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UNEMPLOYMENT INSURANCE, RECALL EXPECTATIONS, AND UNEMPLOYMENT OUTCOMES*

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This paper empirically examines the importance of explicitly accounting for the layoff-rehire process in the analysis of unemployment outcomes in the United States. We find that the spells of individuals' who initially expect to be recalled account for much more of the unemployment of unemployment insurance (UI) recipients than do spells actually ending in recall. Our results indicate that the recall and new job escape rates from unemployment have quite different time patterns and are often affected in opposite ways by explanatory variables. We also find that the probability of leaving unemployment both through recalls and new job finding increases greatly around the time that UI benefits lapse.

Over the past fifteen years many studies have empirically analyzed the determinants of individual unemployment spell durations in the United States.[1] Much of this literature has used a job search framework in which the unemployed are viewed as permanently displaced workers who are unattached to jobs. The possibility of recall to a former job, a process not requiring search, has been omitted from explicit consideration in the majority of these studies.[2]

An explicit treatment of the layoff-rehire process is necessary for a proper understanding of the determinants of unemployment durations and of the impacts of unemployment policies in the United States for at least three reasons. First, temporary layoffs in which workers are often rehired by their original employers are a quantitatively quite important feature of the U. S. labor market.[3] Second, expectations concerning the likelihood of recall are likely to affect the responses of employers and unemployed workers to labor market interventions such as reemployment bonuses and subsidies

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1. See, for example, Ehrenberg and Oaxaca [1976], Kiefer and Neumann [1979], and Moffitt [1985].
2. Exceptions include Classen [1977] and Katz [1986].
3. In fact, Feldstein [1975] and Lilien [1980] conclude that over 70 percent of workers laid off in U. S. manufacturing in the 1970s were subsequently rehired by their former employers, and Katz [1986] finds that the layoff-rehire process also appears to be widespread outside of manufacturing.

for the training of the unemployed. Third, employer recall policies are a primary determinant of the duration of unemployment spells of individuals with nonnegligible recall prospects. Thus, studies examining the unemployment spells of job losers in the United States that fail to account directly for the layoff-rehire process may generate quite misleading inferences concerning the determinants of job search behavior and of unemployment spell durations.

This paper examines how an explicit consideration of the possibility of recalls affects inferences concerning the determinants of the duration of unemployment spells of unemployment insurance (UI) recipients. We first document that the prospect of recall to a previous employer appears to be relevant for the majority of UI recipients in the United States. We then examine how recall expectations are related to job search behavior, reemployment earnings, and unemployment spell durations.

Our analysis is performed using a unique sample of UI recipients from Missouri and Pennsylvania covering unemployment spells in the 1979–1981 period. This data set combines Continuous Wage and Benefit History (CWBH) UI administration records with information from a follow-up survey conducted approximately one year after individuals filed for UI benefits. These data allow us to determine the relationship between recall expectations and unemployment experiences for UI recipients in the two states. We corroborate some of our key findings using an additional CWBH data set that covers a longer time period and a greater number of states than the Missouri-Pennsylvania sample.

We find that about 70 percent of UI recipients expect to be recalled and that these individuals account for more than half of the weeks of unemployment of UI recipients. Furthermore, the spells of individuals' who initially expect to be recalled (ex ante temporary layoffs) account for much more unemployment than do spells actually ending in recall (ex post temporary layoffs). This difference arises because those initially *expecting* recall who are *not* actually recalled tend to have extremely long unemployment spells.

In the second part of our empirical work, we use a competing risks model, in which new job acceptances and recalls are treated as alternative routes of leaving unemployment, to study the impact of recall expectations, worker and job characteristics, and UI variables on the duration of unemployment spells. Our findings indicate that the recall and new job exit probabilities have quite different time patterns and often are affected in opposite ways by explanatory variables. In particular, the recall rate is quite high at short durations and then declines substantially, while the new job finding rate appears to increase with duration up to the point of UI benefits exhaustion. We also find that the probability of leaving unemployment both through recalls and new job finding increases greatly around the time that UI benefits lapse. These results suggest that the duration of UI benefits may have a strong influence on firm recall policies and worker new job finding behavior. Finally, we find that workers who expect to be recalled spend less time searching for jobs and have a lower new job finding rate than other UI recipients.

The remainder of the paper is organized as follows. Section I describes in detail our Missouri-Pennsylvania data set. Section II presents evidence on the fraction of the unemployment of UI recipients accounted for by alternative measures of temporary layoffs using several data sources and presents descriptive information on the relation between recall expectations and unemployment outcomes. Section III discusses the implications of models that explicitly take into account the possibility of recalls for the empirical analysis of unemployment spell durations. Section IV presents empirical evidence on the distribution of unemployment spell durations of UI recipients in Missouri. Section V uses formal econometric duration models to empirically examine the impact of recall expectations, demographic characteristics, and UI variables on unemployment spell durations. Section VI concludes by discussing the implications of our findings for the analysis and design of unemployment policies in the United States.

I. DATA DESCRIPTION: THE MISSOURI-PENNSYLVANIA SAMPLE

Our primary data set for this study consists of a sample of UI recipients from Missouri and Pennsylvania who filed for UI benefits from October 1979 to March 1980. The data set combines records collected by the Unemployment Insurance Service under the Continuous Wage and Benefit History (CWBH) system with information from special supplemental telephone interviews conducted in late 1980 and early 1981. The CWBH data include information from a survey administered when individuals filed for UI as well as UI administrative records of benefit receipt. The follow-up telephone interviews provide information on the starting date of and the weekly wages on the first post-UI job, and allow us to determine whether the job was with the pre-UI employer.[4]

4. Corson and Nicholson [1983] describe the construction and present an interesting empirical analysis of the original data set. Corson and Hilton [1982] provide detailed documentation of the version of the data set we used to make our sample.

This data set allows us to overcome many of the data limitations that confront studies that use either CWBH data or survey data in isolation. Studies using only CWBH data (e.g., Moffitt [1985]) are limited to the study of compensated weeks of unemployment and typically are unable to distinguish spells ending through recall from those ending through the finding of a new job. Studies using micro survey data sets such as the Panel Study of Income Dynamics (e.g., Katz [1986]) tend to have poor information on the UI system parameters facing unemployed individuals and may have significant measurement error because of the retrospective nature of many of the questions.[5] The Missouri-Pennsylvania data set allows us to distinguish compensated from uncompensated weeks of unemployment and to determine how unemployment spells end. It also provides information on the initial recall expectations of UI recipients and accurate administrative information on the level and length of available benefits. The major disadvantage of this data set is that it contains the unemployment spells of individuals from only two states over a short time period. In particular, since the sample contains only individuals who filed for UI benefits in the fourth and first quarters (October 1979 to March 1980), it is likely to overrepresent seasonal temporary layoffs. We present some evidence on the extent to which our conclusions generalize to other states and time periods in Section II.

The original Missouri-Pennsylvania telephone interview data set contains 2,035 observations. Exclusions for missing demographic data and incomplete or inconsistent information on unemployment spells leaves a sample of 1,499 observations. Variable definitions and basic descriptive statistics for this sample are given in Table I.

We focus most of our analysis on the initial spell of unemployment in the benefit year for each individual in the sample since the data set provides much better information on first spells then on total unemployment in the benefit year. We have developed several different measures of unemployment spell durations. IUSR measures the unemployment spell starting from the UI claim date that is available from administrative records, and FSPELL measures the spell from the respondent's self-reported spell start date. These two

5. The availability of both administrative records and survey responses in the Missouri-Pennsylvania data set allows us to assess the validity of survey respondents' retrospective reports of unemployment experiences. A brief analysis of the accuracy of the survey responses is contained in the Appendix.

TABLE I

DESCRIPTIVE STATISTICS FOR MISSOURI-PENNSYLVANIA UI RECIPIENTS DATA SET (UNEMPLOYMENT SPELL START DATES IN 1979–1980)

		Mean (S.D.)		
Variable	Description	Missouri	Penn.	Total
IUSR	Weeks from UI claim date until reemployment or until interview date if spell is censored	16.64	12.91	14.92
		(15.62)	(14.47)	(15.15)
FSPELL	Weeks from end of pre-UI job until reemployment or until interview date if spell is censored	19.35	16.21	17.90
		(16.66)	(16.54)	(16.67)
PAYSPELL	Weeks from UI first payment date until reemployment or until interview date if spell is censored (Missouri only)	15.27	—	—
		(14.81)		
PD1	Potential benefits duration in weeks at claim date	22.92	34.88	28.44
		(4.52)	(4.49)	(7.47)
UI benefit	Augmented weekly benefit amount	88.80	124.76	105.38
		(17.64)	(42.38)	(36.28)
Pre-UI wage	Usual weekly earnings on pre-UI job	258.35	256.13	257.33
		(133.12)	(122.97)	(128.50)
EXPREC	= 1 if expect recall at time of claim	0.74	0.76	0.75
DEFREC	= 1 if have definite recall date	0.12	0.25	0.18
Recall	= 1 if spell ended in recall	0.51	0.64	0.57
New job	= 1 if spell ended in taking a new job	0.40	0.28	0.34
Censored	= 1 if spell is censored at interview date	0.09	0.07	0.08
Age	age in years	36.43	36.80	36.60
		(13.19)	(13.59)	(13.37)
Female	= 1 if female	0.33	0.25	0.30
Married	= 1 if married	0.69	0.63	0.66
Education	Years of schooling	11.37	11.56	11.46
		(2.11)	(1.76)	(1.95)
Spwk	= 1 if spouse works	0.45	0.37	0.41
PA	= 1 if Pennsylvania	0.00	1.00	0.46
Industry dummies				
Mining	= 1 if mining	0.01	0.03	0.02
Construct	= 1 if construction	0.30	0.28	0.29
Durables	= 1 if durable goods manufacturing	0.21	0.24	0.22
Nondurables	= 1 if nondurable goods manufacturing	0.16	0.17	0.16
Transport	= 1 if transportation, communications or utilities	0.06	0.04	0.05
Trade	= 1 if wholesale or retail trade	0.12	0.13	0.12
Admin	= 1 if public administration	0.03	0.03	0.03
Service	= 1 if services	0.11	0.08	0.10
Occupation dummies				
Prof	= 1 if professional, technical, or managerial	0.06	0.05	0.05
Clerical	= 1 if clerical or sales	0.10	0.09	0.10
Supervisor	= 1 if supervisor	0.06	0.04	0.05
Craft	= 1 if craft and related occupations	0.34	0.38	0.36
Operator	= 1 if operator	0.23	0.29	0.26
Laborer	= 1 if laborer	0.21	0.15	0.18
Sample size		808	691	1499

measures can be computed for both Pennsylvania and Missouri. PAYSPELL is an alternative measure that more fully utilizes administrative records on the actual number of weeks of benefits received, but it can be computed only for individuals from Missouri.[6] All three unemployment spell measures lead to similar conclusions concerning the fraction of unemployment accounted for by alternative measures of temporary layoffs. The PAYSPELL measure provides more accurate information for analyzing the distribution of unemployment spell durations.

The descriptive statistics in Table I indicate that the prospect of recall was relevant for a large majority of the UI recipients in the sample. When asked soon after their unemployment spells began, 75 percent expected to be recalled, and 18 percent had a definite recall date from their employer.[7] Thus, the majority of the sample could initially be classified as being on "indefinite" layoff with some expectation of recall but without a known recall date. Fifty-seven percent of the individuals in the sample had initial unemployment spells ending in recall. The mean unemployment spell duration is about 15 weeks when measured from the claim date and about 18 weeks when measured from the end of the pre-UI job.[8]

UI recipients in Missouri and Pennsylvania largely consisted of blue-collar occupations and workers previously employed in construction and manufacturing. The importance of recalls varied substantially across industries. Sixty-six percent of the workers laid off from construction, mining, and manufacturing had spells ending in recall as opposed to 37 percent of the workers from transportation, trade, services, and administration.

6. PAYSPELL is defined as weeks from UI first payment date until reemployment (or until the interview date if the first unemployment spell is still in progress at the interview date). For individuals who had a single compensated unemployment spell in the benefit year and who gained reemployment before benefits were exhausted, PAYSPELL can be computed from CWBH administrative records and equals the weeks of benefits received in the benefit year. We were forced to use respondent retrospective information on weeks of benefits received in the initial spell for individuals with multiple compensated unemployment spells in the benefit year. In this case, PAYSPELL equals the survey respondent's self-reported weeks of benefits received during his or her initial unemployment spell. For individuals who exhausted their benefits during their initial unemployment spell, PAYSPELL is given by weeks from the UI first payment date (from CWBH records) until the self-reported reemployment date (or until the interview date if the spell is censored).

7. The recall expectations information is from a CWBH questionnaire that is administered when an individual files for UI. The questionnaire clearly indicates that the information is confidential and only for statistical and research purposes. The exact question used to determine whether an individual expected to be recalled is "do you expect to be called back to work by any of your past employers?" A response of yes to this question triggers the further question "did a past employer give you a definite recall date?"

8. These means understate the mean duration of completed spells since only incomplete spell durations are available for spells censored at the interview date.

The rules concerning the level and duration of UI benefits were much more generous in Pennsylvania than in Missouri during the period of our sample.[9] In particular, the maximum weekly benefit available was $105 in Missouri and $170 in Pennsylvania in 1980. Pre-UI earnings were similar in the two states leading to a much higher replacement rate in Pennsylvania. Regular UI benefits in Pennsylvania had a uniform duration of 30 weeks, while Missouri had a maximum potential duration of 26 weeks with variation in the potential duration that depended on base period and high quarter earnings. The Missouri sample provides substantial variation in the potential length of benefits, while the Pennsylvania sample provides almost none. Extended benefits were triggered in February 1980 in Pennsylvania and in May 1980 in Missouri. The extensions raised the potential length of benefits to 39 weeks in Pennsylvania and increased the potential length by 50 percent in Missouri.

II. Recall Expectations and Unemployment Outcomes: Some Evidence

In this section we analyze the fraction of the unemployment of UI recipients in our Missouri-Pennsylvania sample that can be accounted for by temporary layoffs. We further examine the fraction of compensated unemployment attributable to different measures of temporary layoffs for a larger and more representative sample of UI recipients. Finally, we analyze the relations among recall expectations, job search behavior, and the reemployment earnings of UI recipients.

We introduce a distinction between ex ante and ex post layoffs. Ex ante layoffs are those that begin with a person expecting to be recalled, while ex post layoffs are those ending in recall. Previous examinations of the fraction of total unemployment time accounted for by the layoff-recall process have used the ex post concept or a hybrid approach. The ex post concept gives the proportion of unemployment from spells involving no job change [Feldstein, 1975; Clark and Summers, 1979]. The hybrid approach looks at in-progress temporary layoffs and is the fraction of the unemployed at a point in time who are "on layoff awaiting recall" by a previous employer [Murphy and Topel, 1987]. These measures are likely to underestimate the total amount of unemployment affected by recall

9. See Corson and Nicholson [1983] for further information on the Missouri and Pennsylvania UI systems during this period.

prospects. The ex post measure does not include the unemployment of those who initially waited for recall but who do not eventually get recalled. The hybrid approach only partially includes people who expect to be recalled, since recall expectations are likely to fade as an unemployment spell continues.

The ex post approach does not take into account the fact that some workers who expect to be recalled at the time of layoff are not recalled or find other jobs before being recalled. Workers expecting recall whose expectations are not met, may have quite long unemployment spells, since they are unlikely to search intensively for a new job as long as they regard the probability of recall to a valuable old job as high. If these workers receive UI benefits, they may be willing to wait as long as the benefits last before searching for another job. Other employers may be unwilling to incur the initial fixed costs of hiring and training workers with reasonable prospects of recall to a more attractive job. These factors suggest an ex ante temporary layoff concept to measure the amount of unemployment affected by the layoff-recall process. The recall expectations information in our Missouri-Pennsylvania data set allows us to compare our ex ante layoff concept with the usual ex post temporary layoff approach.

Table II presents the distribution of first unemployment spells and weeks of first spell unemployment by spell outcome, recall expectations, and definite recall status for our entire sample using the IUSR unemployment concept. Since it is unlikely that many of the long censored spells ended in recall, it appears reasonable to conclude that about 57 percent of the unemployment spells and 32 percent of the weeks of unemployment of UI recipients in our two states are accounted for by ex post temporary layoffs.[10] The typical spell ending in recall was substantially shorter than those ending in the finding of a new job. Less than 10 percent of unemployment is accounted for by spells in which individuals had a definite recall date. On the other hand, almost 64 percent of unemployment is accounted for by ex ante temporary layoffs.

The lower half of Table II provides more detailed information on the relation between recall expectations and unemployment outcomes. Seventy-two percent of those who expected to be recalled and 13 percent of those who did not expect to be recalled had spells

10. The share of unemployment accounted for by temporary layoffs is likely to be overstated in this sample relative to a random sample of unemployment spells over the calendar year since most of the spells started in the peak period for temporary seasonal layoffs (December, January, and February). We provide evidence on the importance of this bias below.

TABLE II
RECALL EXPECTATIONS AND UNEMPLOYMENT SPELL OUTCOMES FOR FIRST SPELLS OF UNEMPLOYMENT USING THE IUSR UNEMPLOYMENT MEASURE
(ENTIRE SAMPLE—MISSOURI AND PENNSYLVANIA—1,499 OBSERVATIONS)

Group	Percentage of spells	Percentage of total weeks of unemployment	Mean duration in weeks
Spell outcome:			
Recall	57.2	32.4	8.4
New job	34.4	39.1	17.0
Censored	8.4	28.5	50.6
Recall expectations:			
Expect recall	75.2	63.8	12.7
Don't expect recall	24.8	36.2	21.8
Definite recall:			
Definite recall date	18.1	9.7	8.0
No definite recall date	81.9	90.3	16.5
Recall expectations and spell outcome:			
Expect recall (n = 1,127):			
Recall	71.7	46.4	8.2
New job	22.2	29.0	16.5
Censored	6.1	24.6	50.8
Don't expect recall (n = 372):			
Recall	13.4	7.6	12.3
New job	71.2	57.0	17.4
Censored	15.3	35.4	50.4

Note. The length of the unemployment spell up to the interview date is utilized as the unemployment spell duration for censored spells in the percentage of total weeks of unemployment and mean duration in weeks calculations.

actually ending in recall. An interesting finding from this table is that, although the vast majority of those who expected to be recalled were recalled, more than 50 percent of the total unemployment of those who expected to be recalled is accounted for by the minority who were not recalled. UI recipients who ex ante expected to be recalled and ex post were not recalled tend to have quite long unemployment spells. While this group accounts for only 21 percent of the entire sample, it accounts for approximately 34 percent of first spell unemployment.

One plausible reason why those who expect be recalled but are not tend to have long unemployment spells is that they may rationally decide to wait for recall and not search very intensively for a new job.[11] (They may also have a difficult time gaining new jobs

11. An alternative possibility is that some of the workers who claimed to expect to be recalled and who do not actually get recalled may simply be "malingerers" who claim to expect recall for the purpose of avoiding UI job search requirements.

TABLE III
SEARCH BEHAVIOR OF UI RECIPIENTS (ENTIRE SAMPLE—MISSOURI AND PENNSYLVANIA—1,499 OBSERVATIONS)

Group	Percent who searched	Mean search hours per week of those who searched	Unconditional mean search hours per week
All	59	12.1	7.1
Spell outcome:			
Recall	41	9.8	4.0
New job	85	14.3	12.1
Censored	78	11.3	8.8
Recall expectations:			
Expect recall	52	10.9	5.7
Don't expect recall	83	14.5	12.0
Definite recall:			
Definite recall date	33	11.7	3.8
No definitie recall date	65	12.2	7.9

Note. The percent who searched calculations are based on the yes-no answers of workers to the following question: "I'd like to ask you about the period of time after that job [pre-UI job] ended. Did you look for work at that time?" Workers who answered yes to this question were later asked "And about how many hours per week on the average would you say you spent looking for work?"

since employers will be reluctant to hire those likely to return to their old jobs.) Table III provides some information on the search behavior of the UI recipients in our sample. Fifty-nine percent of the UI recipients claimed to have looked for work at the time they were laid off. The average searcher spent 12 hours a week looking for work. Those who expected to be recalled were substantially less likely to search than those who did not expect to be recalled, and they searched many fewer hours on average as well. This result is consistent with the finding of Barron and Mellow [1979] that those who classify themselves as being on "temporary layoff" in the Current Population Survey spend less time searching than do other individuals who classify themselves as unemployed. Low search intensity may play a role in the low rate of new job finding of those who expect to be recalled.

Workers with a job to which they are likely to be recalled typically are not required to register with the state employment service and do not have as strict job search requirements as other UI recipients. Although information on the CWBH questionnaire is clearly not used for the purposes of determining job search requirements, we cannot completely rule out the possibility that this explanation is of quantitative significance.

The Contribution of Temporary Layoffs to Total Compensated Unemployment

We next examine the share of total weeks of compensated unemployment accounted for by alternative measures of temporary layoffs. The importance of temporary layoffs in compensated unemployment is of particular interest for UI program purposes. Our primary data set provides information on total weeks of compensated unemployment (weeks of UI benefit receipt) only for the Missouri sample.

The distribution of total compensated unemployment in the benefit year by outcome of the first spell and first spell recall expectations for our Missouri sample is presented in the first row of Table IV. Individuals whose first spell ended in recall account for almost 41 percent of the total weeks of compensated unemployment. This percentage is substantially larger than their share of total weeks of first spell unemployment. This difference arises because those recalled are more likely to have multiple spells of UI receipt in a year and because weeks of unemployment after UI exhaustion are not included.

Since the Missouri-Pennsylvania sample covers only two states over a short period in which seasonal temporary layoffs are likely to be overrepresented, we provide information in Table IV on the importance of recall expectations and actual recalls for UI recipients in a larger number of states over a longer time period using the CWBH administrative data described in Meyer [1989]. This data set provides consistent information on recall expectations, actual recalls, and total compensated unemployment over the benefit year for four states (Missouri, Pennsylvania, Idaho, and Washington) for the period July 1979 to December 1982. Information on a fifth state (New Mexico) is available for a slightly shorter period. Workers expecting to be recalled account for over 55 percent of compensated unemployment in three of the five states, and ex ante temporary layoffs are responsible for much more compensated unemployment than ex post temporary layoffs in all five states. A comparison of the last two rows of Table IV shows that, while the time period covered by our Missouri-Pennsylvania sample does somewhat overstate the importance of temporary layoffs, the basic conclusions that individuals expecting to be recalled account for the substantial majority of insured unemployment spells and that ex ante temporary layoffs are quantitatively more important than ex post temporary layoffs

TABLE IV
DISTRIBUTION OF TOTAL COMPENSATED UNEMPLOYMENT IN BENEFIT YEAR (CWBH ADMINISTRATIVE DATA[a])

State	Time period	Sample size	Expecting Recall[b]: % of individuals	Expecting Recall[b]: % of compensated unemployment	Actually Recalled[c]: % of individuals	Actually Recalled[c]: % of compensated unemployment
Missouri[d]	10/79 to 3/80	808	74.4	69.5	51.1	40.7
Missouri	7/79 to 12/82	35749	63.5	55.7	42.5	30.7
Pennsylvania	7/79 to 12/82	21281	74.7	67.1	51.2	33.3
Idaho	7/79 to 12/82	14261	71.4	64.7	46.9	37.0
Washington	7/79 to 12/82	30523	57.3	49.1	33.4	20.8
New Mexico	4/80 to 12/82	7964	41.1	37.1	23.6	16.9
MO, PA, ID, and WA	10/79 to 3/80	14199	71.6	66.4	44.0	32.3
MO, PA, ID, and WA	7/79 to 12/82	101814	65.1	57.1	42.2	28.6

a. The first row uses the CWBH data described in Section I. The remaining rows present data from the CWBH sample described in Meyer [1989]. Each row includes the compensated unemployment experiences of individuals with benefit year start dates in the indicated time period. Observations with missing values are excluded from the calculations.

b. Individuals are classified as expecting recall if at the time they filed their initial UI claim in the relevant benefit year they responded yes in the CWBH questionnaire to the question, "do you expect to be called back to work by any of your past employers?"

c. An individual is classified as having been actually recalled if his or her employer during any of the three quarters after the last UI payment in the benefit year matches his or her employer during either of the two quarters prior to the claim.

d. Individuals are classified as actually recalled in this sample if their first post-UI job is with the same employer as their pre-UI job.

remain when one examines the entire available sample period for the four states with consistent data.

Furthermore, our findings are quite similar to those of Robertson [1988] for Canada. Robertson finds that 44 percent of total UI weeks in Canada in 1984 were accounted for by ex post temporary layoffs. Thus, we conclude that a substantial proportion of insured unemployment in both United States and Canada appears to be related to the layoff-recall process.

Reemployment Earnings

An important element in the evaluation of the success of a UI program is the effect of UI on the wages of reemployed workers. Table V provides information on the post-UI job earnings relative to pre-UI job earnings of those individuals in the Missouri-

TABLE V

POST-UI JOB EARNINGS RELATIVE TO PRE-UI JOB EARNINGS FOR THOSE REEMPLOYED BY THE INTERVIEW DATE (EARNINGS CHANGE MEASURE = LOG (POST-UI EARNINGS/PRE-UI EARNINGS) MISSOURI-PENNSYLVANIA SAMPLE)

		Change in log weekly earnings		Change in log hourly earnings	
Group	Sample size	Mean	Median	Mean	Median
Entire sample by spell outcome (n = 1,331):					
Recall	838	−0.059	−0.046	−0.014	−0.023
		(0.011)		(0.009)	
New job	493	−0.156	−0.103	−0.128	−0.089
		(0.023)		(0.019)	
New job finders (n = 493):					
Recall expectations:					
Expect recall	240	−0.201	−0.141	−0.151	−0.104
		(0.034)		(0.028)	
Don't expect recall	253	−0.113	−0.081	−0.106	−0.081
		(0.031)		(0.027)	
Whether exhausted:					
Exhausted benefits	67	−0.520	−0.425	−0.301	−0.246
		(0.078)		(0.058)	
Didn't exhaust benefits	426	−0.098	−0.086	−0.101	−0.085
		(0.023)		(0.020)	

Note. The numbers in parentheses are the standard errors of the means. Earnings are deflated by average hourly earnings of U. S. private nonagricultural workers (series AHEEAP from DRI). The base period for the deflator is the second quarter of 1979. Pre-UI job earnings are deflated from the end date of the pre-UI job. Post-UI job earnings are deflated from the interview date.

Pennsylvania sample reemployed by the interview date.[12] Those with unemployment spells ending in recall appear to go back to their old jobs since their post-UI hourly earnings are quite similar to their pre-UI hourly earnings. On the other hand, the usual weekly hours of those rehired by their previous employers do decline by about 4.5 percent on average. The reduced hours of those recalled may be due to the cyclical downturn that gained force by the middle of 1980.

Individuals with spells ending through the finding of new jobs typically experienced substantial earnings declines. In particular, the hourly earnings of those who expected to be recalled but were not fell by 15 percent on average, while new job finders who did not expect to be recalled experienced 11 percent earnings losses on average. Table V also illustrates that individuals who exhausted their benefits experienced the largest earnings declines by a substantial margin. Their hourly earnings declined by 30 percent on average, and their weekly earnings declined even further. The large losses of exhaustees suggest that reservation wages are likely to fall substantially and that the new job finding rate is likely to increase substantially as benefits run out. An alternative explanation for the low relative reemployment earnings of those with long spells is heterogeneity in reemployment prospects. Workers with low job offer arrival rates are likely to have both low reservation wages and low escape rates from unemployment for many plausible wage offer distributions.

III. Theoretical Background

The duration of unemployment is typically analyzed using a standard job search model in which unemployed workers generate job offers by costly search. This approach leads to a single risk model of unemployment spell durations in which unemployment spells can only end through the finding of an acceptable new job. This formulation is less appropriate when analyzing the unemployment durations of workers on layoff with some possibility of recall. The prospect of recall affects the probability of leaving unemploy-

12. Pre-UI earnings are from information provided by respondents at the time that they made their UI claims. Post-UI earnings are from the follow-up survey. The choice of deflator (Average Hourly Earnings Versus CPI) affects conclusions about the magnitude of earnings changes. The earnings losses are substantially larger when the CPI is used as the deflator. On the other hand, the choice of deflator does not substantively affect any conclusions concerning relative earnings changes of any of the group compared.

ment directly through the rate of actual recalls and indirectly by affecting worker search behavior. Katz [1986] finds in a standard job search model extended to include an exogenous recall probability that better recall prospects are likely to reduce the new job finding rate by raising the reservation wage and reducing the likelihood of search.[13] This suggests that workers who expect to be recalled may have extremely long unemployment spells if their expectations are not fulfilled.

Katz [1985] also analyzes a model in which unemployed workers learn about their recall prospects in a Bayesian manner. He shows that the longer a worker is unemployed, all else held constant, the lower will be his or her subjective probability of recall. This result leads to a decreasing reservation wage and possibly increasing search intensity. Consequently, the new job finding rate for those who initially expect to be recalled should rise with unemployment duration (display positive duration dependence) under this scenario. UI benefits of limited potential duration can also generate a smoothly increasing new job hazard up to the point of benefits exhaustion since the value of remaining unemployed decreases as the number of remaining weeks of benefits decreases [Mortensen, 1977]. If individuals can locate jobs and arrange not to begin work until their benefits run out, this effect may generate a discontinuous increase in the escape rate near the point of benefits exhaustion.

Mortensen [1987] incorporates both limited duration UI benefits and the possibility of recalls in a joint wealth-maximizing model of job separations. Layoffs occur in response to reductions in match-specific productivity. The reservation wage decreases over the course of an unemployment spell as a worker approaches benefit exhaustion. This induces an increasing new job finding rate and an increasing recall rate as well. Mortensen shows that for realistic parameter values most of the decline in the reservation wage should occur in the last week or two before exhaustion. The discrete change in the flow value of being unemployed when benefits are exhausted yields the prediction that many firms may recall laid-off workers around the benefit exhaustion point and that the new job finding rate should increase around exhaustion.

The statistical model of unemployment spell durations generated by job search models extended to allow for recalls is a competing risks model in which unemployment spells can end either through recall or through the finding of an acceptable new

13. Burdett and Mortensen [1978] and Pissarides [1982] also analyze job search models that incorporate the possibility of recalls.

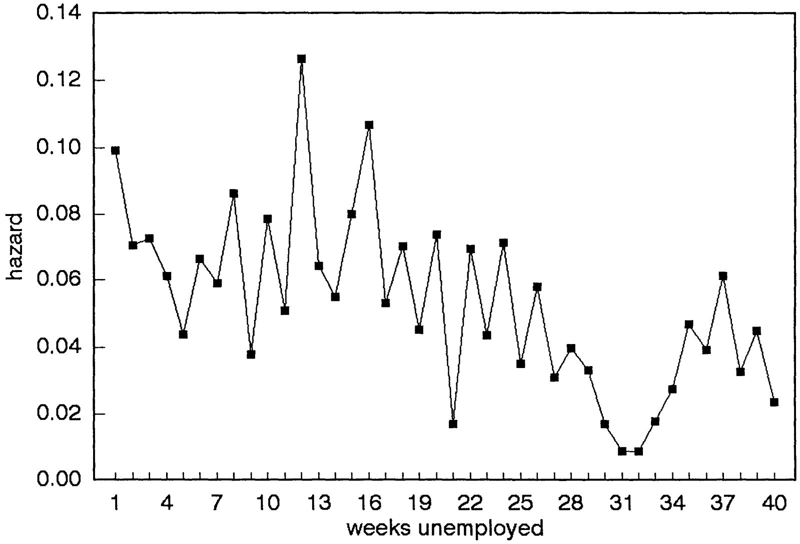

FIGURE I
Total Hazard

job.[14] The predictions of standard job search models for how variables affect the escape rate from unemployment really refer to the new job finding rate and these predictions need not hold for the overall escape rate from unemployment (the sum of the recall and new job finding rates). Information on whether spells ended through recall or the finding of a new job allows an econometrician to estimate a competing risks model. The competing risks specification has the advantage of permitting one to identify the distinct impact of variables on the recall rate and the new job finding rates.

IV. THE DISTRIBUTION OF UNEMPLOYMENT SPELL DURATIONS

The pattern of initial unemployment spell durations in our Missouri sample of UI recipients using the PAYSPELL unemployment spell concept is illustrated in Figures I and II. We focus our duration analysis on the Missouri sample since more information to construct accurate spell durations is available for this sample than

14. See Kalbfleisch and Prentice [1980] for a detailed discussion of competing risks models.

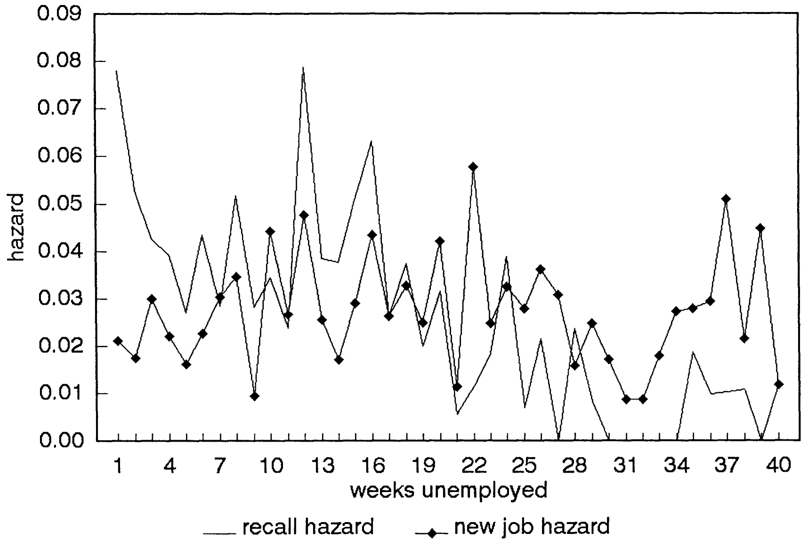

FIGURE II
Recall and New Job Hazards

for Pennsylvania. The overall empirical hazard for a given week is the fraction of spells ongoing at the start of that week which end during the week. The recall and new job empirical hazards are analogously defined as the fraction of spells ongoing at the start of the week that end during the week through recall and through the finding of a new job, respectively. The total hazard basically trends downward except for a rise at 12 and 16 weeks and a valley at around 32 weeks.[15]

The overall hazard masks the quite distinct patterns in the recall and new job hazards.[16] The recall hazard drops sharply over time except for spikes at 12 and 16 weeks and becomes quite low after about 25 weeks. The new job hazard starts out quite low and increases a bit on average until about 25 weeks. More precisely, the new job escape rate grouped into eight-week intervals rises from

15. A pronounced even-odd effect, where the hazard tends to be higher in even weeks, is also evident in Figures I and II. A possible explanation for this anomaly is that the cards used to claim benefits in Missouri are mailed two at a time to potential recipients.

16. These basic differences in the recall and new job finding hazards are quite similar to those found for UI recipients in a national sample of household heads from the PSID analyzed by Katz [1986].

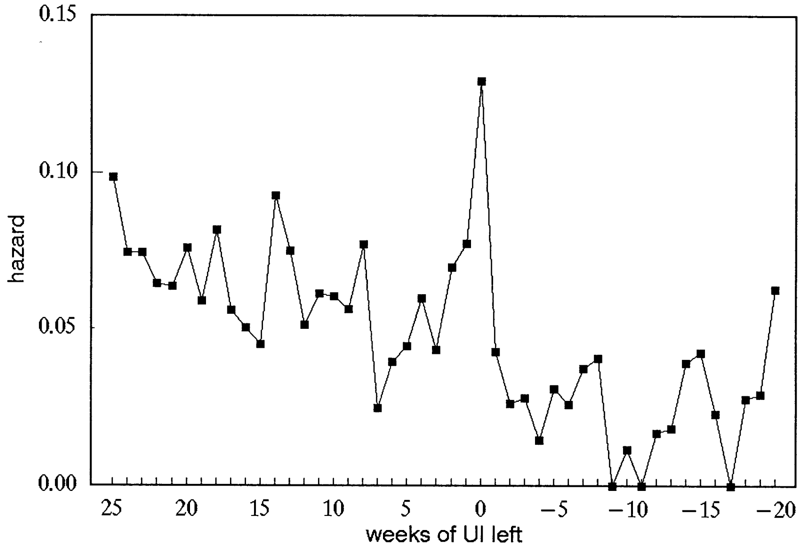

FIGURE III
Total Time Until Exhaustion Hazard

14.8 percent for the first eight weeks of a spell to 19.1 and 20.8 percent, respectively, for the 9-to-16 and 17-to-24 week intervals. Direct evidence on exhaustion effects is somewhat masked in Figures I and II because of the fair amount of variation in potential durations contained in the Missouri sample.

Figures III and IV provide a direct look at possible effects of finite length UI benefits on spell durations. The figures present time until exhaustion empirical hazards analogous to the usual Kaplan-Meier estimators. The time axis is time until benefits lapse rather than time since a spell began. There is a large spike in the hazard at the week of benefits exhaustion.[17] This spike is apparent for both the new job and recall hazards.[18] The new job finding rate remains relatively high after exhaustion, while the recall rate becomes minuscule after exhaustion. This suggests that workers may stop

17. The spike in the hazard function at the week of benefits exhaustion is not primarily a phenomenon related to hiring halls and seasonal fluctuations in the construction industry. Only four of the twenty-six individuals with spells ending in the UI exhaustion week were construction workers.

18. We have taken at face value the reasons people gave for ending receipt of UI. If a person responded that he stopped receiving UI because he found a job, his spell length was set equal to the number of weeks of UI received. This rule may have overstated the rise in the hazard near exhaustion if the responses to this question were in error.

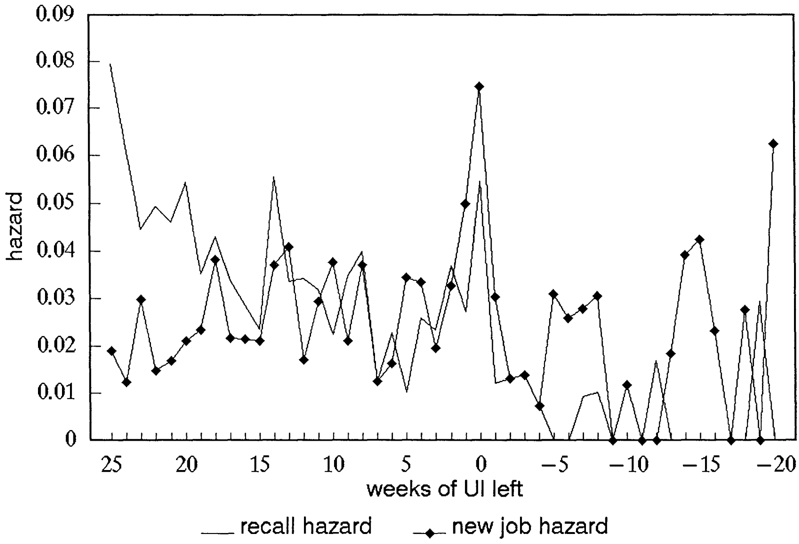

FIGURE IV
Time Until Exhaustion Recall and New Job Hazards

waiting for recall and start taking new jobs as their benefits run out. In fact, when we look only at workers who indicated when their spells began that they expected to be recalled, the new job finding rate is extremely low early in spells, and there is a prolonged sharp increase in the new job escape rate from four weeks before exhaustion through three weeks after exhaustion.

The recall spike around exhaustion in Figure IV provides some support for the Mortensen [1987] joint wealth-maximizing model of the layoff-recall process in which the flow value of being unemployed drops discretely as benefits run out. The exhaustion spikes are consistent with the findings on a PSID sample of Katz [1986] and on a CWBH sample by Moffitt [1985] and Meyer [1990]. Katz also finds that spikes in the hazard near typical exhaustion weeks (26 and 39 weeks) are not apparent for non-UI recipients. The absence of such a pattern for non-UI recipients strongly suggests that the exhaustion spikes for UI recipients are related to the finite length of UI benefits.

V. FORMAL DURATION MODELS FOR THE MISSOURI SAMPLE OF UI RECIPIENTS

In this section we analyze the impact of recall expectations, individual and pre-UI job characteristics, and UI system variables on the total, recall, and new job exit rates from unemployment for the Missouri sample of UI recipients.

Model Specification

The exit rates from unemployment are analyzed using formal hazard model techniques. We use a proportional hazards model estimator that allows for time-varying explanatory variables and that nonparametrically estimates the change in the hazard over time.[19] The estimates are the parameters of a continuous time hazard model and thus retain a clear interpretation. Nonparametrically estimating the change in the hazard over time eliminates the need to impose a potentially restrictive functional form that has no theoretical justification.

Formally, we parameterize the overall hazard rate from unemployment for individual i at time t, $\lambda_i(t)$, using the proportional hazards form. Let T_i be the length of individual i's unemployment spell. Then

$$\begin{aligned}\lambda_i(t) &= \lim_{h \to 0^+} \frac{\text{prob}\,[t + h > T_i \geq t \mid T_i \geq t]}{h} \\ &= \lambda_0(t) \exp\,\{z_i(t)'\beta\},\end{aligned}$$

where

$\lambda_0(t)$ is the baseline hazard at time t, which is unknown,

$z_i(t)$ is a vector of time dependent explanatory variables for individual i, and

β is a vector of parameters, which is unknown.

The probability of a spell lasting until $t + 1$ given that is has lasted until t is easily written as a function of the hazard:

$$(1) \qquad P[T_i \geq t + 1 \mid T_i \geq t] = \exp\left[-\int_t^{t+1} \lambda_i(u)\,du\right].$$

Assuming that $z_i(t)$ is constant between t and $t + 1$, equation (1) can be rewritten as

$$(2) \qquad P[T_i \geq t + 1 \mid T_i \geq t] = \exp\,[-\exp\,\{z_i(t)'\beta + \gamma(t)\}],$$

19. This semiparametric approach is analyzed in detail in Meyer [1986].

where

$$\gamma(t) = \ln\left\{\int_t^{t+1} \lambda_0(u)\, du\right\}. \tag{3}$$

The log-likelihood for a sample of N individuals can be written as a function of terms such as (2):

$$L(\gamma,\beta) = \sum_{i=1}^{N} \left\{ d_i \cdot \ln\left[1 - \exp\left\{-\exp\left[\gamma(k_i) + z_i(k_i)'\beta\right]\right\}\right] - \sum_{t=1}^{k_i-1} \exp\left[\gamma(t) + z_i(t)\beta\right] \right\}, \tag{4}$$

where

k_i = the time a spell ends or is censored, and
d_i = 1 if the spell ends before the survey date and 0 if the spell is censored.

This approach assumes that censoring does not provide any information about T_i beyond that available in the covariates.

We utilize an analogous methodology to estimate the recall and new job hazards within a competing risks model framework. The recall and new job hazards are specified analogously to the total hazard above. In the estimation of the recall hazard, spells ending in the finding of a new job are treated as censored ($d_i = 0$) at the date of new job finding. Spells ending in recall are analogously treated as censored at the recall date in the estimation of the new job hazard.

The effects of UI on the hazard rates are measured using functions of the benefits level and the time until benefits lapse. The level of weekly UI benefits is included as a time-varying covariate whose impact is allowed to vary depending on whether the individual is still receiving benefits or has exhausted benefits. Also included are time until benefit exhaustion dummy variables for five intervals covering both weeks before and after benefits have expired. These variables are designated UI 6–10 through UI ≤ -1. Each of these time-varying exhaustion dummies takes on the value of one in its designated interval and takes on the value of 0 in all other periods. For example, UI 6–10 takes on the value one when the individual is six to ten weeks until exhaustion; UI 0 takes on the value of one in the week of benefits exhaustion; and UI ≤ -1 takes on the value of one when the individual is one week or more after exhaustion. Those 11 or more weeks before exhaustion are the comparison group, the group corresponding to the omitted dummy variable.

Results for the Missouri UI Recipient Sample

Semiparametric hazard model estimates of the total, recall, and new job hazards for the Missouri sample using the PAYSPELL unemployment spell variable are presented in Table VI.[20] Initial recall expectations have a strong effect on the hazards, raising the recall and reducing the new job hazards substantially. Using the estimates in Table VI, those expecting recall have a recall hazard that is almost ten times as high as those who do not expect to be recalled. Furthermore, those expecting recall have a new job hazard that is almost 40 percent lower. The large negative coefficient on expect recall in the new job hazard indicates that workers who expect to be recalled and are not, tend to have much longer unemployment spells than observationally equivalent workers who realized they were permanently displaced at the time of layoff. This result is quite consistent with the findings of Katz [1986] and Gibbons and Katz [1989] that workers permanently displaced by layoffs (slack work or position or shift eliminated) have longer unemployment spells than those displaced in plant closings. The longer spells of those permanently displaced by layoffs are likely to reflect both the depressing effect of recall expectations on job search and a "lemons" effect in which outside employers draw negative inferences about the "quality" of workers who are laid off and not recalled when their original employers have discretion with respect to whom to layoff.

The expect recall and definite recall variables also have strong effects on the total hazard in the estimates presented in Table VI. Those who have a definite recall date (and necessarily expect recall) have a total hazard that is over twice as high as those not expecting recall. A definite recall date also further increases the recall hazard by a factor of 1.7, but has no significant effect on the new job hazard.[21]

20. The sample size falls to 756 in the hazard model estimates, since 52 individuals in the original Missouri sample have missing pre-UI job tenure data.

21. The industry dummy variable coefficients are fairly small and statistically insignificant when expect recall and definite recall date are included in the hazard model estimates. When the expect recall and definite recall date dummies are excluded, the industry dummy variables have much larger and statistically significant effects, with construction, durable goods, and nondurable goods industries having significantly higher recall rates and significantly lower new job finding rates than other industries.

TABLE VI
SEMIPARAMETRIC HAZARD MODEL ESTIMATES FOR MISSOURI UI RECIPIENTS[a]

Variable	Total hazard	Recall hazard	New job hazard
Expect recall	0.423	2.236	−0.500
	(0.099)	(0.272)	(0.135)
Definite recall	0.445	0.509	0.218
	(0.138)	(0.148)	(0.282)
UI benefit ($100s), pre-exhaust[b]	0.381	1.640	−1.115
	(0.322)	(0.438)	(0.447)
UI benefit ($100s), post-exhaust	0.496	—	−0.150
	(0.838)		(1.136)
Pre-UI net weekly wage ($100s)	−0.026	−0.075	0.048
	(0.045)	(0.059)	(0.061)
Age	−0.043	−0.039	−0.054
	(0.024)	(0.031)	(0.040)
Age squared/100	0.046	0.041	0.053
	(0.029)	(0.039)	(0.050)
Pre-UI job tenure (years)	0.0139	0.0260	−0.0304
	(0.0073)	(0.0088)	(0.0191)
Education	0.032	−0.049	0.128
	(0.018)	(0.030)	(0.029)
Black	−0.404	−0.392	−0.459
	(0.193)	(0.247)	(0.288)
Female	−0.161	−0.027	−0.416
	(0.118)	(0.145)	(0.182)
Time until exhaustion dummies:[c]			
UI 0	0.928	0.835	0.789
	(0.235)	(0.371)	(0.329)
UI 1	0.393	0.385	0.410
	(0.300)	(0.479)	(0.405)
UI 2–5	−0.090	−0.045	−0.164
	(0.194)	(0.273)	(0.291)
UI 6–10	−0.167	−0.166	−0.182
	(0.146)	(0.208)	(0.220)
UI ≤ -1	−0.636	−0.470	−1.423
	(0.732)	(0.416)	(0.976)
Log likelihood value	−2,416.2	−1,388.4	−1,275.6

a. The unemployment spell duration measure utilized is PAYSPELL. Other controls included in each of the specifications are number of dependents, spouse works and married dummies, a dummy indicating whether the spell started before February 1, 1980, six industry dummies, weeks from end of pre-UI job until claim date, weeks from claim date until first payment date. In the total and new job hazard models individual baseline hazard parameters are estimated for weeks 1 to 52; spells longer than 52 weeks are censored at 52. In the recall hazard parameters are estimated for the first 30 weeks, after which spells are censored. The number of observations is 756. The numbers in parentheses are asymptotic standard errors.

b. The UI benefit level variable is constrained to have the same effect before and after exhaustion in the recall hazard model. There are too few individuals recalled after exhaustion to estimate an additional coefficient.

c. The time until exhaustion dummy variables are defined in the text.

Increases in pre-UI job tenure, a possible measure of firm-specific human capital or job match quality, are associated with a significantly increased recall hazard and decreased new job hazard. Older workers appear to have longer spells because of both lower recall and new job-finding rates after controlling for tenure. The total hazard estimates mask many large differences between the effects of the covariates on the recall and new job-finding hazards.

The large and significant increases in the recall and new job hazards apparent in Figure IV at the week of benefits exhaustion are strongly confirmed in the more sophisticated hazard model estimates. Higher UI benefits are associated with higher recall rates and lower new job-finding rates. The UI benefit coefficients in the new job hazard appear reasonable; higher benefits greatly depress the new job-finding rate, and this effect disappears after benefits are exhausted. The positive and significant coefficient in the recall hazard is a puzzle. High UI benefits may be linked to the short-term temporary layoff sector of the Missouri economy. The effect of UI and the pre-UI wage on the total hazard are of opposite sign from the findings of most studies, although they are not statistically significant.

We further examine the time pattern of the baseline hazards from these models now that we have controlled for observable differences across individuals. After including explanatory variables, the time pattern of the hazards is captured by the $\gamma(t)$s, the baseline hazard parameters defined in equation (3).[22] These parameters confirm the patterns seen in Figures I and II. A total hazard that falls with unemployment duration masks the combination of an upward sloping new job hazard and a downward sloping recall hazard. A test of these patterns that confirms the visual impression was performed using GLS regressions of the baseline hazard parameters on the length of spell weighted by the inverse sampling variance of the estimated baseline hazard parameters. As a summary of the data, we used the specification $\gamma(t) = a + b \cdot \ln(t) + \epsilon$. This specification roughly corresponds to a Weibull baseline hazard. These regressions yield a positive coefficient on ln (t) of 0.31 with a standard error of 0.12 for the new job hazard (using the baseline hazard parameters up to week 42), and a negative coefficient on ln (t) of -0.12 with a standard error of 0.15 for the

22. The estimated baseline hazard parameters are not reported but are available from the authors.

recall hazard (using the baseline hazard parameters up to week 26). These results show the value of the competing risks specification that allows the disentangling of the two effects which produce the total hazard. Furthermore, the finding that the new job escape rate rises with spell duration, even after including variables which attempt to control for the remaining potential duration of UI benefits, suggests that falling reservation wages from declining assets and changing recall expectations may play an important role in the reemployment process of laid-off workers.[23]

A potential problem with the estimates in Table VI is that it is likely that some individual attributes which affect the hazard rate are omitted from the list of covariates. If unobserved heterogeneity is present, but not allowed for in the estimation, the coefficient estimates will be biased. Estimates which allow for individual specific omitted attributes under the assumption that a gamma distribution is a reasonable approximation to the distribution of heterogeneity in the population are very similar to those in Table VI.

Specifications were also tried that included several additional covariates: a dummy variable set equal to one if the individual engaged in job search at the time of job loss, the time-varying state unemployment rate, and five occupation dummy variables. None of these additions noticeably changed the key findings. The state unemployment rate and occupation dummies were always insignificant. The behavior of the search variable again illustrates the usefulness of the competing risks approach. In the total hazard the search variable comes in negative and highly significant, implying that those who search initially are reemployed less quickly. However, this may arise because initial search acts as a further proxy for the likelihood of recall. Those who strongly expect to be recalled may not search and may also be recalled quickly. The recall and new job hazard estimates provide some support for this interpretation.

23. Although uncontrolled heterogeneity biases estimates of the overall hazard toward spurious findings of negative duration dependence, a bias in the opposite direction is possible for an individual escape route hazard in a competing risks framework. If uncontrolled factors that raise the recall hazard also lower the new job hazard, then one can in theory find spurious positive duration dependence in the new job hazard. Han and Hausman [1986] have developed an estimator to handle correlated, unobserved heterogeneity in a competing risks model. They implement their estimator on the PSID layoff unemployment spell data set developed by Katz [1986] and find essentially zero correlation among the unobserved heterogeneity factors in the new job and recall hazards.

The search variable has a large negative value in the recall hazard, but is small and insignificant in the new job hazard.

Overall, the lack of variation in the UI parameters within Missouri suggests the need to look at a data set covering more states and a longer time period to determine more accurately the impact of the length and level of UI benefits on spell durations. Our recent work [Katz and Meyer, 1990; Meyer, 1989, 1990] on larger CWBH data sets with greater variation in UI system parameters than available in this Missouri sample strongly confirms previous research that has documented that both the level and length of UI benefits have substantial impacts on the duration of unemployment spells of UI recipients. The results with the Misouri sample do indicate that the recall process plays a major part in determining the duration of unemployment spells of UI recipients and the increase in unemployment escape rate around when benefits lapse.

Finally, several authors, including Hamermesh [1977], have suggested a subtle reason why studies that use weeks compensated by UI as the dependent variable might yield biased benefit coefficients. They suggest that higher benefits might induce people to claim UI more promptly, so that a larger fraction of an unemployment spell of a given length would be spent receiving UI. This effect might lead to the finding that higher benefits cause longer compensated spells even when there is no effect on the total length of unemployment. This effect is of potential importance in our Missouri sample where the mean number of weeks from loss of job until UI claim is 3.6 weeks and the standard deviation is 4.3 weeks. This hypothesis was tested by estimating hazard models where the dependent variable is the time from loss of job to the UI claim date. We used a set of control variables like that used for the unemployment spell specifications. The Hamermesh hypothesis would require a large positive coefficient on the benefit level, but the estimated coefficient was close to zero, negative, and insignificant. This result provides some support for the reliability of studies that use weeks compensated as the dependent variable.[24]

VI. CONCLUSION

In this paper we have introduced a distinction between ex ante and ex post temporary layoffs. Ex ante temporary layoffs are layoffs

24. Solon [1981] found a similar result in an examination of CWBH data for three states during the 1978–1979 period.

where an individual initially expects to be recalled, while ex post temporary layoffs are those ending in recall. Ex ante temporary layoffs appear to account for a majority of spells and weeks of unemployment of UI recipients in the United States, while ex post temporary layoffs account for a large, but appreciably smaller, fraction of unemployment. We emphasize the ex ante concept because the expectation of recall seems to affect the behavior of the unemployed. Those expecting recall spend less time searching for new jobs than do other UI recipients and tend to have extremely long unemployment spells if they are not actually rehired by their former employer.

We calculate that ex ante temporary layoffs accounted for over 57 percent of compensated unemployment for a sample of five states covering the 1979–1982 period. This figure suggests that at least 50 percent of UI benefits were paid out to those on ex ante layoffs during this period. Because most firms are quite imperfectly experience rated [Topel, 1983; Meyer, 1989], a large component of UI payments in the United States is likely to represent an implicit subsidy to ex ante temporary layoffs.

Our results combined with the findings of Murphy and Topel [1987] also suggest that ex ante temporary layoffs account for a substantial fraction of total (compensated and uncompensated) unemployment for males in the United States. In particular, Murphy and Topel find, using Current Population Survey (CPS) data, that for the 1967–1985 period the typical share of unemployed males who were on "layoff awaiting recall" was 34 percent, and the typical share of "job losers not currently expecting recall" was 49 percent. Since our findings indicate that many individuals with long spells may initially have expected and waited for recall and since very few of the long-duration unemployed in the CPS classify themselves as currently awaiting recall, we conclude that those who initially expected recall and end up with long unemployment spells eventually stop classifying themselves as awaiting recall. In this case, many of the long-term unemployed who are listed as job losers not currently expecting recall in the CPS may actually represent ex ante layoff spells. Thus, 34 percent may be a significant underestimate of the fraction of male unemployment accounted for by the layoff-rehire process over the 1967–1985 period.

We have also used a competing risks approach to divide the exit rate from unemployment into a recall rate and a new job finding rate. This approach shows that the recall and new job escape rates from unemployment have quite different time patterns and are affected in opposite ways by key explanatory variables. Those expecting recall have a recall hazard that is almost ten times higher and a new job finding rate that is almost 40 percent lower than other UI recipients. We find that the hazard rate of finding a new job rises as a spell progresses as suggested by several search models. This result holds after accounting for observed individual characteristics and the limited duration of UI. Furthermore, we find that the probability of leaving unemployment, both through recalls and new job finding, increases greatly around the time that benefits are exhausted.

Our findings concerning the quantitative importance of ex ante temporary layoff spells in the United States and the behavioral effects of recall expectations have implications for the design and evaluation of unemployment policies. First, since the importance of recall expectations and the value of waiting for recall varies substantially across sectors, the persistence of unemployment may depend greatly on the sectoral distribution of adverse shocks. Second, unemployed workers are likely to have little interest in relocation or retraining programs when they expect to be recalled. In fact, a recent news story discussing difficulties with a subsidized job retraining program reported that "As long as workers think there is hope for a recall and a [high] wage, the retraining experts here say, they are unlikely to join the program" [Kilborn, 1990, p. A10].

Third, the impact of reemployment bonus programs in which large lump sums are offered to UI recipients who end their spells quickly is likely to depend greatly on how such programs treat temporary layoffs. Such bonuses have recently been offered on an experimental basis in four states in attempts to determine whether they provide a cost-effective way of shortening unemployment spells.[25] If a bonus program pays bonuses to people returning to their last employer, they may prove counterproductive even if they shorten the typical insured unemployment spell. This potential for perverse impacts arises since imperfect experience rating means that the availability of such bonuses may create strong incentives for increased use of temporary layoffs.

25. See Woodbury and Spiegelman [1987] and Meyer [1988] for evaluations of a noteworthy reemployment bonus experiment in Illinois.

APPENDIX: ACCURACY OF SURVEY RESPONSES

The combination of administrative records and survey data available in the Missouri-Pennsylvania data set provides an opportunity to explore the accuracy of the retrospective reports on weekly benefit levels, weeks of benefit receipt, and unemployment spell durations. The data set allows us to compare accurate administrative records with the survey responses of the UI recipients. We find that the sample members provide quite accurate information on the level of UI benefits they received and quite poor information on the weeks of benefits received and the dates of the beginning and ending of their unemployment spells. A little over two thirds (67.5 percent) of the 1,408 individuals in our sample that provided information on the level of weekly benefits reported exactly (to the dollar) the benefit level indicated by administrative records. Eighty-five percent of the sample were within $10 of the true amount. The mean self-report was slightly downward biased ($102 reported versus $105 actual), and the variance of the reporting error divided by the variance of the true value was a fairly small 0.26.

On the other hand, very few individuals in the sample reported weeks of benefit receipt the same as indicated by their CWBH records. Only 15 percent of the 561 individuals in Missouri with a single spell of unemployment in the benefit year reported weeks of benefit receipt equivalent to the number provided by administrative records; 35 percent have deviations from CWBH records of over four weeks. The mean absolute difference between weeks reported by respondents and CWBH records is 4.5 weeks. Many inconsistencies in reported dates are apparent in the sample.

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