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Abstract
This paper charts the effects of unemployment insurance (UI) on escape rates from unemployment using data from the Displaced Worker Survey. Transition rates from unemployment to employment are estimated via a flexible semiparametric specification that allows the effects of UI recipiency (and indeed other covariates) to vary through time. In addition, potential biases stemming from sample construction and unobserved individual heterogeneity are accommodated. Contrary to Fallick (this *Review*, 1991), it is found that the spike in the sample hazard rates of UI recipients at benefit exhaustion is not an artifact of the data produced by rounding. Time-varying covariate effects are also detected for several non-UI variables.

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I. Introduction

In the conventional search model, unemployment insurance (UI) raises the reservation wage and lengthens the duration of the unemployment spell. Incorporating finite UI benefits into the model suggests that reservation wages should fall with the approach of benefit exhaustion (Mortensen, 1977). As a result of the decline in the value of remaining unemployed, and increased search intensity, escape rates should rise monotonically.[1] After benefit exhaustion, it is predicted that escape rates should be constant.[2]

Although generally confirming that is inappropriate to hold fixed the effect of UI on hazard rates, the modern empirical literature is not in agreement on the issue of whether UI can account for the observed spike(s) in the empirical hazard at benefit exhaustion. In particular, Fallick (this *Review*, 1991) has noted that the spike in the empirical hazards at 26 weeks (and also at 39 weeks, the point of extended benefit exhaustion) is observed for both UI recipients and nonrecipients alike. He reports that shortly before and shortly after benefits commonly expire the cet. par. effect of UI is statistically insignificant. He speculates that the familiar jumps in the life-table estimates of hazard rates are instead a function of rounding on the part of both uninsured and insured survey respondents (see Poterba and Summers, 1984; Sider, 1984).

Fallick also raises the possibility that the growing convergence in the behavior of recipients and nonrecipients might reflect unobserved individual heterogeneity (UIH). That is, those with higher hazard rates might have unobserved characteristics that make them less likely to apply for benefits, so that with the passage of time the two UI recipient categories become more similar in unobserved characteristics. Although he claims to find little (indirect) support for this possibility, his estimates do not control for UIH. Furthermore, they are subject to endogenous stratification bias if, as is indeed likely, UI benefit status depends on duration (see section III).

As we have intimated, other U.S. studies of the time-varying effects of UI, using different data sets, reach rather different conclusions. Thus, for example, Meyer (1990) reports that the probability of escaping from unemployment rises dramatically just before benefits lapse; and, interestingly enough, when the length of benefits is extended the probability of a spell ending is high in the week benefits were previously expected to lapse. The innovation in his study is the use of a spline function in time to benefit exhaustion.[3] That being said, results more in line with Fallick are reported in a British study by Narendranathan and Stewart (1993) with no UI effects persisting beyond week 12, well below the 12-month National Insurance benefit then payable or the 6-month interval of earnings-related supplementation.

In the present treatment, we examine the effects of UI recipiency, using displaced worker data after Fallick - albeit more recent data no longer contaminated by multiple spells of joblessness - and again using semiparametric models for the baseline hazard function. Quite apart from allowing the effect of UI to vary through time using (three) linear segments, the other covariates will also be allowed to vary (either continuously or discretely) through time. Throughout, care is also taken to avoid the problem of endogenous stratification while accounting for unobserved individual hetereogeneity.

The plan of the paper is as follows. Section II introduces the flexible representations of the baseline hazard and the manner of incorporation of time-varying effects and UIH. Section III briefly reviews the data and the way we dealt with endogenous stratification. Section IV provides the empirical results. A brief summary concludes.

II. Model Specification

Consider a time axis that is divided into M intervals by points $k_1, k_2, \ldots, k_{m-1}$. Assuming a piecewise-constant specification for the baseline hazard function, we can write the hazard function as

$$h(t) = \phi[X_i,\beta(t)]e^{\lambda_m}, \quad k_{m-1} \leq t < k_m \qquad m = 1,\ldots, \quad M \tag{1}$$

where X is a vector of explanatory variables for worker i, β identifies the regression coefficients, and λ_m is the exponential parameter for the m interval (Prentice and Gloeckler, 1978; Fahrmeir and Tutz, 1994, pp. 314-325)

For a constant effects (that is, a proportional hazards) specification, we have simply

$$\phi[X_i,\beta(t)] = \exp(\beta X_i). \tag{2}$$

For a specification that allows for discrete change in the regression coefficients in each interval, we have

$$\phi[X_i,\beta(t)] = \exp(\beta_m X_i). \tag{3}$$

In practice, a more parsimonious model was used, absent which (M-1) regression coefficients would have had to be estimated for each regressor. Assuming just one discrete change at an arbitrary time, t^*, we can write

$$\phi[X_i,\beta(t)] = \exp[\beta_1 \ X_i \ I(t \leq t^*) + \beta_2 \ X_i \ I \ (t > t^*)], \tag{4}$$

where I denotes the indicator function.

Because we are mostly concerned with the time-varying effects of unemployment insurance recipiency, we will consider three different duration periods. The first characterizes the time before exhaustion of benefits; it comprises the intervals 5 to 23 weeks and 29 to 36 weeks.[4] The second period identifies the neighborhood of exhaustion; namely, 24 to 28 weeks and 37 to 42 weeks.[5] Finally, the third period accounts for the time after exhaustion, defined as more than 43 weeks of unemployment. We also wish to allow for time-varying coefficients in respect of the other regressors. To this end, we set arbitrary breakpoints at 4, 12, 26, and 52 weeks in four separate regressions. We note parenthetically that the selection of alternative breakpoints yielded coefficient estimates that did not differ materially from those reported below.

For a specification that allows for continuous changes in covariate effects over the spell of joblessness we have, assuming a logarithmic functional form for t,

$$\phi[X_i, \beta(t)] = \exp[\beta_1 X_i + \beta_2 X_i \log(t)]. \qquad \textbf{(5)}$$

Also, we can incorporate unobserved individual heterogeneity through a multiplicative error term, v, as follows

$$h(t) = \phi[X_i, \beta(t)] e^{\lambda_m} v. \qquad \textbf{(6)}$$

For this purpose we will consider a conventional parametric form for v, namely, the standard gamma distribution. The Monte Carlo simulations provided by Ridder (1987) show that as long as a flexible baseline hazard specification is employed the choice of a mixture distribution is not critical and that, in particular, the standard gamma distribution appears to perform well.[6]

Turning to the issue of implementation, we employed a piecewise-constant specification with jobless durations grouped into 33 intervals. As noted below, our unemployment data is weekly spell length and is top coded at 99 weeks. The choice of 33 intervals rather than 99 is dictated by the relative frequency of the observations within each (weekly) cell. The intervals are weekly observations up to and including week 22, then 7 intervals of 4 weeks up to and including week 50, followed by 3 intervals of 12 weeks' duration, and a final interval encompassing the balance of the observations.

In order to estimate the model, the survivor function has first to be defined. For the m interval this can be written as

$$S(k_m) = \exp\left[-\sum_{j=1}^{m} h_j\right]. \tag{7}$$

The likelihood contribution for an individual who exits at interval *m* is given by S (k_m) - S (k_{m-1}) and for an individual whose duration is censored at *m* by S (k_{m-1}).

In general, the likelihood function can be expressed

$$L_i = \prod_{m=1}^{M}\left[(S_i(k_{m-1}) - S_i(k_m))^{\delta_i}(S_i(k_{m-1})^{1-\delta_i}\right]^{\delta_{m_i}}, \tag{8}$$

where δ_i identifies an uncensored duration and δ_{m_i} equals 1 if the individual's duration falls in the *m* interval, 0 otherwise.

Finally, in order to provide a check on our results, we also employed a Cox proportional hazards partial likelihood estimator. The advantage of this approach is of course that it can equally well accommodate time-varying effects of the covariates without the need to specify a parametric form for the baseline hazard function.

III. Data

The dataset used in this inquiry is the nationally representative, five-year retrospective January 1988 Displaced Worker Survey (DWS). The dataset is well-described elsewhere (see, for example, the essays contained in Addison, 1991, in addition to Fallick, 1991) so that only brief introductory remarks are required here. The DWS has been conducted biennially since 1984, and is attached to the January (currently February) Current Population Survey (CPS). It contains information on the nature of the lost job and subsequent joblessness for workers displaced by reason of plant closure, slack work, abolition of shift or position, failure of a self-employment business, termination of a seasonal job, and "other" causes. Such data are supplemented by extensive information on the personal characteristics of the worker contained in the parent CPS.

The DWS has the advantages over administrative data elaborated by Fallick, namely, information on the jobless duration of nonrecipients.[7] The principal problem with use of administrative data is that there is no counterfactual, with the result that it difficult to tell whether rising hazards observed at or around the time of benefit exhaustion truly reflect the impact of impending or actual benefit exhaustion. And, as noted earlier, Fallick argues that the escape rates of nonrecipients are also characterized by humps at 26 and 39 weeks. In short, the value of the DWS is that it allows us to observe what occurs before, at, and, indeed, after benefit exhaustion (unlike administrative data which are automatically censored at expiration).

There are inevitably some shortcomings of the DWS. Two obvious difficulties are recall bias - individuals experiencing displacement in past years may be more likely to understate their jobless duration than are more recent job losers - and the rounding of reported spells of unemployment. As a practical matter, restricting the sample to those workers who last worked at their jobs within two years of the survey (rather than the full five years) did not change the results reported below, while the use of a flexible baseline hazard function circumvents the second, rounding problem.

Rather more problematic, however, is the issue of reverse causation. The data merely identify whether the respondent was a UI recipient or nonrecipient. An unknown number of those classified as nonrecipients may in fact have been eligible for UI benefits but failed to draw them because they expected to find work within an interval corresponding to the waiting times and filing delays associated with drawing benefits. In short, unemployment insurance receipt is not the same as eligibilty for benefits and may depend on the duration of a spell. (Administrative data are not immune from this problem either.) Treating nonrecipients as non-eligibles may upwardly bias the effect of UI on unemployment duration. This reverse causation problem was handled here by not considering the effect of UI during the first three weeks of the unemployment event while continuing to use these duration data. This procedure is somewhat better, albeit similar in spirit, than the alternative of simply truncating the data at four weeks with appropriate modification to the estimation procedure to account for sample truncation.[8] We note in passing that, while recognizing the problem of reverse causation, Fallick in effect argues that it provides a further reason for estimating time-varying effects of UI.

Use of the 1988 DWS has major advantages over the 1984 DWS employed by Fallick. The definition of unemployment in the earlier survey (and indeed the 1986 DWS) gives the number of weeks workers were without employment and "available for work" in the wake of the displacement event. This unemployment measure may include one or more periods of suspended job search and intervals of labor force withdrawal because the definition does not encompass active search, unlike the familiar measure of unemployment duration in the parent CPS. Moreover, the definition of unemployment in the 1984 DWS admits of multiple spells of joblessness; if displaced workers obtained a temporary job followed by another spell of unemployment, it is the combined total that is reported. The innovation of the 1988 DWS (and subsequent surveys) is that the measure of unemployment now refers to the length of the single spell of joblessness that followed the displacement event and resulted in reemployment. In other words, the multiple spells problem is eliminated, even if there is still the possibility that intervals of withdrawal may be included. Not surprisingly, therefore, estimated mean unemployment using the 1984 DWS considerably exceeds that from the 1988 DWS (see Addison and Portugal, 1992).

Observe also that the parent CPS allows us to identify those who failed to find work after displacement but who were nevertheless economically active as of the survey data. We include such unemployed workers and their necessarily incomplete spells of joblessness within our sample. The unemployment measure for the bulk of the sample who found work is also right censored for those whose unemployment exceeds the 99 weeks at which the DWS data proper are top coded. Both forms of right censoring are of course explicitly accommodated in the likelihood function given in equation (8).

In addition to excluding those with zero spells of unemployment (i.e. transitions from employment to employment) and those who were never economically active after being displaced, the following restrictions were placed on the data. Because the nature of displacement is not well defined for certain individuals and sectors, those employed part time and in agriculture at the point of displacement were excluded, as were those aged less than 20 years and above 61 years as of January 1988. Similar reasoning explains the exclusion of self-employed individuals, together with those displaced for seasonal and "other" reasons. It was also decided to exclude females because of well-known gender differences in supply behavior.[9] Altogether, these restrictions yielded an unweighted sample of 2,345 individuals.

[Table 1 and Figure 1 near here]

Table 1 provides a fairly complete illustration of the weekly pattern of failures, censoring, and the corresponding empirical hazard (and survival) rates for the first 52 weeks of jobless duration. Consistent with Fallick, there is a heaping of hazard rates for both UI recipients and nonrecipients at (or around) weeks 26 and 39. (Additionally, the many spikes in the data for both recipients and nonrecipients clearly reflect the rounding phenomenon emphasized by Fallick.) By the same token, Figure 1 also makes it clear that the escape rate from unemployment of recipients is substantially higher - almost double - than that of nonrecipients at week 26, the conventional point of benefit expiration, even if the difference in favor of recipients is altogether less marked at week 39. More broadly, it can be seen that the hazard rates of nonrecipients are sharply higher over the first 9 weeks of jobless duration. Up to week 26, the pattern is more mixed with some obvious reversals. Thereafter, hazard rates are if anything modestly higher among recipients. These results, though suggestive, do not speak for themselves since they reflect a variety of influences other than UI receipt, to which we next turn.

IV. Findings

Results of fitting the piecewise-constant hazards model are given in Table 2. The first column of the table provides results for a parsimonious specification that excludes UI and constrains the others regressors to have effects that do not vary through time. These basic regressors are of course supposed to capture factors influencing the reservation wage and/or the arrival rate of job offers. It appears that age (AGE) and tenure with the firm at the time of displacement (TEN), as well as the then obtaining state unemployment rate (UN), are each associated with significantly lower escape rates from joblessness. The effect of the first variable most probably captures the unrealistically high reservation wages of long-serving workers, while the two latter arguments proxy the reduced arrival rate of job offers with age and increasing labor market slack.

In contrast, race (WHITE), being a family head (HEAD), and job loss by reason of plant closure or relocation (CLOSE) are associated with more rapid job finding after displacement. The result for race is familiar, and captures the poorer job opportunities confronting blacks as a result of both objective and discriminatory factors. The result for household heads is also thoroughly conventional, and presumably reflects a higher opportunity cost of unemployment for such individuals and their greater search intensity. The better reemployment prospects of those displaced by plant closings reflects a number of influences. One of these might be compositional: in the case of plant closings, all workers are "canned" whereas in the case of the omitted category of slack work only a subset of workers, presumably largely chosen by management, lose their jobs. The former group of displaced workers may well be of better quality than the latter and hence locate jobs more rapidly. But there are other factors at work here, because plant closings are usually accompanied by a larger volume of job search assistance, as well as advance notice. Moreover, workers displaced by reason of slack work (the omitted category) might harbor unrealistic expectations of recall and hence search less intensively for work, at least initially. Finally, the insignificant effect of years of education (SCHOOL) is of interest because more educated workers should have higher escape rates because of their greater search efficiency, higher opportunity costs of remaining unemployed, and generally better job prospects. As can be seen, this expectation is not borne out, but we shall have occasion to revisit this finding.

[Table 2 near here]

Column (2) adds UI recipiency to this list of basic regressors, and allows for time-varying effects in this regard. Some slight amplification of the coefficient estimates is required. Segments A, B, and C pick up the effects of UI before, at/around, and after benefit exhaustion, respectively. The coefficient estimate for A in column (2) is straightforwardly to be interpreted as meaning that UI receipt prior to exhaustion (i.e. during weeks 5 through 23 and 29 through 36) is associated with recipient escape rates that are 46.6 percent lower than those of nonrecipients. The effect of benefit exhaustion (i.e. measured across weeks 24 through 28 and 37 though 42), given by the coefficient estimate for B, is strongly positive however. To gauge the precise effect on relative escape rates, it has to be added to the coefficient for segment A. The effect of approaching and actual exhaustion, then, is to elevate escape rates by 24.6 percent vis-à-vis nonrecipients. As for segment C, unequivocally a period after exhaustion (and corresponding to more than 43 weeks of unemployment), the coefficient estimate should be zero were the effect of UI after exhaustion the same as prior to exhaustion. This is manifestly not the case: the coefficient is both positive and strongly significant. Overall, after exhaustion, the escape rates of recipients can be seen to be some 12.4 percent lower than those of nonrecipients but, as a practical matter, this difference is not statistically significant.

On this evidence, the clear inference is that UI lowers escape rates very sharply prior to exhaustion and raises them very strongly at or around exhaustion. Incorporating UI leaves the significance of the other regressors unaffected, while largely preserving their magnitudes. And it will be recalled that the estimates control for unobserved individual heterogeneity.

Column (3) of Table 1 provides results for a specification in which, in addition to discrete time-varying UI effects, all the other regressors are allowed to change continuously through time. The UI results are basically unaffected; that is, vis-à-vis nonrecipients, UI adversely impacts job search activity prior to exhaustion (- 42.6 percent) and elevates job finding very sharply at expiration (+ 36.3 percent). On this occasion, the escape rates of recipients are actually higher than those of nonrecipients after exhaustion but, as before, the difference between the two groups is not statistically significant over this interval. More interesting is the suggestion of time-varying effects of the other regressors. In particular, significant coefficient estimates are found for the interaction terms CLOSE(t), TENURE(t), and SCHOOL(t). These results are not unexpected. In the case of CLOSE, we would argue that the beneficial effect of greater job search assistance and advance notice on unemployment should erode through time, while in the case of TENURE initially ambitious reservation wages should accommodate to labor market realities. And for SCHOOL, the advantages of broadly more favorable employment opportunities are expected eventually to manifest themselves.

There are no signs that the other regressors are of a time-varying nature, although in each case the effect of this specification is to strengthen the absolute magnitude of the coefficient estimate for the first week of unemployment. There is little reason to anticipate that the negative effects of being either black or old should ameliorate through time. And if we have a time-consistent measure of labor market slack, the effect of unemployment should be the same across comparable phases of unemployment.

But it may be that variation in the effect of the non-UI regressors over time is not smooth. For this reason column (4) of Table 2 allows for a discrete change in these covariate effects at 4 weeks. As before, the UI variable is measured across segments A through C. The effects of the UI variables again prove robust to re-specification - relative escape rates fall by 45.4 percent prior to exhaustion and rise by 28 percent at exhaustion. As far as the other covariates are concerned, the only change is a marked strengthening in the coefficients for CLOSE, SCHOOL, and TENURE, even if the latter is less precisely estimated than before. In other words, the insignificant effect of SCHOOL in the fixed coefficient model in columns (1) and (2) is indeed misleading; after an initial period in which more educated workers find jobs at much the same rate as their less-skilled counterparts, their anticipated labor market advantage is observed in the data. For CLOSE, the advantage associated with this form of job loss clearly seems to be confined to the first month of job search. For TENURE, there are again signs of a fairly drastic downward revision of reservation wages with unemployment exposure after 4 weeks unemployment.

The final three columns of Table 2 impose different breakpoints for the non-UI covariate effects at greater than 12 weeks, 26 weeks, and 52 weeks in columns (5), (6), and (7), respectively. In each case, the effect of UI is largely unchanged across segments; particularly noticeable in this regard is the broad stability of the segment B coefficients (even though the net effects range from 16.4 to 27.8 percent). As for the other regressors, time-varying effects are less and less easy to detect as the breakpoint is extended. Thus, significant effects are no longer encountered for CLOSE and TENURE, while SCHOOL remains significant only in column (5). We would surmise in these cases that, with the possible exception of the SCHOOL variable, the relevant distinction is between short- and long-term unemployment.

[Table 3 near here]

As noted earlier, as a check we reestimated the model using a specification that makes no assumption as to the shape of the baseline hazard. The results of using the Cox specification are given in Table 3, where the fitted equations otherwise follow those of Table 2. There is a gratifying similarity of result both with respect to the UI variables and the other covariates. Again, there is palpable evidence of time-varying UI effects. The principal differences are that the depressing effect of UI on escape rates prior to exhaustion is modestly reduced and that, after exhaustion, the escape rates of recipients at all times actually exceed those of nonrecipients (even if the differences between the two groups remain statistically insignificant at conventional levels). Interestingly, the coefficient estimates for segment B very closely correspond to those reported in Table 2. The effects of the non-UI covariates are also close and importantly the pattern of time-varying effects is to all intents and purposes identical to that reported earlier for the piecewise-constant hazards model.

A final observation, therefore, is that it does not make a difference - either in terms of the regression coefficient estimates or the baseline hazard - whether or not one controls for unobserved individual heterogeneity.

V. Conclusions

This analysis has demonstrated that the effects of UI on jobless duration in the wake of job displacement are real and not an artifact of the data, stemming from respondent rounding or unobserved individual heterogeneity. UI receipt is associated with a very marked reduction in escape rates from unemployment prior to exhaustion. Specifically, the hazard rates or recipients are some 40 percent lower than those of nonrecipients over this interval. (Note that this value is unlikely to be contaminated by a reverse line of causation from duration to UI receipt because by construction UI was not allowed to have any effect during the first month of unemployment experience.) At or around benefit exhaustion, on the other hand, the escape rates of recipients were found to increase sharply, exceeding those of nonrecipients by more than 20 percent. Finally, following benefit exhaustion, there are no statistically significant differences in the rates of job finding of the two groups. Overall, these results inescapably point to delaying tactics on the part of the insured unemployed, especially when taken in conjunction with the finding from other research that UI does not lead to material improvement in postdisplacement wages (e.g. Addison and Blackburn, 1997).

These findings differ markedly from Fallick's analysis of the 1984 DWS, but they are consistent with received theory and empirical work using administrative data (e.g. Meyer, 1990). Fallick's unemployment data are frankly inadequate for the task at hand, being contaminated by multiple jobless spells and made opaque by reason of the high degree of right censoring associated with the use of a competing risk model, although he is to be credited with the use of appropriate (i.e. semiparametric) estimation techniques, and above all in emphasizing the central importance of a counterfactual in UI duration analysis.

The implication of our analysis for policy seems clear: the UI system should be modified to stimulate more rapid job finding on the part of the insured unemployed population. Policy reform should seek to secure a general reduction in the disincentive effects of UI insurance, even if the income maintenance function might even have to be strengthened for problem groups. Immediately practical measures include job search assistance, allied with more stringent application of the job search test, because this has been shown to offer gains to the unemployed and the public purse alike (Meyer, 1995). There would also seem to be scope for further experimentation with reemployment bonuses although, as our attempt to deal with sample construction bias implies, we are cognizant of the moral hazard problem in this regard.

Endnotes

1. Even if workers are not engaged in productive search, as in the static labor-leisure model, the more modest prediction of a clustering of observations around the exhaustion point may be inferred (Moffitt and Nicholson, 1982).

2. Although Meyer (1990, 2) notes that the hazard rate may be discontinuous at benefit exhaustion if the marginal utility of leisure is not independent of income.

3. Two disadvantages of the administrative data examined by Meyer are that the information pertains to recipients alone and spells of joblessness are only observed while benefits are paid (at which point they are censored).

4. We do not include in the definition of the UI variables the initial four weeks of unemployment in order to avoid the problem of reverse causation; that is, very short spells of unemployment may determine UI recipiency (see Portugal and Addison, 1990, and the next section).

5. Clearly, other segment definitions are possible, but experimentation with different intervals did not materially affect our results.

6. See Addison and Portugal (1997) on the consequences of neglecting unobserved individual heterogeneity.

7. In providing information on completed spells of unemployment, the DWS is particularly suited to investigation of distributional assumption in duration analysis, time aggregation problems, and competing risks.

8. Use of other knot points did not make much difference to the results.

9. Although it transpired that estimates of the time-varying effects of UI for females were in fact quite consistent with those reported below for males.

Figure 1: Differences in the Empirical Hazard Rates of Nonrecipients and Recipients of Unemployment Insurance

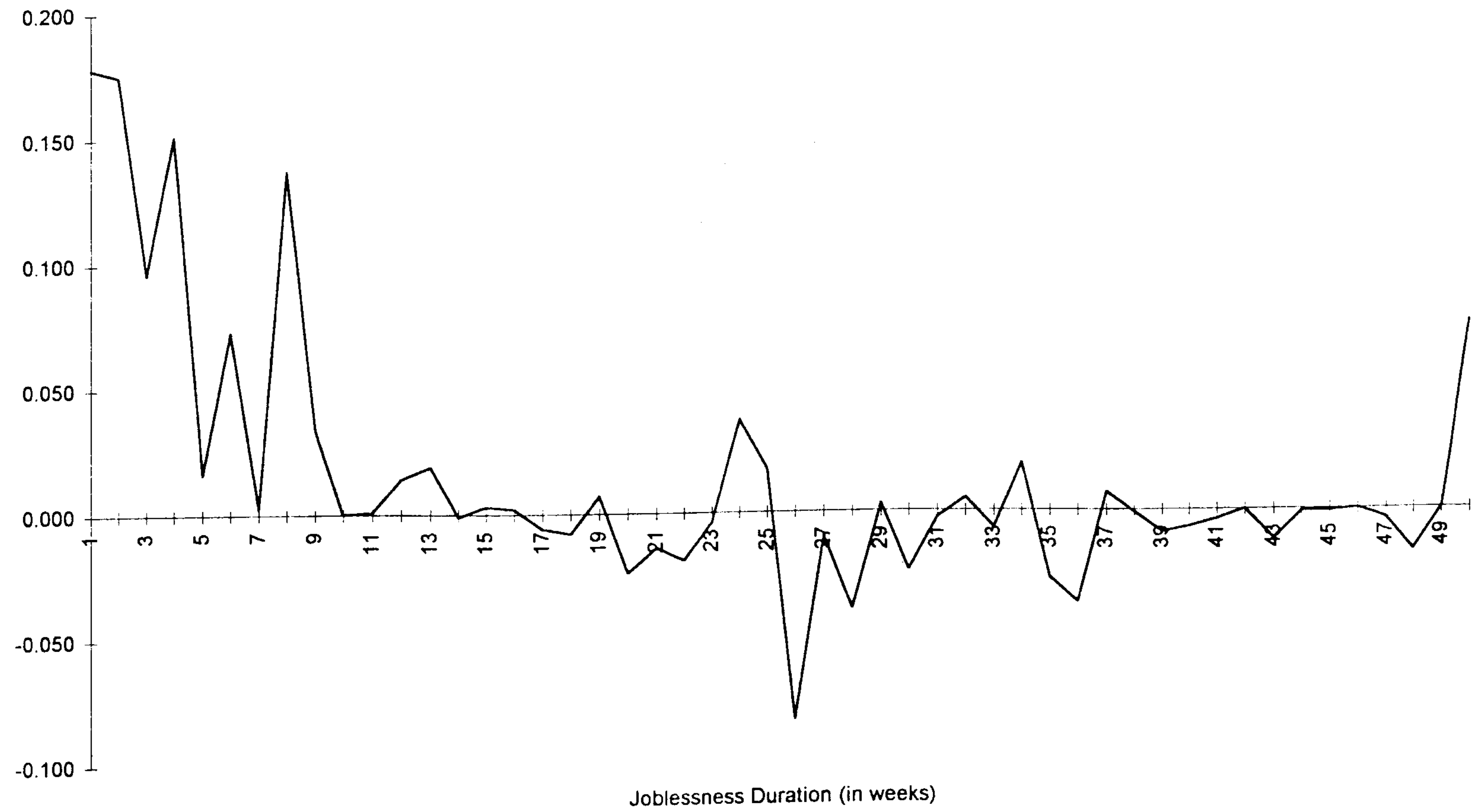

Table 1: Empirical Survival and Hazard Rates

Week	UI Recipients						Nonrecipients					
	Entered	Censored	At Risk	Exited	Survival Rate	Hazard Rate	Entered	Censored	At Risk	Exited	Survival Rate	Hazard Rate
0	1464	0	1464	0	1.000	0.000	881	0	881	0	1.000	0.000
1	1464	2	1463	59	1.000	0.041	881	6	878	173	1.000	0.219
2	1403	6	1400	60	0.960	0.044	702	8	698	138	0.803	0.219
3	1337	8	1333	57	0.919	0.044	556	11	550	72	0.644	0.140
4	1272	11	1266	92	0.879	0.075	473	11	467	95	0.560	0.226
5	1169	7	1165	33	0.815	0.029	367	2	366	16	0.446	0.045
6	1129	12	1123	58	0.792	0.053	349	7	345	41	0.427	0.126
7	1059	4	1057	16	0.751	0.015	301	1	300	5	0.376	0.017
8	1039	19	1029	83	0.740	0.084	295	8	291	58	0.370	0.221
9	937	3	935	23	0.680	0.025	229	1	228	13	0.296	0.059
10	911	8	907	34	0.664	0.038	215	4	213	8	0.279	0.038
11	869	2	868	8	0.639	0.009	203	2	202	2	0.269	0.010
12	859	13	852	80	0.633	0.099	199	5	196	21	0.266	0.113
13	766	3	764	22	0.574	0.029	173	1	172	8	0.238	0.048
14	741	3	739	19	0.557	0.026	164	0	164	4	0.227	0.025
15	719	6	716	7	0.543	0.010	160	1	159	2	0.221	0.013
16	706	7	702	48	0.537	0.071	157	2	156	11	0.218	0.073
17	651	4	649	17	0.501	0.027	144	1	143	3	0.203	0.021
18	630	6	627	9	0.488	0.015	140	2	139	1	0.199	0.007
19	615	2	614	0	0.481	0.000	137	0	137	1	0.197	0.007
20	613	8	609	32	0.481	0.054	136	4	134	4	0.196	0.030
21	573	1	572	8	0.455	0.014	128	2	127	0	0.190	0.000
22	564	3	562	15	0.449	0.027	126	0	126	1	0.190	0.008
23	546	3	544	2	0.437	0.004	125	0	125	0	0.189	0.000
24	541	5	538	38	0.435	0.073	125	1	124	13	0.189	0.110
25	498	2	497	14	0.405	0.029	111	1	110	5	0.169	0.046
26	482	4	480	85	0.393	0.194	105	1	104	11	0.161	0.111
27	393	0	393	4	0.324	0.010	93	0	93	0	0.144	0.000
28	389	4	387	19	0.320	0.050	93	1	92	1	0.144	0.011
29	366	1	365	3	0.305	0.008	91	0	91	1	0.143	0.011
30	362	1	361	32	0.302	0.093	90	1	89	6	0.141	0.069
31	329	0	329	1	0.275	0.003	83	0	83	0	0.132	0.000
32	328	2	327	22	0.275	0.070	83	0	83	6	0.132	0.075

33	304	0	304	2	0.256	0.007	77	0	77	0	0.122	0.000
34	302	0	302	2	0.254	0.007	77	0	77	2	0.122	0.026
35	300	0	300	8	0.253	0.027	75	0	75	0	0.119	0.000
36	292	0	292	18	0.246	0.164	75	0	75	2	0.119	0.027
37	274	0	274	2	0.231	0.007	73	0	73	1	0.116	0.014
38	272	0	272	4	0.229	0.015	72	0	72	1	0.114	0.014
39	268	0	268	6	0.226	0.023	71	1	70	1	0.113	0.014
40	262	1	261	13	0.221	0.051	69	0	69	3	0.111	0.044
41	248	0	248	1	0.210	0.004	66	1	65	0	0.106	0.000
42	247	0	248	0	0.209	0.000	65	0	65	0	0.106	0.000
43	247	0	247	3	0.209	0.012	65	0	65	0	0.106	0.000
44	244	0	244	4	0.206	0.017	65	0	65	1	0.106	0.016
45	240	1	239	4	0.203	0.017	64	0	64	1	0.105	0.016
46	235	0	235	0	0.120	0.000	63	0	63	0	0.103	0.000
47	235	1	234	1	0.200	0.004	63	0	63	0	0.103	0.000
48	233	0	233	4	0.199	0.017	63	0	63	0	0.103	0.000
49	229	0	229	0	0.195	0.000	63	0	63	0	0.103	0.000
50	229	2	228	10	0.195	0.045	63	2	62	7	0.103	0.120
51	217	0	217	0	0.187	0.000	54	0	54	0	0.091	0.000
52	217	6	214	86	0.187	0.503	54	4	52	26	0.091	0.667

Table 2: The Determinants of Unemployment, Piecewise-Constant Hazards Specification (n=2,345)

Independent variable	Specification						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
AGE	-.017*** (.003)	-.018*** (.003)	-.020*** (.005)	.020*** (.005)	-.019*** (.004)	-.018*** (.004)	-.018*** (.004)
UN	-.092*** (.013)	-.096*** (.013)	-.098*** (.020)	-.087*** (.017)	-.095*** (.015)	-.093*** (.015)	-.095*** (.014)
CLOSE	.119** (.049)	.117** (.054)	.265*** (.089)	.250*** (.078)	..114* (.062)	.133** (.061)	.130** (.059)
SCHOOL	.009 (.009)	.005 (.010)	-.034 (.018)	-.038** (.015)	-.008 (.012)	.001 (.012)	.004 (.011)
TENURE	-.019*** (.005)	-.019*** (.005)	-.039*** (.008)	-.029*** (.008)	-.022*** (.006)	-.024*** (.006)	-.021*** (.005)
HEAD	.180*** (.058)	.222*** (.064)	.312*** (.101)	.235*** (.089)	.260*** (.072)	.250*** (.072)	.227*** (.069)
WHITE	.402*** (.082)	.455*** (.089)	.365** (.144)	.399*** (.130)	.383*** (.103)	.483*** (.101)	.460*** (.096)
AGE (t)			.002 (.002)	.004 (.006)	.005 (.006)	-.006 (.008)	-.006 (.017)
UN (t)			.007 (.008)	.008 (.025)	.009 (.027)	-.070* (.037)	-.075 (.063)
CLOSE (t)			-.080** (.037)	-.235** (.098)	-.008 (.104)	-.008 (.138)	-.162 (.291)
SCHOOL (t)			.020*** (.007)	.075*** (.019)	.042** (.021)	.032 (.026)	.024 (.056)

TENURE (t)			.010***	.016*	.008	.009	.015
			(.003)	(.009)	(.010)	(.011)	(.020)
HEAD (t)			-.062	-.040	-.143	-.033	-.091
			(.042)	(.115)	(.117)	(.157)	(.363)
WHITE (t)			.015	.055	.132	-.008	-.130
			(.058)	(.167)	(.177)	(.205)	(.379)
UI EFFECT							
A.		-.628***	-.556***	-.606***	-.605***	-.677***	-.641***
		(.082)	(.069)	(.084)	(.087)	(.093)	(.087)
B.		.848***	.866***	.853***	.850***	.829***	.842***
		(.181)	(.175)	(.181)	(.182)	(.187)	(.183)
C.		.511***	.561***	.536***	.517***	.512**	.532***
		(.191)	(.175)	(.187)	(.187)	(.207)	(.198)
σ	.261	.455	0	.377	.376	.589	.498
	(.225)	(.145)		(.222)	(.251)	(.164)	(.164)
Log-likelihood	-7351.41	-7318.99	-7308.35	-7307.10	-7314.75	-7315.24	-7316.65

Asymptotic standard errors in parentheses

Notes: ***, **, * denote significance at the .01, .05 and .10 levels respectively

The independent variables are AGE, age at time of displacement; UN, state unemployment rate at time of job loss; CLOSE, dummy variable for job loss by reason of plant closing or relocation; SCHOOL, years of schooling completed; TENURE, years of tenure with former employer, HEAD, dummy variable for head of household; WHITE, dummy variable for white.

Table 3: The Determinants of Unemployment, Cox Specification (n=2,345)

Independent variable	Specification						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
AGE	-.015***	-.015***	-.018***	.018***	-.017***	-.015***	-.016***
	(.003)	(.003)	(.005)	(.004)	(.003)	(.003)	(.003)
UN	-.081***	-.079***	-.092***	-.078***	-.085***	-.073***	-.080***
	(.010)	(.010)	(.020)	(.017)	(.013)	(.011)	(.010)
CLOSE	.109**	.096**	.260***	.235***	..105*	.108**	.108**
	(.046)	(.046)	(.088)	(.074)	(.058)	(.051)	(.046)
SCHOOL	.011	.010	-.031*	-.033**	-.004	.005	.009
	(.009)	(.009)	(.018)	(.015)	(.012)	(.010)	(.010)
TENURE	-.016***	-.015***	-.036***	-.027***	-.019***	-.019***	-.017***
	(.004)	(.004)	(.009)	(.008)	(.006)	(.005)	(.004)
HEAD	.168***	.182***	.293***	.219***	.236***	.197***	.183***
	(.051)	(.052)	(.101)	(.084)	(.065)	(.057)	(.052)
WHITE	.378***	.390***	.351**	.371***	.358***	.424***	.397***
	(.074)	(.074)	(.150)	(.129)	(.097)	(.086)	(.076)
AGE (t)			.002	.005	.006	.001	.019
			(.002)	(.005)	(.005)	(.007)	(.015)
UN (t)			.007	.002	.019	-.024	.064
			(.008)	(.021)	(.021)	(.026)	(.055)
CLOSE (t)			-.078***	-.223**	-.024	-.050	-.297
			(.037)	(.094)	(.094)	(.118)	(.256)
SCHOOL (t)			.020***	.069***	.038**	.022	.039
			(.007)	(.019)	(.019)	(.023)	(.050)

TENURE (t)			.009***	.017*	.010	.015	.031*
			(.003)	(.009)	(.008)	(.009)	(.017)
HEAD (t)			-.053	-.058	-.144	-.073	-.093
			(.042)	(.107)	(.106)	(.134)	(.316)
WHITE (t)			.017	.026	.073	-.139	-.201
			(.039)	(.157)	(.150)	(.170)	(.341)
UI EFFECT							
A.		-.518***	-.515***	-.517***	-.515***	-.516***	-.514***
		(.071)	(.071)	(.071)	(.071)	(.071)	(.071)
B.		.854***	.835***	.846***	.840***	.852***	.853***
		(.175)	(.175)	(.175)	(.175)	(.175)	(.175)
C.		.537***	.538***	.543***	.525	.548***	.541***
		(.187)	(.187)	(.187)	(.187)	(.187)	(.189)
χ^2	174.42	229.04	251.26	252.58	238.64	235.16	238.11
Log-likelihood	-13782.16	-13754.85	-13743.73	-13743.07	13750.05	13751.79	-13750.31

Asymptotic standard errors in parentheses

Notes: ***, **, * denote significance at the .01, .05 and .10 levels respectively

The independent variables are AGE, age at time of displacement; UN, state unemployment rate at time of job loss; CLOSE, dummy variable for job loss by reason of plant closing or relocation; SCHOOL, years of schooling completed; TENURE, years of tenure with former employer, HEAD, dummy variable for head of household; WHITE, dummy variable for white.

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Appendix: Descriptive Statistics

Variable	UI Recipients	Nonrecipients
CLOSE	0.450	0.494
HEAD	0.721	0.670
WHITE	0.886	0.873
COMPLETED SPELL	0.840	0.876
AGE	37.350 (10.177)	34.828 (10.210)
UN	7.760 (2.322)	7.317 (2.234)
SCHOOL	11.439 (2.399)	11.540 (2.587)
TENURE	5.763 (6.767)	3.561 (5.964)
DURATION	22.512 (23.152)	10.725 (17.918)
n	1464	881

Standard deviations in parentheses

Note: DURATION is the length of the unemployment spell in weeks, and COMPLETED SPELL refers to the proportion of unemployment spells that are completed. The definition of the covariates is as reported in Table 2.