UNEMPLOYMENT DURATIONS AND EXTENDED UNEMPLOYMENT BENEFITS IN LOCAL LABOR MARKETS

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Many empirical studies have confirmed the theoretical prediction that longer-term Unemployment Insurance (UI) entitlement leads to longer unemployment duration. Most of those studies have examined special programs that provide extra weeks of unemployment benefits when unemployment rates in the region are higher. Hence, they must distinguish if the longer unemployment duration among UI claimants observed in these cases is due to the extended benefits or to the adverse labor market conditions that trigger those extensions. In contrast, this paper measures the effect of identical entitlement extensions across two labor markets facing very different demand conditions—Pittsburgh and Philadelphia, over the years 1980–85. The results confirm findings of the existing literature and indicate that the adverse effect of longer entitlement changes relatively little in response to variation in demand conditions.

According to job search theory, longer Unemployment Insurance (UI) entitlement subsidizes job search and leads to longer unemployment spells. A large body of empirical literature supports this prediction (for example, Moffitt and Nicholson 1982; Ham and Rea 1987; Meyer 1990). These studies rely on variation in the maximum benefit duration coming from extended benefit programs to separate the effect of UI entitlement from the effects of spell duration. However, because the additional weeks of UI compensation offered through these state programs are triggered when the unemployment rate reaches a legally mandated threshold, the interpretation of the existing empirical results as a causal relationship is open to question. Specifically, additional weeks of benefits are provided during economic downturns when spell length also increases, which may lead to overestimation of the entitlement effect on unemployment duration.

This paper sets up a stronger test of job search theory predictions by employing a different identification strategy. We estimate the effect of changes in UI entitle-

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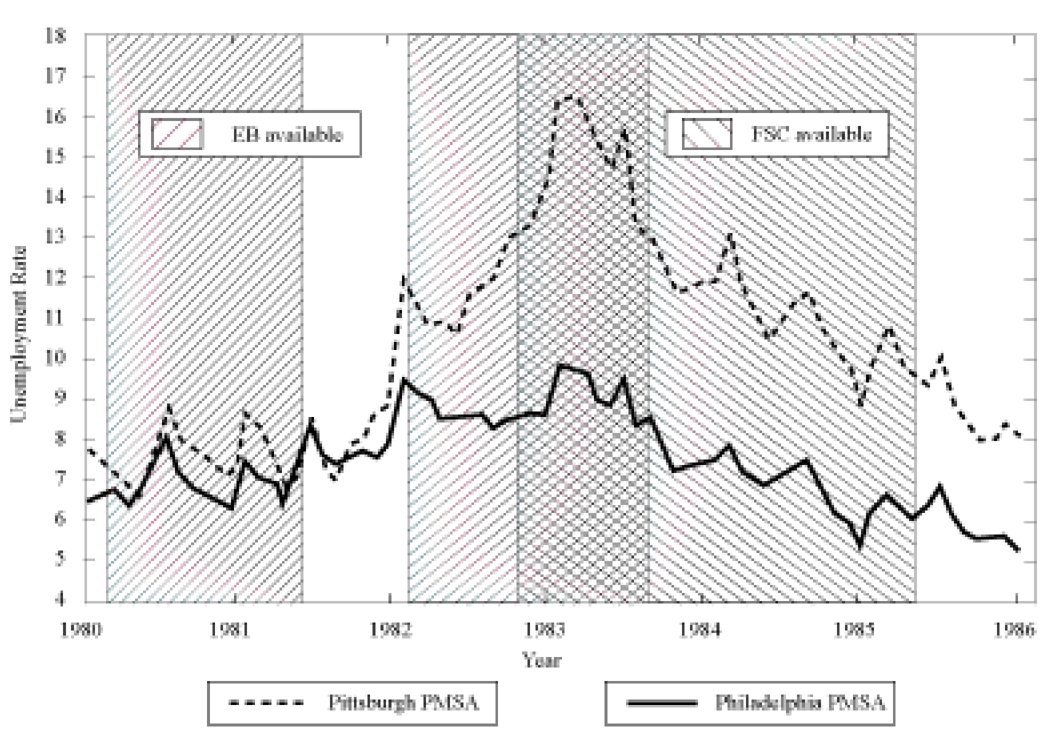
Copies of the computer programs and the data set used to generate the results are available from Frederick J. Tannery at the Department of Economics, University of Pittsburgh, Pittsburgh, PA 15260.

Industrial and Labor Relations Review, Vol. 56, No. 2 (January 2003). © by Cornell University.
0019-7939/00/5602 $01.00

Figure 1. Unemployment Rates in Sample Labor Markets.



ment on the duration of unemployment in two distinct labor markets experiencing very different business conditions: Pittsburgh and Philadelphia, from 1980 through 1985. Figure 1 indicates the dramatic differences in performance between these two labor markets during this 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. In contrast, 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%.

Despite differences in local economic conditions, the maximum benefit duration followed an identical pattern in the two regions. High unemployment rates in the early 1980s led to two temporary increases in the duration of UI benefits. The first was provided under the Federal-State Extended Benefit (EB) program, which increased entitlement by 50% in states where unemployment reached a statutory threshold. Longer entitlement was also available under Federal Supplemental Compensation (FSC), which operated nationally between 1982 and 1985 and authorized more benefit weeks in states with higher total unemployment rates.[1]

Using a competing risk hazard model, which separately estimates the duration of

[1]The timing of the extended benefits programs is also notable. As illustrated in Figure 1, FSC began in September 1982 when the unemployment rate in Philadelphia had been relatively constant for the previous six months. On the other hand, EB ended when the unemployment rate was 13% in Pittsburgh. Moreover, when EB ended in August 1983, unemployment was higher in both regions than when EB was triggered "on" in February 1980.

unemployment spells ending in recall and those ending in new jobs, we quantify how employers and unemployed workers in our sample responded to changes in UI entitlement. We contrast estimates from different samples to highlight the influence of different sources of identification. The results based on the pooled sample of Philadelphia and Pittsburgh claimants rely, in part, on UI entitlement variation independent of demand conditions, which reduces the potential endogeneity of benefit duration. The split-sample results, based separately on the unemployed in each labor market, rely only on time variation in entitlement, which is tied to time variation in demand conditions.

The sample design also allows us to directly investigate the sensitivity of the entitlement effect to demand conditions by estimating the effects on re-employment probabilities of entitlement and unemployment rates jointly with the effect of their interaction. (Again, we can identify the interaction effect using variation in unemployment both across and within local labor markets.) The significance of such analysis for policy purposes is discussed below.

The rationale for the extended benefits programs is that they direct benefits to high-unemployment areas and should have only 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.[2] There is no evidence, theoretical or empirical, on whether the adverse effect of entitlement on job finding rates changes with local demand conditions. In particular, the search subsidy provided by longer entitlement 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.

While the EB state trigger mechanism provides additional benefits in relatively prosperous areas where they may not be needed, it also withholds benefits from high-unemployment regions within low-unemployment states. Denying extended benefits to depressed labor markets may also raise political pressure for ad hoc legislation authorizing even less precisely targeted additional UI compensation on a national basis (Blaustein et al. 1993).[3] One way of helping the unemployed in high–unemployment-rate areas, without the expense of providing extended benefits at the state or national level, is to base benefit extensions on sub-state triggers. Local unemployment rates currently allocate training funds under the Job Training Partnership Act, and the feasibility of sub-state triggers for EB has been studied by the U.S. Department of Labor.[4]

By contrasting the adverse effect of extended benefits on the duration of unemployment in tight and slack labor markets, this paper complements the cost analysis of implementing sub-state triggers.

Finally, we also extend the existing UI literature in terms of data quality. Our competing risk hazard estimates rely entirely on administrative data. We augment

[2]California, Texas, and Pennsylvania, together accounting for 20% of the U.S. population, provide an example of intrastate variation in annual unemployment rates in Standard Metropolitan Areas (SMSAs) in the early 1980s that exceeded the between-state national variance (Employment and Earnings 1985).

[3]For example, EB was seldom available during the recession of the 1990s, which may have led to the passage of the national Emergency Unemployment Compensation program in November 1991. See also Blank and Card (1991).

[4]Czajka, Long, and Nicholson (1989) evaluated the administrative costs of implementing EB programs based on Primary Metropolitan Statistical Area (PMSA) labor market areas.

the UI data collected under the Continuous Wage and Benefit History program with quarterly earnings records reported by each UI-covered employer for each worker. Employer identifiers on earnings records allow us to distinguish unemployment spells ending in new jobs from those ending in a recall and indicate 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. We are also able to precisely date the exhaustion of benefits by accounting for the exact trigger dates of the extended benefits programs.

Theory

Job search theory models the response of unemployed job seekers to changes in both UI benefit parameters and demand conditions (for a survey, see Mortensen 1986). The probability of finding a job depends both on the probability of receiving a job offer and on the optimal reservation wage, which determines the probability of accepting the offer. An increase in UI generosity leads to higher reservation wages and lengthens the expected duration of unemployment. The effect of changing demand conditions is ambiguous. A decrease in unemployment normally leads to an increase in the probability of receiving a job offer and shortens the unemployment spell. However, it will also have an indirect effect on the reservation wage. Job search becomes more productive and the value of unemployed search increases, leading to a higher reservation wage, which offsets the direct effect.

While the effects of both UI entitlement and the search environment have been addressed within job search theory, there has been no attempt to account for interactions between them.[5] We conjecture that the disincentive effect of UI is stronger in tighter labor markets, where the likelihood of a job offer is higher, making the job search model more applicable. Longer entitlement subsidizes job search, and the effect on search strategies is likely to be stronger in labor markets where the productivity of search is higher. Further, job seekers in depressed labor markets may be more reluctant to reject job offers, for fear of not finding a job before all UI benefits end. Risk aversion among workers in depressed labor markets is likely, but has been largely ignored in the mainstream job search theory.

Even though unemployment spells often end in recall, there has been relatively little theoretical work examining recall decisions of firms. Pissarides (1982) developed a static model of workers' job search and firms' recall decisions in which firms correctly anticipate workers' optimal job search strategy. Employers are assumed to incur costs of losing a worker's firm-specific skills when the worker takes a job with another firm. Employers therefore respond to workers' incentives in an effort to minimize these costs. In a dynamic job search model, firms are more likely to recall workers when benefits are about to lapse, as they know workers nearing exhaustion are more likely to take a new job (Jurajda 1998). Firms also recall workers when demand conditions recover. However, if the adverse effect of entitlement on workers' search intensity is stronger when demand conditions improve, firms will respond with a lower recall probability. The extent to which the worker's search strategy is mirrored in the firms' recall decisions is an empirical question providing motivation for a separate estimation of the recall and new job hazards.

Econometric Model

We measure the effect of unemployment insurance on unemployment spell duration in a competing risk hazard model for new job and recall hazards. The new job hazard is motivated by job search theory. It equals the probability that a wage offer is received times the probability that it is ac-

[5]The interaction between demand conditions and entitlement in a standard job search model appears to consist of two offsetting effects much as does the direct effect of demand conditions.

ceptable. The resulting estimate can be interpreted as an approximation to comparative statics implied by a corresponding model of job search.[6]

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

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

where

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

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.[7]

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

$$(3) \qquad L^r(t) = \lambda_r(t,x_t) \prod_{v=1}^{t-1} [1 - \lambda_r(v,x_v)]\,[1 - \lambda_n(v,x_v)],$$

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 that is still in progress at the end of our sampling frame (that is, no transition out of unemployment has been observed until duration T), the likelihood contribution is the survivor function

$$(4) \qquad S(T) = \prod_{v=1}^{T} [1 - \lambda_r(v,x_v)]\,[1 - \lambda_n(v,x_v)].$$

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 may be biased. We control for the unobserved heterogeneity using the flexible nonparametric approach of 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.

Data and Descriptive Statistics

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 files include an administrative record detailing the claimants' initial entitlement, weekly benefit amount, number of weeks claimed, and individual characteristics such as race, sex, and county of residence. Also included are responses to a questionnaire administered at the time of each claim, which reports education, marital status, and other family income. The survey ended in August 1984, a victim of federal budget cuts. 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 seasonality problems arising from a short sample, as noted in Katz and Meyer (1990a).

The CWBH data have 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 from those end-

[6]For a survey of search approach empirical literature, see Devine and Kiefer (1991).

[7]To streamline notation, we omit use of the i subscript (for individual) from all formulas.

ing 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. They 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 almost 24% of all claimants exhaust their UI entitlement.[8]

Claims data differ from spell data. Initially, laid off workers file for UI benefits, which begins a 52-week benefit year.[9] 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 procedure under-samples 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 in 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.

We focus on claims from the Philadelphia and Pittsburgh Primary Metropolitan Statistical Areas (PMSAs).[10] As noted above, these areas had dramatically different unemployment rates in the sample period 1980–85. Throughout the paper we will use the monthly PMSA unemployment rates as our main measure of demand conditions in each region.[11] We also control for demand conditions using an annual measure of employment growth that is both SMSA- and industry-specific. The relatively large labor markets, combined with the deep recession, result in 7,750 spells of compensated unemployment (representing 1% of all claimants). Deleting observations with missing variables and omitting left-censored spells[12] reduces the sample size to 6,658 spells for 5,134 individual workers. Nearly as many spells end in a new job as in a recall, and 14.4% are censored. The censored spells include out-of-the-labor-force transitions as well as out-of-state moves and employment.[13] Potential interstate migration

[8]Previous research either had no information about employment subsequent to collecting UI benefits (for example, Katz and Meyer 1990b; Meyer 1990) or supplemented the administrative data with information from a follow-up telephone survey (for example, Katz 1986; and Katz and Meyer 1990a). Survey-based data are likely to be less accurate in measuring the duration of unemployment spells. For example, Katz and Meyer (1990a) noted 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.

[9]Given the nature of the data, we calculate the duration of unemployment from the date of claim, not the date of job loss.

[10]The Philadelphia PMSA (as defined in 1979) includes Philadelphia, Bucks, Chester, Delaware, and Montgomery Counties in Pennsylvania and Burlington, and Camden 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.

[11]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%, which represents an extreme outlier even in the more depressed Pittsburgh region.

[12]We do not know when these interrupted spells started.

[13]Workers who do not report any employment within the sampling frame are coded as censored at the moment of benefits exhaustion.

Table 1. Individual and Spell Characteristics.

Independent Variable	*New Job*	*Recall*	*Censored*
	Pittsburgh		
Duration in Weeks	25.4 (19.6)	14.4 (14.2)	41.6 (25.1)
Age	34.8 (11.7)	38.8 (11.8)	39.1 (12.7)
Male	0.73	0.81	0.69
Married	0.44	0.55	0.37
White	0.91	0.92	0.86'
Base Period Earnings	13,542 (8,144)	16,932 (8,683)	14,193 (9,038)
UI Benefits	143.1 (49.6)	159.5 (40.1)	144.2 (46.4)
Initial UI Entitlement	38.6 (7.06)	38.1 (7.65)	36.0 (8.77)
Unemployment Rate	11.1 (3.51)	11.4 (3.88)	11.5 (4.06)
Employment Growth	0.31 (5.02)	–2.12 (5.32)	–0.64 (5.17)
Number of Spells	1,089	1,551	422
	Philadelphia		
Duration in Weeks	22.1 (17.3)	12.0 (12.2)	38.9 (23.5)
Age	34.3 (11.1)	38.4 (12.2)	37.9 (11.8)
Male	0.65	0.68	0.59
Married	0.32	0.42	0.28
White	0.79	0.72	0.71
Base Period Earnings	12,471 (7,760)	13,955 (7,883)	12,447 (8,320)
UI Benefits	137.8 (48.7)	147.9 (45.4)	135.6 (48.2)
Initial UI Entitlement	37.8 (7.61)	37.2 (7.61)	35.7 (9.00)
Unemployment Rate	7.51 (1.14)	7.58 (1.24)	7.21 (1.42)
Employment Growth	2.89 (1.97)	2.03 (2.11)	2.64 (1.97)
Number of Spells	1,671	1,390	535

Notes: Standard errors in parentheses. Earnings and UI benefits are in 1992 dollars.

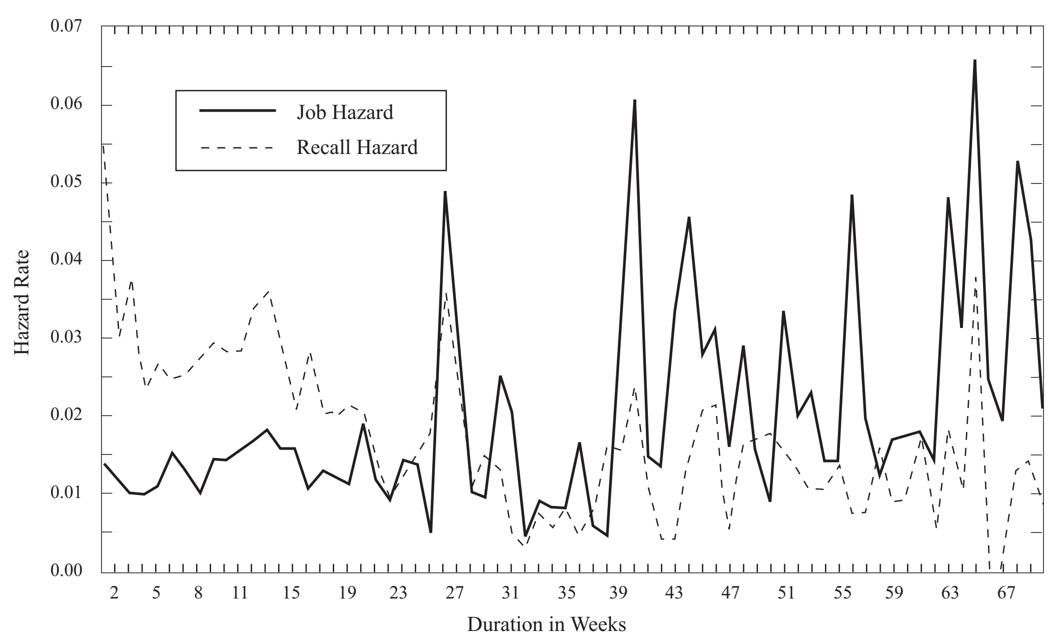
is a major drawback of the data and could be important both in Philadelphia, which lies on the border of the state, and in Pittsburgh, where the reduction in heavy industry employment led to shrinkage of the local labor force.[14]

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.[15] 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 did. 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. Both the likelihood and the duration of censored spells are similar in the two regions, perhaps suggesting little difference in the extent of interstate migration. Claimants in the two labor market areas also differ in several respects. Pittsburgh claimants are more likely to be white, married, and male, and they are likely to enjoy higher base period earnings than the Philadelphia unemployed.

[14]Unfortunately, published data do not provide a reasonable estimate of out-migration among the unemployed. Census data list the characteristics of in-migrants into the study areas, but are silent about those who leave. The Current Population Survey reports estimates of out-migration of the unemployed only for the entire nation.

[15]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.

Figure 2. Empirical Hazards for Competing Risks in Pittsburgh.



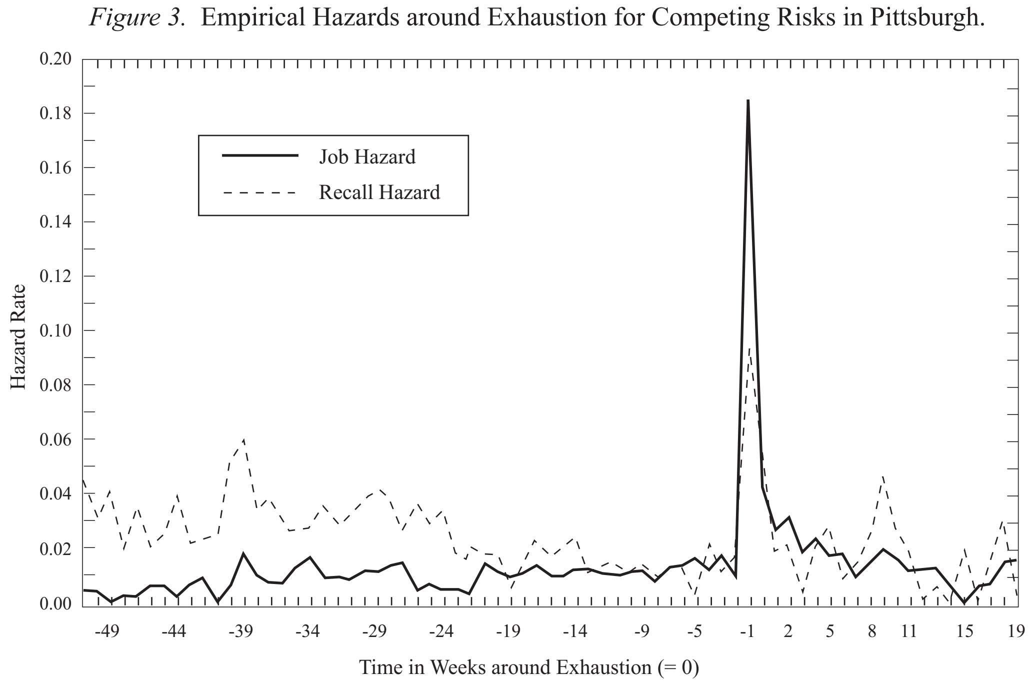
Recall is more prevalent in Pittsburgh, while new job findings are more likely to occur in Philadelphia.

The unusually high variation in entitlement over time allows one to separate the effect of entitlement from duration dependence. The variation is due to 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 weeks. Moreover, EB triggers and FSC authorizations often changed the available remaining entitlement while a spell of unemployment was in progress. Over 75% of the spells started when extended benefits were available, and more than 15% of the spells were in progress while one of the extended benefits programs increased entitlement. On the other hand, about 14% 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.

Empirical Hazards and UI Exhaustion

In order to collect the EB or FSC benefit extensions, the unemployed first have to exhaust their regular UI benefits. In Pittsburgh, 35% of claimants exhausted regular benefits throughout the sample period, compared to 32% in Philadelphia. Of those exhausting regular benefits, 75% received benefit extensions in Pittsburgh, and 69% in Philadelphia. The exhaustion rates for benefits collected under EB and FSC substantially exceed those for regular benefits:

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



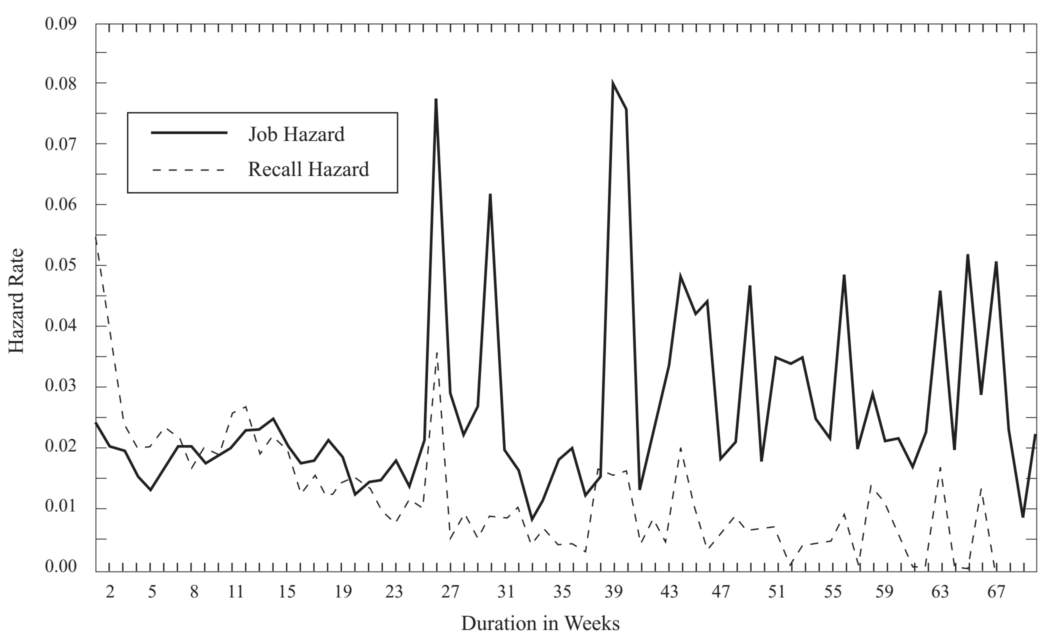
in Pittsburgh and Philadelphia, respectively, the exhaustion rates for EB (among workers entering that program) were 74% and 73%; for FSC, 65% and 63%. The exhaustion rates in the two regions are comparable in spite of the sizable difference in demand conditions. Collecting extended benefits therefore strongly predicts prolonged spells of unemployment not only in depressed areas but also in tighter labor markets. Overall, benefit exhaustion is about three times more likely to occur for job changers than for recalled workers.

Figure 2 shows the Kaplan-Meier empirical hazards for the first 70 weeks of unemployment for 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; spells lasting at least six months more often end in a new job. 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 weeks until exhaustion rather than weeks unemployed. There is a very large spike in the hazard at the week benefits lapse (corresponding to time 0). Nearly 19% of the unemployed exhausting their UI benefits find jobs in the next week, and almost another 10% 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 they increase by factors of 19 and 5, respectively, in the week benefits lapse.

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, however, again coincide with the potential duration of entitlement. The

Figure 4. Empirical Hazards for Competing Risks in Philadelphia.



recall hazard is depressed in the weeks immediately preceding exhaustion and more than triples in the week benefits lapse. The new job hazard rate rises to a dramatic 24% spike from a little above 2% in the preceding week. Nearly one-quarter of those unemployed in Philadelphia at the exhaustion week find a new job, and another 8% are recalled.

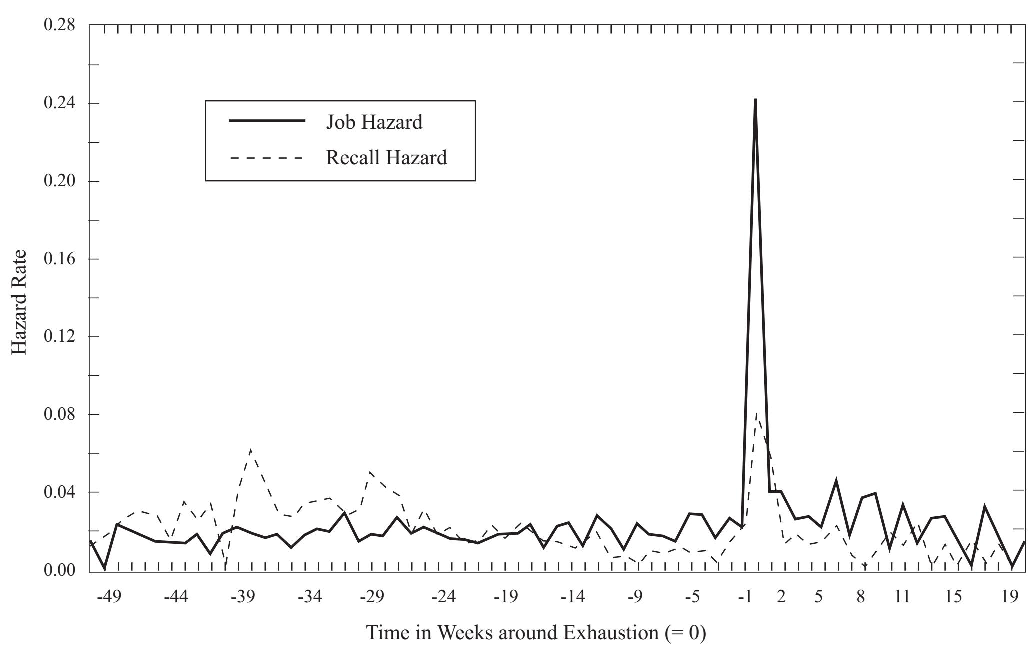
The high exit rates at exhaustion serve as persuasive evidence of the strategic use of compensated unemployment by both workers and firms. They also indicate that strategic use of entitlement is important even in very depressed labor markets. Furthermore, the exhaustion spikes in the two regions are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

New Estimates of the UI Entitlement Effect

The estimated unemployment hazard models use flexible parameterizations of the effects of both spell duration and remaining entitlement. Entitlement is specified as 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[16] 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

[16]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.

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



function is normalized 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 (the employment growth measure and the regional unemployment rate discussed in the data section), previous employment variables, year dummies, and a relatively parsimonious step function in duration to control for duration dependence.[17]

[17]Each of the steps was chosen to represent approximately 4% of the transitions. Specifically, the break points for the steps in the new job hazard are at duration weeks 4, 6, 8, 10, 12, 14, 17, 20, 24, 26, 30, 38, 40, 46, 56, and 71. For the recall hazard, the steps start in weeks 2, 3, 4, 5, 6, 8, 10, 12, 14, 16 19, 23, 26, 34, 44, and 66. In specifications with no unobserved heterogeneity we also experimented with finer parameterizations (2% 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).

New Job Hazard

Table 2 reports the sensitivity of the new job hazard to UI compensation. Our first estimates in column (1) are based on the pooled sample of Pittsburgh and Philadelphia claimants. Even though these estimates are based on a new identification strategy, they generally accord with the existing literature. The precisely estimated coefficients indicate that entitlement depresses the new job hazard for those with at least one week of remaining eligibility. The negative effect is large and remarkably similar for those with longer entitlement. The UI effect does not depend on the level of remaining entitlement as long as exhaustion is sufficiently far in the future. Workers are more likely to find jobs in the weeks just before exhaustion. Further, the exhaustion week coefficient is consistent with the large spikes at exhaustion found in the empirical hazards.

Columns (3) and (5) list estimates of separate hazard functions for claimants in

Table 2. New Job Hazard Estimates.

Sample:	*Pooled*		*Pittsburgh*		*Philadelphia*	
Heterogeneity:	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
UI Compensation and Demand Conditions						
Log Weekly Benefits	–0.175*	–0.209**	–0.217	–0.250	–0.131	–0.137
	(0.105)	(0.104)	(0.160)	(0.168)	(0.141)	(0.142)
Entitlement 37 and Over	–0.812***	–0.865***	–0.730**	–0.745**	–0.912***	–0.929***
	(0.194)	(0.187)	(0.317)	(0.322)	(0.254)	(0.244)
28 to 36	–0.784***	–0.868***	–0.723***	–0.674**	–0.859***	–0.880***
	(0.171)	(0.163)	(0.275)	(0.278)	(0.222)	(0.212)
19 to 27	–0.830***	–0.939***	–0.849***	–0.815***	–0.866***	–0.879***
	(0.145)	(0.138)	(0.234)	(0.229)	(0.189)	(0.180)
04 to 18	–0.697***	–0.812***	–0.606***	–0.585***	–0.785***	–0.800***
	(0.109)	(0.103)	(0.171)	(0.168)	(0.143)	(0.136)
01 to 3	–0.272**	–0.105***	–0.121	–0.109	–0.100**	–0.111***
	(0.117)	(0.110)	(0.176)	(0.171)	(0.157)	(0.148)
–1 to 0	1.70***	1.627***	1.80***	1.81***	1.61***	1.60***
	(0.097)	(0.097)	(0.148)	(0.152)	(0.130)	(0.130)
Expected Exhaustion[a]	0.447***	0.431***	0.179	0.175	0.571***	0.567***
	(0.139)	(0.138)	(0.255)	(0.253)	(0.167)	(0.166)
Unemployment Rate	–0.061***	–0.074***	–0.055***	–0.060***	–0.035	–0.038
	(0.011)	(0.011)	(0.014)	(0.015)	(0.039)	(0.040)
Employment Growth[b]	0.034***	0.026***	0.031	0.038	0.044***	0.042**
	(0.009)	(0.009)	(0.034)	(0.045)	(0.016)	(0.017)
Demographics						
Constant	–2.58***	—	–2.71***	—	–2.63***	—
	(0.359)		(0.624)		(0.503)	
Philadelphia	0.026	0.021	—	—	—	—
	(0.060)	(0.060)				
Log Base Period Earnings	0.050	0.039	0.057	0.072	0.036	0.046
	(0.065)	(0.065)	(0.100)	(0.100)	(0.087)	(0.090)
White	0.289***	0.270***	0.192*	0.237**	0.314***	0.315***
	(0.055)	(0.052)	(0.112)	(0.114)	(0.064)	(0.065)
Male	0.115***	0.111***	0.132	0.112*	0.139**	0.138**
	(0.048)	(0.048)	(0.082)	(0.085)	(0.061)	(0.062)
Married, Spouse Present	–0.020	–0.020	–0.019	–0.012	–0.021	–0.027
	(0.044)	(0.043)	(0.068)	(0.067)	(0.059)	(0.060)
Age 25 to 34	–0.225***	–0.214***	–0.083	–0.087	–0.313***	–0.320***
	(0.056)	(0.055)	(0.089)	(0.092)	(0.073)	(0.073)
35 to 49	–0.300***	–0.281***	–0.165*	–0.168*	–0.372***	–0.380***
	(0.062)	(0.059)	(0.099)	(0.096)	(0.080)	(0.080)
50 and Over	–0.511***	–0.481***	–0.375***	–0.368***	–0.600***	–0.600***
	(0.070)	(0.067)	(0.108)	(0.104)	(0.092)	(0.095)
Log-Likelihood	–12,734.2	–26,030.1	–5,154.8	–12,050.9	–7,550.4	–13,892.6

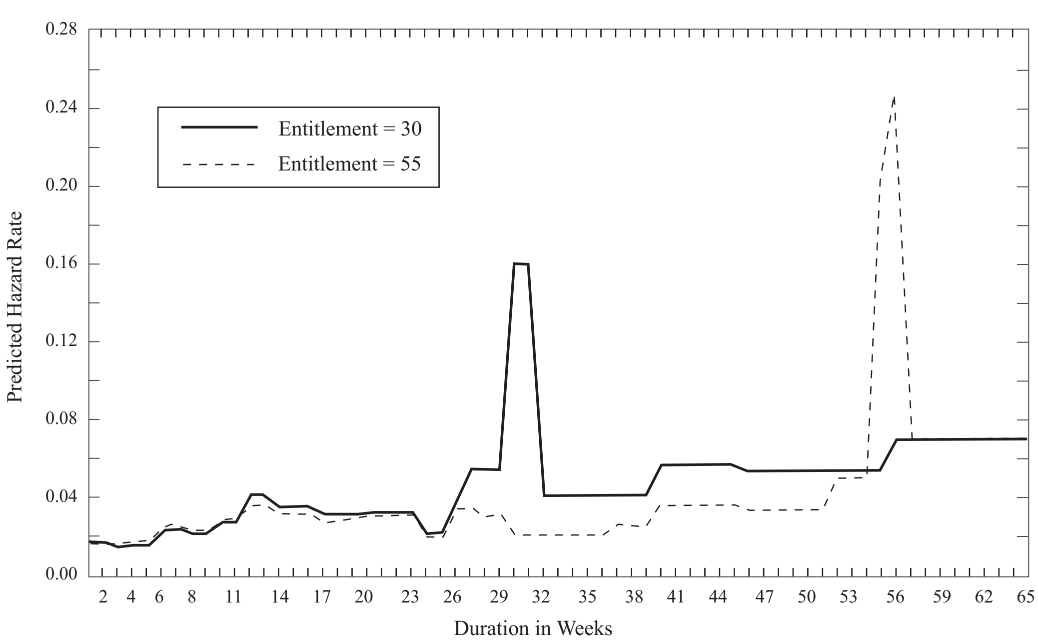
Notes: Standard errors in parentheses. All specifications include year and industry dummies and a step function in duration, which are available from the authors on request. See Appendix A for specification of the unobserved heterogeneity distribution.

[a]Exhaustion of all benefits was expected by recipients but did not occur, because a benefits extension became effective after the regular benefits ran out.

[b]Employment growth is an industry- and SMSA-specific measure.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Figure 6. Predicted New Job Hazard in Pittsburgh.



Pittsburgh and Philadelphia, respectively.[18] The estimated entitlement coefficients identified off only time variation in entitlement within each local labor market are very similar to those estimated using both sources of variation. The effect of changes in UI entitlement tied to changes in demand conditions appears similar to that identified using an additional "cleaner" source of identification.[19]

Following Meyer (1990), our specifications also include a dummy variable capturing the effect of expected regular benefit exhaustion that did not occur due to a benefit extension being triggered on. This variable equals 1 in the week when regular benefits were previously expected to lapse and in the immediately following week. It does not turn to 1 for those who started their unemployment claim when extended benefits were available. The coefficient is positive and statistically significant, which was also found in the existing literature.[20] Higher unemployment rates and greater weekly benefits, controlling for previous earnings, significantly depress the new job hazard, while employment growth boosts the hazard. The estimated baseline hazard coefficients for all specifications are available on request.

We also investigate the sensitivity of our estimates to unobservable person-specific factors. 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.[21] Estimated sample

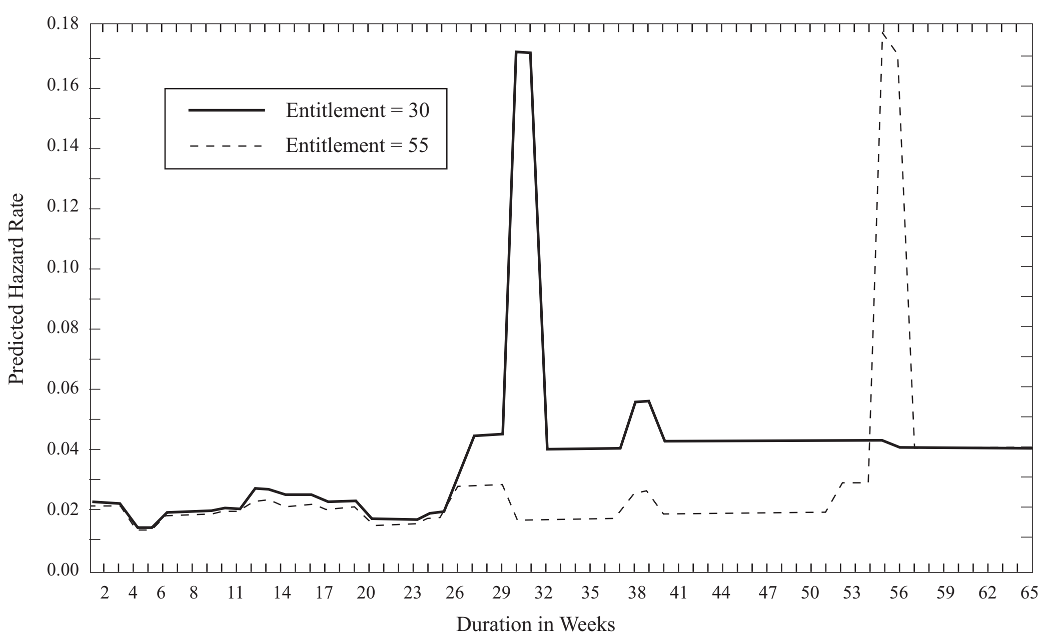
[18]The likelihood ratio test comparing the pooled-sample and split-sample results suggests using the latter at a marginal level of significance: at 45 degrees of freedom, the χ^2 p-value is 0.106.

[19]The next section discusses the differences in the estimated entitlement effects across the two regions.

[20]In an earlier version of this paper we also included an indicator for the weeks when extended benefits were suddenly triggered off. This indicator was positive and statistically significant.

[21]All of our estimates allowing for unobserved heterogeneity are based on specifications with two

Figure 7. Predicted New Job Hazard in Philadelphia.



likelihoods strongly support including unmeasured heterogeneity, as the heterogeneity distribution is precisely estimated. Adding unmeasured heterogeneity in columns (4) and (6) slightly widens the gap between the regional entitlement effects as entitlement coefficients in Pittsburgh move toward zero, while Philadelphia coefficients become more negative. The effect of controlling for unobservables on the pooled-sample results in column (2) is also small relative to standard errors.

We illustrate the estimated entitlement effects in Figures 6 and 7, which use the heterogeneity estimates from columns (4) and (6) of Table 2 to plot the estimated new job hazard in the two areas. We evaluate the hazard assuming that all claimants are entitled to either 55 or 30 weeks of UI.[22] The figures underscore two main findings. The large spikes move according to the timing of exhaustion, and following exhaustion hazards are about 2 percentage points higher than they were when UI has been available.

Recall Hazard

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 for the pooled sample of Pittsburgh and Philadelphia unemployed. The first column of Table 3 supports the hypothesis that firms strategically use compensated unemployment to hoard workers and

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.

[22]The hazard is evaluated for each spell, assuming individual-specific average values of other covariates and adjusting for time-changing values of entitlement and duration. To obtain the mean hazard rate, we integrate over the heterogeneity distribution and average over all spells.

Table 3. Recall Hazard Function Estimates.

Sample:	*Pooled*		*Pittsburgh*		*Philadelphia*	
Heterogeneity: *Variable*	*No* *(1)*	*Yes* *(2)*	*No* *(3)*	*Yes* *(4)*	*No* *(5)*	*Yes* *(6)*
UI Compensation and Demand Conditions						
Log Weekly Benefits	–0.281***	–0.378***	–0.440***	–0.487***	–0.033	0.012
	(0.107)	(0.130)	(0.149)	(0.151)	(0.158)	(0.209)
Entitlement 37 and Over	–0.014	–0.346*	0.200	0.156	–0.321	–0.677**
	(0.216)	(0.206)	(0.293)	(0.321)	(0.327)	(0.341)
28 to 36	–0.146	–0.479**	–0.068	–0.020	–0.296	–0.586*
	(0.201)	(0.192)	(0.270)	(0.292)	(0.306)	(0.317)
19 to 27	–0.362**	–0.585***	–0.261	–0.514*	–0.511*	–0.707**
	(0.179)	(0.181)	(0.238)	(0.258)	(0.276)	(0.288)
04 to 18	–0.580***	–0.749***	–0.575***	–0.560***	–0.603**	–0.754***
	(0.153)	(0.163)	(0.198)	(0.209)	(0.242)	(0.259)
01 to 3	–0.350*	–0.457**	–0.281	–0.272	–0.463	–0.573*
	(0.183)	(0.183)	(0.232)	(0.223)	(0.300)	(0.312)
–1 to 0	1.67***	1.61***	1.58***	1.59***	1.80***	1.77***
	(0.145)	(0.158)	(0.186)	(0.201)	(0.232)	(0.249)
Expected Exhaustion[a]	–0.326	–0.359	–0.213	–0.216	–0.469	–0.457
	(0.232)	(0.242)	(0.305)	(0.327)	(0.359)	(0.367)
Unemployment Rate	–0.020**	–0.033***	–0.029***	–0.035***	0.058	0.041
	(0.008)	(0.010)	(0.010)	(0.011)	(0.041)	(0.047)
Employment Growth[b]	–0.004	0.007	0.012	0.021	–0.037**	–0.053**
	(0.008)	(0.010)	(0.033)	(0.036)	(0.017)	(0.022)
Demographics						
Constant	–3.371***	—	–3.928***	—	–3.578***	—
	(0.367)		(0.573)		(0.579)	
Philadelphia	–0.201***	–0.261***	—	—	—	—
	(0.057)	(0.070)				
Log Base Period Earnings	0.246***	0.231***	0.346***	0.350***	0.129	0.129
	(0.061)	(0.077)	(0.082)	(0.081)	(0.092)	(0.126)
White	–0.029	–0.026	0.170*	0.208**	–0.136**	-0.075
	(0.052)	(0.065)	(0.096)	(0.103)	(0.065)	(0.090)
Male	0.096*	0.119*	0.109	0.128	0.065	0.052
	(0.052)	(0.063)	(0.077)	(0.077)	(0.070)	(0.093)
Married	0.196***	0.201***	0.208***	0.233***	0.159***	0.158*
	(0.041)	(0.050)	(0.056)	(0.058)	(0.061)	(0.081)
Age 25 to 34	0.042	–0.013	0.129	0.126	–0.041	–0.093
	(0.066)	(0.077)	(Q.092)	(0.094)	(0.094)	(0.0121)
35 to 49	0.232***	0.186**	0.324***	0.338***	0.123	0.069
	(0.067)	(0.080)	(0.094)	(0.095)	(0.097)	(0.126)
50 and Over	0.259***	0.294***	0.278***	0.247**	0.252**	0.297**
	(0.070)	(0.082)	(0.098)	(0.100)	(0.101)	(0.127)
Log-Likelihood	–13,378	–26,030.1	–6,929.9	–12,050.9	–6,402.7	–13,892.6

Notes: Standard errors in parentheses. All specifications include year and industry dummies and a step function in duration, which are available upon request. See Appendix A for specification of the unobserved heterogeneity distribution.

[a]Exhaustion of all benefits was expected by recipients but did not occur, because a benefits extension became effective after the regular benefits ran out.

[b]Employment growth is an industry- and SMSA-specific measure.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

smooth production. The recall hazard entitlement effect is precisely estimated for values of remaining entitlement below 27, but is close to 0 for the highest entitlement brackets. Firms recall workers in the period unemployment benefits end, and the exhaustion spike coefficient is similar in magnitude to that found in the new job hazard. One explanation for this finding is that firms recall workers at exhaustion in order to avoid losing them to other employers.[23] Again, even though based on a new identification strategy, our results confirm those found in the existing studies.

Table 3 also reports the influence of demographic characteristics and demand conditions on the recall hazard. High unemployment rates depress recall transitions. Philadelphia UI claimants face a lower overall recall probability than do Pittsburgh UI claimants. Age affects recalls differently from new job transitions, because older, more experienced workers are more likely to be recalled. Higher earnings on the last job increase the recall hazard. Unlike in the new job hazard, the expected exhaustion coefficient (capturing expected regular benefits exhaustion that did not occur) is negative and not statistically significant.

Columns (3) and (5) present separate hazard functions for Pittsburgh and Philadelphia. The likelihood ratio test strongly rejects the pooled-sample model of column (1) in favor of a split-sample specification.[24] Firms in both regions are much more likely to recall workers as soon as benefits lapse, and the exhaustion spikes are comparable across the two labor markets. Controlling for unmeasured heterogeneity in columns (2), (4), and (6) has an effect on the entitlement coefficients. The negative impact of long remaining entitlement is larger and precisely estimated in the pooled-sample specification and in Philadelphia. Pittsburgh estimates and all exhaustion spike coefficients are little affected. Even though some of the entitlement step coefficients are not statistically significant in the Pittsburgh hazard, the split-sample estimates of the entitlement effect are again consistent with those based on the pooled sample.

The average predicted recall hazards are compared under two different initial entitlement values in Figures 8 and 9. We follow the same computational strategy we did for new job hazards, and we use the heterogeneity estimates from columns (4) and (6) in order to provide an upper bound on the estimated UI effect. The results are similar to the new job findings, except for the high value of the Pittsburgh hazard at low duration. This is due to imprecisely estimated positive coefficients on the longest entitlement brackets in Pittsburgh.

In both the new job and recall hazards, the pooled-sample results appear as a weighted average of city-specific coefficients. The comparison of individuals with equal entitlement across labor markets facing different demand conditions does not affect the qualitative conclusions based solely on time variation in entitlement.

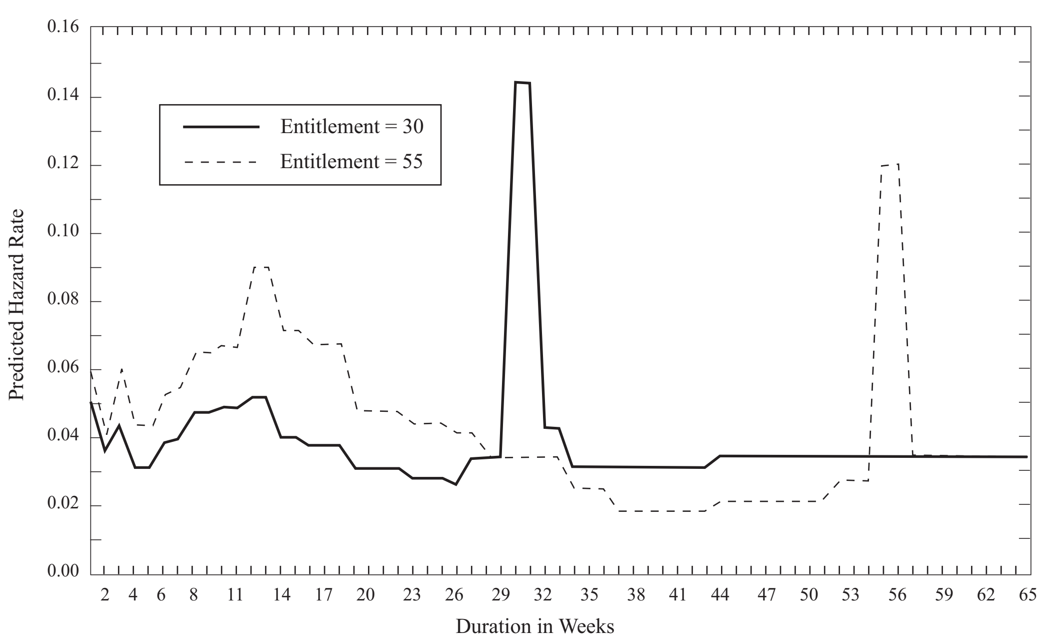
Entitlement Effect and Demand Conditions

Comparing the UI entitlement effect across the two labor markets in columns (3) and (5) of Tables 2 and 3 speaks about differences in the effect of UI on unemployment durations related to differences in demand conditions. At all remaining entitlement steps prior to exhaustion, the entitlement effect is larger, that is, more negative, in Philadelphia, where the average unemployment rate was about 5 percentage points lower. However, the differences are not statistically significant, either individually or jointly, in either of the hazards. This is consistent with the finding based on the empirical-hazard spikes at exhaustion and the unconditional exhaustion rates of extended benefits programs

[23]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 experience-rated.

[24]At 45 degrees of freedom, the χ^2 p-value is 0.00005.

Figure 8. Predicted Recall Hazard in Pittsburgh.



that there is surprising similarity in the strategic use of compensated unemployment by both workers and firms across labor markets facing dramatically different demand conditions.[25]

Regional differences or similarities in the entitlement coefficients may, however, be difficult to interpret as being solely due to the differing demand conditions. For example, the UI entitlement effect in Pittsburgh and Philadelphia may differ under comparable demand conditions but appear similar when unemployment in Pittsburgh is higher. In the subsequent analysis we therefore explicitly parameterize the interaction effect to provide direct evidence on the issue.

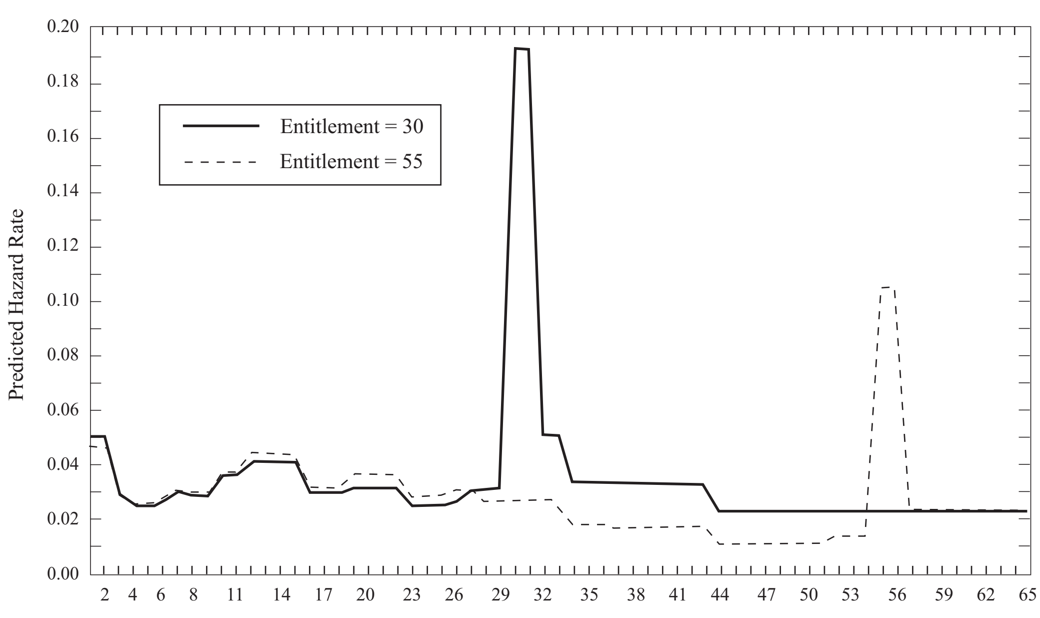
Variation in the interaction between unemployment and entitlement comes both from temporal changes in unemployment and entitlement within each area and from differences in demand conditions across the two labor markets. This allows for estimating our specification in each labor market separately as well as in the pooled sample. The interactions between entitlement and demand conditions are normalized to those who have exhausted benefits. Hence, when we interact entitlement steps with unemployment, we essentially ask whether the effect of aggregate unemployment level on individual unemployment hazards differs between those with positive remaining entitlement and those with no UI entitlement left.

New Job Hazard with Interactions

The estimated entitlement-unemployment rate interactions not controlling for

[25]The end of regular benefits in spells that were in progress when UI triggers increased entitlement has a positive and statistically significant effect on the new job hazard in Philadelphia. One explanation for this result is that workers in less depressed areas are more likely to make arrangements to begin a new job before benefits run out. On the other hand, the negative effect of weekly UI benefits on the recall hazard is large and precisely estimated in the Pittsburgh sample, but cannot be detected in the Philadelphia sample.

Figure 9. Predicted Recall Hazard in Philadelphia.



heterogeneity are presented in columns (1), (3), and (5) of Table 4.[26] The entitlement-unemployment interactions are jointly significant at the 0.1% level in all specifications. Further, the 12 entitlement coefficients (entitlement steps and their interactions) are jointly significantly different between Pittsburgh and Philadelphia.[27] A likelihood ratio test also rejects imposing the equality of all coefficients across the two areas.

The exhaustion-week spike in the new job hazard falls when unemployment rates increase in all specifications. At times of higher unemployment, fewer unemployed workers are able to begin working as soon as benefits expire. The weaker negative effect of few weeks of benefits remaining when unemployment rates are higher is consistent with the exhaustion-week spike interaction. Unemployed workers in low unemployment labor markets find it easier to become re-employed when benefits end, and they are also more likely to wait until benefits lapse before returning to work.[28]

The pooled-sample interacted results not allowing for heterogeneity suggest a stronger disincentive effect of long remaining entitlement when unemployment is higher, which conflicts with our theoretical considerations. However, this evidence is based

[26]Because neither the demographic coefficients nor the baseline hazard estimates are affected by inclusion of the unemployment-entitlement interactions, they are not reported.

[27]Differences in the interaction effects across the two labor markets may suggest non-linearities in the entitlement-unemployment effect. However, there is not enough variation in our data to estimate such non-linear interactions precisely. Alternatively, there may be no non-linear effect to estimate, as differences across areas may be due to fundamentals of job search technology.

[28]Note that the rules governing extended benefits constrain the variation in unemployment levels within labor markets for those claimants with large levels of remaining entitlement. Hence, it is perhaps not surprising that we do not find significant interactions at high levels of entitlement.

Table 4. New Job Hazard Function Estimates with Interactions.

Sample:	*Pooled*		*Pittsburgh*		*Philadelphia*	
Heterogeneity:	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
UI Compensation and Demand Conditions						
Log Weekly Benefits	–0.021	–0.193*	–0.218	–0.490***	–0.134	–0.141
	(0.070)	(0.104)	(0.161)	(0.190)	(0.141)	(0.143)
Entitlement 37 and Over	–0.353	–0.515	–0.166	–0.941	–2.157***	–2.208***
	(0.416)	(0.321)	(0.615)	(0.634)	(0.808)	(0.825)
28 to 36	–0.523	–0.549*	–0.702	–1.489***	–1.173	–1.212
	(0.397)	(0.289)	(0.532)	(0.560)	(0.738)	(0.754)
19 to 27	–0.625	–0.604**	–1.05**	–1.699***	–1.959***	–1.969***
	(0.384)	(0.285)	(0.496)	(0.565)	(0.677)	(0.699)
04 to 18	–0.770**	–0.879***	–0.846*	–1.330***	–2.408***	–2.430***
	(0.357)	(0.252)	(0.444)	(0.502)	(0.636)	(0.681)
01 to 3	–0.950**	–1.104***	–0.860	–1.350**	–3.891***	–3.936***
	(0.402)	(0.327)	(0.548)	(0.630)	(0.897)	(0.897)
–1 to 0	2.440***	2.290***	3.263***	3.084***	2.993***	2.991***
	(0.385)	(0.278)	(0.520)	(0.538)	(0.716)	(0.799)
Expected Exhaustion[a]	0.468***	0.506***	0.230	0.217	0.632***	0.628***
	(0.140)	(0.138)	(0.256)	(0.257)	(0.169)	(0.167)
Unemployment Rate	–0.033	–0.053**	–0.045	–0.094***	–0.136*	–0.139*
	(0.021)	(0.022)	(0.029)	(0.034)	(0.081)	(0.083)
Employment Growth[b]	0.035***	0.025***	0.029	0.011	0.043**	0.041**
	(0.009)	(0.009)	(0.035)	(0.051)	(1.66)	(0.017)
Unemployment Rate * Remaining Weeks of UI Entitlement						
Entitlement 37 and Over	–0.061***	–0.045	–0.055	–0.001	0.138	0.142
	(0.027)	(0.027)	(0.041)	(0.043)	(0.099)	(0.102)
28 to 36	–0.045***	–0.042	–0.015	0.038	0.020	0.022
	(0.027)	(0.026)	(0.038)	(0.041)	(0.094)	(0.097)
19 to 27	–0.039**	–0.041	0.009	0.051	0.128	0.127
	(0.027)	(0.028)	(0.037)	(0.043)	(0.089)	(0.092)
04 to 18	–0.005	0.008	0.018	0.044	0.213**	0.213**
	(0.024)	(0.025)	(0.034)	(0.039)	(0.084)	(0.090)
01 to 3	0.062**	0.079**	0.062	0.086*	0.466***	0.470***
	(0.030)	(0.032)	(0.042)	(0.049)	(0.116)	(0.116)
–1 to 0	–0.096***	–0.081***	–0.135***	–0.130***	–0.205**	–0.206*
	(0.030)	(0.030)	(0.045)	(0.046)	(0.099)	(0.111)
Log-Likelihood	–12,719.5	–26,013.8	–5,142.9	–12,012.0	–7,527.1	–13,860.5

Notes: Standard errors in parentheses. All specifications include year and industry dummies, a step function in duration, and demographic controls, all of which are available upon request. See Appendix A for specification of the unobserved heterogeneity distribution.

[a]Exhaustion of all benefits was expected by recipients but did not occur, because a benefits extension became effective after the regular benefits ran out.

[b]Employment growth is an industry- and SMSA-specific measure.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

on imposing equality of coefficients across the two areas, which is rejected by the data. Estimated sample likelihoods allowing for unobserved heterogeneity in columns (2), (4), and (6) of Table 4 again strongly support the presence of unobserved person-specific factors affecting the unemployment hazard. Yet, the effect of unmeasured heterogeneity on the parameters of interest within each labor market is small.[29]

In summary, while many of the estimated interaction coefficients in both cities are not statistically significant, their overall pattern suggests that the adverse effect of entitlement strengthens when demand conditions improve, and that this interaction effect occurs when benefits are about to lapse.

Recall Hazard with Interactions

Introducing unemployment rate–entitlement interactions (Table 5) does not allow us to precisely estimate differences in recall entitlement disincentives tied to changing demand conditions, as none of the interaction coefficients are statistically significant.[30]

Conclusion

By measuring the effect of UI entitlement on unemployment duration using within-state independence of entitlement and unemployment, this paper overcomes a potential weakness of the existing literature. Our estimates support job search theory predictions and the conclusions of the existing literature by finding a positive effect of entitlement on spell duration even in relatively tight labor markets. Contrary to our theoretical conjecture, and despite dramatic differences in demand conditions between the two areas we examine, we find only weak support for the presence of stronger UI disincentive effects in tighter labor markets.

The empirical hazards provide compelling visual evidence of the strategic use of longer entitlement, as almost a third of workers who exhausted benefits managed to find work in the next week. These reemployment spikes coincided with the potential duration of UI benefits including extensions, and they were much larger than those reported in previous studies. This may be due to the particularly deep recession that overlapped our sample period and the long UI entitlement available. However, the accurate administrative data used in this study precisely date the transitions out of unemployment, which may also contribute to the spikes at exhaustion. Over 28% of claimants even in the depressed Pittsburgh labor market were able to find work as soon as benefits ended, and two-thirds of this group found new jobs.[31] The strategic impact of exhausting benefits therefore appears to have been similar across demand conditions. Further, the probability of exhausting the various tiers of UI benefits was large and relatively little affected by differing unemployment rates in the two regions, especially for the extended benefits tiers. For a majority of workers who collected either EB or FSC, larger entitlement led to increases in unemployment for at least as many weeks as benefits were available, as the exhaustion rates for the benefits extension programs reached far over 60% in both regions.

This similarity between the regions is not eliminated by conditioning on other factors. We estimated new job and recall hazards in the two cities controlling for both the effect of observable region-specific and person-specific variables and the influence of unobservables. While there are some differences in the estimated entitlement coefficients across the two areas facing different levels of unemployment, these differences are neither statistically nor quantitatively significant.

[29]We were unable to identify the interaction effect between the dollar amount of the UI benefits and the unemployment rate. This may be due to a lack of variation in the level of benefits separate from the variation in previous wages. Unlike the frequently changing entitlement level, the generosity of UI benefits (expressed as the ratio of wages to benefits) never changed. The UI benefits coefficient is therefore identified solely off the minimum and maximum levels of benefits.

[30]With the exception of two coefficients in the Pittsburgh recall hazard after controlling for unobservables. As in the case of the new job hazard, pooling the sample was strongly rejected.

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

Table 5. Recall Hazard Function Estimates with Interactions.

Sample:	*Pooled*		*Pittsburgh*		*Philadelphia*	
Heterogeneity:	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*
Variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
UI Compensation and Demand Conditions						
Log Weekly Benefits	–0.192**	–0.351***	–0.442***	–0.710***	–0.029	0.016
	(0.093)	(0.129)	(0.149)	(0.175)	(0.158)	(0.209)
Entitlement 37 and Over	–0.642	–0.053	0.497	–0.877	–0.783	–1.414
	(0.514)	(0.414)	(0.567)	(0.757)	(1.197)	(1.356)
28 to 36	–0.986*	–0.436	–0.379	–1.698**	–2.028*	–2.337*
	(0.508)	(0.407)	(0.550)	(0.738)	(1.189)	(1.341)
19 to 27	–1.032**	–0.508	–0.211	–1.372*	–0.989	–1.071
	(0.504)	(0.410)	(0.541)	(0.727)	(1.148)	(1.299)
04 to 18	–1.385***	–0.966**	–0.664	–1.587**	–1.866	–1.996
	(0.500)	(0.413)	(0.537)	(0.718)	(1.151)	(1.304)
01 to 3	–1.894***	–1.211**	–1.217	–2.105**	–1.619	–1.869
	(0.574)	(0.511)	(0.754)	(0.830)	(1.592)	(1.966)
–1 to 0	1.151***	1.830***	1.784***	1.345*	3.373***	3.417**
	(0.534)	(0.449)	(0.611)	(0.778)	(1.252)	(1.451)
Expected Exhaustion[a]	0.061	–0.325	–0.190	–0.123	–0.385	–0.372
	(0.234)	(0.242)	(0.306)	(0.331)	(0.361)	(0.368)
Unemployment Rate	0.004	–0.031	–0.025	–0.108**	–0.055	–0.079
	(0.029)	(0.036)	(0.039)	(0.054)	(0.1,13)	(0.174)
Employment Growth[b]	–0.267	0.765	0.012	–0.011	–0.037**	–0.053**
	(0.827)	(0.995)	(0.033)	(0.039)	(0.017)	(0.022)
Unemployment Rate * Remaining Weeks of UI Entitlement						
Entitlement 37 and Over	–0.038	–0.016	–0.028	0.054	0.059	0.091
	(0.031)	(0.036)	(0.041)	(0.056)	(0.158)	(0.179)
28 to 36	–0.009	0.006	0.021	0.099*	0.221	0.222
	(0.031)	(0.036)	(0.041)	(0.056)	(0.158)	(0.179)
19 to 27	–0.027	–0.010	–0.011	0.057	0.061	0.045
	(0.032)	(0.037)	(0.042)	(0.057)	(0.156)	(0.177)
04 to 18	–0.006	0.014	0.003	0.052	0.181	0.177
	(0.032)	(0.038)	(0.043)	(0.056)	(0.156)	(0.176)
01 to 3	0.066	0.067	0.077	0.122*	0.169	0.188
	(0.040)	(0.046)	(0.053)	(0.065)	(0.216)	(0.270)
–1 to 0	–0.036	–0.037	–0.020	–0.002	–0.226	–0.236
	(0.038)	(0.043)	(0.051)	(0.064)	(0.173)	(0.202)
Log-Likelihood	–13,355.6	–26,013.8	–6,924.3	–12,012.0	–6,393.3	–13,860.5

Notes: Standard errors in parentheses. All specifications include year and industry dummies, a step function in duration, and demographic controls, all of which are available upon request. See Appendix A for specification of the unobserved heterogeneity distribution.

[a]Exhaustion of all benefits was expected by recipients but did not occur, because a benefits extension became effective after the regular benefits ran out.

[b]Employment growth is an industry- and SMSA-specific measure.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

The Philadelphia-Pittsburgh research design further allows one to directly evaluate the impact of UI rules under different economic conditions by estimating entitlement-unemployment interactions. There are two sources of variation one could potentially use. First, the interactions estimated separately for each region using only temporal variation suggest that the entitlement effect on the new job hazard was sensitive to demand conditions when benefits were about to expire. The effect of an impending lapse of benefits retreated from the week of actual exhaustion as unemployment rates increased. In times of higher unemployment, fewer unemployed workers were able to begin working in the week when benefits expired; the exhaustion hazard spike is less concentrated.[32] Second, the research design suggests the possibility of combining the within-region temporal variation with the across-region differences in demand conditions. Unfortunately, the data reject constraining the effect of overtime variation within regions to be the same as the effect of across-region variation. Future work employing a research design similar to ours, perhaps using data from multiple states, can test this constraint and attempt to provide an estimate of the interaction effect based on both sources of variation.

Our findings have two implications for policies designed to aid the unemployed while minimizing the distorting effects on decision-makers. First, the high incidence of exhausted benefits in both extended benefits programs, combined with the dramatic spike at the moment of exhaustion even in deeply depressed labor markets, suggests that greater focus needs to be put on incentives for rapid reemployment. It may be possible to experiment with a reemployment bonus that allows workers to keep a fraction of future extended benefits if they find new jobs as previously tested with regular UI benefits.

Second, our estimates measure the size of the disincentive effect of providing extended UI coverage across different demand conditions; therefore, we can evaluate the cost of the extended benefits programs in terms of lengthening the average duration of unemployment. A comparison of this cost across tight and slack labor markets is important for evaluating the potential benefits of implementing sub-state trigger mechanisms. If the entitlement effect were larger in areas or times of lower unemployment, implementing sub-state trigger EB would lower the disincentive of UI while providing the benefits of EB to depressed areas. However, we find a broadly similar entitlement effect in low- and high-unemployment areas. Strategic use of UI entitlement appears strong and only mildly related to demand conditions.

We cannot fully evaluate the costs and benefits of the extended benefits system based on our results. It is plausible that extended coverage programs provide the unemployed with benefits that are not measured in this paper. If these benefits are small, then our findings call the entire EB program into question.

Benefits provided by extended coverage programs may be important only in depressed labor markets, where the UI disincentive effect may be offset by improved worker-firm matches, which raise post-unemployment earnings. To the extent that one may expect a stronger positive effect of providing extended coverage in the depressed areas, where workers who otherwise might be forced into low-wage jobs or onto welfare rolls may thus be enabled to wait until the economy improves, such coverage would appear less efficient in tighter labor markets. 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 relationships among earnings, entitlement, and demand conditions remain an important

[32]We believe that this finding is not in opposition to our earlier conclusion, based on the comparison of the traditional specifications across the two regions, that there is only weak evidence of stronger UI disincentive effects in tighter labor markets. The two sets of results differ in source of identification. Comparing labor markets with different levels of unemployment does not allow us to detect temporal changes in the entitlement effect related to temporal changes in demand conditions. Further, the interaction effect is not pervasive: (a) it is not present for long remaining values of entitlement; (b) the interaction near exhaustion is strong in Philadelphia, which had less variation in demand conditions than Pittsburgh; and (c) we find no sensitivity of the UI effect to demand conditions in the recall hazard.

area of research that needs to be explored in order to fully assess the impact of additional weeks of UI compensation.[33]

Extended benefits programs could target depressed areas using sub-state triggers. The ad hoc legislation providing extra weeks of compensation could also be directed more precisely to regions or groups most affected by economic downturns. 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. In our case, such policy would deny the benefits extensions to the unemployed in Philadelphia.

To simulate the effect this policy would have on the average duration of unemployment, we use the estimated hazard functions to calculate the expected duration of unemployment for the sample of Philadelphia claimants. (See Appendix B for the formulas used in these calculations.) We compare the expected duration based on the actual level of initial entitlement for each worker to the simulated expected duration assuming that the extended benefits programs are not available so that claimants are only entitled to regular UI.[34] Denying the benefits extensions over the sample period shortens the expected duration of unemployment in Philadelphia by about one week, from 18.7 weeks to 17.7 weeks.[35]

A final caveat is that our sample period covers a particularly deep recession. It remains to be seen if estimates based on less depressed labor markets would provide similar findings.

[33]The evidence on the earnings effect of UI is mixed. Recently, Addison and Blackburn (2000) provided estimates based on U.S. data on displaced workers from the late 1980s. Relying on an array of approaches from the existing literature, they found little evidence of beneficial effects of UI on wages using samples consisting of claimants only.

[34]To provide an upper bound on the effect, we use the heterogeneity estimates from columns (6) of Tables 2 and 3.

[35]We also simulate the decrease in expected duration when entitlement is lowered from 55 to 30 weeks for all Philadelphia claimants. That is, we first assign all claimants 55 weeks of entitlement (irrespective of whether they were actually entitled to benefits extensions) and then, in a second simulation, lower the maximum benefit duration to 30 weeks. This results in a 1.87 week reduction in the expected duration.

Appendix A
Parameterization of the Heterogeneity Distribution

Here, following Heckman and Singer (1984), we extend the description of the econometric duration model by introducing the unobserved heterogeneity. Let $\lambda_j(t,x_t|\theta_k^j)$ be the hazard of leaving unemployment at duration t for someone with person-specific characteristics x_t, conditional upon this person having the unobserved factor θ_k^j, $k = 1,2,\ldots, N_\theta^j$, $j \in \{r,n\}$:

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

where

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

The example of a likelihood function contribution for someone with two completed spells of unemployment is given below. Assume that the first spell starts in week *1* and ends with a recall in week *s*, and the second spell starts in week *p* and ends in a new job in week *w* (at duration $w-p$). Then,

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

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

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

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

One can compute the individual contributions to the sample likelihood for other scenarios 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.

Following McCall (1996), we are assuming an *M*-tuple distribution of unobservables, where *M* is the number of hazards to be jointly estimated. The 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.

Heterogeneity Distribution with M*-tuples*

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

Appendix B
Expected Duration Simulations

The expected duration is computed as

(10) $$E(t) = N^{-1}\sum_{n=1}^{N}\{\sum_{t=1}^{99} tf(t|\bar{x}_{it}) + 100(1-F(99|\bar{x}_{i99}))\},$$

where N is the number of spells, the vector $\bar{x}_{it}$ includes the time-changing duration and entitlement as well as the averages of all other explanatory variables for spell *i*, and $f(t|\bar{x}_{it}) = F'(t|\bar{x}_{it})$ denotes the unconditional probability of leaving employment at duration *t* through either recall or new job finding, calculated as

(11) $$f(t|\bar{x}_{it}) = \sum_{k=1}^{N_\theta^n}\sum_{m=1}^{N_\theta^r} p(\theta_k^n,\theta_m^r)\,f(t|\bar{x}_{it},\theta_k^n,\theta_m^r),$$

(12) $$f(t|\bar{x}_{it},\theta_k^n,\theta_m^r) = \{\lambda_n(t,\bar{x}_{it}|\theta_k^n) + \lambda_r(t,\bar{x}_{it}|\theta_m^r) - \lambda_n(t,\bar{x}_{it}|\theta_k^n)\lambda_r(t,\bar{x}_{it}|\theta_m^r)\}\times \prod_{v=1}^{t-1}[1-\lambda_n(v,\bar{x}_{iv}|\theta_k^n)]\,[1-\lambda_r(v,\bar{x}_{iv}|\theta_m^r)].$$

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