The Match Quality Gains from Unemployment Insurance

Mário Centeno

ABSTRACT

This paper assesses the benefits of unemployment insurance (UI) by measuring its effect in match quality. We note that UI generosity should affect the decision to match or not and should therefore have some effect on match quality. Using NLSY data, we analyze the relationship between post-unemployment job tenure and measures of the state-level UI generosity and the unemployment rate at the time the job is started. We show that greater UI generosity leads to longer job tenure. Furthermore, we find some evidence that this effect is more pronounced during busts, UI having a limited dampening effect on the cyclical variation in match quality.

I. Introduction

In the vast literature examining the consequences of the unemployment insurance (UI) system, there are only a few studies addressing the benefits of the program to post-unemployment outcomes. Furthermore, the few papers on this subject concentrate on the effects of UI on post-unemployment wages (see Burtless 1990, Cox and Oaxaca 1990 and, more recently, Addison and Blackburn 2000), while the effects of UI benefits on the quality of labor market adjustments, namely on job/employment stability, have been neglected. This is rather surprising because one possible beneficial effect of the UI system arises precisely if it allows workers to search longer before taking a job. In this case, they might be able to secure more desirable jobs, which they will be less likely to quit, probably because these jobs represent better matches between employer and worker and end up lasting longer as a result.

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The goal of this paper is to assess the benefits of UI by measuring the effect of this program generosity on post-unemployment job tenure over the business cycle. One approach to doing so would be to specify a structural model which, allowing for business cycle variation, incorporated features such as the full response of individuals and firms, and hence match quality, to changes in UI. The comparative statics of this model would yield the effect of changes in UI generosity on match quality over the cycle. Such a model, however, would not only require a number of assumptions to be tractable but its calibration would involve a variety of parameters which have not been empirically estimated to date.

We propose an alternative reduced-form strategy, which is to directly estimate the relationship between match quality (measured by job tenure) and the level of UI generosity. The resulting regression model is identified by using differences in UI benefits availability across individuals in the U.S. Differences in the benefits available to the unemployed arise from the substantial variation in the generosity of the state-administered programs across states and over time within states. This kind of variation has been used in previous studies to assess the benefits of UI, namely in Levine (1993) to estimate the spillover effect between insured and uninsured unemployed, and in Gruber (1997) to estimate the consumption smoothing effect of UI generosity. Previous empirical research on the UI impact on subsequent job duration includes Belzil (1995) and (2001). These papers focus on UI duration and explore the change in the initial entitlement period rule implemented in Canada in 1977, and found a weak positive impact of the maximum benefit duration on subsequent job duration for a sample of young Canadian male workers.

Match quality is difficult to quantify empirically. In this paper, we rely on theory to identify job tenure with match quality. The idea of representing a good match by a lengthy duration comes from Jovanovic (1979), where a match is a pure experience good: the quality of a match is not known ex ante, but must be experienced. Additionally, the evidence from Akerloff, Rose, and Yellen (1988) that nonpecuniary match aspects have a negative impact on the probability of individual quits can also be seen as supportive of the interpretation of tenure as a proxy to match quality. Under the assumption that "good matches endure", we can argue that looking at match duration, while conditioning on the starting wage, implies that the focus is on the other aspects of match quality that impact upon mobility and other labor market adjustments.

The available models of match quality say that outside option matters. From the worker's point of view, this outside option is higher in booms because the market is thicker. Therefore, jobs formed during booms should have higher match quality and last longer. A substitute for a thick market at a point in time can be longer periods of search in a thin market. We argue that the UI benefit system can play an important role in the job-search process by thickening the market and improving post-unemployment match quality.

The hypotheses to be tested in this paper fit in the following general framework: is the quality of matches affected by UI generosity, with more generous benefits leading to an increase in match quality? Furthermore, is the cycle effect on match quality smoothed out by more generous UI systems meaning that the UI benefit impact is greater in recessions?

The model presented in Marimon and Zilibotti (1999) produces these basic predictions. It illustrates the impact of UI policies on the tradeoff between unemployment and job mismatch and generates the key predictions between UI and post-unemployment match quality tested in this paper, namely that a more generous UI benefit system helps in reducing job mismatch. Their model does not explicitly address the relationship between economic fluctuations and job mismatch. However, it relates, on the one hand, the tightness of the labor market with the level of match quality and, on the other hand, UI generosity and labor market tightness, which are the other hypotheses being tested in this paper.

The intuition behind these hypotheses is that people receiving higher UI benefits will have a lower opportunity cost for search (this operates through the income effect of UI). The UI benefit is seen as a search subsidy (Burdett 1979), allowing both more search effort and a longer search period. The unemployed are given time to find, not just a job, but the right job. As a result, better matches are expected to come out of the search process because people can afford to wait longer for a better match.

This also relates to the cyclical behavior of match quality. If the quality of matches is adversely affected by the cycle (as shown in Bowlus 1995) and if the UI benefit allows people to be more "selective," the congestion pressure and negative spillovers that result from a large number of searchers can be attenuated, and the expected quality of job matches increases. The UI system should be more relevant during busts because it is the only method of "thickening" the market (more search effort per period might be useless). This will have an important implication on the cyclical behavior of match quality. In fact, we should expect the procyclical behavior of match quality to be smoother in more generous UI systems.

We use NLSY79 data for the period 1979–98 covering more than one entire business cycle. In order to test the hypotheses presented above, we will follow previous work by Topel (1984) which suggests that we can use differences in UI benefit systems across states and over time within states as a proxy for changes in the opportunity cost of being unemployed. The quality of a job match is identified by the duration of the employment spell (as in Bowlus 1995), and the business cycle is measured with the state unemployment rate.

There are two key results in this paper. First, we show that job tenure is positively related with the generosity of the UI system. The total effect of the UI benefit level is a reduction of the employment hazard: larger UI benefits lead to longer, post-unemployment, employment spells after controlling for region of residence and year fixed effects. This result has a difference-in-difference interpretation because the coefficient on the UI benefit only captures changes across both regions and years. An increase in UI generosity equal to one standard deviation of the UI benefit measure increases median tenure from 57 to 61 weeks and shifts the survival probability evaluated at two years of tenure by three percentage points. Secondly, we show that the benefit level smoothes out the cyclicality of match quality. This result means, on the one hand, that the higher the UI benefit the smaller the effect of the starting unemployment rate on the employment hazard, and, on the other hand, that at higher levels of the unemployment rate the UI benefit has a larger impact on the employment hazard reduction.

The remainder of the paper is organized as follows. In Section II we present a brief theoretical motivation and review the existing evidence on the impact of UI on post-unemployment outcomes. Section III describes the data and the criteria used to construct the sample. The econometric methodology is described in Section IV and

Section V presents the main empirical results from the hazard rate model estimation. Section VI summarizes our main findings and presents suggestions for future research.

II. Theory and Previous Evidence

There are basically two alternative views about the way the job matching process evolves. We can see the job matching process as one of search for new employment positions. A Diamond-type model (Diamond 1982) or Jovanovic's (1979) model can be used to describe the mechanisms that allow workers to achieve a better job match, usually characterized as a job with higher wage. In this way, the matching process depends on factors such as outside opportunities and expectations about future wages. Alternatively, one can see the process of job matching in the context of a nonmarket clearing model of the labor market, in which wages are fixed above the equilibrium level, jobs are rationed, and the main force behind the process of job changes is the so called vacancy chain. In this kind of model quits are procyclical because vacancy chains are longer when unemployment is low (see Akerlof, Rose, and Yellen 1988).

Each of these views yields a different set of predictions about the results of the job matching process in different phases of the cycle. During cyclical downturns we can identify two competing forces in action. One is a congestion pressure from the increased number of workers not employed, making it harder for workers to secure employment. The other is an agglomeration effect: Because more people are around unemployment is higher, and if the number of vacancies is countercyclical, we can expect employers to make better matches. Stylized facts in labor economics indicate that wages are procyclical and most of the quits are voluntary, concentrated in low-wage jobs and motivated by an improvement in the wage earned. This might imply that a person who finds a job during a recession, when wages are depressed, should be more likely to change jobs in the future, when better paid matches are available. Evidence of this is reported, among others, by Bowlus (1995). This points out the crucial fact that the acceptance of a job match is a function of the expectations about finding better jobs and the outside opportunities available for each worker.

The impact of labor market policies, particularly the UI system, on productivity and job mismatch has recently been examined in several theoretical papers. Marimon and Zilibotti (1999) present a model of the role of UI on mismatch and unemployment and show the positive impact of the UI system on the reduction of job mismatch. In a related paper, Acemoglu and Shimer (2000) analyze the productivity gains from more generous UI systems. Considering risk-averse workers, they show that UI increases labor productivity by encouraging workers to seek higher-productivity jobs and by encouraging firms to create these jobs. In their setting, the UI is more than a search subsidy, and affects the type of jobs that workers look for and accept.

The model in Marimon and Zilibotti (1999) can be used to generate the key predictions tested in this paper. They show that more generous UI systems lead to lower mismatch and higher productivity per worker. In their model the UI has the standard effect of reducing employment, but also helps workers to get a suitable job. Furthermore, in tight labor markets people tend to be very choosy, and to only accept highly suitable jobs. They also prove that market tightness can be affected by the UI benefit system's generosity, higher benefits leading to an increasing pickiness by workers and reducing job mismatch. In this paper we empirically investigate these predictions and assess if they translate into higher match quality, as measured by longer job tenure. We extend their analyses to study the impact of the UI generosity on the cyclical behavior of match quality, namely its ability to smooth job tenure cyclicality.

In addition, we need to take into account that benefits do not last forever and that the effect of UI in the reservation wage might not be constant over time. UI benefits are received for a fixed period of time. If someone unemployed reaches the end of this period without finding an "acceptable match", we might expect the reservation wage to gradually fall over time. Following Mortensen (1977),[1] the escape rate from unemployment decreases for a newly laid-off worker eligible for UI benefits with both the benefit duration and the benefit amount. This result implies that the congestion pressure will decrease with both UI parameters. For a noneligible or exhausted worker the opposite result applies, given that the value of being unemployed and eligible for UI benefits following an employment spell increases with the benefit level. This effect might be different during booms and recessions. In fact, this restriction is expected to be more active during recessions, when it is more difficult to find "better" jobs, leading to a higher impact of the UI benefit. In other words, during recessions the worker is more likely to exhaust the UI benefits and to be in the region of his reservation wage where it is falling steeply over time—the UI benefit being more effective in the reduction of the congestion pressure and in thickening the market.

On the empirical side, there are only a limited number of studies addressing the impact of UI on post-unemployment outcomes, and they have concentrated almost exclusively on the wage dimension of match quality. Belzil (2001) is the only study that we are aware of that looks at job duration. He explores a reduction in the initial entitlement period rule in Canada to study the impact of UI duration on subsequent job duration, and reports a weak but positive impact of the maximum benefit duration on subsequent job duration, for a sample of young Canadian male workers. Addison and Blackburn (2000) analyze the impact of UI on post unemployment job stability using a sample of displaced workers taken from the Current Population Survey. They find weak positive evidence that UI recipients are more likely to have had only one job following displacement (rather than holding more than one job).

The literature on the impact of UI on post-unemployment earnings is not completely conclusive. While most studies found a positive (frequently small) impact; some studies found it to be statistically nonsignificant. The best-known paper is probably the one by Ehrenberg and Oaxaca (1976). They found evidence that higher UI benefits make the process of job search more productive, resulting in longer unemployment spells and higher post-unemployment wages. Their results indicate that an increase in UI benefits has a significantly greater impact in the older male sample, than in the female and younger male samples. A number of other studies are reviewed in Burtless (1990), and Cox and Oaxaca (1990), which draw seemingly opposite conclusions from the available evidence. More recently, Addison and Blackburn (2000) conclude for the existence of, at most, a weak effect of UI on post-unemployment

1. See Mortensen (1977) Propositions 3 and 4.

earnings. Their results point to a positive, but statistically nonsignificant, effect of UI for a sample taken from the 1988, 1990 and 1992 Displaced Workers Supplement from the January Current Population Survey. The main conclusion from this literature is that studies using samples of UI claimants found little beneficial effects of UI on wages, while studies comparing recipients with nonrecipients usually found more significant impact estimates.

As I have advanced above, match quality is not only made of wages, but also of other job aspects that together combine to impact worker mobility. The use of tenure as a measure of these other characteristics, and thus of match quality, can be justified by either characterizing the match as a pure experience good as in Jovanovic (1979) or by taking the evidence on the importance of nonpecuniary job characteristics on voluntary quits presented in Akerloff, Rose, and Yellen (1988). The motivation for the approach followed in this paper comes from the fact that looking at match duration after controlling for the starting wage will place our focus on these other characteristics of match quality.

III. Data

The adequacy of the data to be used in this paper can be assessed by two factors: the coverage of a complete business cycle and the quality of tenure data. In fact, the ideal data should cover multiple business cycles in order to be able to capture more of the timing issues mentioned above.

The data used come from the NLSY79. The NLSY79 is a panel started in 1979 with 12,686 men and women with ages ranging from 14 to 21. In this study we include data through 1998. The subsample used in this study includes only men due to the higher probability that a female employment spell terminates for reasons different from poor quality of the match, for example, due to personal reasons such as pregnancy, marriage or moving due to spousal relocation. The male NLSY79 subsample consists of 6,403 individuals.

The NLSY79 panel was transformed so that each observation corresponds to an employment spell. For each individual several observations were created. In the NLSY79 this is possible to do using the variable in the Workhistory database that links jobs over years. Because each individual reports up to five jobs each year, the employer code is used to construct a variable for each job that contains information on whether the job was a match reported in the previous year. The database resulting from this procedure has 39,401 employment spells, an average of 6.15 for each individual.[2]

For each observation we have collected specific worker and match information. The match information includes the initial and final month and year of the employment spell, total tenure in weeks, union coverage status, the starting wage and information on the industry and occupation. For each individual the following additional personal characteristics also were obtained: age at the beginning of the job, race, highest grade completed, school enrolment status, state of residence, and marital status. To

2. In the empirical analysis we did not include the military subsample. Self-employment and government job spells were also excluded from the final sample.

each observation we added data on the monthly unemployment rate for the state of residence at the beginning of the employment spell (taken form the Local Area Unemployment Statistics of the Bureau of Labor Statistics) and a measure of the state UI system generosity.

We considered two measures of UI generosity: the maximum weekly benefit allowed by the state of residence of the individual and a simulated UI benefit. If the hypothesis being tested is correct, the source of variation being the UI benefit level, the maximum benefit is an upper bound of the actual value that each individual can claim, and so it is the maximum expected revenue during the unemployment period.[3] However, using the maximum benefit is not altogether satisfactory. Although it has the advantage of being independent of each individual experience, and thus avoids simultaneity problems of using the actual benefits received, it does not use the actual structure of each state's UI benefit system. The maximum benefit also can be quite responsive to the state's subsidy policy, reflecting, for example, changes in the system motivated by cyclical adjustments of the state's policy. In an attempt to overcome these problems, we use a simulated UI benefit as a measure of UI generosity. The simulated benefit allows us to use individual variation in how each state's UI system structure applies, rather than each individual's actual experience. We follow previous work by Levine (1993) and compute this measure using the knowledge of each state's UI benefit structure in each year and the information on prior jobs and on worker's characteristics to construct individual-based measures of UI benefit.[4] For each pair (worker i, state j) we computed the benefit that worker i would receive if she were to be unemployed in state j. The simulated benefit for each state/year pair is the average benefit that the individuals in the sample would receive in each state and year. We present results using both variables as proxies to UI generosity.[5]

The following criteria were applied to reject observations: we did not include observations with a tenure less than or equal to two weeks, only post-education employment spells were considered and the individual must be 16 years or older. Given that the variable of interest is job tenure in matches preceded by unemployment spells, we have included only employment spells that follow an unemployment experience in which the individual was eligible to receive some kind of unemployment compensation.[6] This was done by matching information about the current employment spell with information about the previous employment experience. Due to the significant number of nonresponses, all the observations were checked for missing information. Those with missing information were deleted. After this we were left with 5,011 observations from 2,688 individuals, an average of 1.86 observations for

3. A measure of the minimum state UI benefit and of the state replacement ratio computed as in Levine (1993) were used in the estimation of the proportional hazard model, the results obtained were qualitatively similar but weaker than those obtained with the simulated and maximum state benefits.

4. The computation of this measure of UI benefit requires information about marital status, number of children, total income in previous year, and weekly wage. All of these variables are available in the NLSY79.

5. Note that these measures are all income-based. It would be interesting to use measures of eligible UI benefit duration, but there is no state-level variation in UI duration to allow us to identify its relationship with job tenure.

6. The NLSY79 does not have information on UI eligibility and actual eligibility is hard to impute. We used information on the reason for job displacement and on the number of weeks worked at the previous job to define the eligibility criteria. Note that eligibility criteria vary over time and states and the criteria used in this paper are only a raw approximation to the actual criteria applied in each state and year.

Table 1
Variables used, Description, Mean and Standard Deviation

Variable	Description	Mean	Standard Deviation
Tenure	Tenure in weeks	95.42	124.57
Censor	0 = censored 1 = not censored	0.87	0.33
Age	Age in years at the beginning of the spell	26.37	4.25
Union	0 = not union sponsored job 1 = union sponsored job	0.23	0.42
HS	0 = did not obtain high school diploma 1 = obtained high school diploma	0.69	0.47
CLG	0 = did not complete 16 years of school 1 = completed 16 years of school	0.12	0.32
White	0 = Black or Hispanic 1 = not Black or Hispanic	0.70	0.46
NU0	National monthly civilian unemployment rate	6.49	3.39
STU0	State monthly civilian unemployment rate	6.63	3.87
MS	0 = not married 1 = married	0.30	0.45
StW	Hourly real starting wage (at 1979 USD)	5.05	19.19
Max UIB	Maximum weekly state unemployment insurance (at 79 USD)	118.88	21.84
Sim UIB	Simulated weekly state unemployment insurance (at 79 USD)	75.39	22.42

The data are composed of male workers in NLSY79 over the period 1979–98. The construction and definition of variables are contained in Section III. When required, variables are deflated using the CPI (1979 = 100). The maximum weekly state benefit and the simulated weekly benefit were constructed from a file kindly provided by Marianne Bertrand, updated with the new regulations concerning UI benefits up to 1998, and follow the procedure used in Levine (1993). The state monthly civilian unemployment rate is from the Local Area Unemployment Statistics of the Bureau of Labor Statistics, for the period 1978–98. Starting hourly wages correspond to the variable "Hourly rate of pay" as computed in the NLSY79. This variable records the hourly wage rate for each reported job.

each individual. Table 1 presents a description of the most important variables used in the study, their sample means and standard deviations.[7]

With these data a first look at the relationship between tenure, the UI system and the unemployment rate was conducted by examining median tenure across levels of the state monthly unemployment rate and the real simulated UI benefit (Figures 1 and 2). The sample in Figure 1 includes all the employment spells following a period

7. The procedure used to construct the final sample might end up biasing it with respect to the initial one. In order to check for this possibility, Table A1 in the Appendix provides the same set of summary statistics for this larger sample. The comparison of Tables 1 and A1 shows that, in both samples, the main variables have very similar values. In particular, this is true for the demographic indicators, such as age, schooling, or race, and for the match specific indicators, such as tenure or the starting wage.

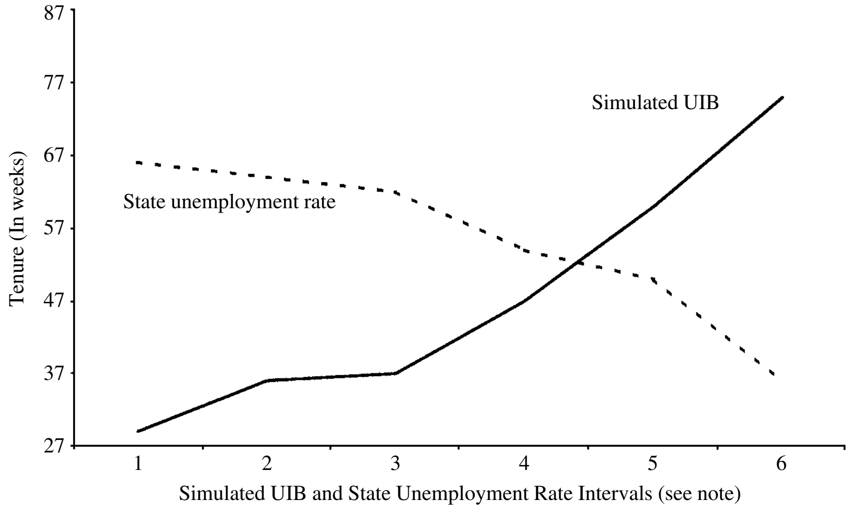

Figure 1
Relationship between Tenure, State Unemployment Rate and Unemployment Insurance Benefits (UI eligible sample)

Notes: This graph plots the median tenure for each of six groups of observations. Each observation corresponds to an employment spell that follows a period of unemployment during which the individual was eligible to receive UI benefits. The number of observations is 5,011. The observations are grouped into six levels of the real simulated UI benefit and into six levels of the state monthly unemployment rate at the start of the job. For the real simulated UI benefit the groups were defined by the following limits: < 56 USD, 56–63 USD, 64–71 USD, 72–78 USD, 79–88 USD, and > 88 USD. For the state monthly unemployment rate at the start of the job the groups were defined by the following limits: < 5.4 percent, 5.4 percent-6.2 percent, 6.3 percent-7.1 percent, 7.2 percent-7.9 percent, 8.0 percent-9.1 percent, and > 9.1 percent. For each group the median tenure using the Kaplan-Meier nonparametric procedure is then computed.

of unemployment in which the individual was eligible to receive the subsidy. The sample observations were grouped according to the unemployment rate at the start of the job and of the real simulated UI benefit in the state and year.[8] For each group we calculated the median tenure using the Kaplan-Meier density function method (see Lancaster 1990) to take into account the presence of right-censored observations.

In Figure 1 median tenure declines from 66 weeks when the unemployment rate at job initiation is less than 5.4 percent to around 36 weeks for the group of observations for which it is higher than 9.1 percent (similar trends show up in the Bowlus 1995 data). Median tenure is 29 weeks for the group of observations with real UI benefit less than 56 USD and it is 75 weeks for the group of observations with real UI benefit greater than 89 USD. The graph shows that job tenure and state unemployment rate

8. We grouped the observations into six levels of the starting monthly unemployment rate and than calculated the median tenure for each group (dashed line in Figures 1 and 2). Next, we grouped the observations into six levels of the real simulated UI benefit and calculated the median tenure for each group (solid line in Figures 1 and 2).

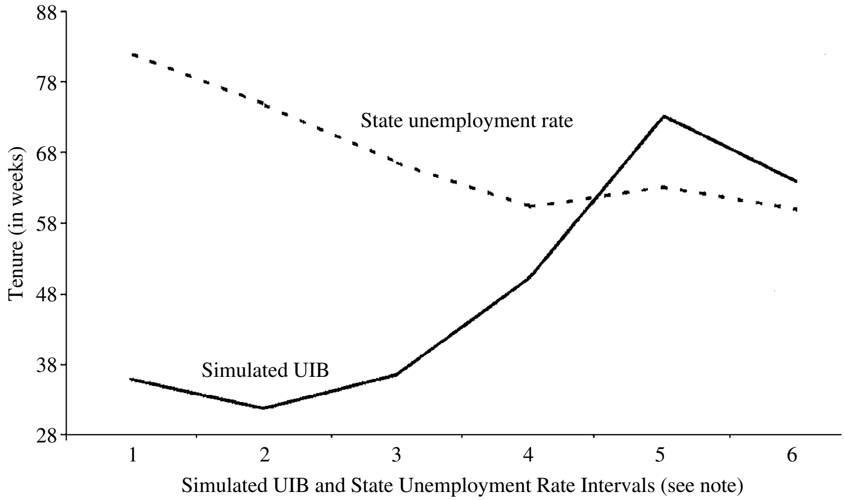

Figure 2
Relationship Between Tenure, State Unemployment Rate and Unemployment Insurance Benefits (Noneligible/Not unemployed sample)

Notes: This graph plots the median tenure for each of six groups of observations. Each observation corresponds to an employment spell that either was not preceded by a period of unemployment or that followed an unemployment spell in which the worker was not eligible to receive UI benefits. The number of observations is 34,390. The observations are grouped into six levels of the real simulated UI benefit and into six levels of the state monthly unemployment rate at the start of the job. For the real simulated UI benefit the groups were defined by the following limits: < 55 USD, 56–61 USD, 62–69 USD, 70–77 USD, 78–88 USD, and > 88 USD. For the state monthly unemployment rate at the start of the job the groups were defined by the following limits: < 5.0 percent, 5.0 percent-5.8 percent, 5.9 percent-6.6 percent, 6.7 percent-7.4 percent, 7.5 percent-8.7 percent, and > 8.7 percent. For each group the median tenure using the Kaplan-Meier nonparametric procedure is then computed.

are negatively related, whereas job tenure and the simulated UI benefit are positively related.

We also have calculated these medians using the employment spells that were not preceded by a period of unemployment or that follow an unemployment spell in which the worker was not eligible to receive UI benefits. This larger sample has 34,390 observations.

The results presented in Figure 2 indicate that for this sample the relationship between median tenure and real UI benefit is not as clear as the one presented in Figure 1, especially at low levels of the simulated UI benefit, but we still found a negative relationship between median tenure and the state level unemployment rate. This is an interesting result because this larger group might be considered a "control group" as opposed to the "treated group" of unemployed people eligible to receive some kind of unemployment compensation before starting a new job. In some sense, the fact that the positive relationship between the simulated UI benefit and job tenure did not show up in the "control group" can be seen as evidence favorable to the hypothesis being tested.

IV. Econometric Methodology

The econometric methodology consists of estimating a proportional hazard model, including information about the UI benefit level for each worker's state. The hazard function represents the probability of employment termination conditional on the duration lasting up to t. It has the following form:

$$(1) \quad \lambda(t|X(t) = \lambda_0(t)\exp(\beta' X(t))$$

and the vector X includes both individual and job characteristics at the start of the match. The individual characteristics included are age, marital status, schooling level, and race; the available job characteristics are union coverage status, industry and occupation dummies. We also included in X a measure of the UI benefit, the unemployment rate at the start of the job and an interaction between the state UI benefit and the unemployment rate, which are the key variables to test the hypotheses presented above.[9] In Equation 1, λ_0 is the baseline hazard rate at time t for the covariate vector $X(t) = 0$. The estimation procedure follows Topel and Ward (1992) and Bowlus (1995).

In order to estimate β, we use Cox's (1972) semiparametric estimation technique. The likelihood function is the sum of the probabilities that an employment spell terminates at time t_i, given that one employment spell terminates at time t_i. This is given by

$$(2) \quad \frac{\exp(\beta' X_i(t_i))}{\sum_{j \varepsilon R_i} \exp(\beta' X_j(t_i))}$$

where R_i is the index set of employment spells at risk just prior to time t_i: $R_i = \{j{:}t_j \geq t_i\}$.

The conditioning eliminates the baseline hazard rate and the censored observations enter only to the sum in the denominator, that is, they enter the risk set at each observation but do not contribute to the numerator of the partial likelihood. Several estimation issues arise in this context. The availability of panel data in which we have more than one observation for the same individual implies the possibility that the unobserved components may be correlated across observations of the same individual. This unobserved term is usually included in the model in the following way:

$$(3) \quad \lambda_i(t \mid X_i(t_i), \theta_i) = \lambda_0(t)\exp(\beta X_i(t_i) + \theta_i)$$

This means that the unobserved heterogeneity does not cancel out in Equation 2. If we have the same number of completed spells per individual, these spells are the ones that contribute to Cox's partial likelihood and the individual specific component drops out along with λ_0. But Chamberlain (1985) pointed out that this is not suitable when we have a different number of spells for different individuals. In this case the number of spells is not independent of their duration. Chamberlain proposed to use only the first two spells for each individual. This can be done as long as the rejected part of the

9. Equation 1 indicates the presence of time-varying independent variables in $X(t)$. In the empirical implementation the current unemployment rate was included but it never proved to be statistically significant at conventional levels. In this section we keep the time indexation in order to make the presentation easier.

Table 2

Estimates for the Proportional Hazard Model – NLSY79 Sample.
Risk Variable: Tenure on the Job.
Measure of UI System: Log Simulated Weekly State Unemployment Benefit

Equation	Log Unemployment Rate at Start of Job	Log Simulated UIB	Log Unemployment Rate * Log Simulated UIB	Log Starting Wage	Wald X^2(df) Statistic	Starting Month Dummy	Region of Residence Dummy	Starting Year Dummy	N
1	0.3846				135 (25)	No	No	No	2,688
	(0.0709)								
2	0.2719	–0.7352			179 (26)	No	No	No	2,688
	(0.0722)	(0.0962)							
3	0.2926	–0.7225	–0.4223		202 (27)	No	No	No	2,688
	(0.0793)	(0.0990)	(0.2336)						
4	0.2658			–0.3490	123 (26)	No	No	No	2,367
	(0.0752)			(0.0562)					
5	0.1648	–0.6366		–0.3411	154 (27)	No	No	No	2,367
	(0.0763)	(0.1044)		(0.0552)					
6	0.1846	–0.6269	–0.3956	–0.3730	185 (28)	No	No	No	2,367
	(0.0847)	(0.1068)	(0.2535)	(0.0561)					
7	0.1870			–0.3182	188 (37)	Yes	No	No	2,367
	(0.0775)			(0.0550)					
8	0.1122	–0.5608		–0.3152	210 (38)	Yes	No	No	2,367
	(0.0781)	(0.1077)		(0.0542)					
9	0.1301	–0.5514	–0.3785	–0.3153	235 (39)	Yes	No	No	2,367
	(0.0858)	(0.1101)	(0.2578)	(0.0542)					

10	0.2914			−0.3520	130 (29)	No	Yes	No	2,367
	(0.0770)			(0.0562)					
11	0.1840	−0.6431		−0.3439	161 (30)	No	Yes	No	2,367
	(0.0781)	(0.1103)		(0.0549)					
12	0.1963	−0.6365	−0.2249	−0.3440	178 (31)	No	Yes	No	2,367
	(0.0859)	(0.1127)	(0.2513)	(0.0549)					
13	0.0655			−0.3441	146 (48)	No	Yes	Yes	2,367
	(0.0983)			(0.0548)					
14	0.0714	−0.3414		−0.3435	234 (49)	No	Yes	Yes	2,367
	(0.1009)	(0.1131)		(0.0548)					
15	0.1215	−0.3351	−0.4568	−0.3439	251 (50)	No	Yes	Yes	2,367
	(0.1069)	(0.1169)	(0.2817)	(0.0548)					
16	0.0319			−0.3175	280 (59)	Yes	Yes	Yes	2,367
	(0.1001)			(0.0549)					
17	0.0448	−0.2919		−0.3163	281 (60)	Yes	Yes	Yes	2,367
	(0.1027)	(0.1131)		(0.0549)					
18	0.0903	−0.2588	−0.4206	−0.3164	305 (61)	Yes	Yes	Yes	2,367
	(0.1085)	(0.1160)	(0.2802)	(0.0549)					

Data are composed of a panel of workers from the NLSY79 over the period 1979–98. The generosity of the unemployment insurance system is measured by the simulated weekly state unemployment benefit. Variables are defined in Section III. Covariates included in each regression are 11 industry dummies and 10 occupation dummies. Additional covariates included in each regression are high school graduate, college graduate, marital status, white, and union status dummies. Standard errors are in parentheses. They are corrected to allow for group effects within state-year cells. N is the sample size. The sample size is smaller when the wage variable is included due to missing data for the starting wage in some observations.

sample is random and negligible. Here this is not feasible because of the criteria used to select the spells.

Note that when we are estimating a model in which the explanatory variables and the unobserved components are independent, we can still make inferences about the model parameters even if the proportional hazard model is misspecified. In this case the coefficients will be biased toward zero but the standard errors are approximately correct (see Lancaster 1990).

We work with a data set that has only one spell per individual in order to avoid problems of correlation between observations from the same individual that result from unobserved heterogeneity. Under the assumption that the unobserved components are random and uncorrelated across individuals and that the observed and unobserved components are independent, we can satisfy the distribution restriction placed on the error term by the proportional hazard model.

This spell is randomly selected from the set of spells for each individual that "survived" the selection criteria mentioned in the data section. This results in a sample of 2,688 observations, 13 percent of which are censored. We have also estimated the same model using several observations per worker. The results obtained using all the observations were qualitatively similar to those reported in the next section, as were those obtained by applying Chamberlain's criteria to the selection of the observations.

The problems associated with the process of sample construction in studies of the impact of UI, namely on UI duration, were addressed in several papers, an example being Portugal and Addison (1990). One of the most important issues of sample selection relates to the impact of UI on the search behavior on the part of the unemployed. The procedures described above try to cope with these problems of sample selection biases. For example, the use of an imputed UI benefit for the nonrecipients follows a suggestion of Portugal and Addison (1990), and makes the UI benefit variable exogenous to UI receipt, thus avoiding simultaneity problems. However, other issues related with sample selection biases might still be present. In an attempt to check our basic findings' robustness, we report results from estimating the main model for a sample on noneligible workers. As these workers might have different expected search benefits under the UI program, the impact of UI generosity must have a smaller impact on post-unemployment outcomes for this group of workers.

V. Estimation Results

Tables 2 and 3 present the results of the parameter estimation from different proportional hazard model specifications using the NLSY79 panel and the criteria described in Sections III and IV to define the sample composition.

The coefficients of interest are those on the state monthly unemployment rate at the beginning of the spell, the measure of the UI benefit (either the simulated or the maximum benefit), and the interaction between these two terms.[10] Other covariates were also included in all specifications of the model. Their coefficients are not presented in

10. The unemployment rate and the UI benefit measure are included as deviations from the sample means. This implies that the impact of each variable is being evaluated at the sample mean since the interaction would then be zero.

Table 3
Estimates for the Proportional Hazard Model—NLSY79 Eligible Workers Sample.
Risk Variable: Tenure on the Job
Measure of UI System: Log Maximum Weekly State Unemployment Benefit

Equation	Log Unemployment Rate at Start of Job	Log Simulated UIB	Log Unemployment Rate ∗ Log Simulated UIB	Log Starting Wage	Wald X^2(df) Statistic	Starting Month Dummy	Region of Residence Dummy	Starting Year Dummy	N
1	0.2658			–0.3490	123 (26)	24No	No	No	2,367
	(0.0752)			(0.0562)					
2	0.3135	–0.5153		–0.3367	141 (27)	No	No	No	2,367
	(0.0753)	(0.1145)		(0.0561)					
3	0.4637	–0.4533	–1.6217	–0.3322	181 (28)	No	No	No	2,367
	(0.0790)	(0.1146)	(0.3309)	(0.0548)					
4	0.0319			–0.3175	280 (59)	Yes	Yes	Yes	2,367
	(0.1001)			(0.0549)					
5	0.1063	–0.4142		–0.3093	292 (60)	Yes	Yes	Yes	2,367
	(0.1014)	(0.1248)		(0.0547)					
6	0.2651	–0.3468	–1.4628	–0.3024	321 (61)	Yes	Yes	Yes	2,367
	(0.1044)	(0.1262)	(0.3431)	(0.0542)					

Data are composed of a panel of workers from the NLSY79 over the period 1979–98. The generosity of the unemployment insurance system is measured by the maximum state unemployment benefit. Variables are defined in Section III. Covariates included in each regression are 11 industry dummies and 10 occupation dummies. Additional covariates included in each regression are high school graduate, college graduate, marital status, white and union status dummies. Standard errors are in parentheses. They are corrected to allow for group effects within state-year cells. N is the sample size.

Tables 2 and 3 because they do not vary significantly across specifications. In Table A2 in the Appendix we present the full set of results for two of the model specifications of Table 2. These estimates are in line with what is found in the literature on job tenure (see, for example, Farber 1999): white, married and more educated workers experience longer employment spells, and workers with union covered contracts have longer expected tenure.

In Table 2 the first specification for each model tests the procyclicality of match duration. As previously found, there is a negative effect of the cycle on the quality of matches—jobs initiated during booms last longer than those initiated during recessions.

In Table 2 the simulated UI benefit is used to measure the generosity of the UI system. In Line 2 we add the log of the maximum state UI benefit. The negative coefficient implies that higher UI benefits reduce the employment hazard, leading to longer expected match tenure. However, the main test of our hypotheses is carried out in Line 3, adding the interaction term between the unemployment rate and the UI benefit measure. The impact of the UI benefit evaluated at the mean log unemployment rate is –0.723 (t-statistic equal to –7.298), indicating that higher benefits decrease the probability of a job being terminated. The estimated effect of the log unemployment rate, evaluated at the mean log simulated UI benefit, is 0.293 (with a t-ratio equal to 3.690), meaning that higher state unemployment rates at the start of the match lead to shorter job tenures and match quality has a procyclical behavior. These results seem to support one of the hypotheses being tested, namely that higher UI benefits tend to reduce the employment hazard rate over the business cycle.

The second hypothesis is related to the effect that higher UI benefits have in the cyclical behavior of match quality. If the cyclicality of match quality is related to the congestion pressure, one can investigate whether the UI system is more effective in attenuating this pressure during recessions or during booms. This hypothesis is related to the sign of the interaction coefficient. The estimates in Table 2 imply that the UI benefit system has a greater impact on the employment hazard at higher levels of the unemployment rate (the interaction term is negative). This result can also be interpreted as the UI system having the effect of knocking the cyclical behavior of match quality out. It is an indication that the reduction on the congestion pressure operated through the UI system is more effective in recessions than in booms, and implies that UI benefits smooth out the cyclical pattern of match quality.

In order to test the possibility that these results are driven by factors other than the effect of the UI benefit, we estimated alternative specifications of the model. One possible effect is through initial wages. It might be that the labor market internalizes most of the cycle and reservation wage effects through initial wages, such that after controlling for the initial wage the effect of the cycle and of UI benefits is knocked out. In Lines 4–6 we include the starting wage. The results show that the impact of the UI benefit is only slightly reduced to –0.627 and is still significantly different from zero. The unemployment rate coefficient is also smaller, equal to 0.185, but remains statistically significant. The effect of the starting wages in the probability of a spell being terminated points to the importance of including more controls in the model specification in order to eliminate, or at least reduce, further omitted variable bias.[11]

11. In Bowlus (1995) the starting wage has a much greater impact on the coefficient for the unemployment rate at the start of the job. The difference between the results can be twofold. First, the results in Table 2

Next, we control for seasonal as opposed to cyclical patterns by including dummy variables indicating the starting month of the employment spell (Lines 7–9). The statistical significance of the simulated UI benefit effect on the hazard rate does not change much. In fact, the marginal effect remains quite stable for the UI measure (it changes from –0.627 to –0.551). On the contrary, the coefficient of the unemployment rate is further reduced to 0.130, and is now not statistically different from zero. The coefficient on the starting wage decreases slightly.

An analysis of the distribution of high UI benefit states shows that they are disproportionately concentrated in the Midwest and Northeast while low UI benefits states are located mainly in the South. We include region dummies to control for regional effects (Lines 10–12) trying to identify time invariant regional differences (for example, differences in productivity that affect both starting wages and spell durations). Again, the results did change significantly, neither for the unemployment rate nor for the UI benefit.[12]

Of more consequential impact on the results was the inclusion of year dummies (Lines 13–15 of Table 2). These are meant to capture time effects due to the evolution of job tenure and UI generosity. The estimated impact of the UI benefit is much less, estimated at –0.335 and statistically significant (a *t*-ratio of –2.867). The year dummies might be capturing the opposite trends of job tenure and UI generosity observed in the sample.[13] Note also that the coefficient on the starting unemployment rate is no longer statistically significant in Specification 13. This result might be an indication of differences in match cyclical behavior between the 1980s and '90s and that are captured through the year dummies. Taken together, these results might imply a policy interpretation that state governments use the UI benefit in recession years as a social subsidy. This effect will be reflected in the year dummies, and the UI benefit again has the reservation wage interpretation.

Finally, we present a model with month, region, and year controls. The results for this model are shown in Lines 16–18. In Model 18, the coefficient estimate for the UI measure is smaller, but the *t*-statistic (–2.23) still provides support for the hypothesis that UI generosity increases the duration of job tenure upon unemployment. However, in this model, there is only weak evidence in favor of the dampening effect of UI on the cyclical variation in match quality.

In Table 3 we present the results obtained replacing the simulated UI benefit with the maximum state UI benefit for Specifications 6 and 18 in Table 2. The results

cover a much larger period (twice as many years and two complete business cycles). Secondly, and more important, the sample selection criteria are quite different in the two papers. While Bowlus used all employment spells in the NLSY79, we are only considering post-unemployment spells, further restricting them to jobs following unemployment experiences in which the worker was eligible to receive UI benefits. We can argue that the overall effect of the cycle through wages that operates at a general level might not be present in a sample of displaced workers, whose wages tend to be lower and more affected by the reservation wage effect of the UI benefit. In fact, from recent work by Barlevy (2001) we have learned that wage cyclicality is lower in high UI states, a fact consistent with the above results. Furthermore, using Bowlus' sample selection criteria, we find the starting wages to have a much greater impact on the estimated coefficient for unemployment. Thus, the difference in the results could be based on a different cyclical behavior of wages and match tenure for laid-off workers experiencing unemployment.

12. Results with state fixed effects did not change the basic results obtained with region fixed effects.

13. In our sample, average tenure falls over the sample period from 97 weeks to 76 weeks, while the real simulated UI benefit increases from 63 USD to around 90 USD by the end of the period.

obtained with the maximum benefit are supportive of the two hypotheses being tested. However, it is worth noting that in Table 3 the statistical significance of the coefficients is much higher than the one obtained using the simulated UI, in particular for the interaction coefficient. A possible interpretation for these differences is that the maximum UI benefit is capturing something about the state economy or the state's UI policy that it should not, as mentioned in the Data section, and that motivated the use of the simulated UI benefit.[14]

As a robustness check on these results, we estimated the main model for a sample of noneligible workers. These workers might have lower expected search benefits under the UI program and, as a consequence, the impact of UI generosity on post-unemployment outcomes should be smaller (or even negligible). The employment spells included in the noneligibles sample were those following an unemployment spell in which the individual was not eligible to receive UI, either because the worker quit the previous job or because the number of weeks of work during the previous year did not reach the minimum required for the worker to be eligible for UI. The results from the estimation of these models are reported in Table 4. For the sample of workers noneligible to receive UI, after the inclusion of the month, region and starting year dummies, the impact of UI on job tenure is not statistically different from zero and the UI does not have any sizable impact on the cyclicality of match quality. Despite the fact that the magnitude of the estimated coefficients in Tables 2 and 4 is not statistically different, match tenure for noneligible workers seems to be unaffected by UI.

From the proportional hazard model coefficients one cannot tell the magnitude of the impact of the UI system generosity on job tenure. In order to be able to evaluate the magnitude of the estimated effects in Table 2 we compute two alternative indicators. First, we estimate the shift in the baseline survivor function in response to an increase in UI generosity. Next, we use the conditional cumulative density function of the proportional hazard model and estimate median tenures for different values of the UI system measure.

The baseline survivor function $S_0(t) = \exp(-\int_0^t \lambda_0(u)\,du)$ gives us the probability of a match surviving just beyond time t, when the covariates in X are all set at zero. We start by evaluating the baseline survivor function at the mean values of the simulated UI, the unemployment rate and the starting wage, for a single, black, high-school graduate worker with a nonunion contract.[15] Having estimated the baseline survivor function, we can use it to obtain estimates of the survivor function for any covariate vector X using

$$S(t \mid X) = S_0(t)^{\exp(\hat{\beta}'X)}.$$

In Table 5, we evaluate the survivor function shift in response to an increase in UI generosity, at different values of tenure. We study the impact on the survival probability of increasing the simulated UI benefit: in the second column we report the impact of a 10 percent increase (0.1 change in log UIB) and in the third column, a standard deviation increase (a 0.316 change in log UIB).

14. We performed several tests of the proportional hazard specification. In all these tests the results confirmed the correctness of the proportional hazard model assumption.

15. Table A3 in the Appendix reports the estimates of the baseline survivor function for different values of tenure, using the results from Model 18 in Table 2.

Table 4
Estimates for the Proportional Hazard Model—NLSY79 Sample.
Robustness Check: Noneligible Sample. Risk Variable: Tenure on the Job.
Measure of UI System: Log Simulated Weekly State Unemployment Benefit

Log Unemployment Rate at Start of Job	Log Simulated UIB	Log Unemployment Rate ∗ Log Simulated UIB	Log Starting Wage	Wald $X^2(df)$ Statistic	Starting Month Dummy	Region of Residence Dummy	Starting Year Dummy	N
0.2259 (0.0480)	–0.3466 (0.0563)	–0.1484 (0.2117)	–0.3110 (0.0237)	554 (25)	No	No	No	3,747
0.1243 (0.0569)	–0.0808 (0.0929)	–0.3236 (0.2294)	–0.2997 (0.0242)	693 (61)	Yes	Yes	Yes	3,747

Data are composed of a panel of workers from the NLSY79 over the period 1979–98. The generosity of the unemployment insurance system is measured by the simulated weekly state unemployment benefit. Variables are defined in Section III. Covariates included in each regression are 11 industry dummies and 10 occupation dummies. Additional covariates included in each regression are high school graduate, college graduate, marital status, white, and union status dummies. Standard errors are in parentheses. They are corrected to allow for group effects within state-year cells. N is the sample size.

Table 5
Impact on Survival Probability of an Increase in Unemployment Insurance Benefit Generosity

	Estimates based on Model 18 from Table 2		
		Simulated UI Increase Equal to	
At Tenure Equal to	Baseline Survival Function (1)	10 percent (2)	One Standard Deviation (3)
26 weeks	0.7129	0.0062	0.0192
52 weeks	0.5347	0.0086	0.0269
78 weeks	0.4019	0.0095	0.0298
95 weeks (mean tenure)	0.3537	0.0095	0.0301
104 weeks	0.3348	0.0095	0.0300
156 weeks	0.2305	0.0088	0.0282
260 weeks	0.1319	0.0070	0.0227

The baseline survivor function for a given time t gives us the probability of a match surviving just beyond that time t and is reported in Column 1. The baseline survivor function is evaluated at the mean values of the unemployment rate, the UI measure and the starting wage. Other relevant characteristics are: nonunion, black, high school graduate and single worker. The table presents the shift in survival probabilities at different levels of tenure. The figures in Columns 2 and 3 correspond to the increase in estimated survival probability when we increase UI generosity from its baseline value by 0.1 and 0.316, respectively.

The results show that at the mean value of tenure (95 weeks) the survival probability increases by three percentage points in response to a standard deviation increase in the simulated UI benefit from its mean value (all other variables remaining at their original values). The shift in survival probability is of a reasonable magnitude given that most new jobs in the U.S. labor market end early in the relationship, especially during their first year (see Farber 1999).

An alternative measure to evaluate the UI impact on match tenure consists in estimating median tenure for different values of UI generosity. To do that we use the baseline survivor function estimated above and set the conditional cumulative density function equal to 0.5 as follows:

$$F(t \mid X) = 1 - \exp\left(-e^{\hat{\beta}'X} S_0(t)\right) = 0.5$$

Using the estimated βs and setting the explanatory variables at the desired values we can back out a level for the integrated hazard function that is associated with the median tenure for that model specification. Using the results from Model 18 in Table 2 and considering the same base values for the covariates as above, median tenure is 57 weeks for an unemployment rate equal to 6.6 percent and a simulated UI equal to 75 USD (their mean values). If we increase the simulated UI by 10 percent, median tenure increases to 59 weeks, while with a standard deviation increase of the UI measure, median tenure is equal to 61 weeks. The impact is an increase in duration equal to four weeks for each standard deviation increase of the UI benefit measure.

Overall, the evidence presented lends some support to the hypotheses being tested. On the one hand, there is evidence that higher UI benefits tend to increase the duration of subsequent employment spells, and on the other hand, we find some limited evidence that more generous benefits reduce the cyclicality of job match quality, the impact on employment duration is greater, the higher the unemployment rate.

VI. Conclusions

The impact of social insurance programs, particularly of unemployment insurance programs, has increasingly attracted the attention of empirical economists. However, this attention has primarily been directed toward estimating the disincentive effects of these programs. The purpose of this paper was to analyze the relationship between the quality of job matches (measured by job tenure), the business cycle, and the UI system generosity. We found some evidence that UI generosity increases the duration of subsequent job tenure upon unemployment and more limited support in favor of its dampening effect on the cyclical variation in match quality.

We relate these findings to the job search behavior of the unemployed. We note that the generosity of UI benefits should affect the decision to match or not and should have some effect on the cyclical behavior of match quality. The unemployed are given time to find not just a job, but also the right job. As a result, better matches are expected to come out of the search process since people can afford to wait longer for a better match.

We show that job tenure is positively related to the generosity of the UI system. The total effect of the UI benefit level is a reduction of the employment hazard: Larger UI benefits lead to longer subsequent employment spells after controlling for region of residence and year fixed effects. An increase in UI generosity equal to one standard deviation of our simulated UI benefit measure increases the match survival probability at one year of tenure by almost three percentage points and median tenure by four weeks. Furthermore, we present some evidence that this impact is greater during busts than during booms. Thus UI smoothes out the cyclical variation in match quality.

These results have two possible interpretations that open up new directions for research. The first one is that more generous UI increases the expected match quality for a given worker, and the second is that workers face a given match quality distribution and the UI simply leads some to stay unemployed and cuts off the lower tail (short duration matches). In this sense analyzing the long-run productivity effect of longer matches would be of key importance. Along the lines of the work by Acemoglu and Shimer (2000), as a result of more generous UI benefits there might be efficiency gains from the truncation of the lower tail of match quality distribution.

These results are also important from a policy perspective. In fact, to the potential efficiency gains identified in Acemoglu and Shimer, we must add the benefits from the dampening effect on the cyclical variation in match quality of more generous UI.

In particular, the ability to limit the negative effect of recessions with exogenous variables, such as the UI benefit or other welfare policy instruments, opens up new possibilities for policy intervention. It will also be interesting to attempt an analysis of the trade-off in terms of social welfare between the costs of having shorter employment spells or a more generous UI benefit system, a discussion that began in the early 1970s, and recently has been partially re-examined in Marimon and Zilibotti (1999).

Table A1

Variables used, Description, Mean and Standard Deviation (Computed for the Starting Sample—all Employment Spells)

Variable	Description	Mean	Standard Deviation
Tenure	Tenure in weeks	102.26	136.92
Censor	0 = censored 1 = not censored	0.85	0.36
Age	Age in years at the beginning of the spell	25.10	5.09
Union	0 = not union sponsored job 1 = union sponsored job	0.13	0.33
HS	0 = did not obtain high school diploma 1 = obtained high school diploma	0.61	0.49
CLG	0 = did not complete 16 years of school 1 = completed 16 years of school	0.19	0.39
White	0 = Black or Hispanic 1 = not Black or Hispanic	0.68	0.47
NU0	National monthly civilian unemployment rate	7.11	2.22
STU0	State monthly civilian unemployment rate	7.21	2.34
MS	0 = not married 1 = married	0.25	0.43
StW	Hourly real starting wage (at 1979 USD)	4.77	18.21
Max UIB	Maximum weekly state unemployment insurance (at 79 USD)	113.09	24.17
Sim UIB	Simulated weekly state unemployment insurance (at 79 USD)	69.63	22.94

See notes to Table 1.

Table A2

Estimates for the Proportional Hazard Model—NLSY79 Eligible Workers Sample
Risk Variable: Tenure on the Job.
Measure of UI System: Log Simulated Weekly State Unemployment Benefit.
Complete Results from Models 6 and 18 in Table 2

	Equation 6		Equation 18	
Variable	Coefficient	Standard Error	Coefficient	Standard Error
Log unemployment rate	0.1846	0.0847	0.0903	0.1085
Log simulated UI	–0.6269	0.1068	–0.2588	0.1160
Log unemployment rate				
Log simulated UI	–0.3956	0.2535	–0.4206	0.2802
Union dummy	–0.1174	0.0561	–0.1320	0.0565
White dummy	–0.0658	0.0487	–0.0773	0.0513
High-school graduate	–0.1127	0.0503	–0.1307	0.0501
College graduate	–0.0452	0.0672	–0.0224	0.0687
Married dummy	–0.2004	0.0480	–0.1454	0.0499
Log starting wage	–0.3413	0.0552	–0.3164	0.0549
Wald $X^2(df)$ statistics		185 (28)		305 (61)
Number of observations		2,367		2,367

Data are composed of a panel of workers from the NLSY79 over the period 1979–98. The generosity of the unemployment insurance system is measured by the maximum state unemployment benefit. Variables are defined in Section III. Additional covariates included in each regression are 11 industry dummies and 10 occupation dummies. Standard errors are corrected to allow for group effects within state-year cells. The sample size is smaller when the wage variable is included due to missing data for the starting wage in some observations

Table A3
Baseline Survival Function Estimates

Tenure (in weeks)	Baseline Survival Function
12	0.8831
24	0.7367
36	0.6360
48	0.5548
60	0.4898
72	0.4228
84	0.3790
96	0.3517
108	0.3225
120	0.2930
132	0.2671
144	0.2463
156	0.2305
168	0.2145
180	0.2016
192	0.1889
204	0.1754
216	0.1639
228	0.1528
240	0.1437
252	0.1358

The baseline survivor function is evaluated at the mean values of the unemployment rate, the UI measure and the starting wage using results from Model 18 in Table 2. Other relevant characteristics are: nonunion, black, high school graduate, and single worker.

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