time on layoff this week
4 .Looking for a job this week
5 .Without a job, not looking for a job, not on layoff

A job separation is defined as a change in ES from 1 or 2 to 3, 4, or 5. The duration of unemployment is computed by summing the number of consecutive weeks that ES $>=$ 3, starting at the date of job separation and stopping when the individual finds a job that lasts for at least one month (i.e. reports a string of four consecutive ES=1 or ES =2). Individuals are defined as being on temporary layoff if they report ES = 3 at any point in the spell. They are included as "searching" if they report ES = 4 at any point during their spell.

This method of computing durations results in a slightly different mean duration than that found in the CPS data. The mean spell in the SIPP lasts for 20.95 weeks before ending or being censored, whereas the US Department of Labor reports a mean duration of approximately 15 weeks. The official figure is computed from the length of ongoing spells for the cross-section of unemployed individuals who report they are looking for work in the CPS. The official definition therefore excludes the spells of individuals who become discouraged and stop searching for work. Unfortunately, these individuals cannot be identified in the SIPP because of the lack of reliable information on search behavior. At a weekly frequency, reports of job search are frequently interspersed with reports that the individual is not looking for a job; moreover, individuals often find jobs after reporting that they were not looking for one. Therefore, the only feasible measure of the length of an unemployment spell is to count the weeks from job separation to either job finding or censoring. While this is a valid definition of "unemployment," it should be distinguished from the more familiar measure, especially when the empirical results of this paper are compared to those of other studies.

TABLE 1a

Summary Statistics by Wealth Quartile in SIPP Sample

		Net Liquid Wealth Quartile			
	Pooled	1 (< -$1,115)	2 (-$1,115-$128)	3 ($128-$13,430)	4 (>$13,430)
Median Liq. Wealth	$1,763	$466	$0	$4,273	$53,009
Median Debt	$1,000	$5,659	$0	$353	$835
Median Home Equity	$8,143	$2,510	$0	$11,584	$48,900
Median Annual Wage	$17,780	$17,188	$14,374	$18,573	$23,866
Mean Years of Education	12.07	12.21	11.23	12.17	13.12
Mean Age	36.99	35.48	35.18	36.64	41.74
Fraction Renters	0.39	0.43	0.61	0.35	0.16
Fraction Married	0.61	0.64	0.59	0.60	0.63

TABLE 1b

Summary Statistics by Spousal Work and Mortgage Status in SIPP Sample

	Dual Earner?		Has Mortgage?	
	No (0.63)	Yes (0.37)	No (0.55)	Yes (0.45)
Median Liq. Wealth	$1,193	$3,001	$630	$4,855
Median Debt	$778	$1,357	$523	$1,725
Median Home Equity	$3,838	$15,801	$0	$30,421
Median Annual Wage	$16,472	$20,331	$15,946	$20,792
Mean Years of Education	11.84	12.46	11.88	12.53
Mean Age	35.33	39.79	35.96	38.66
Fraction Renters	0.44	0.30	0.71	0.00
Fraction Married	0.38	1.00	0.55	0.70

TABLE 2a

Hazard Model Estimates by Quartile of Net Liquid Wealth

	(1)	(2)	(3)	(4)	(5)
	Pooled	By Quartile of Net Liquid Wealth			
				Pre-wave	State
	Full cntrls	No cntrls	Full cntrls	Full cntrls	FE's
log UI ben	0.671				
	(0.132)				
Q1 x log UI ben		0.452	0.466	0.470	0.360
		(0.135)	(0.154)	(0.294)	(0.128)
Q2 x log UI ben		0.492	0.579	0.448	0.437
		(0.225)	(0.235)	(0.245)	(0.180)
Q3 x log UI ben		0.848	0.747	0.750	0.595
		(0.255)	(0.203)	(0.290)	(0.204)
Q4 x log UI ben		1.117	1.119	1.213	0.922
		(0.385)	(0.322)	(0.475)	(0.371)
Q1=Q4 p-val		0.043	0.045	0.245	0.024
Q1+Q2=Q3+Q4 p-val		0.012	0.052	0.050	0.028
Observations	83834	81307	75739	35291	75739

TABLE 2b

Additional Hazard Model Estimates by Quartile of Net Liquid Wealth

	(1)	(2)	(3)	(4)	(5)
	Low-wage	Maximum	Actual	Temp	Home
	Full cntrls	Benefits	Benefits	Layoffs	Equity
Q1 x log UI ben	0.376	0.466	0.619	0.471	0.722
	(0.119)	(0.154)	(0.109)	(0.151)	(0.453)
Q2 x log UI ben	0.473	0.579	0.582	0.583	0.866
	(0.199)	(0.235)	(0.098)	(0.223)	(0.456)
Q3 x log UI ben	0.874	0.747	0.593	0.788	0.709
	(0.192)	(0.203)	(0.086)	(0.208)	(0.437)
Q4 x log UI ben	1.250	1.119	1.049	1.156	0.882
	(0.585)	(0.322)	(0.296)	(0.334)	(0.362)
Q1=Q4 p-val	0.029	0.045	0.127	0.032	0.784
Q1+Q2=Q3+Q4 p-val	0.004	0.052	0.174	0.041	0.990
Observations	56107	75739	75739	80574	29549

NOTE-Coefficients reported are hazard ratios from a Cox hazard model. Standard errors clustered by state in parentheses.

TABLE 3a

Hazard Model Estimates by Spousal Work Status

	(1)	(2)	(3)	(4)	(5)
	Full sample No cntrls	Middle netliq Qs No cntrls	Middle netliq Qs Full cntrls	Middle netliq Qs State FE's	netliq Q=1 Full cntrls
Single earner x log UI ben	0.642	0.577	0.545	0.380	0.443
	(0.171)	(0.164)	(0.141)	(0.125)	(0.197)
Dual earner x log UI ben	0.735	1.116	1.148	0.637	0.482
	(0.210)	(0.510)	(0.485)	(0.231)	(0.218)
Single = Dual p-val	0.590	0.057	0.070	0.217	0.901
Observations	84363	40905	36828	36828	19130

TABLE 3b

Hazard Model Estimates by Mortgage Ownership

	(1)	(2)	(3)	(4)
	Full sample No cntrls	Full sample Full cntrls	Full sample State FE's	netliq Q <=2 Full cntrls
No mortgage x log UI ben	1.309	1.380	1.301	0.874
	(0.382)	(0.301)	(0.592)	(0.341)
Mortgage x log UI ben	0.377	0.392	0.384	0.212
	(0.160)	(0.164)	(0.186)	(0.141)
No mortg. = Mortg. p-val	0.002	0.005	0.008	0.047
Observations	37087	35291	35291	16656

NOTE-Coefficients reported are hazard ratios from a Cox hazard model. Standard errors clustered by state in parentheses.

Figure 1

Seam Effect: Unemployment Exit Hazards by Week

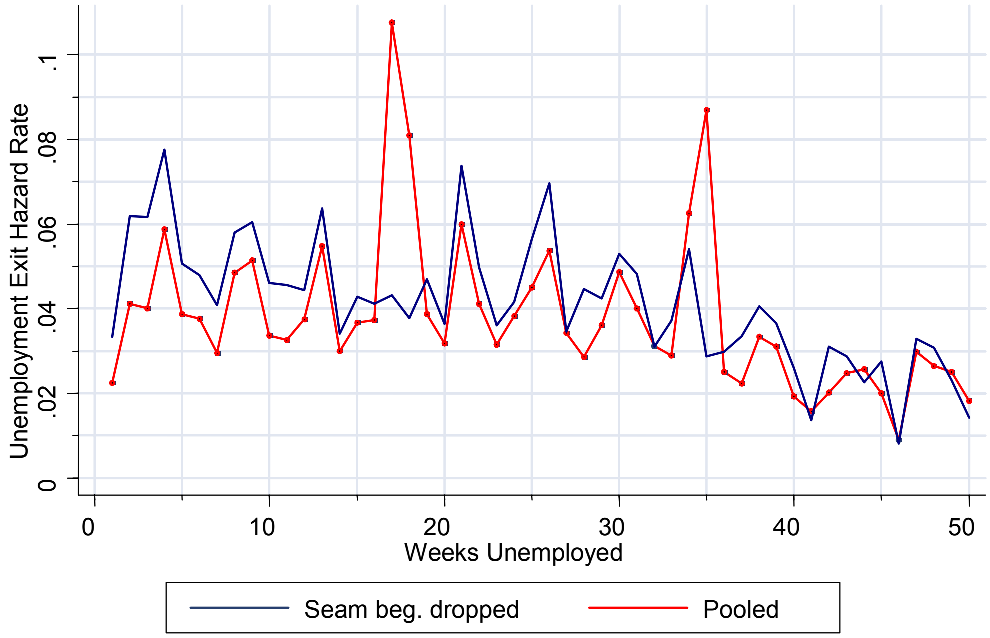

Figure 2a

Effect of UI Benefits on Durations: Lowest Quartile of Net Wealth

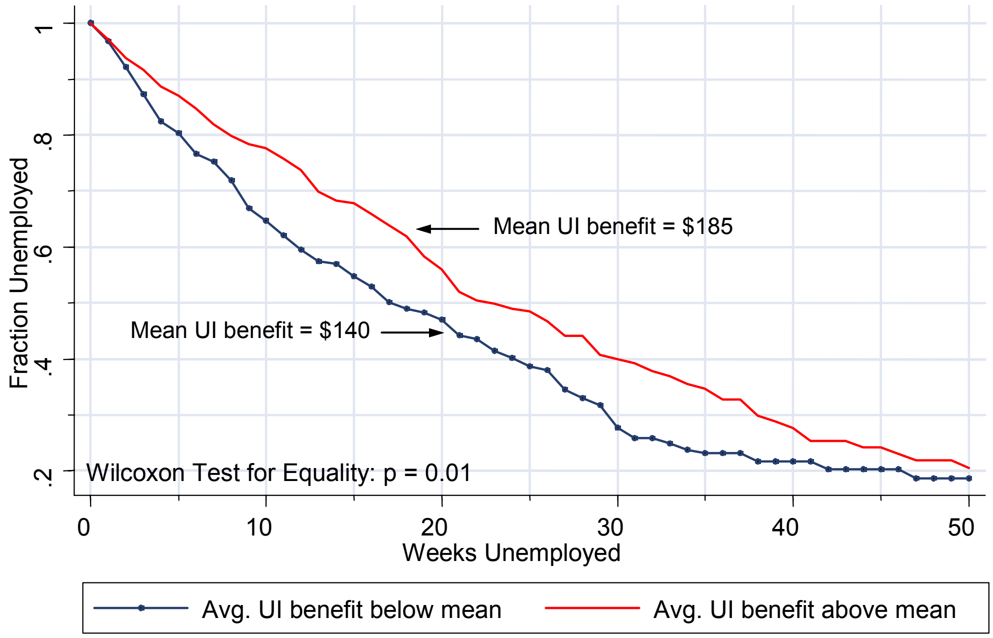

Figure 2b

Effect of UI Benefits on Durations: Second Quartile of Net Wealth

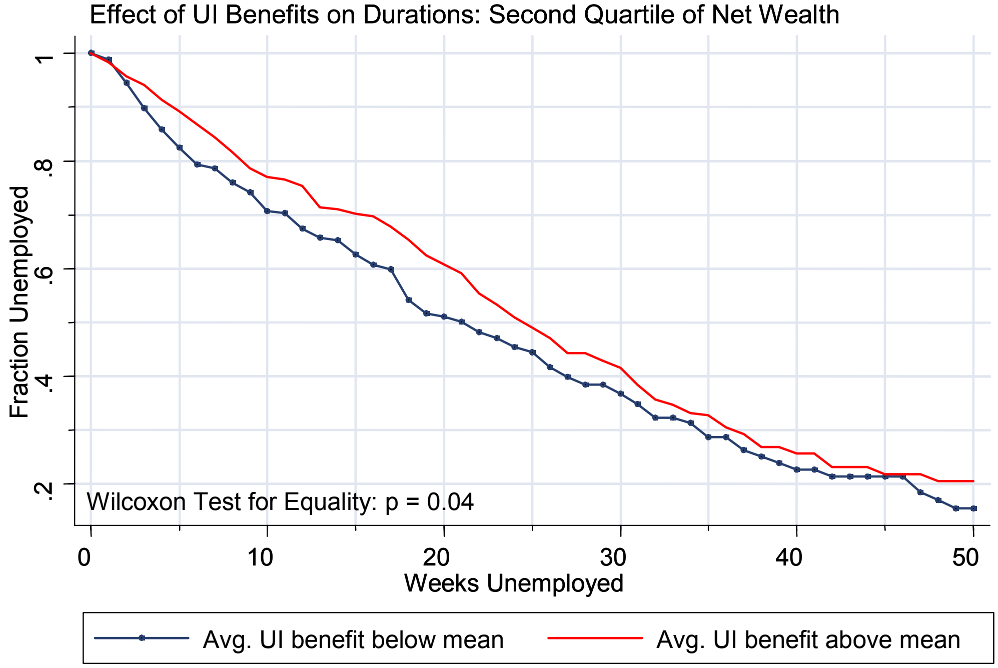

Figure 2c

Effect of UI Benefits on Durations: Third Quartile of Net Wealth

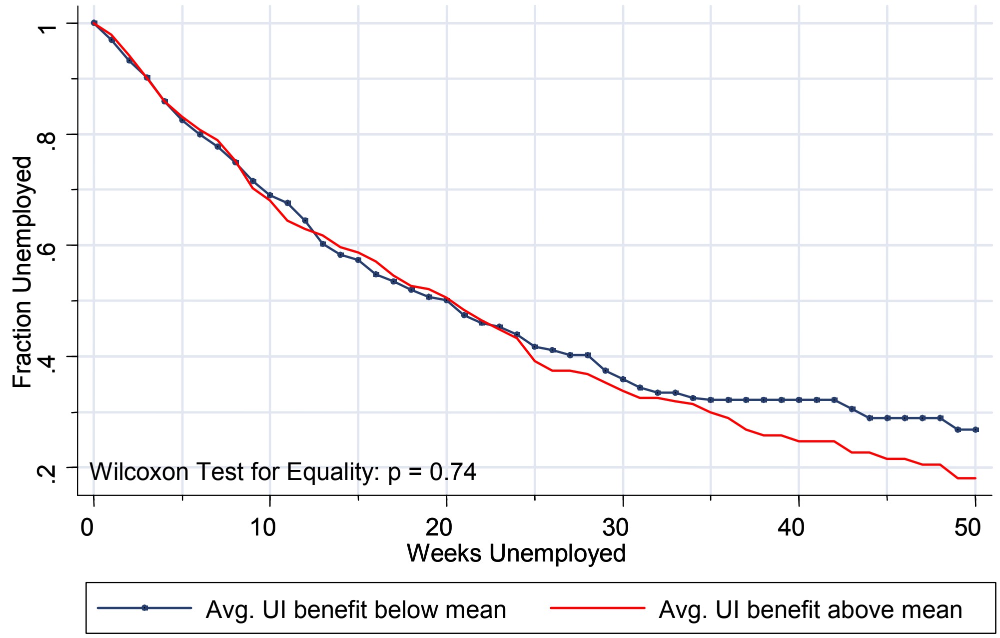

Figure 2d

Effect of UI Benefits on Durations: Highest Quartile of Net Wealth

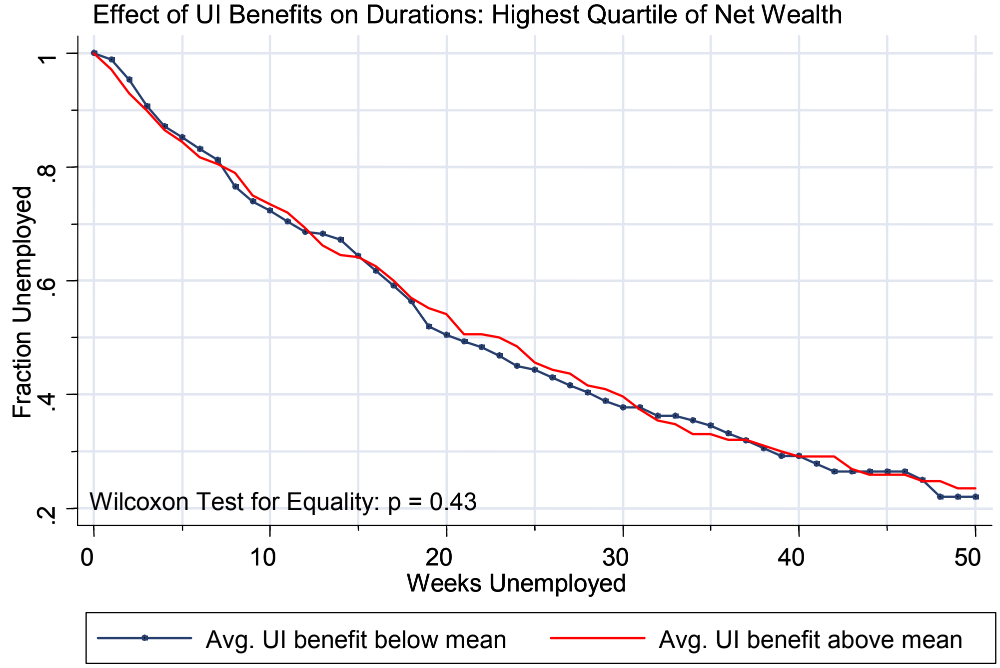

Figure 3a

Effect of UI Benefits on Durations: Single-Earner Households

Figure 3b

Effect of UI Benefits on Durations: Dual-Earner Households

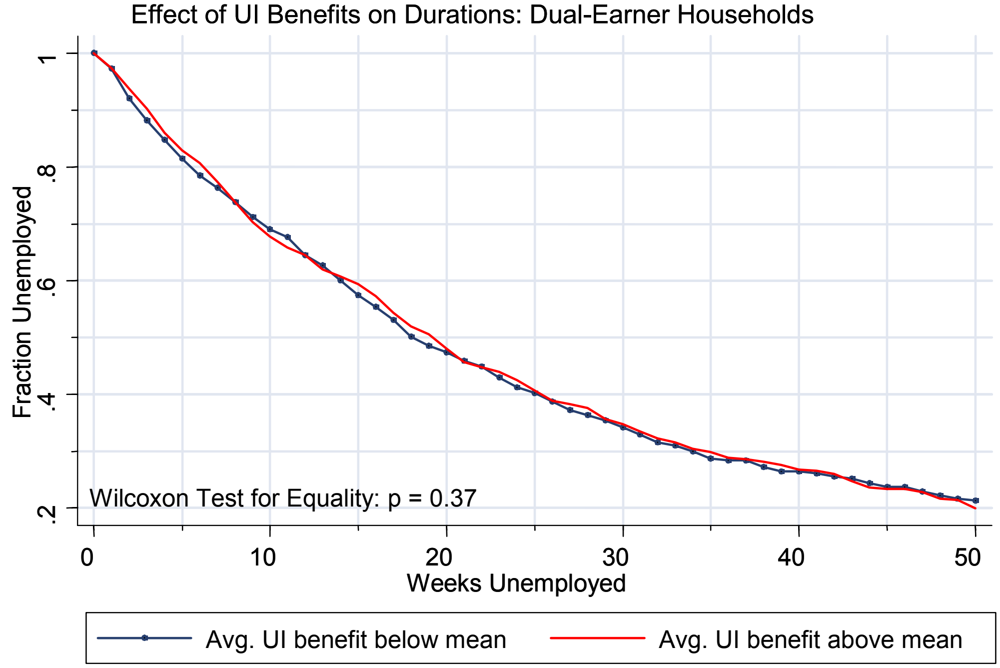

Figure 4a

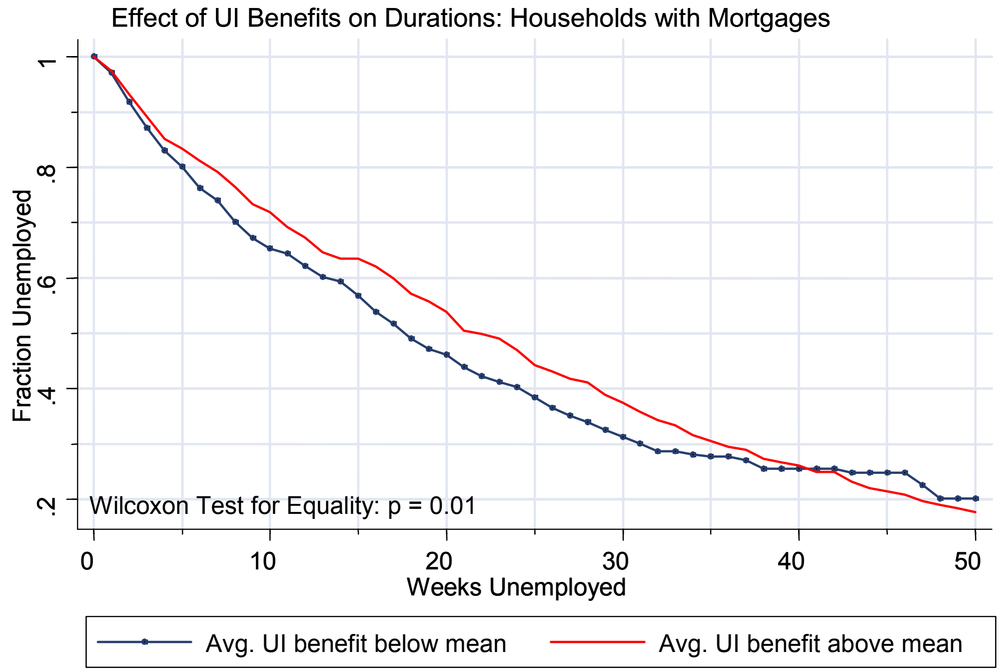

Figure 4b

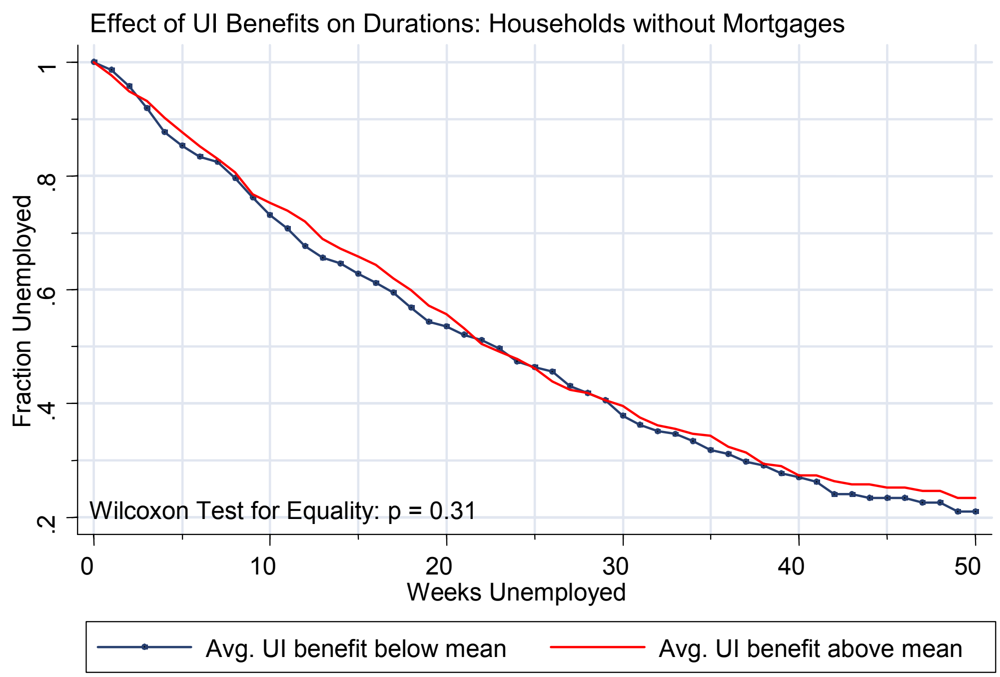

Figure 5

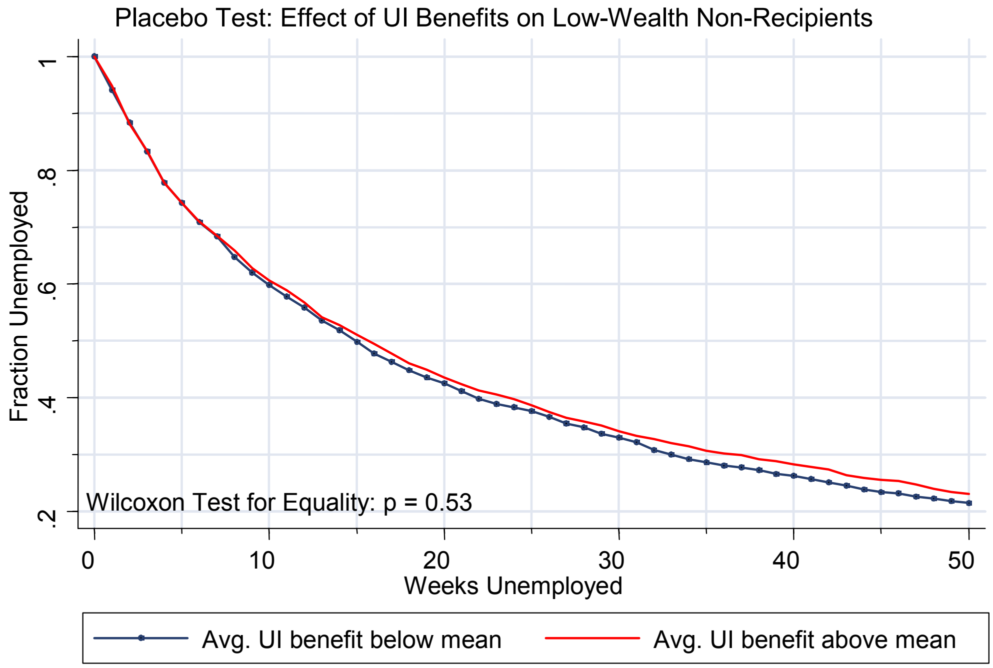
Placebo Test: Effect of UI Benefits on Low-Wealth Non-Recipients
Fraction Unemployed
.2
.4
.6
.8
1
0
10
20
30
40
50
Weeks Unemployed
Wilcoxon Test for Equality: p = 0.53
Avg. UI benefit below mean
Avg. UI benefit above mean

Figure 6

Effect of Severance Pay on Durations