Unemployment Durations and Extended Unemployment Benefits in Local Labor Markets *

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Abstract

Extended unemployment benefits programs are triggered by the state insured unemployment rate while intrastate demand conditions often vary dramatically. Some tight local labor markets may therefore exhibit a large adverse effect of extended unemployment benefits. Using a competing risk duration model, this paper measures the size of the entitlement effect across two labor markets facing dramatically different demand conditions. This exercise is important for evaluating potential benefits of proposed sub-state trigger extended benefits programs. The empirical results indicate that, in both recall and new job hazard, the entitlement effect is stronger in low unemployment labor markets. This finding is robust across a number of alternative specifications and econometric approaches. Implementing sub-state trigger extended benefits programs may therefore yield substantial benefits in terms of reducing the adverse incentives of unemployment insurance.

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1 Introduction

High unemployment rates in the early 1980's led to two temporary increases in the duration of unemployment insurance (UI) benefits. The first was provided under the Federal-State Extended Benefit (EB) program, which increased entitlement by 50% in states where the insured unemployment rate reached a statutory trigger. Longer entitlement was also available under Federal Supplemental Compensation (FSC), which operated nationally between 1982 and 1985 and authorised more benefit weeks in states with higher total unemployment rates. The rationale for these programs is that they direct benefits to high unemployment areas and should have a small adverse incentive effect. Longer entitlement offsets some of the impact of the recession and allows unemployed workers to wait until the economy improves, rather than forcing them into low wage jobs or onto welfare rolls. Even the more precisely targeted EB program, however, fails to exploit within-state variation in labor market conditions which is often greater than the between state differences.[1] According to job search theory longer entitlement subsidises job search and leads to longer durations of unemployment. There is no evidence (either theoretical or empirical) on how the value of the search subsidy changes with local demand conditions. In particular, the search subsidy could be larger in tight labor markets where ample employment opportunities exist. If this is the case, the adverse incentives of longer UI entitlement may be substantial in tight labor markets.

This paper compares the effect of increased UI entitlement on the duration of unemployment in two distinct labor markets subject to the same UI benefits but experiencing much different business conditions: Pittsburgh and Philadelphia, Pennsylvania from 1980 through 1985.[2] Figure 1 indicates the dramatic difference in the performance of each labor market during the study period. The recession was relatively mild in Philadelphia, with the unemployment rate reaching 9.9% compared to a national average of 10.7% in 1983. At the same time, however, structural changes in steel and other durable manufacturing industries pushed the Pittsburgh unemployment rate to 16.9%. In one Pittsburgh area county, the unemployment rate reached a depression-like level of 30%. The timing of the extended benefits programs is also notable. FSC began in September 1982 when the unemployment rate in

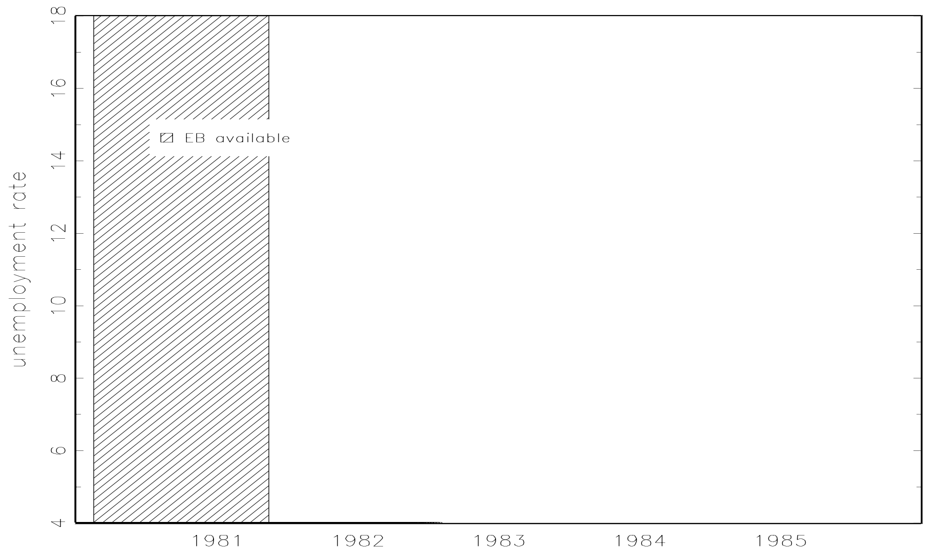

Figure 1: Unemplo

Partnership Act (JTPA) and the feasibility of substate triggers for EB has been studied by the US Department of Labor.[5] This paper complements the analysis of implementing substate triggers by measuring the effect of extended benefits on the duration of unemployment in tight and slack labor markets. Using a competing risk hazard model, which separately estimates the duration of unemployment spells ending in recall and those ending in new jobs, we quantify how unemployed workers and employers respond to changes in UI entitlement and contrast the estimated effects across the two labor markets.

Our competing risk hazard estimates are the first to be based entirely on administrative data. Survey based data used in previous studies (e.g. Katz 1986 and Katz and Meyer 1990a) is likely to be less accurate in measuring the duration of unemployment spells. For example, Katz and Meyer (1990a) note the poor quality of survey responses on weeks of compensated unemployment and on the duration of unemployment compared to the same information in the administrative UI records. We augment the administrative UI data with quarterly earnings records reported by each employer for each worker. Employer information on earnings data distinguishes unemployment spells ending in new jobs from those ending in a recall and indicates when unemployment ends for a person who has exhausted benefits. This characteristic of the data allows us to track individuals over long spells without relying on survey data.

Two additional features make the data particularly useful for measuring the impact of UI entitlement on the duration of unemployment. First, extra weeks of benefits under EB and FSC programs, along with a reduction in regular benefits from 30 to 26 weeks, provide substantial variation in UI entitlement needed to separate the effect of entitlement from duration dependence. Second, variation in UI entitlement independent of demand conditions helps disentangle the influence of remaining weeks of benefits from demand effects. For example, longer entitlement under EB occurs when demand conditions deteriorate, which confounds the entitlement effect with the impact of high unemployment rates.[6] Differences in unemployment rates across labor markets subject to the same UI coverage and a widening gap between the IUR (used to trigger EB) and the TUR (which more accurately reflects demand conditions) provide leverage to isolate the entitlement effect from the demand effect.[7]

Our results indicate that UI entitlement depresses the new job and recall hazards more in low unemployment labor markets than in high unemployment areas. As exhaustion approaches, those unemployed in a depressed area find new jobs faster. Further, both hazards increase sharply in the period benefits end. The spikes in both of the hazards at the moment of exhaustion are much larger than those reported in previous studies and are comparable across the two labour markets, suggesting extensive strategic use of entitlement even in depressed labor markets. These findings are robust across a number of alternative specifications (using both cross-sectional and time variation in unemployment rates) and econometric approaches, including those accounting for unobserved heterogeneity.

The paper is organized as follows. Section 2 gives a detailed account of the data set and provides descriptive statistics of the variables of interest in each region, based on whether a spell of unemployment ends in a new job or recall. Empirical hazard estimates for each type of transition out of unemployment are also presented in this section. Section 3 develops the econometric model and presents two types of hazard function estimates. A likelihood ratio test suggests splitting the sample of Pittsburgh and Philadelphia UI claimants. Hence, we first estimate the hazard functions for each labor market separately and then, within each labor market, we allow the entitlement effect to vary across demand conditions. Section 4 qualifies our findings, discusses UI policy implications of our results and offers suggestions for further research.

2 Data Description

The data set is a 1% random sample of claimants for UI benefits from Pennsylvania. The information was collected under the Continuous Wage and Benefit History (CWBH) program. The CWBH data includes an administrative record detailing the claimants initial entitlement, weekly benefit amount, the number of weeks claimed, and individual characteristics such as race, sex, and county of residence. The CWBH also includes responses to a questionnaire administered at the time of a claim which reports education, marital status, and other family income. The survey was a victim of federal budget cuts and ended in August 1984.

Claims after this date contain survey information only if the worker had a prior claim. The study period includes claims between January 1980 and December 1985. This covers six full years and avoids problems of seasonality arising from a short sample, as noted in Katz and Meyer (1990a).

The CWBH data has been used to study the duration of unemployment by Moffitt (1985), Katz and Meyer (1990a,1990b) and Meyer (1990). Unfortunately, administrative records follow claimants for only as long as they collect UI. No information is available after benefits lapse. Furthermore, the CWBH data cannot distinguish spells ending in a new job versus those ending in a recall. We overcome this deficiency by appending quarterly wage records (collected by the Pennsylvania Department of Labor and Industry) to the administrative data. Wage records are reported by each employer covered by the UI law and are used to determine eligibility and the amount of benefits. Wage records extend from the second quarter of 1979 through the first quarter of 1986 and contain quarterly earnings, weeks worked, and the principal industry of operation. An employer identification number distinguishes recalled workers from those who change jobs. Wage records also determine when those who exhaust benefits return to work. This is an important feature of the data set, since over 21% of all claimants, including 30% of job changers, exhaust their UI entitlement.[8]

Claims data differs from spell data. Initially, laid off workers file for UI benefits, which begins a 52 week benefit year. Subsequent spells of unemployment within this time period must draw benefits from unused entitlement, including EB or FSC, before another claim can be established. We restrict our analysis to the first spell of unemployment within a claim. While this undersamples spells from cyclical and seasonal industries, it has the advantage of precisely determining the start of each spell. It also more accurately measures the remaining entitlement, since workers on a subsequent spell within a benefit year may have sufficient earnings credits to open another valid claim if and when current benefits lapse. The result is a highly accurate record of the earnings and unemployment experience of a large number of workers who filed for unemployment benefits during a particularly sharp recession. While the data has the drawback of including only Pennsylvania workers, it is extremely well suited to our purpose of examining the differential effects of extended benefits in tight and slack labor markets. Furthermore, even though it is not representative of the entire nation, it is large and free of the survey response problems encountered in some previous competing risk studies (e.g. Katz and Meyer 1990a). We focus on claims from the Philadelphia and Pittsburgh Primary Metropolitan Statistical Areas (PMSA).[9] As noted above, these areas had dramatically different unemployment rates in the sample period 1980-1985.[10] Further, the rapid reduction of the Pittsburgh unemployment rate after 1983 creates the misconception of improving labor market conditions. Actually, employment in Pittsburgh fell by 1.4% from 1983 to 1985, a decline which was masked by a 4.8% drop in the labor force. At the same time employment grew by 7.2% in Philadelphia.

The relatively large labor markets, combined with the deep recession, result in 7750 spells of compensated unemployment (representing 1% of all claimants). Deleting observations with missing variables and omitting left censored spells[11] reduces the sample size to 6658 spells for 5134 individual workers. Nearly as many spells end in a new job as in a recall, and 12.3% are censored.[12] The average duration of an unemployment spell is about five months. Table 1 reports the means for selected variables by reemployment outcome in each labor market.[13] Differences in the unemployment experiences and claimant characteristics for recall and new job transitions (see Table 1) are similar to those noted by Katz (1986) and Katz and Meyer (1990a). Short spells usually end in a recall, while younger workers, women and the unmarried are more likely to change employers. Further, claimants who find new jobs earned less on their previous jobs than recalled workers, and this gap is more pronounced in the Pittsburgh area. The potential duration of UI entitlement and the unemployment rate at the start of an unemployment spell are similar for job changers and recalled workers.

A comparison of the two labor market areas reveals substantial differences. Pittsburgh claimants are more likely to be white, married and male. The unemployment rate at the start of their spells is below the 5 year Pittsburgh average of 12.5%, and their base period earnings are high compared to the Philadelphia unemployed. Recall is more prevalent in Pittsburgh, while new job findings are more likely to occur in Philadelphia. In Pittsburgh, censored spells end at short durations compared to Philadelphia.

The data set exhibits unusually high variation in entitlement. This is partly due to

Table 1: Individual and Spell Characteristics

Pittsburgh

Spell Type	New Job	Recall	Censored
Duration in Weeks	25.8 (18.9)	14.3 (14.0)	25.8 (18.9)
Age	34.9 (11.6)	38.8 (11.8)	39.3 (13.1)
Male	0.73	0.81	0.67
Married	0.44	0.55	0.33
White	0.91	0.92	0.85
Base Period Earnings	13740. (8270.)	16954. (8696.)	13593. (8774.)
UI Benefits	144.1 (49.2)	159.5 (40.2)	141.2 (46.9)
Initial UI Entitlement	38.6 (7.06)	38.1 (7.63)	34.9 (8.89)
Unemployment Rate	10.9 (3.79)	11.56 (4.22)	11.1 (4.46)
Number of Spells	1153	1553	356

Philadelphia

Spell Type	New Job	Recall	Censored
Duration in Weeks	22.4 (17.0)	12.2 (12.6)	32.4(18.2)
Age	34.3 (11.4)	38.4 (12.1)	38.0 (11.9)
Male	0.64	0.68	0.60
Married	0.31	0.42	0.26
White	0.78	0.72	0.70
Base Period Earnings	12454. (7716.)	13972. (7875.)	12467. (8585.)
UI Benefits	137.7 (48.8)	147.9 (45.4)	135.2 (47.8)
Initial UI Entitlement	37.9 (7.61)	37.2 (7.61)	34.5 (8.88)
Unemployment Rate	7.78 (1.92)	7.87 (1.99)	7.78 (2.08)
Number of Spells	1739	1394	463

Standard errors in parenthesis. Earnings and UI Benefits are in 1992 dollars.

the combined effects of the EB and FSC extensions and changes in the state's UI laws (reducing regular benefits from 30 to 26 weeks at the beginning of 1984). These changes and extensions resulted in four different initial entitlement levels for workers who qualified for UI compensation. The EB program extended the available entitlement by 50% up to a maximum of 39 weeks. The FSC was extended several times and increased UI compensation by up to 26 more weeks of entitlement. Moreover, EB triggers and FSC authorizations often changed the available remaining entitlement while a spell of unemployment was in progress.[14] Over 75% of the spells started when extended benefits were available, and more than 19% of the spells were in progress while one of the extended benefits programs increased entitlement. On the other hand, about 13% of the claimants experienced a within-spell reduction in benefit weeks when programs triggered off. Using the dates of extended benefits programs to change the value of remaining entitlement within a spell helps to precisely determine the actual exhaustion dates. Variation in the dollar amount of weekly UI benefits comes mostly from variation in base period earnings and from the existence of maximum and minimum benefit levels.

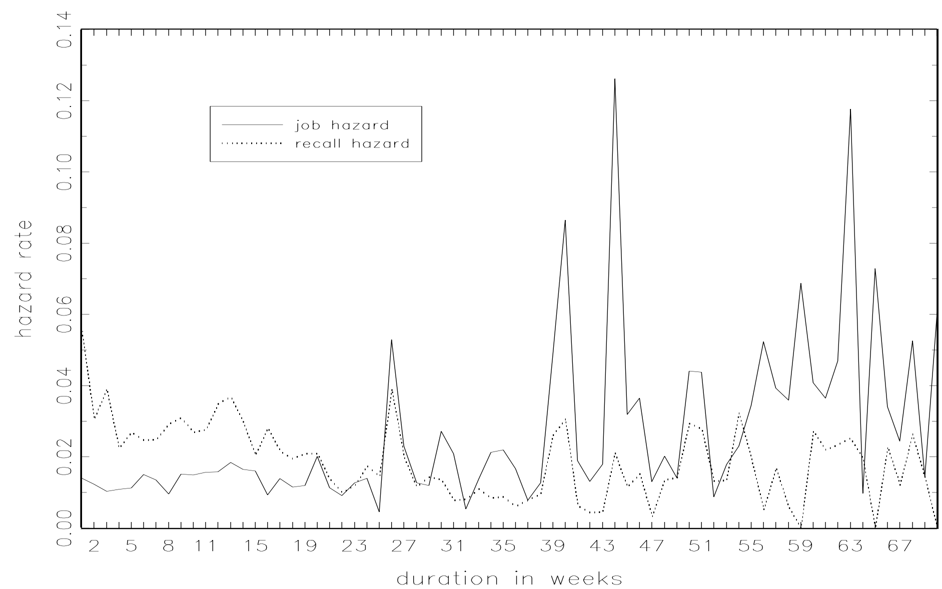

Figure 2: Empirical Hazards for Competing Risks in Pittsburgh

Figure 2 shows the Kaplan-Meier empirical hazards for the first 70 weeks of unemployment for the sample of Pittsburgh claimants. The estimate in a given week is the proportion of the number of unemployed who make a particular type of transition to the number of those who are still unemployed in that week. Reemployment outcomes vary with the duration of unemployment. Shorter spells usually end in a recall, while spells lasting at least six months are more likely to end in a new job.[15] Spikes in the new job hazard coincide with the potential duration of entitlement under one or more of the extended benefits programs. Figure 3 presents Pittsburgh empirical hazards based on time before exhaustion as opposed to time since a spell has begun. There is a very large spike in the hazard at the week benefits lapse (corresponding to time 0). Nearly 25% of the unemployed exhausting their UI benefits find jobs in the next week, and another 12% are rehired by their previous employer. Both the new job and recall hazards are at a relatively low level in the weeks immediately preceding exhaustion, and increase by a factor of 12 and 6 respectively in the week benefits lapse.

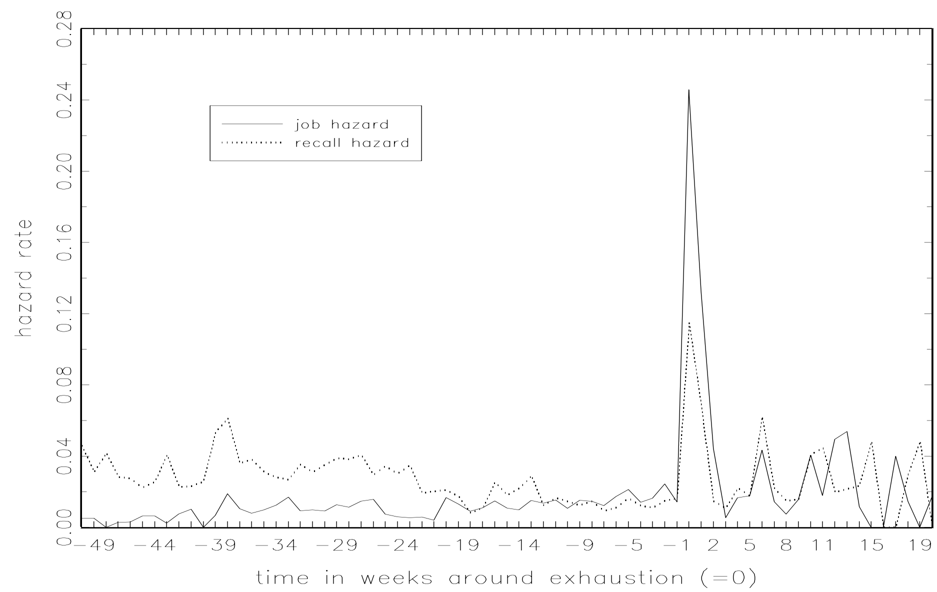

Figure 3: Empirical Hazards around Exhaustion for Competing Risks in Pittsburgh

In Philadelphia, on the other hand, the higher likelihood of a recall in short spells is not as pronounced as in Pittsburgh. Figure 4 reports the Philadelphia empirical hazards and also suggests that new job findings occur more often in spells lasting at least six months. Spikes in the new job and recall hazard again coincide with the potential duration of entitlement, and in spite of the large differences in labor market conditions between Philadelphia and

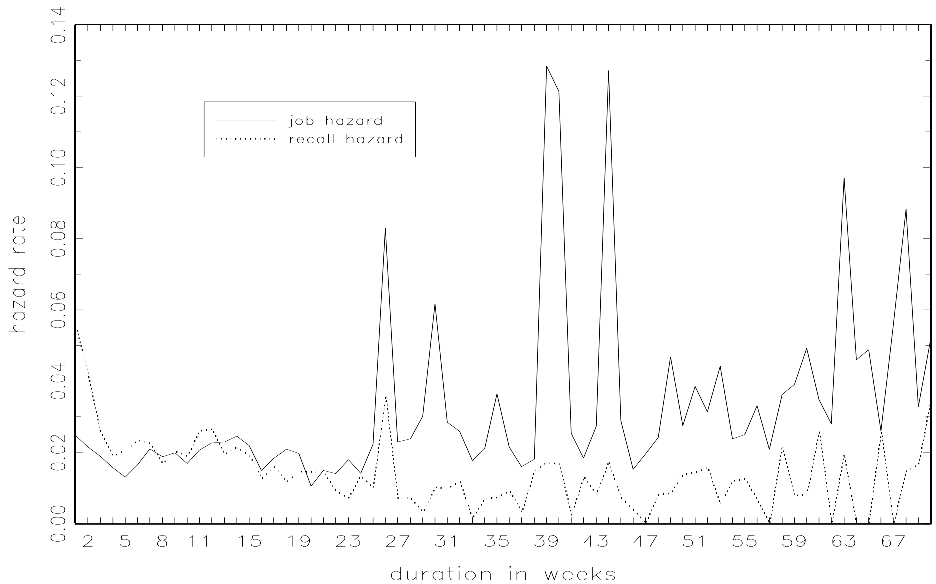

Figure 4: Empirical Hazards for Competing Risks in Philadelphia

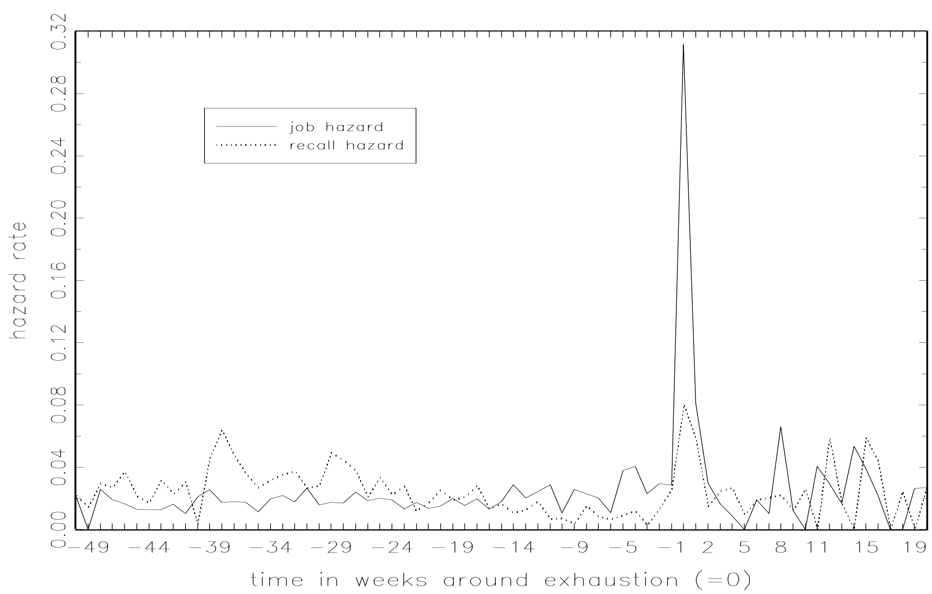

Figure 5: Empirical Hazards around Exhaustion for Competing Risks in Philadelphia

Pittsburgh, Figure 5 reveals exhaustion spikes of similar magnitude. The recall hazard is depressed in the weeks immediately preceding exhaustion and more than doubles in the week benefits lapse. The new job hazard rate rises to the dramatic 32% spike from a little above 3% in the preceding week. Nearly one third of those unemployed in Philadelphia at the exhaustion week find a new job and another 8% are recalled.

The high exit rates at exhaustion serve as persuasive evidence of the strategic use of compensated unemployment by both workers and firms. They also indicate that strategic use of entitlement is important even in very depressed labor markets. The exhaustion spikes in Figures 3 and 5 are substantially larger than those that Katz and Meyer (1990a) find with data that relies on surveys to date when unemployment ends. The magnitude of our exhaustion spikes may be the result of more accurate data and/or the severity of the recession.

In order to collect the EB or FSC benefit extensions, the unemployed have to first exhaust their regular UI benefits. One of the most important findings is that relatively few claimants who collect benefits under either EB or FSC leave unemployment before the benefit extensions end. In Philadelphia, 82.4% of those who collected benefits under EB and/or FSC exhausted all benefits compared to 79.8% in the depressed Pittsburgh region. Collecting extended benefits therefore strongly predicts prolonged spells of unemployment in both slack and tight labor markets.

3 Estimation and Results

3.1 Duration Models

We use a competing risk hazard model for new job and recall hazards. The new job hazard is typically motivated by job search theory, with the hazard equalling the probability that a wage offer is received times the probability that it is acceptable.[16] The resulting estimate can be interpreted as an approximation to comparative statics implied by a corresponding model of job search.

A *hazard* function $\lambda_j(t, x_t)$ is defined as the probability of leaving unemployment by method j at duration t (conditional upon staying there up to duration t) for someone with person specific characteristics x_t. One can leave unemployment for a $n$ew job or for a $r$ecall, i.e. $j \in \{r, n\}$. This is often referred to as a competing risk model. We work in discrete time measured in weeks and use a logit specification:

$$\lambda_j(t, x_t) = \frac{1}{1 + e^{-h_j(t,x_t)}}, \tag{1}$$

where

$$h_j(t, x_t) = r_j(e_t, \alpha_j) + \beta_j' z_t + g_j(t, \gamma_j) + \theta. \tag{2}$$

Here, $r_j(e_t, \alpha_j)$ denotes a function of remaining entitlement e_t, the vector z_t includes levels of benefits, wages, demographics and time changing demand measures, and $x_t' = (e_t, z_t')$. Further, θ is a constant and $g_j(t, \gamma_j)$ is a function capturing the duration dependence.[17]

In a competing risks specification with new job and recall hazards, the probability of an individual being recalled at duration t is

$$L^r(t) = \lambda_r(t, x_t) \prod_{v=1}^{t-1} [1 - \lambda_r(v, x_v)][1 - \lambda_n(v, x_v)],$$

where λ_r and λ_n denote the recall and new job hazards respectively. The likelihood contribution for someone finding a new job is similar.

For an unemployment spell which is still in progress at the end of our sampling frame (i.e. no transition out of unemployment has been observed until duration T), the likelihood contribution is the survivor function

$$S(T) = \prod_{v=1}^{T} [1 - \lambda_r(v, x_v)][1 - \lambda_n(v, x_v)].$$

The sample likelihood then equals the product of individual likelihood contributions. However, in the presence of unobserved person specific characteristics affecting the probability of exit, all of the estimated coefficients will be biased. We control for the unobserved heterogeneity using the nonparametric maximum likelihood estimator (Heckman and Singer 1984). Our specification of the heterogeneity distribution follows McCall (1996) and allows for correlation of unobservables across the two estimated hazards. See Appendix A for more details on this approach.

3.2 Results

This paper measures the effect of extended UI entitlement programs across labor markets facing different demand conditions. We use a flexible parameterization of entitlement–a step function in the weeks of remaining eligibility. Each step equals 1 when remaining entitlement falls within the step boundaries and equals 0 otherwise. The break points for the steps are chosen to encompass approximately 20% of the weekly observations[18] except for the last two, which are strongly suggested by the empirical hazards in Figures 3 and 5 . The next to last step includes the remaining entitlement between 1 and 3 weeks, and the last step equals 1 in the week of exhaustion and the first following week. The step function is normalised to those with two or more weeks of unemployment following exhaustion. The set of explanatory variables also covers demographic characteristics (including industry dummies), local and person-specific measures of demand conditions (including the regional unemployment rates discussed in section 2), previous employment variables, year dummies, and a relatively parsimonious step function in duration to control for duration dependence.[19]

3.2.1 New Job Hazard Entitlement Effect

Table 2 reports the sensitivity of the new job hazard to UI compensation. Our first estimates are based on the pooled sample of Pittsburgh and Philadelphia claimants. Variation in UI entitlement independent of demand conditions purges the influence of remaining weeks of entitlement from the demand effect. Column (1) reports the entitlement step function and the weekly UI benefits, as well as the effect of the regional unemployment rate. The precisely estimated coefficients indicate that entitlement depresses the new job hazard for those with at least four weeks of eligibility. The effect is larger, i.e. more negative, for those with longer entitlement, and workers are more likely to find new jobs in the weeks just before exhaustion. The parameter estimates indicate a tenfold cumulative increase in the hazard as one moves from the maximum entitlement to the exhaustion week.[20] Such an increase is consistent with the empirical hazards in Figures 3 and 5. On the other hand, the entitlement coefficients are normalized to the time after exhaustion and using this comparison, the hazard almost doubles as we move from any of the three largest entitlement brackets to the period after exhaustion. Finally, higher unemployment and a higher amount of weekly benefits, controlling for previous earnings, significantly depress the new job hazard.

In columns (2) and (4) we split the sample and estimate separate hazard functions for Pittsburgh and Philadelphia. The likelihood ratio test comparing the pooled-sample and split-sample results suggests using the latter.[21] These estimates, based on the divided sample, allow us to compare the entitlement effect across the two labor markets. For all entitlement steps, the effect is larger, i.e. more negative, in Philadelphia, where the average unemployment rate was about 5 percentage points lower. For example, the Philadelphia hazard improves by 110% as one moves from the highest entitlement bracket to the time after exhaustion. The same movement translates into an 80% increase in the Pittsburgh hazard. Further, impending exhaustion (1 to 3 weeks of remaining entitlement) leads the unemployed in Pittsburgh to find new jobs, but has no influence on the Philadelphia unemployed. This large difference when remaining entitlement is between 1 and 3 weeks shows that workers in relatively depressed labor markets react more rapidly to a nearing lapse of benefits. Similarly, the exhaustion spike is larger in Pittsburgh. Even though the differences in the coefficient estimates are seldom statistically significant, the pattern of the estimates suggests that (i) large values of entitlement depress the Philadelphia hazard more, and (ii) nearing exhaustion leads the high-unemployment Pittsburgh claimants to find new jobs, while Philadelphia unemployed wait until the week benefits lapse. Adding unmeasured heterogeneity in columns (3) and (5) slightly decreases most of the entitlement coefficients in Pittsburgh but has virtually no impact in Philadelphia. We use a 2-tuple heterogeneity distribution (McCall 1996), which allows the unobserved factors from the two hazards to be correlated and requires a joint estimation procedure.[22] Estimated sample likelihoods strongly support including unmeasured heterogeneity.

Table 7 in Appendix B presents the effects of a subset of other variables in the new job hazards without heterogeneity reported in Table 2. In both regions, the new job hazard is higher for men, whites and workers under 25 years old. Higher industry-level unemployment rates depress new job transitions, while workers in industries experiencing employment growth are more likely to find new jobs. The latter effect is precisely estimated in the

Table 2: New Job Hazard Function Estimates

Sample	Pooled	Pittsburgh		Philadelphia	
Heterogeneity	No	No	Correlated	No	Correlated
Variable	(1)	(2)	(3)	(4)	(5)
Log Weekly Benefits	-0.241**	-0.342**	-0.395**	-0.152	-0.152
	(0.105)	(0.161)	(0.173)	(0.141)	(0.142)
Entitlement 37 and over	-0.742***	-0.602*	-0.595	-0.837***	-0.837***
	(0.204)	(0.331)	(0.366)	(0.266)	(0.262)
28 to 36	-0.683***	-0.569**	-0.518*	-0.768***	-0.767***
	(0.179)	(0.286)	(0.311)	(0.234)	(0.226)
19 to 27	-0.649***	-0.638***	-0.610***	-0.684***	-0.684***
	(0.154)	(0.246)	(0.256)	(0.201)	(0.194)
04 to 18	-0.426***	-0.335*	-0.321*	-0.513***	-0.513***
	(0.119)	(0.181)	(0.193)	(0.158)	(0.152)
01 to 3	0.253**	0.437***	0.443***	0.0895	0.089
	(0.114)	(0.167)	(0.176)	(0.157)	(0.159)
-1 to 0	2.61***	2.69***	2.70***	2.52***	2.51***
	(0.106)	(0.158)	(0.164)	(0.144)	(0.148)
Unemployment Rate	-0.059***	-0.055***	-0.0604***	-0.053	-0.0537
	(0.0110)	(0.013)	(0.014)	(0.040)	(0.0413)
Log-Likelihood	-12563.	-5076.5	-11894.7	-7448.4	-13758.5

Standard errors in parentheses. All specifications include a standard set of regressors reported for columns (2) and (4) in Table 7 in the appendix.
$*$ denotes significance at 10% level;
$**$ denotes significance at 5% level;
$***$ denotes significance at 1% level

Table 3: New Job Hazard Function Estimates with Interactions

Sample	Pittsburgh				Philadelphia			
Heterogeneity	No		Correlated		No		Correlated	
Variable	(1)		(2)		(3)		(4)	
Log Weekly Benefits	-0.357**	(0.161)	-0.350**	(0.169)	-0.155	(0.141)	-0.156	(0.142)
Entitlement 37 and over	-1.855	(0.743)	-0.183***	(0.679)	-3.916***	(0.913)	-3.919***	(1.003)
28 to 36	-2.215***	(0.662)	-0.219***	(0.587)	-3.048***	(0.858)	-3.05***	(0.924)
19 to 27	-2.55***	(0.633)	-0.253***	(0.593)	-3.01***	(0.787)	-3.011***	(0.855)
04 to 18	-1.947***	(0.572)	-0.193***	(0.51)	-2.961***	(0.719)	-2.963***	(0.823)
01 to 3	-1.947***	(0.606)	-0.192***	(0.571)	-4.952***	(0.888)	-4.955***	(0.957)
-1 to 0	2.017***	(0.566)	2.027***	(0.519)	2.37***	(0.752)	-2.37***	(0.875)
Unemployment Rate	-0.171***	(0.043)	-0.169***	(0.037)	-0.350***	(0.095)	-0.350***	(0.104)
Entitlement 37 and over	0.088*	(0.052)	0.087*	(0.046)	0.378***	(0.112)	0.378***	(0.123)
28 to 36	0.120**	(0.049)	0.119***	(0.043)	0.283***	(0.110)	0.283**	(0.118)
19 to 27	0.151***	(0.049)	0.150***	(0.045)	0.296***	(0.103)	0.296***	(0.112)
04 to 18	0.129***	(0.045)	0.128***	(0.039)	0.321***	(0.095)	0.321***	(0.109)
01 to 3	0.191***	(0.047)	0.190***	(0.043)	0.661***	(0.113)	0.661***	(0.123)
-1 to 0	0.496	(0.046)	0.049	(0.040)	-0.001	(0.100)	-0.001	(0.118)
Log-Likelihood	-5062.9		-11866.4		-7416.7		-13713.1	

Standard errors in parentheses. All specifications include the same set of regressors as in Table 7.
$*$ denotes significance at 10% level;
$**$ denotes significance at 5% level;
$***$ denotes significance at 1% level

Philadelphia region. Table 7 also reports the effects of abrupt within-spell changes in entitlement owing to the start and end of extended coverage programs. Specifically, we include dummy variables equal to 1 in the four-week period starting in the week entitlement increases or decreases.

The "trigger" variables are also interacted with the unemployment rate and remaining eligibility. Since we simultaneously control for the changing level of entitlement, the trigger variables capture the "surprise" effect of changes in entitlement separate from the change in entitlement itself. Conditional on the actual entitlement, triggering off the benefits leads the unemployed to find new jobs more quickly. This impact is statistically significant in the Pittsburgh labor market. Such a finding suggests that workers are surprised by the sudden lapse of benefits.[23] As expected, this impact is stronger when few weeks of compensation remain. The surprise effect of additional weeks of entitlement (trigger "on") is not statistically significant in either of the regions. Finally, Table 7 in the appendix also contains the baseline hazard estimates.

While the comparison of the slack and tight labor market reveals differences in the level of the entitlement effect, we can also use the temporal variation in unemployment to measure how entitlement effect changes within each labor market as demand conditions change.[24] The entitlement-unemployment rate interactions in columns (1) and (3) of Table 3 show that the entitlement effect varies with local demand conditions in both regions.[25] The estimates from both labor markets suggest that the negative effect of entitlement on new job finding is weaker, i.e. closer to zero, when unemployment rates are high. Except for the exhaustion spike, all of the interactions are precisely estimated. This result is consistent with the unemployed in tight labor markets using entitlement to search longer since the returns to search can increase with the availability of jobs. Alternatively, the unemployed in depressed labor markets take any job they find. The sharp increase in the interaction coefficient for those with one-to-three weeks of eligibility shows that incentives to leave unemployment are strongest among those in high unemployment areas when benefits are about to expire. More unemployed workers in tight labor markets may be able to arrange to begin working as soon as benefits expire or are confident in their ability to find a new job as soon as benefits end.

To evaluate how sensitive the entitlement effect is to changes in the unemployment rate, one has to account for how the hazard is affected by both the changing unemployment rate and its entitlement interactions. The estimates imply that a given decrease in the unemployment rate results in a dramatically larger increase of the entitlement effect in the low-unemployment Philadelphia region. Consider, for example, the total change in the hazard as one moves from the largest entitlement bracket to the period after exhaustion. We evaluate this effect in each labor market at the local average of the unemployment rate, and then we decrease the unemployment rate by 2.5%. The total *change* in the hazard increases by approximately 25% in Pittsburgh as the unemployment rate decreases from the 12.5% to 10% level. In contrast, reducing the unemployment rate in Philadelphia from 7.5% to 5% results in a 144% increase of this entitlement effect in Philadelphia.[26]

We obtain similar pattern of estimates when we allow for interaction of unemployment rates and duration. The results are available from the authors on request. Such interactions are potentially important as they allow the influence of duration to change with local demand conditions. As the entitlement and duration are closely correlated, such specifications can purge the unemployment rate-duration effect from the unemployment rate-entitlement effect.[27] Finally, columns (2) and (4) of Table 3 report the estimates after controlling for unobserved heterogeneity. Even though the estimated sample likelihood again strongly supports including unobserved heterogeneity, the estimates with a 2-tuple heterogeneity distribution are virtually identical to those without heterogeneity.

3.2.2 Recall Hazard Entitlement Effect

The importance of recall for unemployment spells has been well documented by Katz (1986) and Katz and Meyer (1990a, 1990b). Our sample has about as many spells ending in recall as in a new job. Again, we start by estimating the recall hazard of the pooled sample of Pittsburgh and Philadelphia unemployed. The first column of Table 4 supports the hypothesis that firms strategically use compensated unemployment to hoard workers and smooth production. The recall hazard entitlement effect is precisely estimated, but it is not monotonically increasing, unlike the new job hazard entitlement effect. Firms recall workers in the period unemployment benefits end in order to avoid losing them to other employers.[28] The large estimate of the exhaustion dummy is consistent with the empirical hazard in Figure 3. The hazard improves by a factor of 7 as one moves from the highest entitlement bracket to the week benefits lapse.

Columns (2) and (4) present separate hazard functions for Pittsburgh and Philadelphia. The likelihood ratio test again rejects the pooled-sample model of column (1) in favor of a split-sample specification.[29] When we split the sample and estimate a separate recall hazard for each region, we find that long remaining entitlement has no effect in the Pittsburgh hazard (column 2). This contrasts with the sizeable and significant negative effect of large values of entitlement in Philadelphia in column (4). The weaker disincentive at longer entitlements in Pittsburgh could be the result of firms in durable manufacturing industries using short-duration temporary layoff unemployment more often than employers in Philadelphia. The depressing influence of long entitlement on the new job hazard of workers in Philadelphia could also lower the recall rate of local employers as firms delay recalling workers who remain unemployed. Similar to the new job hazard, all of the recall entitlement steps are more negative in Philadelphia. There is little difference, however, in the estimates for those with one to three weeks of eligibility or in the exhaustion dummy coefficient. Firms in both regions are much more likely to recall workers as soon as benefits lapse, as the exhaustion spikes are comparable across the two labor markets. When we consider the effect of positive values of remaining entitlement on each hazard, the Pittsburgh hazard increases by a cumulative 105% as we move from the first significantly estimated entitlement step (remaining entitlement between 19 and 27 weeks) to the last step before exhaustion (remaining entitlement between 1 and 3 weeks).[30] In Philadelphia, on the other hand, the percentage point increase in the hazard as one moves from the highest to the lowest entitlement bracket is 144%. A comparison of the level of the hazard with positive remaining entitlement to the hazard after exhaustion confirms the differences between the two labor markets. The total improvement in the hazard as one moves from having the maximum entitlement to having none is only 26% in Pittsburgh, but 118% in Philadelphia.

Controlling for unmeasured heterogeneity has a larger effect on recall hazards than on any other estimates. The gaps between the estimated entitlement effects widen as we introduce unobserved heterogeneity. The entitlement effects become smaller in absolute value in Pittsburgh, but more negative in Philadelphia. The exhaustion spike coefficients are not affected and remain comparable across the two labor markets.

The influence of demographic characteristics, demand conditions and within-spell changes in entitlement is listed for each region in Table 8 in the appendix. Age affects recalls differently than new job transitions as older, more experienced workers are more likely to be recalled. Higher earnings on the last job increase the recall hazard, and the effect is precisely estimated in Pittsburgh. The estimated constants suggest that Philadelphia UI claimants face a lower recall probability. High unemployment rates depress recall transitions in Pittsburgh. In Philadelphia the effect of local unemployment is insignificant, while higher employment growth results in a lower likelihood of recalls. Conditional on the changing value of entitlement, the estimates of the trigger dummies indicate that increases in entitlement during an unemployment spell reduce the incidence of recall in Pittsburgh. The sudden end of extended benefits leads firms to recall workers more quickly in Philadelphia. However, this last estimate does not reach conventional levels of statistical significance.

Allowing for unemployment rate-entitlement interactions (Table 5) shows that recall disincentives are stronger for firms in both labor markets when unemployment rates are lower. This interaction effect is precisely estimated in all entitlement brackets except for the exhaustion spike. Similar to the new job hazard, the estimate is largest just before benefits end. Further, the previously positive and imprecise estimate of the unemployment rate effect in Philadelphia becomes negative and statistically significant. Combining the impact of changing entitlement and the unemployment rate shows that the sensitivity of the entitlement effect to the level of unemployment is more pronounced in the low unemployment Philadelphia region. Consider, again, the total change in the hazard as one moves from the maximum entitlement bracket to the period after exhaustion. While a reduction in the unemployment rate from 12.5% to 10% increases this entitlement effect by 40% in Pittsburgh, a similar change in unemployment from 7.5% to 5% expands the Philadelphia entitlement effect by a dramatic 222%.[31]

Table 4: Recall Hazard Function Estimates

Sample	Pooled	Pittsburgh		Philadelphia	
Heterogeneity	No	No	Correlated	No	Correlated
Variable	(1)	(2)	(3)	(4)	(5)
Log Weekly Benefits	-0.311***	-0.506***	-0.568***	-0.029	0.036
	(0.107)	(0.149)	(0.153)	(0.157)	(0.21)
Entitlement 37 and over	-0.507**	-0.236	-0.174	-0.861***	-1.07***
	(0.222)	(0.307)	(0.352)	(0.328)	(0.359)
28 to 36	-0.479**	-0.385	-0.310	-0.631**	-0.820**
	(0.205)	(0.280)	(0.318)	(0.303)	(0.331)
19 to 27	-0.617***	-0.51**	-0.467	-0.759***	-0.871***
	(0.184)	(0.249)	(0.287)	(0.276)	(0.302)
04 to 18	-0.785***	-0.730***	-0.705***	-0.856***	-0.944***
	(0.158)	(0.208)	(0.238)	(0.244)	(0.273)
01 to 3	-0.472***	-0.462**	-0.457**	-0.468*	-0.534*
	(0.176)	(0.232)	(0.239)	(0.272)	(0.284)
-1 to 0	1.66***	1.70***	1.70	1.63***	1.64***
	(0.154)	(0.201)	(0.224)	(0.243)	(0.281)
Unemployment Rate	-0.0192**	-0.0290***	-0.0360***	0.039	0.0192
	(0.0083)	(0.0102)	(0.0121)	(0.042)	(0.0485)
Log-Likelihood	-13250.	-6846.1	-11894.7	-6356.2	-13758.5

Standard errors in parentheses. All specifications include a standard set of regressors reported for columns (2) and (4) in Table 8 in the appendix.
$*$ denotes significance at 10% level;
$**$ denotes significance at 5% level;
$***$ denotes significance at 1% level

Table 5: Recall Hazard Function Estimates with Interactions

Sample	Pittsburgh				Philadelphia			
Heterogeneity	No		Correlated		No		Correlated	
Variable	(1)		(2)		(3)		(4)	
Log Weekly Benefits	-0.513***	(0.149)	-0.614***	(0.187)	-0.027	(0.158)	0.031	(0.21)
Entitlement 37 and over	-1.822***	(0.698)	-2.214**	(0.881)	-4.494***	(1.189)	-4.785***	(1.492)
28 to 36	-2.297***	(0.679)	-2.633***	(0.849)	-4.809***	(1.178)	-5.059***	(1.462)
19 to 27	-2.135***	(0.666)	-2.392***	(0.826)	-3.514***	(1.115)	-3.532**	(1.421)
04 to 18	-2.234***	(0.645)	-2.405***	(0.813)	-3.928***	(1.106)	-4.038***	(1.429)
01 to 3	-3.251***	(0.753)	-3.511***	(0.907)	-5.136***	(1.389)	-5.397***	(1.765)
-1 to 0	0.481	(0.671)	0.412	(0.826)	0.715	(1.232)	0.870	(1.609)
Unemployment Rate	-0.167***	(0.052)	-0.193***	(0.066)	-0.429***	(0.153)	-0.449**	(0.190)
Entitlement 37 and over	0.132**	(0.054)	0.151**	(0.068)	0.488***	(0.157)	0.495**	(0.197)
28 to 36	0.16***	(0.054)	0.174***	(0.067)	0.561***	(0.159)	0.565***	(0.196)
19 to 27	0.137**	(0.055)	0.148**	(0.067)	0.379**	(0.155)	0.362*	(0.193)
04 to 18	0.128**	(0.054)	0.133**	(0.067)	0.428***	(0.152)	0.427**	(0.194)
01 to 3	0.234***	(0.060)	0.248***	(0.073)	0.647***	(0.186)	0.672***	(0.236)
-1 to 0	0.103*	(0.057)	0.106	(0.070)	0.109	(0.171)	0.0878	(0.224)
Log-Likelihood	-6836.6		-11866.4		-6343.1		-13713.1	

Standard errors in parentheses. All specifications include the same set of regressors as in Table 8.
$*$ denotes significance at 10% level;
$**$ denotes significance at 5% level;
$***$ denotes significance at 1% level

Again, we tested our specification to alternatives including unemployment rate-duration interactions, but these either were not significant or did not affect the parameters of interest.[32] Further, the results were not materially affected by controlling for unmeasured heterogeneity. Estimates of the entitlement effect in columns (2) and (4) are somewhat stronger, but the changes are small relative to standard errors.

4 Conclusion

This paper contrasts the effects of longer UI entitlement in labor markets experiencing much different labor demand conditions. Longer UI entitlement leads to greater increases in the duration of both new job and recall unemployment spells in relatively low unemployment areas. The adverse incentives of entitlement on finding a new job are stronger in lower unemployment rate areas at all levels of entitlement, and the gap is widest for those with between one and three weeks of eligibility remaining. The comparison of the entitlement effect is similar in the recall hazard, but the gap here is most pronounced at the longest remaining entitlement values. Even though the differences in the parameter estimates are, for the most part, statistically insignificant, our interpretation of the results is supported by the estimates allowing for entitlement-unemployment interactions within each of our two labor markets. First, both new job and recall entitlement effects are stronger when unemployment rates are lower. Second, the entitlement effect is more sensitive to the changing demand conditions in the labor market with persistently lower unemployment.

Visual evidence of the strategic use of longer entitlement is provided by the empirical hazards as 36% of workers who exhaust all benefits manage to find work in the next week. The spikes in the hazards at exhaustion are much larger than those reported in previous studies. This may be due to the particularly deep recession covered by our sample and the long UI entitlement available. However, the accurate administrative data used in this study precisely dates the transitions out of unemployment, which may also contribute to the spikes at exhaustion. Surprisingly, 34% of claimants in the depressed Pittsburgh labor market are able to find work as soon as benefits end, and three quarters of this group finds new jobs.[33] The strategic impact of exhausting benefits is similar across demand conditions (as further confirmed by our estimated hazard coefficients). The spikes in the empirical hazards coincide with the potential duration of UI benefits *including extensions*. For most workers who collected either EB or FSC, larger entitlement led to increases in unemployment for at least as many weeks as benefits were available. Around 80% of those who collected EB or FSC also drew their last check, with 82.4% in Philadelphia and 79.8% in Pittsburgh.

These findings have two implications for policies designed to aid the unemployed while minimising the distorting effects on decision makers. First, a stronger entitlement effect in low unemployment areas adds to the cost of the extended coverage programs by making the average duration of unemployment longer. The relatively smaller effect of UI entitlement in the high unemployment Pittsburgh labor market, therefore, argues for directing the UI benefits using substate triggers and for more precisely directed ad hoc legislation providing extra weeks of compensation. The Federal-State Extended Benefit law could be amended to base future extended benefits on labor market conditions in more economically integrated regions such as Metropolitan Statistical Areas or other collections of counties. The current policy of state wide extended benefits also denies extended UI compensation to workers in economically depressed areas in states that, on average, have low unemployment. A substate EB program would therefore be particularly valuable in the current environment in which some areas are experiencing depression-like unemployment rates, yet extended benefits are not available. Second, the high incidence of exhausting either of the two extended benefits programs, combined with the dramatic spike at the moment of exhaustion even in deeply depressed labor markets, suggests greater focus be put on incentives for rapid reemployment. It may be possible to experiment with a reemployment bonus which allows workers to keep a fraction of future claims if they find new jobs as previously tested with regular UI benefits.

Our sample period covers a particularly deep depression. It remains to be seen if estimates based on less depressed labor markets would provide similar findings. Furthermore, the focus of this research on the adverse incentives of longer entitlement on the duration of unemployment ignores potential benefits of longer entitlement on improved worker-firm matches which raise post-unemployment earnings. While estimating the benefits of entitlement on earnings is outside the scope of this research, earnings records can be used to show the influence of entitlement on earnings change. The relationship between earnings, entitlement, and demand conditions remains an important area of research that should be investigated in order to fully assess the impact of additional weeks of UI compensation.

Endnotes

1. California, Texas, and Pennsylvania with 20% of the U.S. population are examples where intrastate variation in unemployment rates in Standard Metropolitan Areas (SMSA) in 1983 exceeded the between state variance.

2. For both cities we focus on the Standard Metropolitan Statistical Areas as they were defined in 1982.

3. The IUR is the percentage of workers covered by the UI who are collecting regular UI benefits, i.e. those getting EB or FSC are not counted as unemployed in the IUR. The TUR is the unemployment rate based on the Currect Population Survey.

4. During the 1990's extended benefits were available only in Alaska, Maine, Oregon, Puerto Rico, Rhode Island, Vermont, Washington and West Virginia.

5. Czajka, Long, and Nicholson (1989) evaluate the *administrative* costs of implementing EB programs based on Primary Metropolitan Statistical Area (PMSA) labor market areas.

6. Many empirical studies measuring UI effects (e.g. Ham and Rea 1987, Meyer 1990, Engberg 1992a) rely on variation in UI benefits across states, coming from the extended benefits programs.

7. In our sample period the IUR declined when compared to the TUR because insured unemployed collecting FSC benefits extensions are not included in calculating the insured unemployment rates. The percentage point difference first jumped from 2.5 to 4 in mid 1981, went up again from 4 to 6 over the first half of 1983, and decreased in the second half of 1984.

8. Previous research either had no information about employment subsequent to collecting UI benefits (e.g. Katz and Meyer 1990b, Meyer 1990) or appended the administrative data with information from a follow-up telephone survey (e.g. Katz and Meyer 1990a). Survey data is often subject to response errors, whereas using the wage records provides more precise information.

9. The Philadelphia PMSA (as defined in 1979) includes Philadelphia, Bucks, Chester, Delaware, and Montgomery counties in Pennsylvania and Burlington, Camdem and Gloucester counties in New Jersey. Our sample only includes the Pennsylvania counties. The Pittsburgh PMSA includes Allegheny, Washington, and Westmoreland Counties. Beaver county, adjacent to Pittsburgh PMSA, is also included in our Pittsburgh sample.

10. Throughout the paper we will use the PMSA unemployment rates as our main measure of demand conditions in each region. We use the PMSA rates as opposed to county unemployment rates because of the large measurement error often involved in computing the county rates. The only exception is Beaver county in the Pittsburgh area, representing 4% of the sample. There are two reasons for this exception. First, even though Beaver county was included in the Pittsburgh SMSA until 1984, it is now its own PMSA. Second, in 1983, its unemployment rate reached a level of almost 30% and represents an extreme outlier even in the more depressed Pittsburgh region.

11. We do not know when these interrupted spells started.

12. The censored spells include out-of-the-labor-force transitions as well as interstate migrants.

13. Spells were divided based on the type of transition out of unemployment. All means except for the mean of the completed duration were taken in the first week of a spell.

14. The unemployed can collect EB or FSC only after exhausting benefits under the regular UI program.

15. Similar findings were noted by Katz and Meyer (1990a).

16. For a survey of search approach empirical literature see Devine and Kiefer (1991).

17. In order to streamline notation, we do not use individual i subscript in any of the formulas.

18. An entitlement specification in which the two longest steps were specified according to the length of UI extensions produced similar results in both the new job and recall hazards.

19. Each of the steps was chosen to represent approximately 5% of the transitions. In specifications with no unobserved heterogeneity we also experimented with finer parametrizations (2.5% steps) with no effect on the coefficients of interest. For a discussion of the advantages of such a semi-parametric specification of duration dependence see Meyer (1990).

20. The remaining entitlement dummies are precisely estimated unlike in Katz and Meyer (1990a). As one moves from remaining entitlement between 4 and 18 weeks to the week benefits lapse, the parameter estimates imply an eight-fold cumulative increase in the hazard. The estimated effect of entitlement is therefore larger than that reported in Katz and Meyer (1990b) or Meyer (1990), who report a three-fold cumulative increase as one moves from 6 weeks of remaining entitlement to the exhaustion week.

21. At 51 degrees of freedom, the χ^2 p-value is 0.014.

22. All of our estimates allowing for unobserved heterogeneity are based on specifications with two points of support of the discrete heterogeneity distribution. We searched for more points of support, but could not find them. For details on the heterogeneity estimation see Appendix A.

23. To correctly predict the triggers the workers would have to know the trigger formula and be able to forecast the insured unemployment rate or congressional action.

24. A likelihood ratio test again rejected pooling the sample.

25. We did not include any interactions of unemployment rates with the UI benefits dollar amount as the differential effect of the benefit amount is not a policy issue.

26. At a 7.5% unemployment rate, the Philadelphia hazard improves by a factor of 2.87 as one moves from maximum entitlement to the time after exhaustion. If one moves down the entitlement schedule at a 5% unemployment rate, the Philadelphia hazard grows by a factor of 6.99. The difference in the sensitivity of the entitlement effect is not caused by the different level of the initial unemployment rate. As unemployment falls from 12.5% to 10% in Philadelphia, the change in the entitlement effect is even larger at 155%.

27. We first tried interacting the unemployment rate with polynomials of a logarithm of duration. These interactions had no impact on the parameters of interest and were never significant. Second, we included a compressed six-step function in duration interacted with unemployment, but could not precisely estimate unemployment rate-entitlement interactions and the unemployment rate-duration interactions at the same time.

28. Few firms are financially liable for the last weeks of UI benefits as these workers are often getting extended benefits or are employed by firms that are at the maximum UI tax rate and hence are not experienced rated.

29. At 51 degrees of freedom, the χ^2 p- value is 0.00013.

30. If the insignificant steps (entitlement 28 and over) were also included, the improvement would be only 80%.

31. At a 7.5% unemployment rate, the Philadelphia hazard improves by a factor of 2.26 as one moves from maximum entitlement to the time after exhaustion. If one moves down the entitlement schedule at a 5% unemployment rate, the Philadelphia hazard grows by a factor of 7.29.

32. First, we interacted the unemployment rate with polynomials in a logarithm of duration. Second, we included a compressed step function in duration interacted with the unemployment rates.

33. Transitions from non-reported employment could be one of the causes of the large new-job hazard spike at exhaustion.

A Parametrization of the Heterogeneity Distribution

Here, following Heckman and Singer (1984), we extend the description of the econometric duration model by introducing the unobserved heterogeneity. Most of the used notation was introduced in section 3.1. Let $\lambda_j(t, x_t|\theta_k^j)$ be the conditional probability (hazard) of leaving a given state at time (duration) t for someone with person specific characteristics x_t, conditional upon this person having the unobserved factor θ_k^j, $k=1,2,...,N_\theta^j$, $j \in \{r, n\}$

$$\lambda_j(t, x_t|\theta_k^j) = \frac{1}{1+e^{-h_j(t,x_t|\theta_k^j)}}, \tag{3}$$

where

$$h_j(t, x_t|\theta_k^j) = r_j(e_t, \alpha_j) + \beta_j' z_t + g_j(t, \gamma_j) + \theta_k^j. \tag{4}$$

The likelihood function contribution accounting for unobserved factors for someone leaving unemployment at duration t for a $n$ew job is

$$L^n(t) = \sum_{k=1}^{N_\theta^n} \sum_{m=1}^{N_\theta^r} p(\theta_k^n, \theta_m^r) L^n(t|\theta_k^n, \theta_m^r), \tag{5}$$

where $p(\theta_k^n, \theta_m^r)$ is the probability of having the unobserved components θ_k^n and θ_m^r in the new job and recall hazards respectively, and where

$$L^n(t|\theta_k^n, \theta_m^r) = \lambda_n(t, x_t|\theta_k^n) \prod_{v=1}^{t-1} [1 - \lambda_n(v, x_v|\theta_k^n)][1 - \lambda_r(v, x_v|\theta_m^r)]. \tag{6}$$

Previous discussion used only single spell duration scenarios. Equation 7 gives the likelihood contribution of a person with two completed spells of unemployment. The first spell starts in week 1 and ends with a recall in week s; the second spell starts in week r and ends in a new job in week w (at duration $w - r$):

$$L^{r,n}(s, w) = \sum_{k=1}^{N_\theta^n} \sum_{m=1}^{N_\theta^r} p(\theta_k^n, \theta_m^r) L^r(s|\theta_k^n, \theta_m^r) L^n(w|\theta_k^n, \theta_m^r). \tag{7}$$

Here θ^n and θ^r denote the unobserved terms entering new job and recall hazards respectively and

$$L^r(s|\theta_k^n, \theta_m^r) = \lambda_r(s, x_s|\theta_m^r) \prod_{v=1}^{s-1} [1 - \lambda_n(v, x_v|\theta_k^n)][1 - \lambda_r(v, x_v|\theta_m^r)], \tag{8}$$

$$L^n(w|\theta_k^n, \theta_m^r) = \lambda_n(w, x_w|\theta_k^n) \prod_{v=r}^{w-1} [1 - \lambda_n(v, x_v|\theta_k^n)][1 - \lambda_r(v, x_v|\theta_m^r)]. \tag{9}$$

One can compute the individual contributions to the sample likelihood for other scenarios of durations and transitions in a similar way. The number of points of support of the distribution of unobservables (N_θ^r and N_θ^n) is determined from the sample likelihood.

We are using the following assumptions on the distribution of the unobserved heterogeneity. First, we estimate the hazard functions with no unobserved heterogeneity, i.e. $\theta_k = \theta \ \forall k$. Second, we estimate the competing risks with an M-tuple distribution of unobservables (McCall 1996), where M is the number of hazards to be jointly estimated. This distribution is described in the following table, where r and n denote recall and new job unemployment hazards respectively, and N is the number of points of support to be estimated.

Table 6: Heterogeneity distribution with M-tuples

$p(\Theta_1)$	$\Theta_1 = \{\theta_1^r, \theta_1^n\}$
$p(\Theta_2)$	$\Theta_2 = \{\theta_2^r, \theta_2^n\}$
...	...
$p(\Theta_N)$	$\Theta_N = \{\theta_N^r, \theta_N^n\}$

The M-tuple heterogeneity distribution allows the unobserved factors from the two hazards to be correlated and requires a joint estimation procedure.

B Unemployment Hazard Function Estimates

Table 7: New Job Hazard Function Estimates

Variable	*Pittsburgh* (1)		*Philadelphia* (2)	
UI Benefits and Remaining Weeks of UI Entitlement				
Log Weekly Benefits	-0.342**	(0.161)	-0.152	(0.14)
Entitlement 37 and over	-0.602*	(0.331)	-0.837***	(0.266)
28 to 36	-0.569**	(0.286)	-0.768***	(0.234)
19 to 27	-0.638***	(0.246)	-0.684***	(0.201)
04 to 18	-0.335*	(0.181)	-0.513***	(0.158)
01 to 3	0.437***	(0.167)	0.0895	(0.157)
-1 to 0	2.698***	(0.158)	2.523***	(0.144)
Extended Benefits Triggers				
Trigger On	0.023	(0.974)	0.724	(1.645)
Trigger On ∗ Unempl. Rate	-0.040	(0.061)	-0.107	(0.185)
Trigger On ∗ Rem. Entitlement	0.0239	(0.016)	-0.0002	(0.014)
Trigger Off	1.929**	(0.833)	1.224	(1.075)
Trigger Off ∗ Unempl. Rate	-0.114	(0.078)	-0.143	(0.144)
Trigger Off ∗ Rem. Entitlement	-0.049**	(0.020)	-0.003	(0.013)
Demand Conditions				
SMSA Un. Rate (monthly)	-0.0555***	(0.0139)	-0.0537	(0.040)
Industry Un. Rate (national)	-0.019	(0.032)	-0.028	(0.027)
Employment Growth †	2.035	(3.258)	4.569***	(1.64)
Demographics				
Constant	-2.746***	(0.682)	-2.325***	(0.561)
Log Previous Wage	0.115	(0.100)	0.049	(0.087)
White	0.211*	(0.110)	0.385***	(0.063)
Male	0.15*	(0.081)	0.139**	(0.060)
Age 25 to 34	-0.035	(0.089)	-0.313***	(0.072)
35 to 49	-0.123	(0.099)	-0.405***	(0.080)
50 and over	-0.374***	(0.107)	-0.644***	(0.091)
Married, Spouse Present	0.026	(0.066)	-0.014	(0.058)
Log-Likelihood	-5076.5		-7448.4	

Standard errors in parentheses. All specifications include year and industry dummies, as well as a step function in duration. See the next table for the estimated baseline hazards. We do not control for the unobserved heterogeneity here. †Employment growth is an industry and SMSA specific annual measure.

Table 7 Continued

Variable	*Pittsburgh* (1)		*Philadelphia* (2)	
Duration = 2	-0.247	(0.191)	-0.362***	(0.136)
3 to 4	-0.025	(0.192)	-0.524***	(0.151)
5 to 6	-0.146	(0.203)	-0.236	(0.144)
7 to 8	0.108	(0.198)	-0.331**	(0.153)
9 to 10	0.157	(0.203)	-0.175	(0.152)
11 to 12	0.298	(0.217)	-0.110	(0.162)
13 to 14	-0.002	(0.218)	-0.367**	(0.162)
15 to 17	0.043	(0.221)	-0.483***	(0.168)
18 to 21	-0.215	(0.243)	-0.369**	(0.180)
22 to 26	-0.286	(0.274)	-0.297	(0.204)
27 to 30	-0.072	(0.283)	-0.372*	(0.215)
31 to 35	-0.433	(0.316)	-0.364	(0.237)
36 to 39	-0.014	(0.327)	-0.302	(0.256)
40 to 43	0.373	(0.341)	-0.533*	(0.280)
44 to 48	0.182	(0.355)	-0.653**	(0.286)
49 and over	1.181***	(0.369)	0.215	(0.301)

Standard errors in parentheses. Each duration step was chosen to cover 5% of transitions.
$*$ denotes significance at 10% level;
$**$ denotes significance at 5% level;
$***$ denotes significance at 1% level

Table 8: Recall Hazard Function Estimates

Variable	*Pittsburgh* (1)		*Philadelphia* (2)	
UI Benefits and Remaining Weeks of UI Entitlement				
Log Weekly Benefits	-0.506***	(0.149)	-0.0294	(0.157)
Entitlement 37 and over	-0.236	(0.307)	-0.861***	(0.328)
28 to 36	-0.385	(0.280)	-0.631**	(0.303)
19 to 27	-0.511**	(0.249)	-0.759***	(0.276)
04 to 18	-0.730***	(0.208)	-0.856***	(0.244)
01 to 3	-0.462**	(0.232)	-0.468*	(0.272)
-1 to 0	1.70***	(0.201)	1.634***	(0.244)
Extended Benefits Triggers				
Trigger On	-1.421**	(0.677)	-0.662	(1.435)
Trigger On ∗ Unempl. Rate	0.0547*	(0.029)	0.0260	(0.146)
Trigger On ∗ Rem. Entitlement	0.0226	(0.016)	0.0250	(0.017)
Trigger Off	-0.070	(0.670)	2.056	(1.31)
Trigger Off ∗ Unempl. Rate	-0.005	(0.060)	-0.210	(0.185)
Trigger Off ∗ Rem. Entitlement	0.010	(0.0151)	-0.015	(0.017)
Demand Conditions				
SMSA Un. Rate (monthly)	-0.029***	(0.010)	0.039	(0.042)
Industry Un. Rate (national)	-0.037	(0.029)	0.002	(0.029)
Employment Growth †	1.411	(3.333)	-3.915**	(1.67)
Demographics				
Constant	-3.077***	(0.630)	-2.795***	(0.636)
Log Previous Wage	0.386***	(0.082)	0.134	(0.092)
White	0.179*	(0.097)	-0.109*	(0.065)
Male	0.092	(0.077)	0.0580	(0.070)
Age 25 to 34	0.122	(0.091)	-0.041	(0.093)
35 to 49	0.313***	(0.094)	0.117	(0.097)
50 and over	0.252***	(0.097)	0.228**	(0.101)
Married, Spouse Present	0.216***	(0.056)	0.155**	(0.060)
Log-Likelihood	-6846.1		-6356.2	

Standard errors in parentheses. All specifications include year and industry dummies, as well as a step function in duration. See the next table for the estimated baseline hazards. We do not control for the unobserved heterogeneity here. †Employment growth is an industry and SMSA specific annual measure.

Table 8 Continued

Variable	*Pittsburgh* (1)		*Philadelphia* (2)	
Duration = 2	-0.600***	(0.135)	-0.279**	(0.114)
= 3	-0262**.	(0.133)	-0.859***	(0.142)
4 to 5	-0.693***	(0.133)	-1.162***	(0.132)
= 6	-0.679***	(0.164)	-0.973***	(0.156)
7 to 8	-0.561***	(0.135)	-1.129***	(0.137)
9 to 10	-0.450***	(0.139)	-1.123***	(0.143)
11 to 12	-0.328**	(0.141)	-0.787***	(0.139)
13 to 14	-0.175	(0.153)	-1.005***	(0.163)
15 to 16	-0.507***	(0.167)	-1.251***	(0.177)
17 to 19	-0.630***	(0.167)	-1.365***	(0.174)
20 to 23	-0.946***	(0.188)	-1.578***	(0.191)
24 to 26	-0.779***	(0.205)	-1.323***	(0.208)
27 to 34	-1.171***	(0.217)	-2.236***	(0.239)
35 to 44	-1.338***	(0.263)	-2.098***	(0.277)
45 to 72	-1.181***	(0.302)	-2.416***	(0.33)
73 and over	-1.822**	(0.775)	-2.632***	(0.781)

Standard errors in parentheses. Each duration step was chosen to cover 5% of transitions.
$*$ denotes significance at 10% level;
$**$ denotes significance at 5% level;
$***$ denotes significance at 1% level

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