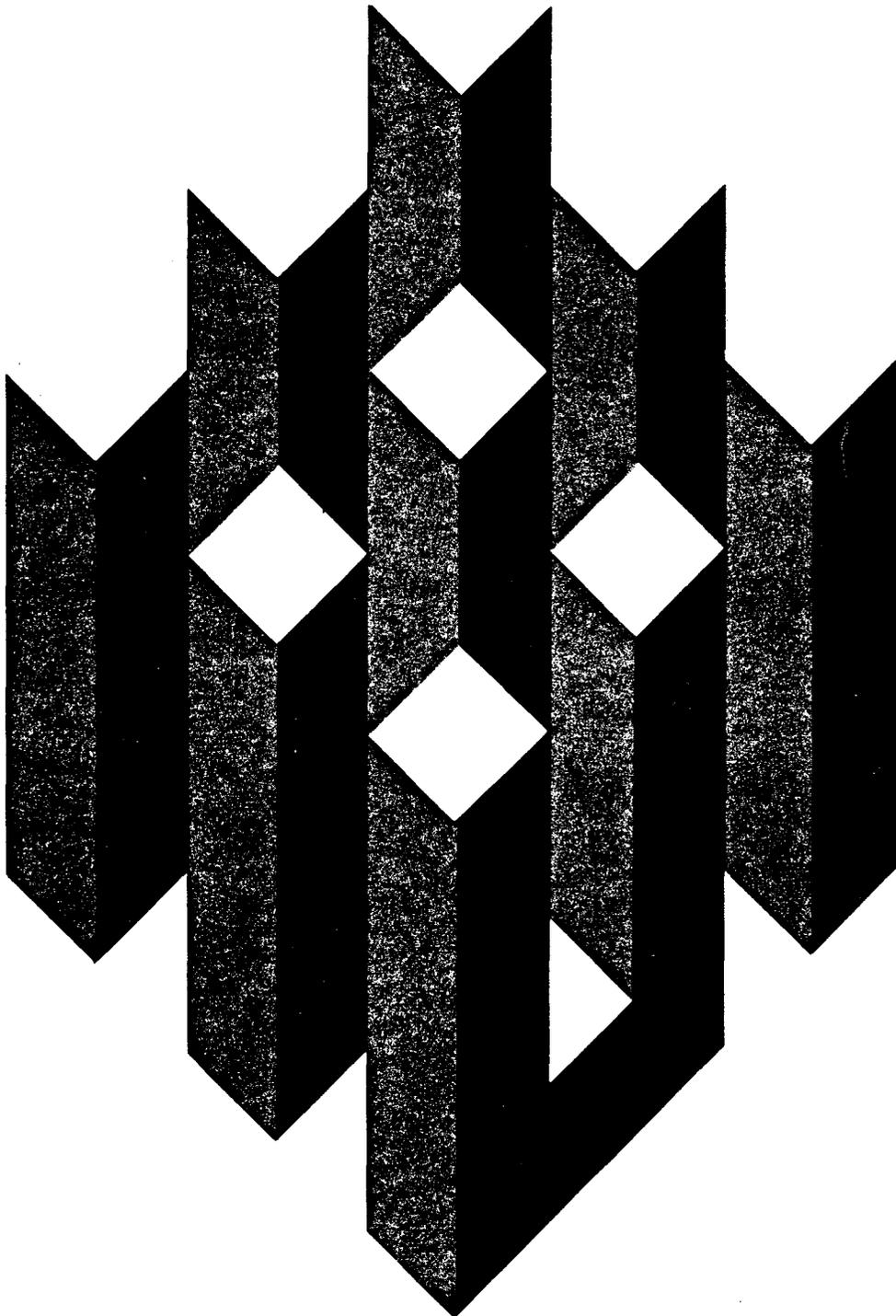


An Analysis of UI Recipients' Unemployment Spells



Unemployment Insurance
Occasional Paper 83-1

U.S. Department of Labor
Unemployment and Training Administration



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U.S. Department of Labor
Raymond J. Donovan, Secretary

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Albert Angrisani, Assistant Secretary for
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This report was prepared by Walter Corson, Senior Economist, Mathematica Policy Research, and Walter Nicholson, Professor of Economics, Amherst College, and Senior Fellow, Mathematica Policy Research, under sponsorship of the Unemployment Insurance Service of the Employment and Training Administration, U.S. Department of Labor. Because researchers are encouraged to express their own viewpoints, the opinions offered in this document do not necessarily represent the official position or policy of the Department of Labor.

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I: INTRODUCTION AND SUMMARY

A. PURPOSE OF THE STUDY

The purpose of this study was to evaluate how information collected by the Unemployment Insurance Service under the Continuous Wage and Benefit History (CWBH) system can be used to analyze UI recipients' unemployment experiences. The study had both analytical and methodological focuses. With respect to analyzing UI recipients' unemployment experiences, three general issues were investigated in detail:

1. Providing detailed description of such unemployment measures as initial unemployment spells, weeks of UI benefits collected, exhaustion rates for both regular and extended UI benefits, post-exhaustion unemployment, and post-unemployment wage rates.
2. Developing analytical models to explain and predict these unemployment outcomes using data from the CWBH system; and
3. Using the data to attempt an evaluation of the services provided to UI recipients by state Employment Services (ES).

The study also had major methodological components related both to the overall suitability of the CWBH data set for UI research and to ways in which that data set might be supplemented. Specifically, three questions related to the supplementation of CWBH data were addressed:

1. Is there a need to supplement the CWBH data set to include information on individuals' complete unemployment spells?
2. If the CWBH data set were to be supplemented with additional data on unemployment experiences, what form would that data collection effort take?

3. Can the CWBH data be supplemented with data from the ESARS system to provide reliable evaluations of services provided through the ES?

In the final section of this chapter we provide a summary of our results as they relate to each of these analytical and methodological questions. First, however, we provide a brief outline of the design of the study.

B. STUDY DESIGN

Designing the present study involved making decisions about the kinds of data to be collected, sample sites, and questions of sample size, allocation and composition. Since the basic data set for the study came from the administrative and interview data that comprise the CWBH system, design decisions were focused on devising strategies for utilizing and supplementing those basic data. In order to obtain additional information on individuals' unemployment experiences, it was decided to conduct a supplemental interview at the end of the benefit year that covered topics such as length of unemployment spells and post-unemployment wage rates that are not currently available from the CWBH file. The timing of that interview was chosen both to permit most individuals to complete their unemployment spells and to provide data that were comparable in the period of coverage to those in the CWBH system. Conducting the supplemental interview at the end of the benefit year also had the advantage of occurring relatively close to individuals' unemployment experiences so that problems of respondent recall could be minimized. In addition to this supplemental interview it was also decided to supplement the CWBH data with respondents' ESARS records (where they existed) so that effects of ES-provided services

could be examined. Presumably ESARS records on such services are more accurately reported (especially with respect to timing and the nature of services received) than would be respondents' own reports on such matters.

Since interviewing for the study was to be conducted in late 1980 and early 1981, CWBH data were required for individuals beginning their benefit years during the period October 1979-March 1980. Only a few states had sufficient numbers of recipients in their CWBH files during that period to provide the sample sizes that were deemed necessary for the study. From among those states, Missouri and Pennsylvania were selected as most representative of the national population of UI recipients. Those states also had differences in their UI laws that offered some degree of variation in the study. Hence, it appeared to be feasible to examine some of the potential effects of such variations on workers' behavior. Because there was no specific policy interest favoring one state's results over the other's, it was decided to divide the sample equally between them.

Because a principal purpose of the study was to explore possible ways of supplementing the CWBH data, three different interviewing techniques were employed: (1) a telephone interview; (2) a detailed mail interview; and (3) an abbreviated mail interview. These three techniques were believed to span the range of alternatives that states might employ in supplementing their data. A considerable body of research suggested that we could expect both lower response rates and poorer quality data from our mail interviews than from our telephone interviews. Hence, we decided to choose a sample size for the telephone interview that was sufficiently large to permit all of the analysis to be done with that data set alone. Computations based on prior studies of unemployment experiences suggested

that a completed sample size of 2,000 telephone interviews would provide reasonably precise estimates of the responses that might be expected. It was also expected that the sample of exhaustees would be large enough for analysis purposes. This basic sample was then to be supplemented with a sample of 400 for each of our mail interviews. This relatively smaller sample size for the mail interviews was selected both because of the greater policy interest in the telephone interview data and because it was believed that such mail interview sample sizes were sufficiently large to permit detection of major differences in response quality among the interview types. In all then, the target sample size for the study totaled 2,800.^{1/} It was also decided to restrict the sample to UI recipients as opposed to claimants since the focus of the research questions was on this group. Finally, only individuals who had completed the initial CWBH interview were sampled so that individuals who completed our interview would have the basic demographic and economic data needed for analysis.

C. SUMMARY OF RESULTS

Our presentation of results of the study is divided into two additional chapters. In Chapter II we describe the basic analytical results of the study. Basic findings include:

- Initial completed unemployment spells averaged 13.6 weeks for respondents in the sample. That figure was lower than the mean value of total weeks of UI benefits received during the year (15.8) or of total weeks of unemployment (17.3). The 13.6 week figure exceeded average weeks of compensated unemployment in the initial spell by about 3

^{1/}Actual completed sample sizes departed somewhat from these targets. See Chapter III for a complete discussion of the outcomes of the interviewing process.

benefit year were in fact quite different on average. The measures were, however, highly correlated for individual recipients.

- At the time they were laid-off, nearly three-quarters of the respondents in our sample expected to be recalled to their jobs. For most of these workers their expectations were met. Both those individuals who were expecting recall and those who were actually recalled had much shorter unemployment spells (by all of our measures) than did other recipients. They also had significantly lower exhaustion rates for regular UI and higher subsequent wages than did other UI recipients without recall expectations.
- Results of analyzing data on initial unemployment spells were consistent with prior research. We found that each 10 percentage point increase in the net wage replacement ratio was associated with about one-third of a week of additional unemployment. Higher wage-replacement ratios were also associated with higher post-unemployment wages (after controlling for wages on the pre-UI job).
- Examination of UI records data on unemployment experiences (weeks of benefits, exhaustion rates, and so forth) did not yield results that were so consistent with prior research. The net wage-replacement variable, for example, was frequently insignificant by standard statistical tests. A possible reason for this result is that existing caps on weekly UI benefit amounts tend to impart a negative correlation between wage-replacement rates and weeks of UI benefits collected. That is, high wage workers tend to have both low wage-replacement rates and longer potential weeks of UI eligibility. Such a correlation tends to bias estimated wage-replacement effects toward zero. Since this problem does not occur in examinations of the (independently measured) initial unemployment spell variable, supplementation of the basic UI data in the CWBH system may be warranted for research purposes.
- For all equations that were estimated our ability to predict unemployment experiences on the basis of background data on respondents was quite low. Values of R^2 of less than .10 were common. Only recall expectation proved to be a significant predictor in all of the equations. Hence, other than the importance of knowing about recalls, the statistical analyses offered relatively little guidance to UI administrators about better ways to predict unemployment outcomes. In particular, there was no evidence that collecting supplemental data on the length of initial unemployment spells would improve the ability to predict UI outcomes such as total benefit payments or exhaustion rates.

- Determinants of ES usage corresponded to prior expectations. Specifically, low wage individuals were more likely to use the ES and those expecting recall were much less likely to do so. ES use tended to increase with the duration of unemployment so there was some evidence that individuals regarded it as a "backstop" job search technique.
- Individuals' decisions to use the ES later in their job searches imparted serious biases to simple ordinary least squares estimates of ES effectiveness. Taken at face value these estimates implied that ES use made individuals worse off by increasing their unemployment spells and reducing their subsequent wage rates. The significance of these biases clearly warned against any kind of simple attempt to use non-experimental cross-section data to judge ES effectiveness.
- Utilization of more sophisticated estimation procedures succeeded in reversing the biases involved in the ordinary least squares estimates of the effect of the ES. That is, as expected on prior grounds, ES use was found to reduce unemployment durations and increase subsequent wage rates. But the quantitative sizes of these estimates were not particularly robust to the alternative estimation techniques employed. The close interconnection between search strategies, recall expectations, and ES use proved to be very difficult to disentangle in our non-experimental data. Similar conclusions applied to the study of specific ES-provided services, notably job referrals.

A major component of our study involved survey methodology. Highlights of our findings regarding non-response included:

- Non-response occurred in this study both because all potential respondents (36 percent for telephone and mail interviews combined) did not complete an interview and because "completed" interviews sometimes contained missing information (24 percent of all initial attempts) that was considered important for the analysis of unemployment spells.^{1/} This non-response was large enough that study findings could be biased.

^{1/} Some of the non-response occurred because CWBH data were unavailable in the federal CWBH data bank for some of our sample. This problem would presumably not occur if the research were done by state UI research departments.

- The probability of response was positively related to demographic variables such as age and education and to other variables that were positively related to the likelihood of finding a job (e.g., expectation of recall). Blacks were also less likely to respond than whites.
- The determinants of non-response and of labor market outcomes were related in such a way that the study sample underrepresented long spells of unemployment; however, the size of this bias was small and study results did not need to be adjusted for non-response.
- No evidence was found that indicated that non-response biased our estimates of the size of the effects of various variables on labor market outcomes, such as the length of the initial unemployment spell.
- Comparisons between CWBH/ESARS and interview data suggested that the interview estimates were similar to the CWBH/ESARS data at the mean, but that there was considerable noise in the interview data (i.e., there were a number of positive and negative differences between the interviews and CWBH/ESARS data).

The mail and telephone interviews were also compared, and our findings concerning this comparison were:

- Non-response was significantly higher on the mail than the telephone interviews, the difference in response rates being 23 percentage points.
- This non-response difference resulted because of both a higher level of non-response on the mail than the telephone interview for the survey and because of missing data. Some constructed data items were missing from the mail interview for as much as 40 percent of the completion sample.
- Although non-response bias was small, overall, it was larger on the mail than the telephone interview because of the overall difference in response rate. Determinants of non-response did not differ between the two interview methods.
- Data quality on the interview types was generally similar although there was some evidence that it was slightly worse on the mail interviews. There was more noise in the data for those interviews.

- The telephone interview was more expensive to administer than the mail interview, but because of the lower mail response rate, the cost advantage of the mail interview was on the order of only 5 to 10 percent.
- These findings suggest that either telephone or mail interviews could be used successfully by future studies of this nature, with the choice of method depending on whether the additional accuracy and higher response rates of the telephone interview are thought to justify the additional cost. If the mail interview is chosen, our findings suggest that the detailed version be used rather than the abbreviated version.

II: ANALYZING UNEMPLOYMENT SPELLS

A. INTRODUCTION

In this chapter we will examine the unemployment experiences of the UI recipients in our sample. Besides describing these experiences in some detail, the chapter also develops a variety of models that seek to explain and predict unemployment outcomes. The chapter is divided into five additional sections. Section B is largely descriptive in nature. It provides a detailed picture of the types of outcomes that were experienced by individuals in our sample. Statistical models that seek to explain these outcomes are developed in Section C. Those models are quite similar to others that have appeared in the literature on unemployment insurance and the results reported are contrasted to those from the other studies. Next, in Section D, we examine the extent to which the individuals in our sample made use of the Employment Service (ES). The nature of services received by these individuals using the ES are also described. Section E then draws together the analysis of the two prior sections in an attempt to evaluate whether or not use of the ES improved the experiences of UI recipients relative to what would have happened without such services. A principal purpose of that section is to illustrate some of the methodological problems involved in making such an assessment. Finally, Section F summarizes our results and outlines some of the implications they have for future research.

B. DESCRIPTION OF UNEMPLOYMENT EXPERIENCES

In this section we provide a relatively detailed examination of the unemployment experiences of the UI recipients in our sample. A summary of

those experiences is provided in Table II.1. As for most of the analysis in this chapter, data in the table refer only to responses on our telephone interview in the belief that these data are of a higher and more consistent quality than is obtainable from our mail surveys (see Chapter III for a complete analysis of differences in the surveys). Overall 1457 telephone respondents had sufficiently completed data to permit inclusion in Table II.1.

The first three variables in Table II.1 report various measures of unemployment duration. The initial unemployment spell (IUS) measures the length of time (in weeks) between an individual's layoff date and the date at which he or she either becomes re-employed or leaves the labor force. This duration measure is the one most frequently used in theoretical analyses of unemployment since it reflects the length of the period of post-layoff search. In many data sets, particularly those based on UI administrative records or those (such as the Current Population Survey) in which unemployment spells are recorded in progress, this variable is not available. Its presence in our data will permit comparisons to analyses based on other types of data. The total weeks of unemployment (WKSUN) and weeks of UI benefits (WKSUI) variables provide other measures of unemployment experiences during the benefit year.^{1/} On the average, the

^{1/} Although we did not analyze it in detail, our survey also contained information on the initial spell of compensated unemployment--that is, the individual's initial period of UI collection. That variable averaged 10.5 weeks in our sample (with a standard deviation of 9 weeks). Its value differed from the initial unemployment spell data both because of lags in applying for UI and because, for recipients with longer spells, UI benefits were exhausted before the initial spell ended. Regression results for this variable were quite similar to those for total weeks of UI benefits and are not reported separately. In particular, it should be noted that the initial compensated spell data did not seem to be as well explained by the search-type models we used as were the initial unemployment spell data.

TABLE II.1

BASIC MEASURES OF RECIPIENTS' UNEMPLOYMENT
EXPERIENCES FOLLOWING INITIAL LAYOFF

Variable	Description	Mean	Standard Deviation
IUS	Initial Unemployment Spell, in weeks	13.6	14.2
WKSUN	Total Weeks Unemployed During Year	17.3	15.1
WKSUI	Total Weeks of UI Benefits Collected	15.8	12.3
EXTUI	Percent Exhausting Regular UI	24.9	—
EXTEB	Percent Exhausting EB	10.0	—
REEMPLOY	Percent Reemployed Following Unemployment Spell	91.0	—
WKLYWG	Weekly Wage of Those Reemployed (Dollars)	255.8	143.0
WKGPRE	Weekly Wage on Pre-UI Job for Those Reemployed (Dollars)	254.2	129.0
RWKLYWG	Weekly Wage of Those Reemployed (1979 Dollars)	232.5	130.0
RWKGPRE	Weekly Wage on Pre-UI Job for Those Reemployed (1979 Dollars)	239.1	120.8
RHRWG	Hourly Wage of Those Reemployed (1979 Dollars)	5.94	3.51
RHRWGPRE	Hourly Wage on Pre-UI Job for Those Reemployed (1979 Dollars)	5.93	3.65

SOURCE: Telephone interview only.

workers in our sample were unemployed for over 17 weeks during that period and collected nearly 16 weeks of UI benefits. Initial unemployment spells were shorter, averaging 13.6 weeks in duration. This figure fell short of the other two duration measures mainly because some unemployment was experienced after the initial spell ended. For example, some individuals lost their first post-unemployment job. Part of the difference between the unemployment spell and weeks of UI data may also be explained by the possibility that some recipients did not (in our survey) report being actively engaged in job search during some of the periods in which they reported receiving UI benefits.^{1/}

Twenty-five percent of the individuals in the sample exhausted their regular UI entitlement during the benefit year. Average exhaustion rates were lower in Pennsylvania than in Missouri (see Table II.5) reflecting the uniform UI duration provision of thirty weeks for the former state.^{2/} Exhaustion rates in the sample closely approximated those reported in UI Statistics during the period. Because both states in our sample were triggered on to regular extended benefits (EB) during most of the period under investigation, exhaustion of regular UI did not result in a cutoff of benefits. Rather, most exhaustees could begin collecting EB benefits as soon as they had drawn their complete UI entitlement. Only about 40 percent of regular UI exhaustees (10 percent of the entire sample) went on

^{1/} Following CPS procedures, however, individuals on temporary layoff were counted as being unemployed whether or not they engaged in active search efforts.

^{2/} This uniform duration has been changed since the study sample began receiving benefits. Eligible claimants in Pennsylvania now have potential UI durations of either 26 or 30 weeks, depending on the number of weeks worked in the base period.

to exhaust their EB entitlement as well. This EB exhaustion rate was lower than the 60-70 percent rate found during the 1975 recession (see Corson and Nicholson, 1982). The lower figure reported here may be explained by the relatively stronger 1980 labor market, by a possible truncation of our EB exhaustion rate by the interview date at the end of the benefit year, or by the possibility that some recipients may have underreported their total UI collections including EB.^{1/}

Most (91 percent) of the individuals in our sample became re-employed at some point during the year following their initial layoffs. Of those who did not find work, most were older workers who left the labor force. Relatively few respondents (about 2 percent) continued to be unemployed at our interview date. Weekly wages for re-employed individuals were, on average, virtually identical to wages those workers had earned on their pre-UI jobs. In nominal terms average wages rose slightly whereas in real terms they fell slightly. Real hourly wages were also relatively little changed. Of course, as for the other figures in Table II.1 there is substantial variation in the average wage figures, so a more detailed investigation (to be presented below) may reveal patterns obscured by aggregation.

A brief summary of the demographic characteristics of the individuals in our sample is provided in Table II.2. Because there is substantial policy interest in recipients who exhaust their UI entitlements, Table II.2 also illustrates separate summary characteristics of

^{1/} Comparison of reported data to administrative data in the CWBH system did not reveal any such disparity for Missouri, however. Absence of such data for Pennsylvania (see Chapter I), precluded a detailed investigation.

TABLE II.2

SELECTED CHARACTERISTICS OF UI CLAIMANTS, BY EXHAUSTEE STATUS

Characteristic	Regular UI		Extended Benefits ^{a/}	
	Exhaustee	Non-Exhaustee	Exhaustee	Non-Exhaustee
		Total		Total
Percent Female	31.5	30.1	30.8	30.4
Percent Black	8.0	3.5	9.6	4.0
Mean Age	36.3	36.5	38.3	36.3
Percent Expecting Recall	61.9	78.8	62.3	76.0
Mean Months on Pre-UI Job	46.6	59.2	48.3	56.8
Real Hourly Wage on Pre-UI Job	5.76	5.89	5.98	5.85
Percent Ever Reemployed	75.1	95.7	65.1	93.4
Percent Reemployed at Same Job	35.1	65.7	31.5	61.0
Weeks Unemployed After Exhaustion				
0-4	46.1	n.a.	40.8	n.a.
5-8	16.0	n.a.	14.6	n.a.
9-12	9.8	n.a.	13.6	n.a.
More than 12	28.1	n.a.	31.1	n.a.
Total	100.0	n.a.	100.0	n.a.
Median	4.9	n.a.	7.0	n.a.
Sample Size	365	1098	146	1318
		1464		1464

NOTE: Percent Exhausting Regular UI: 24.9

Percent Exhausting EB: 10.0

^{a/} All individuals in the sample are included in this table. Non-exhaustees include individuals who began collecting EB and did not exhaust and individuals in the sample who did not begin collecting EB.

regular UI and EB exhaustees compared to all other individuals in the sample. In terms of general characteristics, Table II.2 shows that our sample was predominantly (70 percent) male and that recipients had worked substantial periods (an average of 4.5 years) on their pre-UI jobs. The small fraction of blacks in the sample (4.6 percent) was explained partly by a somewhat lower rate of UI eligibility for blacks than for whites and by the fact that a substantial amount of racial identity data were missing from the Pennsylvania CWBH data. In order to conserve sample sizes, those with such missing data were defined to be "white." Finally, three-quarters of all the individuals in our sample expected to be recalled to their pre-UI jobs and a high fraction ultimately did return to that job. All of these general characteristics of our sample will be investigated in considerably more detail in the next section.

In many respects both the UI and EB exhaustees in our sample closely resembled non-exhaustees. The age and sex composition of the two groups were virtually identical. Blacks were more heavily represented among exhaustees than in the total sample, however. Exhaustees were also less likely to expect to be recalled to their pre-UI jobs and less likely ultimately to find work. About 40-50 percent of both UI and EB exhaustees were reemployed within four weeks of exhaustion. Median weeks unemployed following exhaustion were 4.9 for regular UI exhaustees and 7.0 for EB exhaustees. Those figures were somewhat below durations reported in other studies of exhaustion, perhaps because of the truncation of the data by the end of the benefit year.

Table II.3 illustrates the distribution of various measures of UI recipients' unemployment durations. In general, there is considerable

TABLE II.3

DISTRIBUTION OF INITIAL UNEMPLOYMENT
DURATIONS BY STATE
(Percent Distribution)

Number of Weeks	Missouri			Pennsylvania		
	Initial Spell	Total Weeks Unemployed	Total Weeks of UI	Initial Spell	Total Weeks Unemployed	Total Weeks of UI
5 weeks or less	27.1	17.1	21.6	41.3	29.3	24.9
6-10	18.6	16.7	20.7	21.7	19.8	19.4
11-15	18.5	16.6	17.1	12.6	11.7	12.3
16-20	11.2	13.1	13.3	6.1	10.1	10.4
21-30	10.6	16.3	15.2	8.4	12.0	14.1
31-40	5.2	7.9	8.9	3.3	7.4	14.2
41-52	6.1	8.3	1.9	5.2	8.2	4.1
over 52	2.7	3.4	—	1.3	1.6	—
Total	100.0	100.0	100.0	100.0	100.0	100.0
Number in Sample	774	771	774	690	686	690

SOURCE: Telephone interview only.

bunching of the data around relatively short durations. That is particularly true in the Pennsylvania sample where more than 40 percent of all respondents had initial unemployment spells of 5 weeks or less. As we show in the next table, a principal factor influencing the incidence of short durations in the sample was workers' being recalled to their former jobs. Practically all of the very short durations resulted from such recalls. In other respects the distributions of unemployment durations were relatively uniform with few recipients experiencing extremely long (say, more than one year) periods of unemployment.^{1/}

The importance of recalls in influencing unemployment durations is clearly illustrated in Table II.4. At the time of the CWBH interview recipients were asked whether they expected to be recalled to their jobs. In all three-quarters of our sample had such expectations. Of course those expectations were not always realized--about 27 percent of those expecting recall did not in fact return to their jobs. And a few workers who did not expect to be recalled actually were. But in all, recall experiences played a major role in determining unemployment outcomes. For example, initial unemployment spells were less than half as long for individuals who expected to be and actually were recalled as they were for individuals who neither expected to be nor were actually recalled. Similar differences occurred for most of the other unemployment duration measures. Individuals who expected to be recalled but were not seem to

^{1/}The fact that a few respondents reported receiving more than 39 weeks of UI benefits during the year may indicate weeks of partial benefits. The collection of TRA benefits also contributed to this finding since these benefits were reported as UI by some recipients.

TABLE II.4

ALTERNATIVE MEASURE OF UNEMPLOYMENT
DURATION BY STATE AND NATURE OF LAYOFF
(Mean Number of Weeks)

Duration Measure	Expecting Recall		Not Expecting Recall	
	Returned to Job	Did Not Return	Returned to Job	Did Not Return
MISSOURI SAMPLE				
Initial Unemployment Spell	9.7	19.7	15.1	22.4
Total Weeks Unemployed	14.0	22.6	18.3	25.2
Total Weeks of UI Benefits	11.8	17.4	15.5	19.1
Sample Size	375	189	18	192
PENNSYLVANIA SAMPLE				
Initial Unemployment Spell	7.9	20.3	10.0	16.9
Total Weeks Unemployed	12.4	23.5	13.8	20.0
Total Weeks of UI Benefits	13.8	23.3	15.5	19.9
Sample Size	422	106	35	127

SOURCE: Telephone survey only.

have fared relatively poorly. In Missouri initial unemployment spells for this group were nearly as long as for those not expecting recall and in Pennsylvania they were four weeks longer. Hence, failure of recall expectations seemed to be a relatively important cause for long unemployment durations.

Similar conclusions about the incidence of recalls are suggested by the data on other unemployment outcomes reported in Table II.5. Those whose expectations of recall were met (about 54 percent of the entire sample) were significantly less likely to exhaust either their regular UI entitlements or all benefits provided under UI and EB together than were claimants in most other categories.

Even when the sample is limited only to individuals who ultimately found jobs, recall experiences continued to exert major influence on unemployment outcomes. For example, the unemployment durations data reported in Table II.6 show that initial unemployment spells were only about 60-70 percent as long for recipients who expected to be and were recalled as for other recipients.

A final descriptive table that illustrates the importance of recalls in our sample (Table II.7) provides information on real wage change experienced by re-employed workers. Relatively few of those recipients recalled to their jobs suffered significant wage losses. Indeed, the median recalled worker experienced a slight gain. For those workers not recalled the picture was quite different. More than 40 percent of workers who were not recalled experienced losses of greater than 5 percent in real weekly wages with significant numbers of workers having losses of more than 25 percent. Of course, some job changers ended up doing rather well: in

TABLE II.5

OTHER OUTCOMES FOLLOWING LAYOFF BY
STATE AND NATURE OF LAYOFF

Duration Measure	<u>Expecting Recall</u>		<u>Not Expecting Recall</u>		Total
	Returned to Job	Did Not Return	Returned to Job	Did Not Return	
MISSOURI SAMPLE					
Percent Reemployed	100.0	73.3	100.0	79.2	89.0
Percent Exhausting UI	14.9	34.9	27.8	43.7	27.3
Percent Exhausting EB	5.9	10.0	5.6	16.7	9.6
Sample Size	375	189	18	192	774
PENNSYLVANIA SAMPLE					
Percent Reemployed	100.0	75.5	100.0	78.7	91.7
Percent Exhausting UI	14.2	41.5	20.0	33.9	22.3
Percent Exhausting EB	5.2	26.4	2.9	16.5	10.4
Sample Size	422	106	35	127	690

SOURCE: Telephone interview only.

TABLE II.6

ALTERNATIVE MEASURES OF UNEMPLOYMENT DURATION FOR REEMPLOYED INDIVIDUALS
BY STATE AND NATURE OF LAYOFF
(Mean Number of Weeks)

Duration Measure	Expecting Recall		Not Expecting Recall		Total
	Returned to Job	Did Not Return	Returned to Job	Did Not Return	
MISSOURI SAMPLE					
Initial Unemployment Spell	9.7	15.1	15.5	16.9	12.5
Total Weeks Unemployed	13.9	18.7	18.3	20.7	16.5
Total Weeks of UI Benefits	11.7	16.1	15.5	16.9	13.8
Sample Size	373	146	18	151	688
PENNSYLVANIA SAMPLE					
Initial Unemployment Spell	7.9	15.2	10.0	13.5	9.8
Total Weeks Unemployed	12.5	19.5	13.8	17.4	14.2
Total Weeks of UI Benefits	13.8	20.6	15.5	17.7	15.4
Sample Size	418	80	35	100	633

SOURCE: Telephone interview only.

TABLE II.7

DISTRIBUTION OF CHANGES IN REAL WEEKLY WAGES FOR ALL
REEMPLOYED INDIVIDUALS BY STATE AND NATURE OF LAYOFF
(Percent Distribution)

Percent Change in Real Weekly Wage	<u>Expecting Recall</u>		<u>Not Expecting Recall</u>		Total
	Returned to Job	Did Not Return	Returned to Job	Did Not Return	
MISSOURI SAMPLE					
Loss of 25% or More	3.4	28.8	0.0	19.9	12.4
Loss of 5-25%	14.2	23.3	38.9	25.9	19.3
No Change (-5-+5%)	37.5	18.5	44.4	13.9	28.5
Gain of 5-25%	32.2	13.0	11.1	17.9	24.4
Gain of More Than 25%	12.6	16.4	5.6	22.5	15.4
Total	100.0	100.0	100.0	100.0	100.0
Sample Size	373	146	18	151	688
PENNSYLVANIA SAMPLE					
Loss of 25% or More	3.4	26.3	0.0	26.0	9.6
Loss of 5-25%	12.0	16.3	17.1	18.0	13.7
No Change (-5-+5%)	49.3	26.3	45.7	14.0	40.6
Gain of 5-25%	27.5	15.0	25.7	17.0	24.2
Gain of More Than 25%	7.9	16.3	11.4	25.0	11.8
Total	100.0	100.0	100.0	100.0	100.0
Sample Size	418	88	35	100	633

both Missouri and Pennsylvania about one-fourth of all claimants who changed jobs had gains of 25 percent or more. But the majority of job changers were not so fortunate since they ended up with losses in real earnings.

C. BASIC REGRESSION RESULTS

In this section we present our basic regression results for the outcomes of unemployment spells experienced by individuals in our sample. The presentation covers three general topics. First, we describe the basic approach we will take in the analysis and the nature of the sample data used. Then we examine the general regression results for that total sample and contrast them to the results of other studies of UI recipients. Finally, we investigate estimated responses in various subsets of the data in an effort to illuminate further basic influences on workers' experiences.

1. Analytical Model and Data Description

The basic analytical model we employed for our initial results was a very simple one. Unemployment outcomes (duration, exhaustion rates, subsequent wage rates, and so forth) were assumed to be linearly related to various exogenous influences by the equation:

$$Y = \sum_{i=1}^K \beta_i X_i + U \quad (1)$$

where Y is the outcome of interest, the X_i include both demographic variables (age, sex, and so forth) and economic variables (such as location, date, and various parameters of the UI system) believed to affect

Y, and U is a random disturbance. In Section E we will present results of somewhat more sophisticated efforts at modeling unemployment outcomes and a few other modeling approaches will be described very briefly later in this section. But for the most part we will adhere to a simple regression approach to the analysis in the belief that such an approach is most helpful in providing a broad overview of the data.

Two criteria, analytical tractability and data quality, guided our selection of a basic sample for analysis. With respect to the first of these, we wished to analyze a sample of individuals for whom it was reasonable to assume that a single structural equation explained their behavior. For this reason, we decided to focus only on those individuals in the entire sample who had become re-employed by the interview data. In this way, we could study both unemployment durations and subsequent wage rates for a single sample, and we could avoid analytical problems raised by the need to model participation decisions if labor market dropouts had been included in the analysis. Focusing only on individuals who ultimately found a job also had some benefits for data quality since problems in differentiating between unemployment and labor market withdrawal were mitigated. In any event, since 91 percent of the individuals in our initial sample ultimately found jobs, the decision to examine only the re-employed subset did not have a major effect on most of our results: they were virtually identical for the larger, more inclusive sample.

In order to utilize the best quality data for our investigations, two decisions were made. First, only data from the telephone interview were used since we generally believed that those data were less subject to measurement error (see Chapter III). Second, for inclusion in our sample

individuals were required to have data on all of the variables used in the analysis. Although it might have been possible to increase sample sizes slightly by requiring that data be available only for the particular regression being run, we believed that these gains were not worth the loss in comparability that such sample selection criteria would have entailed.

Application of our two selection criteria resulted in a total sample of 1,269 individuals. Variables used in the analysis of this sample are described in Table II.8 with means and standard deviations of these variables being shown in the first two columns of the table. Only a few of these measures warrant specific mention. First, the decision to focus only on re-employed recipients reduced all of the average unemployment duration measures by about two weeks (compare Tables II.1 and II.7). Standard deviations of those variables were also reduced appreciably as were exhaustion rates for both regular UI and EB. Qualitatively, however, the unemployment data in Table II.8 closely resembled the data from our entire sample.

Among the independent variables in Table II.8, three deserve specific discussion. First, the net wage replacement ratio (NWRR) was defined as the ratio of UI benefits to after-tax weekly wages and was calculated from administrative records on UI benefits, interview data on weekly wages and a program that imputed state and Federal income taxes. Overall, the mean value reported for NWRR of 0.57 was quite close to that reported in other studies. Second, the small representation of black workers (4 percent) as mentioned previously, resulted both from lower UI eligibility for such workers and some missing data in the Pennsylvania sample. Finally, the data in Table II.8 indicated that more than three-

TABLE II.8

DESCRIPTION OF BASIC DATA USED FOR ANALYSIS

Variable Name	Description	Total Sample		Active Searcher		Job Changer	
		Mean	S.D.	Mean	S.D.	Mean	S.D.
IUS	Initial Unemployment Spell (weeks)	11.3	10.8	14.5	11.2	15.5	13.0
WKSUN	Total Weeks Unemployed	15.4	12.7	18.8	12.8	19.0	13.9
WKSUI	Total Weeks of UI Benefit	14.5	11.6	16.3	11.2	17.3	11.9
EXTUI	=1 if Exhaust Regular UI	0.20	—	0.26	—	0.30	—
EXTIEB	=1 if Exhaust EB	0.07	—	0.08	—	0.10	—
REWRG	Real Hourly Wage on Post- UI Job	5.92	2.88	5.71	2.97	5.25	2.81
NWRR	Net Wage Replacement Ratio	0.57	0.22	0.56	0.23	0.57	0.24
STATE	=1 if Pennsylvania =0 if Missouri	0.48	—	0.30	—	0.38	—
PJN80	=1 if Layoff Prior to January 1980	0.44	—	0.54	—	0.51	—
AJN80	=1 if Layoff Prior to January 1980	0.26	—	0.16	—	0.20	—
AGE	Age in Years	36.2	13.2	34.0	12.5	33.1	12.0
SEX	=1 if Female	0.30	—	0.28	—	0.29	—
ED	Education in Years	11.5	1.9	11.7	1.9	12.0	1.8
BLACK	=1 if Black	0.04	—	0.05	—	0.05	—
SPOUSE	=1 if Working Spouse in Household	0.42	—	0.40	—	0.41	—
HESIZE	Household Size, Excluding Respondent	2.17	1.52	2.17	1.55	2.14	1.51
REWGPFE	Real Hourly Wage on Pre-UI Job	5.90	2.82	5.87	2.99	5.58	2.81

Table II.8 (continued)

Variable Name	Description	Total Sample		Active Searcher		Job Changer	
		Mean	S.D.	Mean	S.D.	Mean	S.D.
XPRCL	=1 if Respondent Expects to be Recalled	0.77	—	0.67	—	0.48	—
PD	Potential Duration, Including EB Extensions (weeks)	36.7	5.2	35.6	6.2	35.7	6.2
ESARS	=1 if has ESARS record	0.40	—	0.57	—	0.66	—
REFER	=1 if Received Job Referral	0.13	—	0.22	—	0.29	—
Number in Sample		1269		718		456	

quarters of the individuals in our sample expected to be recalled to their prior jobs at the time they responded to the CWBH questionnaire. Although, as we describe in the next paragraph, these expectations were not always realized, the high incidence of short-term layoffs followed by recalls in our sample provided a thread that runs through most of our analysis in the remainder of this report.

Two subsets of our data that were widely used in analysis are also described in Table II.8. The first of these we refer to as the "active searcher" sample. It consisted of those individuals who responded on our interview that they did look for work upon being laid-off. Omitted from the sample are those who did not search either because they had left the labor force or, more commonly, because they were awaiting recall. Our second subsample consisted of "job changers"--that is, individuals who did not return to their pre-UI jobs following their initial unemployment spells. Although such individuals constituted only about 36 percent of our entire sample, they are of particular interest since they represent a group for whom standard job search theory may be most directly applicable.^{1/}

In general, characteristics of the two subsamples described in Table II.8 did not differ appreciably from the total sample. For both subsamples mean unemployment durations were longer than for the total sample, primarily because of lower expectations of recall. Individuals in both subsamples were disproportionately located in Missouri and that fact

^{1/} Still, nearly half (48 percent) of the job changers in our sample expected to be recalled at the time they were laid-off. Because the group of job changers who did not initially expect recall was quite small, we will not analyze it separately in this section. A few results related to that subsample will, however, be discussed in Section E.

also probably reflected the industrial structure and related high recall probabilities in the Pennsylvania sample. Individuals in both subsamples were more likely to make use of the Employment Service (as indicated by the presence of our ESARS record) and to receive a job referral from that agency than were individuals in the total sample. Again, as we show in the next two sections, that finding also derives from the recall phenomenon since those expecting recall were much less likely to make use of the ES. Other than these recall-related differences, the mean characteristics of the three samples outlined in Table II.8 were virtually identical.

2. Results for the Total Sample

Table II.9 reports the results of using the total sample for regressions of the six unemployment outcome measures defined in Table II.8 on the exogenous variables in that table.^{1/} Although the regressions' overall fits were not particularly good, a number of coefficients were statistically significant and some of these might be explicitly highlighted. First, expectation of recall had a significant effect on all of the outcomes examined in the table. Other things being equal, those expecting recall had mean initial unemployment spells and total weeks unemployed that were more than four weeks less than for those not expecting recall. They also collected about 3.5 weeks less in UI benefits, were 13 percent less likely to exhaust their regular benefits entitlement, and were

^{1/}The potential duration variable (PD) was initially included in our regression runs, but its coefficient was never significant and often of the wrong sign. Because of these results and because Pennsylvania's uniform duration policies made effects of the PD variable difficult to interpret, in any case, it was not included in most of our analysis. Results for the ESARS and REFER variables listed in Table II.7 will be discussed in detail in Section E.

TABLE II.9

BASIC REGRESSIONS ON UNEMPLOYMENT OUTCOMES: TOTAL SAMPLE

Independent Variable	IUS		MKSIN		MKSUI		EXUII		EXUIB		HIRMJ	
	COEF.	SE										
CONSTANT	16.0300	2.9600	17.8800	3.5400	14.6500	3.2100	0.3366	0.1116	0.0461	0.0706	-0.2440	0.4634
NWRR	3.3950*	1.6340	2.3020	1.9540	0.0360	1.7750	-0.0870	0.0617	0.0037	0.0390	0.6695*	0.2561
STATE	-1.2130	0.7720	-0.7409	0.9233	3.3480*	0.8390	-0.0036	0.0291	0.0165	0.0184	-0.3031*	0.1210
PJNB0	1.7050*	0.7580	2.2430*	0.9070	2.5030*	0.8240	0.0333	0.0286	0.0102	0.0181	-0.4665*	0.1189
AJNB0	-1.3050	0.8500	-0.0050	1.0160	-0.1003	0.9231	0.0301	0.0321	0.0003	0.0203	-0.1033	0.1332
AGE	-0.0320	0.0254	-0.0405	0.0304	0.0084	0.0276	-0.0002	0.0010	0.0007	0.0006	0.0104*	0.0040
SEX	-0.1864	0.7499	0.1630	0.8969	-1.0510	0.8150	0.1412*	0.0283	0.0115	0.0179	-0.4612*	0.1176
ED	-0.1653	0.1686	-0.1429	0.2017	-0.1030	0.1832	0.0001	0.0064	-0.0004	0.0040	0.0817*	0.0264
BLACK	2.3830	1.5830	4.3890*	1.8930	5.1070*	1.7190	0.1716*	0.0597	0.1510*	0.0378	0.3173	0.2481
SPWOK	0.2481	0.6121	-0.2974	0.7321	-0.8411	0.6650	-0.0345	0.0231	-0.0237	0.0146	-0.0973	0.0950
HHSIZE	-0.3047	0.1949	-0.1858	0.2331	-0.4468*	0.2117	-0.0102	0.0074	-0.0107*	0.0047	-0.0108	0.0306
HMGPRE	0.1521	0.1404	0.3725*	0.1679	0.3748*	0.1525	0.0039	0.0053	0.0068*	0.0034	0.8063*	0.0220
XPRCL	-4.8720*	0.7280	-4.1210*	0.8710	-3.4570*	0.7910	-0.1255*	0.0275	-0.0396*	0.0174	0.3703*	0.1141
Std. Error	10.48		12.54		11.39		0.39		0.25		1.64	
R ²	0.07		0.04		0.05		0.03		0.03		0.70	
Degrees of Freedom	1256		1256		1256		1256		1256		1256	

* Coefficient significantly different from zero at .05 level on a one tail test.

4 percent less likely to exhaust EB. Upon re-employment real hourly wage rates were \$.37 higher for those expecting recall than for those not expecting recall (after controlling for prior wage rates). For all of these findings it should be emphasized that the XPRCL variable was measured at the time of layoff. It did not measure actual recalls, but merely expectations at the start of the unemployment period. The relatively favorable experiences of those expecting recall simply indicated that those expectations were, for the most part, met.

Because much UI research has focused on the net wage replacement ratio as a measure of the impact of UI benefits on job search, coefficients for that variable in Table II.9 are of particular interest. For the regression on initial unemployment spells the coefficient of NWRR had the expected positive sign and was significantly different from zero. The value of that coefficient implied that a 10 percent increase in NWRR was associated with about one-third of an extra week of unemployment. Although that figure was slightly below the figure usually summarized in the UI literature (Hamermesh [1977] puts the consensus estimate at about half an extra weeks of unemployment for such an increase in NWRR) it was well within the range of estimates that have been reported.

In all of the regressions on UI-related outcomes (i.e., those on weeks of benefits and on exhaustion rates), the NWRR variable was not statistically significant. An explanation for this finding is that definitional relationships between UI entitlements and weekly benefit amount computations in state laws may obscure the behavioral impact of UI wage replacement. For example, high wage workers tend to have low values for NWRR because of state caps on UI benefit amounts. They also, in

variable duration states (such as Missouri), tend to have longer potential UI durations. Hence, other things being equal, there will tend to be a negative relationship between NWRR and potential duration and that may result in a negative correlation between NWRR and weeks of UI collected. This correlation may therefore obscure the positive relationship between NWRR and time unemployed and result in an insignificant coefficient for NWRR. Hence, our results indicated that supplemental data on the lengths of the initial unemployment spell may be superior to CWBH data on weeks of UI collection for illuminating basic behavioral relationships in the job search process.

Finally, the significant influence of the NWRR variable in the real wage equation should be mentioned. Traditional job search theory postulates that the prolonged search activity brought about by higher net replacement ratios should lead to better job matches and higher subsequent wage rates. Empirical support for this proposition has been inconclusive, however, and actual estimates have varied widely in their predicted quantitative impact. (For a brief summary see Nicholson, 1981). Our results in Table II.9 suggest that each 10 percent increase in NWRR was associated with an increase of about \$.07 (1.1 percent) in real hourly wages on post-UI job. That figure, while below the estimate obtained by Ehrenberg and Oaxaca (1976) for some of the subgroups they studied, was consistent with the smaller increase in unemployment durations that we estimated for our NWRR variable. Because the pre-UI wage was included in the regression in Table II.9 the coefficient of the NWRR variable was not subject to the same sorts of upward biases that were present in the Ehrenberg-Oaxaca analysis and in some of the research on the Trade

Adjustment Assistance Program.^{1/} That fact, combined with the relative robustness of the NWRR coefficient in other subsample regressions on the post-UI wage, indicated that our estimate of wage replacement effects on subsequent wage rates may reasonably reflect the true job search process.

Although most of the other coefficients reported in Table II.9 had signs that were consistent with prior expectations, few were statistically significant.^{2/} Hence, the ability to predict the length of unemployment spells for specific individuals on the basis of the type of analysis presented here would appear to be rather limited. The low values for the R^2 's in the regressions clearly showed that most of the observed variations in spell lengths remained to be explained. Further, other than the reasonably obvious point that individuals who expected to be recalled fared better, the regression in Table II.9 offered little guidance on how adjustment services might be most effectively targeted to specific workers so as to improve unemployment outcomes.

^{1/} For a discussion, see Corson and Nicholson, (1981).

^{2/} Only the coefficient of the STATE dummy variable might be specifically mentioned. In all of the unemployment duration equations that variable was statistically significant only for the WKSUI regression. Recipients in Pennsylvania collected benefits, on average, 3.3 weeks longer than those in Missouri. That undoubtedly resulted from the uniform duration provisions that existed in Pennsylvania UI law at the time. Whether the result is simply definitional (that is, that Pennsylvania recipients collected more weeks of benefits because they were eligible for more) or behavioral (that is, that Pennsylvania recipients exhibited a disincentive-type response to their lengthy uniform duration of benefits) was difficult to determine within our data set. Addition of the potential duration variable to the regression did not affect the significance of the STATE variable, so its significance may be related to uniform duration per se.

3. Subsample Regressions

Basic regression results for the active searcher and job changer subsamples are reported in Table II.10 and II.11, respectively. Several of the conclusions from the whole sample estimates carried over into the subsample regressions. For example, the NWRR variable continued to have about the same coefficients in the initial unemployment spell and real wage equations as it did in the entire sample. The coefficients were not, however, statistically significant in the job changer subsample--possibly because of its rather small sample size. As before, the STATE dummy variable continued to be significant only in the duration equation representing weeks of UI benefits. This again illustrated how specific state UI provisions (here Pennsylvania's uniform duration policy) may affect duration measures based on UI activities but may have no significant influence on independently measured duration statistics.

A major difference between the subsample and total regressions was in the size and significance of the expect recall variable. In the active searcher subsample the coefficient on XPRCL was always smaller in absolute value than it was in the total sample and, contrary to the previous case, in some equations the coefficient was not significantly different from zero. Hence, it appeared that those who actively searched for work, even though they expected recall, were not so certain of those expectations. That uncertainty proved, in some cases, to be justified since the workers' experiences were not so favorable as were those of workers who expected recall and chose not to search.

For job changers, expectation of recall had no significant effect on any of the unemployment outcomes examined. We had anticipated from the

TABLE II.10

BASIC REGRESSION ON UNEMPLOYMENT OUTCOMES: ACTIVE SEARCHERS

Independent Variable	IUS		MNSM		MNSU		FNUU		EXUB		RHMG	
	COEF.	SE										
CONSTANT	16.3300	4.0500	21.5900	4.6600	15.8000	4.0600	0.4439	0.1600	0.0513	0.0993	-0.4304	0.6665
WRR	4.1090*	2.2980	-0.0780	2.6430	-1.2260	2.3010	-0.1948*	0.0908	-0.0380	0.0564	0.9373*	0.3781
STATE	-0.7220	1.1330	1.1170	1.3030	5.2280*	1.1340	0.0626	0.0447	0.0406	0.0278	-0.4893*	0.1864
RJNB0	1.2390	0.9880	1.8530*	1.1360	1.8950*	0.9890	0.0332	0.0390	0.0033	0.0242	-0.5422*	0.1625
AJNB0	2.2280*	1.4070	2.5670	1.6170	0.7540	1.4080	0.0608	0.0555	-0.0251	0.0345	-0.0485	0.2314
AGE	0.0231	0.0372	0.0261	0.0427	0.0592	0.0372	0.0011	0.0015	0.0006	0.0009	0.0103*	0.0061
SEX	1.6970	1.0460	2.4790*	1.2030	0.3380	1.0470	0.0787*	0.0413	0.0313	0.0257	-0.4586*	0.1721
ED	-0.3486	0.2365	-0.4248	0.2720	-0.1653	0.2368	-0.0055	0.0093	0.0029	0.0058	0.0929*	0.0389
BLACK	3.4690*	1.9830	5.6640*	2.2810	2.9500	1.9660	0.1755*	0.0783	0.1495*	0.0486	0.4375	0.3263
SPWOK	1.2060	0.8590	0.1440	0.9880	-0.6635	0.8603	-0.0326	0.0339	-0.0092	0.0210	-0.0603	0.1414
HHSIZE	-0.5791*	0.2683	-0.4779	0.3085	-0.5525*	0.2686	-0.0153	0.0106	-0.0147*	0.0066	0.0056	0.0441
RHMPRE	0.1462	0.1886	0.1993	0.2169	0.0747	0.1888	-0.0068	0.0074	0.0028	0.0046	0.7691*	0.0310
XPRCL	-3.5800*	0.9310	-2.3230*	1.0700	-1.2470	0.9320	-0.0618*	0.0368	-0.0158	0.0228	0.3487*	0.1531
Std. Error	11.00		12.62		10.99		0.433		0.269		1.81	
R ²	0.06		0.04		0.05		0.04		0.03		0.64	
Degrees of Freedom	705		705		705		705		705		705	

* Coefficient significantly different from zero at .05 level on a one tail test.

TABLE II.11

BASIC REGRESSION ON UNEMPLOYMENT OUTCOMES: JOB CHANGERS

Independent Variable	IUS		MWSIN		MWSII		EXUI		EXUEB		HUNG	
	COEF.	SE	COEF.	SE	COEF.	SE	COEF.	SE	COEF.	SE	COEF.	SE
CONSTANT	20.3800	5.9600	28.0900	6.4000	16.900	5.3700	0.5534	0.2098	0.2183	0.1394	-0.2294	0.9922
WRR	3.8390	3.1750	0.2210	3.4070	-0.3640	2.8600	-0.1227	0.1117	-0.0262	0.0742	0.5947	0.5282
STATE	-2.2340	1.6160	-0.4030	1.7340	4.2240*	1.4560	-0.0092	0.0568	0.0290	0.0378	-0.1447	0.2689
P JIN80	1.3910	1.5060	2.7010*	1.6160	3.2950*	1.3570	0.0545	0.0530	-0.0065	0.0352	-0.6066*	0.2506
A JIN80	1.9110	1.9540	1.9230	2.0960	1.4070	1.7600	0.1049	0.0687	0.0070	0.0457	-0.4055	0.3250
AGE	-0.0040	0.0561	0.0146	0.0602	0.1131*	0.0505	0.0019	0.0020	0.0011	0.0013	0.0216*	0.0093
SEX	1.1910	1.5440	1.5210	1.6570	0.4280	1.3910	0.0716	0.0543	-0.0071	0.0361	-0.7694*	0.2569
ED	-0.5410	0.3594	-0.7250*	0.3857	-0.3736	0.3238	0.0176	0.0126	-0.0053	0.0084	0.1518*	0.0598
BLACK	1.3710	2.8650	2.0390	3.0750	3.6850	2.5810	0.1908*	0.1008	0.1395*	0.0669	0.6048	0.4767
SPWOK	1.9440	1.2830	0.3610	1.3770	-1.3280	1.1550	0.0637	0.0451	-0.0456	0.0300	-0.0643	0.2134
HHSIZE	-0.6775*	0.4049	-0.8200*	0.4345	-1.1050*	0.3650	0.0289*	0.0142	-0.0267*	0.0095	-0.0184	0.0674
HNGHRE	-0.0330	0.2920	-0.2325	0.3133	0.0642	0.2630	0.0010	0.0103	0.0003	0.0068	0.5864*	0.0486
XPRCL	-0.5720	1.3000	-0.4340	1.3950	0.0670	1.1710	-0.0245	0.0457	-0.0245	0.0304	0.0757	0.2162
Std. Error	12.96		13.91		11.67		0.46		0.30		2.16	
R ²	0.04		0.03		0.07		0.04		0.04		0.43	
Degrees of Freedom	443		443		443		443		443		443	

* Coefficient significantly different from zero at .05 level on a one tail test.

raw data that expectations of recall might have been detrimental to those not eventually recalled since they may have delayed active entry into the job search process. For recipients who ultimately found other jobs, that did not seem to be the case, however.

The generally poor fits observed in the total sample regressions continued to persist in the subsample regressions, even though the data were presumably more homogeneous. R^2 's of much less than .10 were common in all except the real wage equations. Even in the real wage equations R^2 's were lower in the subsamples, especially for the job changers. This decline in multiple correlation probably resulted from a diminished explanatory power for the pre-UI wage in the subsamples since these were relatively more dominated by job changers.

Overall then, the subsample regressions served to strengthen our general conclusions about the effects of wage replacement and recall expectations on the length of unemployment spells. But the disaggregation did not noticeably aid in our ability to predict the length of specific individuals' unemployment spells.

D. USE OF THE EMPLOYMENT SERVICE

All state UI laws require claimants to be able and available for work and to accept suitable jobs. Additionally, many states require the claimant to search actively for work. These "work test" laws are applied by requiring claimants to register with the state Employment Service (Job Service) although recipients with definite recall dates or who normally secure work through a union hiring hall are generally not referred to the ES. These individuals are not expected to need ES services, and the provision of ES services to them would be an inefficient use of scarce

resources. Claimants who do register with the ES are expected to accept job referrals and suitable job offers. If they do not, UI administrators are informed, and they determine if the claimant has violated the UI law. The ES, in addition to monitoring UI claimants job search behavior for the UI work test, also tries to find work for its clients by matching available jobs with the clients' job skills. Other services such as counseling and testing may also be provided. These ES activities are intended to place claimants in a suitable job. This activity should reduce unemployment spell lengths and increase wages on subsequent jobs. To investigate the impact of the ES on UI claimants we examine, in this section of our report, data on the use of the ES and the determinants of ES use. In Section E we then attempt to measure the impact of the ES on unemployment spell length and post-unemployment wages.

Data are reported in Table II.12 by state on ES use among our sample. These data come from ESARS records which were matched with our CWBH UI recipient sample.^{1/} An examination of these data indicates that less than half of the sample (40 percent) used the ES as measured by the presence of an ESARS record. Moreover, this number was significantly lower in Pennsylvania than Missouri. One reason many individuals did not register with the ES is that much of our sample expected to be recalled. The importance of this expectation on ES use is discussed below. In addition, individuals who had ES records did not all receive job related services. In both states, approximately 9 percent of this group received

^{1/}The FY80 ESARS records were matched with our sample of recipients all of whom began collecting UI in the first six months of FY80. Thus, ES use that was delayed until FY81 is not counted in our estimates although this should be quite insignificant.

TABLE II.12

USE OF THE EMPLOYMENT SERVICE BY STATE

	Missouri	Pennsylvania	Total
Percent Using the Employment Service	49.5%	29.5%	40.0%
Percent of ES Users Receiving Counseling and Testing	8.5	9.1	8.7
Percent of ES Users Receiving Job Referrals			
One Referral	20.2	18.2	19.5
More Than One Referral	13.1	11.8	12.6
Total	33.3	30.0	32.1
Percent of ES Users Receiving Job Placements	16.1	15.0	15.7
Mean Weeks from Layoff to First ES Contact	6.2	8.1	6.9
Sample Size	691	635	1326

counseling and testing, 32 percent job referrals, and 16 percent job placements. Thus, since some individuals received more than one of these services, many individuals who registered with the ES did not receive any specific job related services. The ESARS records for these individuals generally indicated only that they were UI claimants and no specific record of job search assistance was reported. These individuals might still, however, have utilized job opening lists in the local ES office since such use would probably not show up in the ESARS file.

The final data item in Table II.12 indicates that the date of the first ES contact occurred about 7 weeks after layoff. This provides evidence that UI recipients may have often viewed the ES as a secondary source of jobs, and that they initially utilized other job search methods when they were laid-off. The relationship between unemployment spell length and the use of the ES is discussed further in the next section. Data not reported in Table II.12 also indicated that job referrals and placements occurred at an even later date. For those that received these services the date of the first transaction occurred 14 weeks after layoff, on average. The variance around this mean was, however, quite substantial and a sizeable fraction of recipients (approximately a quarter) did receive services within 5 weeks after layoff.

While the above data indicate that relatively few UI recipients had contact with the ES, and, of these, relatively few received services, ES services might still have been targetted on those most in need, and these services might have been beneficial to the recipients. Before investigating these hypotheses in the next section, we first examine which UI recipients utilized the ES.

An examination of the determinants of ES use is reported in Table II.13 where the effect on the probability of using the ES is reported for our basic list of independent variables.^{1/} Several interesting points can be made by examining these results. First, the STATE dummy variable is significant even though we have controlled for a number of other influences. The probability of using the ES is approximately 23 percentage points less in Pennsylvania than Missouri, a finding which roughly corresponds to that found in the raw data. Why this should be so is, however, not clear, and this finding probably indicates that there is substantial variance among states in the degree to which UI recipients are referred to the ES.

Second, the race variable indicated that blacks were substantially more likely to use the ES than whites; this may be an indication of the types of jobs these two groups are able to find. Third, pre-UI hourly wages were negatively correlated with ES use. This finding conforms to the view that jobs listed with the ES tend to be relatively low paying jobs, and thus, higher wage individuals would not utilize the ES.

Finally, the expect recall variable was significant as we anticipated, and its effect was quite substantial. Individuals expecting recall were over 40 percentage points less likely to use the ES than those not expecting recall. This occurs both because of individual behavior and because UI administrators generally do not refer recipients with a definite recall date to the ES.

^{1/} These effects were estimated using the probit technique.

TABLE II.13

DETERMINANTS OF USE OF THE EMPLOYMENT SERVICE
(Coefficients Estimated by Probit)

Independent Variable	Effect Evaluated at Maximum	Standard Error
NWRR	0.032	0.088
STATE	-0.225*	0.041
PJNSO	-0.001	0.040
AJNSO	-0.049	0.046
AGE	-0.007*	0.001
SEX	0.041	0.040
ED	0.039*	0.009
BLACK	0.235*	0.085
SEWORK	0.008	0.033
HESIZE	-0.008	0.010
REMGPRE	-0.028*	0.008
XPRCL	-0.428*	0.039
Latent R^2		0.381

NOTE: The dependent variable equalled 1 when the respondent used the Employment Service (ES) and 0 otherwise. The mean of this dependent variable was .4. The effect of each independent variable was evaluated at the maximum which occurs when the probability of using the ES equals .5.

*Effect significantly different from zero at .05 level on a one-tailed test.

E. EFFECTS OF THE EMPLOYMENT SERVICE

In this section we present the results of our attempts to model the effects that receipt of services from the Employment Service had on individuals' unemployment experiences. Because those results are both complicated and anomalous, we begin by describing a conceptual model of how the ES is incorporated into individuals' job search activities. That model clearly illustrates why the connection between the ES usage and job search outcomes is so difficult to model from non-experimental data.

The basic problem with modeling effects of the ES is that the decision to use it is an integral part of the search process. Hence, data on ES usage and unemployment outcomes will exhibit a necessary simultaneity: unemployment outcomes will be effected by ES activities, but the decision to use the ES will also be affected by individuals' views of their own job prospects. More specifically, some authors (e.g., Katz, 1978) have suggested that the ES may be regarded by many individuals as a "backstop" search method that is only used when more customary methods (checking with friends, relatives, or directly with employers) fail to yield results. Under this view then the ES will be used rather late in those individuals' job searches and the correlation between ES usage and unemployment duration will be positive. A similar argument can be made with respect to wage rates. Since the ES is known to offer relatively low wage employment opportunities (Camil Associates, 1977) individuals may choose to use it only after they have reduced the minimum wage rate that they are willing to accept. Hence, the (partial) correlation between ES usage and subsequent real wages should be negative.

Inadequate attention to developing statistical procedures that are appropriate to the sorts of joint decisions involved in the job search

process can produce seriously biased estimates. Indeed, as we will show below, ordinary least squares estimation can lead to the conclusion that use of the ES is an unmitigated disaster--increasing unemployment durations and reducing real wage rates. A variety of solutions have been proposed for this problem. Most of these are based, either explicitly or implicitly, on attempting to model the decision to use the ES. That is (as in the previous section) it is hypothesized that:

$$ES = \sum_{i=1}^K \beta_i X_i + U_i \quad (2)$$

where ES is a binary variable indicating ES usage (or receipt of specific ES services) and the X_i are variables thought to affect that usage. In this model then equations (1) (see page 23) and (2) should be estimated simultaneously since ES would be expected to appear as one of the determinants of the unemployment outcomes (Y) in equation (1). Possible estimation techniques include the usual simultaneous equations methods (two and three stage least squares) and related instrumental variables techniques that use predicted values from equation (2) in place of the original ES variable in estimating equation (1). Because the ES variable is binary, the first stage in the instrumental variable procedure might involve techniques other than ordinary least squares (such as probit or logit). This suggestion then leads into a large set of estimation techniques derived from maximum likelihood methods (see Heckman, 1979 and Barnow, Cain, and Goldberger, 1980).

A problem common to all of the procedures that might be employed to estimate equations (1) and (2) consistently is that of identification.

This problem is most obvious in the simultaneous linear equations context. In that case, identification requires the existence of some exogenous influences on individuals' decisions to use the ES that do not influence their unemployment outcomes. But deciding what such variables should be on the basis of theory is a hazardous process. Ultimately, the question of whether or not the variables selected truly do not enter equation (1) is an empirical one since the underlying theory of job search is not developed precisely enough to provide firm a priori guidance. Making the choice of identifying restrictions on empirical grounds is also no simple matter since the number of possible permutations of variables is practically infinite. And, unfortunately, as we demonstrate below, the choices actually made may significantly affect the estimates obtained.

Identification when equation (2) is estimated by a technique such as probit is made somewhat simpler by the non-linearities involved in those techniques. It is theoretically feasible to include exactly the same set of X_1 's in equation (2) as appear in equation (1) and still obtain an estimate for the coefficient of the predicted value of the ES variable. Still, the ES equation may be close enough to the linear case or the procedure used to estimate that equation may encounter other problems so that the identification problem may reappear. We describe some instances of this later in this section.

Our approach to all of these econometric difficulties was to follow the simple research strategy of employing a variety of estimation techniques and report all of them so that the reader may be in a position to judge the meaning of the results. Although the results are subject to a variety of interpretations, our general conclusion is that, given the

currently available statistical tools, it is very difficult to provide a reliable estimate of the "true" effect of the ES from non-experimental data. To reach that conclusion we first focus on initial unemployment spells in our total sample. We then expand the discussion by considering estimates for additional outcomes of interest and for subsamples of the data.

1. Initial Unemployment Spells in the Total Sample

Three different ways of estimating the effect of ES use on the duration of unemployment for individuals in our sample are reported in Table II.14. The first estimation procedure simply added the binary variable representing ES usage (ESARS^{1/}) to the basic regression previously reported in Table II.9. This approach clearly illustrated the biases involved in such a procedure. Taken at face value the coefficient of ESARS implied that those who used the ES were unemployed 4.6 weeks longer than otherwise similar UI recipients who did not. Obviously, as our theory suggested, the simultaneity present in the job search process severely biased the ESARS coefficient in a positive direction.

Most of the other coefficients in the initial unemployment spell regression were not substantially changed by inclusion of the ESARS

^{1/} The results reported in this section primarily used presence of an ESARS record as our measure of ES usage in the belief that this was the most accurately measured of the available variables. Estimates were also made using respondents' own reports of ES usage from our interview and using a measure of ES usage that attempted to adjust for existence of compulsory ES registration requirements. Since the qualitative and quantitative sizes of the estimates obtained with these alternative measures were quite similar to those obtained for the ESARS variable, the other estimates are not reported. Some attempts at estimating the impact of specific ES services will be described below.

TABLE II.14

EFFECT OF EMPLOYMENT SERVICE ON INITIAL UNEMPLOYMENT SPELLS
 UNDER ALTERNATIVE ESTIMATION PROCEDURES: TOTAL SAMPLE
 (Dependent Variable IUS)

Independent Variables	Estimation Procedure ^{1/}					
	OLS		2SLS		2-Stage Probit	
	COEF.	SE	COEF.	SE	COEF.	SE
CONSTANT	12.58*	2.95	-2.87	5.95	24.63*	5.03
ESARS	4.60*	0.69	25.22*	6.12	-24.44	11.56
NWRR	3.27*	1.61	2.71	2.10	5.21*	1.84
STATE	-0.40	0.76	3.26*	1.47	-5.98*	2.38
PJNBO	1.69*	0.75	1.61	0.97	2.05*	0.77
AJNBO	-1.17	0.84	-0.58	1.10	-1.54*	0.86
AGE	-0.01	0.02	0.11*	0.05	-0.18*	0.76
SEX	-0.31	0.74	-0.83	0.97	0.77	0.87
ED	-0.30*	0.17	-0.88*	0.27	0.82	0.49
BLACK	1.58	1.56	-2.04	2.29	7.59*	2.93
SPOUSE	0.22	0.60	0.10	0.79	0.47	0.62
HHSIZE	-0.27	0.19	-0.13	0.25	-0.43*	0.20
RHWGPFE	0.25*	0.14	0.71*	0.22	-0.49	0.34
XPRCL	-3.21*	0.76	4.22*	2.40	-5.03*	0.73
Standard Error	10.30		10.31		10.47	
R ²	0.10		0.09		0.07	
Degrees of Freedom	1255		1255		1255	

^{1/} See text for detailed description of estimation procedures used.

*Coefficient significantly different from zero at .05 level on a one-tailed test.

variable. The most significant change was an increase in the XPRCL coefficient from -4.87 to -3.21. That change provided a further reflection of the close connection between ES use and recall expectation reported in the previous section. Because of that close connection, the coefficient of ESARS effectively reflected, in part, an absence of recall expectations.

Problems in identifying the relationship between expectations of recall and absence of ES use are even more dramatic in the two stage least squares (TSLS) estimates in Table II.14. In that equation the ESARS coefficient increased dramatically as did the coefficient of the XPRCL variable. Since XPRCL was (by far) the most significant variable in the first stage equation predicting ESARS, it was not surprising that the estimation procedure employed had great difficulty in differentiating between the direct effect of recall expectations and its indirect effect through the predicted value of ESARS. Although not reported in the text, results quite similar to the two stage estimates were obtained when the ESARS variable was predicted by the probit procedure: again, the strong influence of the XPRCL variable on ES use resulted in multicollinearity between the two variables.^{1/}

In order to address these identification issues, two general approaches were employed: (1) estimation of alternative models to predict ES use; and (2) estimation over alternative subsamples. The second of these approaches will be discussed at the end of the section; here we will describe the first. Experimentation with a number of possible ways for

^{1/} Similar results were also obtained when the procedure suggested by Heckman (1976) dealing with selectivity bias was employed in this equation.

predicting the ESARS variable led to the general conclusion that it was necessary to eliminate XPRCL from the list of explanatory variables in order to yield results that differed in any major ways from the Two-Stage Least Squares estimates. That is, expectations of recall appeared to be too much an endogenous part of the search process to be included as a predictor of ESARS. Ultimately, we therefore settled on the strategy of using all of the exogenous variables in Table II.14 except XPRCL to predict ES use. Those predictions were then used in place of the actual ESARS variable in the equations. Although predicted values of ESARS were developed using both the logit and probit procedures, results for these two methods were virtually indistinguishable so only the probit results will be reported. We refer to these as "Two Stage Probit" estimates.

In some respects the two stage probit estimates reported in Table II.14 represented a significant improvement. More of the variables were statistically significant than in either our initial estimate (Table II.9) or in the equations estimated by other simultaneous techniques. The coefficient for XPRCL returned to near its previous value (-4.9) and the net wage replacement coefficient came even closer to its consensus value. Unfortunately, although the coefficient of ESARS did as expected change sign, its value became an implausibly large, statistically significant^{1/}

^{1/} Statistical significance for the probit predictions in Tables II.14-II.17 is judged by the t-statistics reported from the second stage OLS regressions. Although those t-statistics are not, strictly speaking, correct (because the equations' standard errors are computed using the predicted rather than the actual values for ESARS), alternative correct standard errors were estimated in a few cases and they did not differ appreciably from those obtained from the ordinary least squares program. Hence, in the interest of convenience, we chose to report those approximate standard errors.

negative number. That coefficient implied that individuals who used the ES had initial unemployment spells that were 2.4 weeks shorter than were those of otherwise identical individuals who did not use the ES. Such beneficial impacts were, of course, as dubious as were the harmful impacts described earlier. Some part of the seemingly large value for the ESARS coefficient can be rationalized by noting that the predicted values for that variable never reached the 0 or 1 bounds--hence, to extrapolate to that range results in unreliable predictions. Still, the results reported in Table II.14 implied that a (say) 10 percent increase in the probability of ES use would reduce mean initial unemployment spells by 2.4 weeks and that value too seemed implausibly large. About all that can be said is that the two stage probit procedure did succeed in reversing the bias in the OLS estimates of the ESARS effect, but that the problems of identifiability and multicollinearity continued to prevent development of a precise estimate. Such results tended to permeate all of our other estimates as well as we now show.

2. Estimates for Other Outcomes

In the top half of Table II.15 we report the coefficient for the ESARS variable in regressions on the six unemployment outcomes we have been examining throughout this chapter. Each regression also contained the other independent variables used in our other estimates and each was estimated by both OLS and the two stage probit procedure. For the OLS estimates, simultaneity bias was clearly evident in all of the equations: the coefficient of ESARS was positive and significant in the duration and exhaustion equations used and, as expected, negative and significant in the real wage equation. Use of the two stage probit procedure succeeded in

TABLE II.15

EFFECT OF EMPLOYMENT SERVICE AND OF JOB REFERRALS ON
VARIOUS MEASURES OF UNEMPLOYMENT OUTCOMES: TOTAL SAMPLE

ES Measure and Estimation Procedure ^{1/}	Unemployment Measure					
	IUS	WKSIN	WKSII	EXTII	EXTIB	RHRMG
ESARS						
OLS						
Coef.	4.60*	6.92*	5.57*	0.13*	0.04*	-0.50*
SE	0.69	0.81	0.74	0.03	0.01	0.11
2-Stage Probit						
Coef.	-24.44*	-24.25*	-22.04*	-0.15	-0.16	0.25
SE	11.56	13.84	12.57	0.43	0.28	1.82
REFER						
OLS						
Coef.	3.75*	4.17*	3.17*	0.05	0.05*	-0.56*
SE	0.95	1.13	1.03	0.03	0.02	0.15
2-Stage Probit						
Coef.	-11.82	-16.20	-33.55*	-0.63	-0.43	2.60
SE	11.61	13.88	12.58	0.43	0.27	1.82
Degrees of Freedom	1255	1255	1255	1255	1255	1255

^{1/}All regressions also contained the other independent variables listed in Table II.14. See text for description of estimation procedures.

*Coefficient significantly different from zero at .05 level on a one-tailed test.

reversing all of those "perverse" signs, but the resulting point estimates remained problematical. For all of the duration outcomes these point estimates were implausibly large and statistically significant. For the exhaustion rates and real wages the coefficients were not so improbable though they were not significantly different from zero. Other than the conclusion that use of the ES is probably not harmful to its users, it would be hazardous to attempt to draw any conclusion about its true effect from such estimates.

The lower half of Table II.15 reports the results of examining one particular service provided by the ES--job referral.^{1/} To investigate that service we used individuals' ESARS records to determine whether or not they had received a job referral from the ES. A binary variable (REFER) representing the existence of such a referral was then used with our two estimating procedures.

As for the case of the ESARS variable, the OLS estimates for the REFER variable seem to imply that obtaining a job referral from the ES is disastrous for UI recipients: unemployment durations and exhaustion rates were higher and real wages lower for such recipients than for those who did not get referrals. Such results, of course, again reflected the simultaneity observed in the previous results for ES use. That is, individuals who decided to go to the ES and succeeded in getting a job referral there were clearly those individuals who had relatively poor job search prospects. Use

^{1/}We also briefly examined both other services (e.g., counseling and testing) and the timing of ES-provided services. Complexities involved in using the ESARS data together with the econometric problems discussed in this section made it difficult to interpret results obtained in this examination.

of the two stage probit procedure again succeeded in reversing all of these perverse signs. But, other than the conclusion that job referrals were probably helpful, the resulting coefficients were simply too erratic to permit definitive statements about the qualitative effects of those referrals.

3. Subsample Results

In Table II.16 we report the results of estimating the effects of ES use and job referrals in three subsamples of the data: (1) active searchers; (2) job changers; and (3) workers not expecting to be recalled. In all of these cases both ESARS and REFER were estimated to have a positive effect on unemployment durations when estimated by ordinary least squares and a negative effect when estimated by the two stage probit procedure.^{1/} Again also all of the negative effects were implausibly large reflecting the identification problems that have been described throughout this section. Progressively limiting the sample to those for whom the traditional job search model seemed most appropriate did not seem to overcome these problems. Similar conclusions were suggested by our results for other unemployment outcomes although these results are not reported here.

Hence, our conclusions about the ability to measure accurately effects of the ES with the types of non-experimental data used in this report were essentially negative. Although the special estimation procedures employed did succeed in eliminating the obvious biases involved in ordinary least squares, problems in identifying the exact structural

^{1/} It should be pointed out that separate probit estimates were made for each sample as the first stage of the two stage estimation process. It would have been inappropriate to use the probit predictions based on the total sample in the subsample regressions.

TABLE II.16

EFFECT OF EMPLOYMENT SERVICE ON LENGTH OF
INITIAL UNEMPLOYMENT SPELL IN FOUR SAMPLES

ES Measure and Estimation Procedure	Sample			
	Total Sample	Active Searchers	Job Changers	Not Expect Recall
ESARS				
CLS				
Coef.	4.60*	2.35*	4.07*	6.97*
SE	0.69	0.95	1.48	1.82
2-Stage Probit				
Coef.	-24.44*	-52.50*	-46.66*	-32.35*
SE	11.56	17.97	21.06	17.33
REFER				
CLS				
Coef.	3.75*	1.49	2.36*	3.49*
SE	0.96	1.04	1.41	1.62
2-Stage Probit				
Coef.	-11.82	-16.43	-96.45*	-36.44
SE	11.61	22.61	43.05	37.72
Degrees of Freedom	1255	704	442	275

*Coefficient significantly different from zero at .05 level on a one-tailed test.

determinants of ES use prevented our obtaining precise and reasonable estimates of ES effectiveness. Improvements in that state of affairs must await the development of better econometric methodologies or the establishment of data bases in which the identification problems are less severe.

F. CONCLUSION AND SUMMARY

In this chapter we showed how data from the CWBH system can be combined with supplementary interview data on UI recipients' experiences to analyze a variety of policy questions. We showed that for some descriptive purposes the CWBH data are quite adequate. Data on weeks of UI collection or exhaustion rates are accurately reported in the CWBH and these data and their values among various subgroups may be of substantial interest to policymakers. The value of these UI data for research purposes is limited, however, by absence of information on completed unemployment spells and by definitional relationships between administrative data and other UI parameters such as potential duration and the weekly benefit amount. Hence, for purposes of behavioral research on the job search process, supplementary data on full unemployment spells may be required. By providing information on subsequent wage rates such data also provide a more complete picture of the search process than is provided by CWBH data.

Although the value of supplementary data for unemployment research seems clear, their value for purposes of UI administration is open to question. Such supplemental data do not seem to aid substantially in predicting weeks of UI collection or UI exhaustion rates, although they provide a more complete picture of recipients' activities while out of work. Our results did suggest that recall expectations had a major impact

on UI outcomes, but that fact is well known by UI administrators and they regularly obtain information on such expectations in any case. The other significant determinants of UI experiences that we identified are also currently available through CWBH.

Whether supplementary data would help to target ES and other services to UI recipients is also open to question. Our results again illustrated the fact that those expecting recall are less in need of such services than are other recipients. But, difficulties in developing a believable model of effectiveness from our non-experimental data made it difficult to offer any detailed guidance on how that effectiveness might be improved. Of course we did not investigate other interviewing strategies (such as targeting supplemental interviews on workers expected to have significant reemployment problems) nor did we examine the possibilities for setting up an experiment with ES-provided services. Those questions might warrant future research priority.

III: EVALUATION OF NON-RESPONSE AND INTERVIEWING METHOD

A. INTRODUCTION

The labor market behavior of unemployment insurance recipients was analyzed in this study with a data set constructed from both program records and interviews with recipients. More specifically, the Continuous Wage and Benefit History (CWBH) system was used to provide a random sample of UI recipients. For this sample the CWBH data system provided information on UI activity collected from program records and on recipient characteristics (e.g., age, sex, race, education) collected from an interview administered at the time of the initial UI claim. ESARS data on recipient activity with the State Employment Service were also added to the analysis file. These data were then supplemented through an interview conducted at the end of the benefit year.^{1/} This interview provided data on labor market activity during the benefit year that were not available from the CWBH system. For example, these data permitted us to construct a measure of the initial completed unemployment spell and of post-unemployment wages so that we could analyze the determinants of unemployment spell lengths and of post-unemployment wages. This follow-up interview was conducted both by telephone and by mail to determine which method would be better for subsequent replication of the study by state or other users.

In using these data for analysis it is important to ask what effect interview non-response, question item non-response, or any other missing

^{1/} Copies of these interviews are contained in Appendix.

data have on the analysis results, and whether these effects differ by mode of data collection. (i.e., telephone or mail). These effects can be of two types. First, the sample with complete data may not be representative of UI recipients, in general, and estimates made for the sample may thus be unrepresentative of the experiences of UI recipients. For example, if women are more likely to respond to the interview than men and if they have longer unemployment spells than men, the sample's experience will overstate the length of unemployment spells of UI recipients, in general. This type of nonresponse effect can, however, be easily addressed by weighting the results to take account of nonresponse. All that is necessary is to determine which factors are both important determinants of nonresponse and of the outcome of interest (e.g., length of unemployment spell in our example) and to construct weights that make the sample look like the true population. ^{1/}

The second possible effect of nonresponse presents a more serious problem. This effect, which may occur if nonresponders differ from responders in a systematic but unobserved way that is correlated with important outcomes and with the probability of response, will be to bias our estimates of the effect of various variables on the outcomes of interest. Biased estimates can also occur if response is a function of the outcome of interest. For example, in the last chapter, we expressed

^{1/} It should be noted that factors that affect nonresponse but do not affect outcomes of interest do not need to be addressed. For example, if women and men had similar unemployment experiences, it would not be necessary to weight the results to take account of differential men's and women's response rates, if we wanted to estimate unemployment durations.

the length of the initial unemployment spell as a function of the demographic characteristics of recipients, their pre-layoff employment experiences and other variables such as the parameters of their UI entitlement. If the length of the unemployment spell is reduced by an unobserved variable such as motivation and motivation is positively correlated with response to the interview, our estimates of the effect of recipient, employment and UI characteristics on unemployment may be biased downward. That is, we may understate the effects of these variables.

This chapter examines these potential nonresponse effects for the analysis results presented in this report.^{1/} A comparison of the differential nonresponse effects of the telephone and mail interview methods and of the quality of the data by interview type is also presented to help determine which interview method is better for subsequent use. The chapter is divided into five additional sections. Section B discusses the extent of nonresponse in the study. Section C then discusses the determinants of nonresponse. The next section, Section D, discusses effects of nonresponse on our interpretation of the results of our analysis of unemployment spells. Section E then examines the quality of the data in the completed interviews. A final section, Section F, summarizes the results and assesses the relative usefulness of the telephone and mail interviewing methods.

^{1/}The effect of nonresponse to the initial CWBH interview was not examined since data on nonresponders to that interview were unavailable.

B. THE EXTENT OF NON-RESPONSE

Data on the extent of nonresponse to the CWBH add-on interview conducted at the end of recipients' benefit years are discussed in this section and reported by type of interview. As was stated above, this interview was used to supplement the basic CWBH data set, and two interviewing methods (telephone and mail) were used to determine which method was more cost-effective. Furthermore, two different mail interviews were used: (1) a "detailed mail" interview collected all the information on the telephone interview, and (2) an "abbreviated mail" interview collected less detailed, more aggregate data. In particular, the abbreviated mail interview collected specific information on only the first unemployment spell and first job after the initial layoff while the larger mail and telephone interviews collected data on as many as four unemployment spells and jobs. Instead of this detail on subsequent jobs the abbreviated mail interview collected data for the entire year's labor force activity through a set of general questions concerning weeks worked, weeks unemployed, and other measures of labor force activity.

The overall results of these surveys are reported in Table III.1. As was anticipated the telephone interview had a significantly higher percentage of completions (68 percent) when compared to either mail interview (56-60 percent).^{1/} Furthermore, completion rates for the two mail interviews were quite similar, being slightly higher for the detailed mail interview. While one might have anticipated a higher response rate

^{1/} It should be noted that the interviews were done by MPR rather than the state UI agencies. Replication of the survey by UI agencies might yield higher response rates because respondents might view response as related to current or future UI claims.

TABLE III.1

RESPONSE RATES BY TYPE OF INTERVIEW

	Telephone	Detailed Mail	Abbreviated Mail
Responded to Survey			
Complete Data	49.0%	25.3%	26.7%
Incomplete Data			
CWBH Data Missing	4.4	4.0	3.8
Key Interview	14.6	30.6	25.9
Data Items Missing			
Total	68.0	59.9	56.4
Did Not Respond to Survey			
Refused	6.0	--	--
Not Located	21.4	--	--
Other ^{a/}	4.6	40.1	43.4
Total	32.0	40.1	43.4
Total Initial Sample	2989	866	866

^{a/} For mail interviews all interviews that were not returned are classified in the "other" category.

for the abbreviated rather than the detailed mail interview, the two interviews were, in practice, quite similar in length because most individuals had only one spell of unemployment and one subsequent job. In fact, in these cases the "abbreviated" mail interview was slightly longer than the "detailed" mail interview because of the separate questions on the entire year's labor market activity.

As in all studies of this nature, return of the mail interviews or completion of a telephone interview did not mean that all interviews could be used in the analysis since some interviews were missing one or more data items that were considered to be key items for the analysis.^{1/} As is reported in Table III.1, 15 percent of the telephone interviews and 25 to 30 percent of the mail interviews were missing one or more "key data" items. The lower missing data rate for the telephone as opposed to mail interviews resulted from the fact that (1) the telephone interviewer insured that the respondent was asked the desired set of questions (i.e., the skip pattern was followed correctly), (2) the respondent was explicitly asked and encouraged to answer all questions, and (3) follow-up calls to the respondent were used if key items were found missing during the quality control process. For the mail interviews follow-up calls for missing data were not made since it was thought such contacts would not be a part of a

^{1/}The definition of key items used for this analysis included a number of complicated constructed variables. It is thus more stringent than necessary for some analyses.

typical mail survey. A further set of interviews (approximately 4 percent) was not useable because CWBH data were not available.^{1/}

The net result of these various reasons for "nonresponse" was that 49 percent of the initial telephone sample yielded data that were complete enough for the principal analyses. For the mail interviews the comparable number was significantly lower, 25 to 27 percent.^{2/} These numbers, however, do not indicate the entire extent of nonresponse since the initial sample included only individuals who had responded to the claim date CWBH interview. This was done to insure that we would start with a sample with relatively complete CWBH data. Response rates on this initial interview range, for the two states we used, from 65 to 70 percent in Pennsylvania and 70 to 75 percent in Missouri.^{3/} Consequently, nonresponse is potentially a serious problem for analysis of these data and we turn, in the next section, to an examination of the causes of nonresponse.

Before proceeding with this examination, however, more detailed data are presented in Table III.2 on item nonresponse. That is, we present

^{1/} The CWBH sample was drawn from state records, but the data were provided through the federal CWBH data bank. Thus, these individuals appeared on state CWBH files but not federal files. They were from the Pennsylvania sample.

^{2/} The completion rate reported for most studies does not take account of the effect on the analysis sample of data that are missing from completed interviews. Two studies for which we can do this are the follow-up study of FSB recipients in which data were collected by telephone (see Brewster et al., 1978) and a study of disqualification provisions of state UI laws in which data were collected by mail (see Felder, 1979). For the first study, 48 percent of the initial sample was both interviewed and had complete data for the regression analysis of labor market outcomes and for the second study the comparable number was 33 percent. Hence, this study is quite comparable to other similar studies.

^{3/} See Richard Strouse (1980) for a discussion of state-by-state response rates.

TABLE III.2

ITEM NON-RESPONSE BY INTERVIEW TYPE FOR SELECTED
 QUESTIONS AND CONSTRUCTED VARIABLES
 (Percent of Data Items Missing for Completed Interviews)

	Interview Type		
	Telephone	Detailed Mail	Abbreviated Mail
Pre-Layoff Employment			
Industry	0.3	2.7	3.2
Occupation	0.0	1.4	1.2
Weekly Wages	2.4	5.6	4.5
Layoff Date	0.6	13.3	11.7
Post-Layoff Job Search Experience			
Did Respondent Search for Work	0.0	2.3	4.9
Number of Weeks Searching	1.9	18.1	16.6
Did Respondent Use ES	0.0	3.3	5.3
Did ES Provide Job Referral	0.2	6.9	9.6
Why Didn't Search for Work	0.0	2.7	5.7
Initial Unemployment Spell Length	1.1	35.5	33.0
UI Experience			
Weeks Collected in First Spell	3.7	16.7	12.3
Weekly Benefit Amount	2.5	7.7	10.6
Post-Layoff Employment			
First Job			
Weekly Wages	2.3	14.3	16.8
Is Job With Pre-UI Employer	0.0	6.2	9.2
Start Date of Job	0.7	20.0	20.3
Entire Period			
Weeks Between Layoff and Interview	0.6	16.6	15.4
Weeks Employed	1.3	29.9	15.2
Weeks Unemployed	1.8	40.3	18.6
Weeks Out of Labor Force	1.9	42.2	29.5
Sample Size	2032	519	488

information on missing data for selected interview questions and for variables constructed from those questions. These data indicate that item nonresponse was generally low for the telephone interview with the largest item nonresponse occurring for income variables (e.g., wages on pre-UI and post-UI jobs) and for variables describing the UI experience (e.g., weeks collected and the weekly benefit amount). For none of these variables did item nonresponse exceed 3.7 percent of all completed interviews. Nevertheless, the overall result (see Table III.1) indicated that 21 percent of all completed telephone interviews had one or more key data items missing,^{1/} although this occurs in part because of item nonresponse in the initial CWBH data set.

For the mail interviews, item nonresponse was substantially higher, particularly for variables that used dates of events in their construction (e.g., the initial unemployment spell length, weeks employed, weeks unemployed). Without interviewer probing on these questions it may have been difficult to obtain a firm answer. For these interviews the overall missing data figures are not substantially higher than the individual data item results, suggesting that individuals who missed or did not answer one set of questions also did the same thing for other sets of questions.

C. DETERMINANTS OF NON-RESPONSE

In this section we present data on the determinants of non-response using a model that explains response as a function of interview type and variables that describe the characteristics of the potential respondents. The dependent variable for this model is a binary variable that takes the

^{1/}This was 14.6 percent of all interview attempts.

value 1 if complete data were available for analysis and the value 0 otherwise. This variable applies our quite stringent definition of non-response and includes both non-response that arose through non-response to the entire survey and non-response that arose because of missing data in the completed interviews. Since the independent variables used in the analysis came from CWBH data, the analysis was restricted to individuals with CWBH data. The four percent of the sample without CWBH data was dropped from this analysis.

The means and standard deviations of the independent variables used in this analysis are reported in Table III.3. Since most of these variables were also used in the analysis of unemployment spells (see Chapter II), we will not comment in detail on them here except for the variable denoting interview type (M).^{1/} This variable (M) is a dummy variable that equals 1 for a mail interview and 0 for a telephone interview, and thus it treats the abbreviated and detailed versions of the mail interview as having the same effect on non-response. Furthermore, interview type is modeled as affecting the response rate intercept but not its slope relative to the other variables in the model (e.g., age, sex, race, etc.). The decisions to combine the two mail interview types and to

^{1/} It is also interesting to note that the mean for the STATE is .44 while the completion sample is split equally between the two states. This occurred because the overall response rate was higher in Pennsylvania than Missouri, but as the data in Table III.4 show this is explained by characteristics of the state sample. In addition, PJN80 and AJN80 are defined for the nonresponse analysis using the UI benefit year begin date and for the unemployment spells analysis of Chapter II using the layoff date. This explains why the mean of PJN80 is lower for the non-response analysis than for the unemployment spells analysis.

TABLE III.3

INDEPENDENT VARIABLES USED IN NON-RESPONSE ANALYSIS

Variable	Description	Mean	Standard Deviation
M	=1 if mail interview =0 if telephone interview	0.37	---
GWRR	Gross Wage Replacement Ratio	0.48	0.23
STATE	=1 if Pennsylvania =0 if Missouri	0.44	---
PJN80	=1 if benefit year begin date prior to January 1980	0.33	---
AJN80	=1 if benefit year begin date after January 1980	0.26	---
AGE	Age, in years	36.54	13.81
SEX	=1 if female	0.32	---
ED	Education, in years	11.16	2.22
BLACK	=1 if Black	0.07	---
SPOUSE	=1 if working spouse in household	0.38	---
HHSIZE	Household size, excluding respondent	2.06	1.60
XPRCL	=1 if expect recall to pre- UI job	0.72	0.45

SOURCE: CWBH data file for telephone and mail interview attempts.

Sample Size: 4226

model interview type solely with a dummy variable were tested statistically and the tests supported these decisions.^{1/}

The results of estimation of this model are reported in Table III.4. The model was estimated using the probit technique because of the binary nature of the dependent variable, and we have reported the estimated effects of the independent variables evaluated at the sample means. This has been done so that the estimates can be interpreted as measuring the effect of each independent variable on the probability of response. Examination of the results shows, as the unadjusted data indicated (see Table III.1), that use of the mail interview lowered the response rate relative to the telephone survey by 27 percentage points, and this result was highly statistically significant. Consequently, non-response would potentially be a more serious problem if mail interviews were used in place of telephone interviews.

A number of other variables also had significant effects on response. Demographic variables such as age and ethnicity had significant effects. Older individuals were more likely to respond and blacks were less likely to respond than whites. Education also had an effect that was statistically significant and large. Each year of education increased the probability of responding and providing completed data by 3.7 percentage points. This suggests that the interviews may have been too complicated

^{1/}For a description of the appropriate tests see Johnston (1972), pp. 192-207.

TABLE III.4

ESTIMATED EFFECTS OF THE DETERMINANTS OF NON-RESPONSE
(Coefficients Estimated by Probit)

Independent Variable	Effect Evaluated at the Mean	Asymptotic t-Statistic
M	-0.269*	-16.20
GWRR	0.032	0.88
STATE	0.016	0.65
PJAN80	-0.050*	-2.34
AJAN80	0.011	0.44
AGE	0.002*	2.52
SEX	-0.016	-0.91
ED	0.037*	9.19
BLACK	-0.121*	-3.70
SPOUSE	0.046*	2.72
HHSIZE	0.018*	3.58
XPRCL	0.035*	1.92
CONSTANT	-0.481	-0.71
(-2.0)* Log Likelihood Ratio		421.467
Degrees of Freedom		12

NOTE: The dependent variable R equalled 1 when complete data were available and it equalled 0 when complete data were not available. The mean of R was .453.

*Effect significantly different from zero at .05 level on a one-tailed test.

and that it would be desirable to simplify them if possible.^{1/} Another demographic variable that deserves mention is sex. We had expected females to have a higher response rate than males, but the sex variable was insignificant. Separate estimates did indicate that females were more likely than males to complete an interview, but, when missing data items were taken account of, this effect was erased.

Other variables in the model which had significant positive effects on response (e.g., presence of a working spouse, household size, expectation of recall) can be thought of as variables that indicate the likelihood that the individual was still at the same address as he or she was when the UI benefit year began, and these individuals were more likely to be found and interviewed than individuals who had moved.^{2/} For example, individuals expecting recall were, as we showed in the last chapter, often recalled and hence it is unlikely that they moved to find work or because of financial needs. Similar arguments apply to individuals with working spouses. One final variable that had a significant coefficient was that individuals with UI benefit year begin dates before January 1980 had lower response rates than those with later benefit year begin dates. Since all individuals were interviewed one year after their benefit year begin date, interview timing has little to do with this result. Instead, this result

^{1/} In fact, one respondent to the mail interview wrote us a note indicating that he had difficulty following the questionnaire.

^{2/} Interestingly, a number of individuals who did not fill out the mail interviews wrote us a note that they had been recalled after a very short unemployment spell, and they thought that we wouldn't be interested in their experience. If these individuals had responded, the sample would have overrepresented the expect recall group to an even greater degree.

may also be related to the probability of moving. Results presented in the last chapter showed that this group had longer unemployment spells than the remainder of the sample and this may have contributed to the lower response rate since some of these individuals may have moved.

The estimates presented above indicate that non-response may be a problem for use of these data not only because it is sizeable but also because the sample of responders is unrepresentative of the full population of UI recipients in what may be important ways. This issue is addressed in the next section.

D. EFFECTS OF NON-RESPONSE

In this section we examine the extent to which the non-response identified above may affect the study's findings. Two non-response effects are examined. These are: (1) the extent to which study findings of, for example, unemployment spell lengths may be biased because the sample is unrepresentative of UI claimants in general, and (2) the extent to which our estimates of the effect of various variables on, for example, unemployment spell lengths may be biased.

The analysis in the previous section identified several variables that had a significant influence on the probability of response and that may also have an effect on labor market outcomes (e.g., unemployment spell lengths). If this is the case, estimates of these labor market outcomes that are made with the study sample may be biased. For example, individuals who expected to be recalled had higher response rates than those not expecting recall, and since recall expectation was shown in the previous chapter to be negatively correlated with unemployment spell

length, the mean spell length for the sample may be biased downward from the true mean. The extent of this potential problem can be examined by comparing the non-response results presented above with our examination of the determinants of labor market outcomes in the last chapter. Using the initial unemployment spell as an example, this comparison shows that three variables are significant determinants of both response and unemployment spell length.^{1/} These variables are PJN80 (layoff prior to January 1980), BLACK (respondent was black), and XPRCL (respondent expected recall). Furthermore, the effects of all three variables are such that non-response leads to a reduction in the estimated duration of the initial unemployment spell. To judge the importance of this bias we have estimated the separate effect of non-response on the mean duration estimate for each variable and reported the results in Table III.5. These estimates are done separately for the telephone and mail samples and are based on the overall response rate for those samples, the non-response coefficient estimates presented in Table III.4 and the unemployment duration coefficient estimates presented in the previous chapter. The results show that mean spell length is biased downward for the sample by about one-quarter of a week for the telephone sample and one-half of a week for the mail survey, if we sum the separate effects of each variable. The difference between the two survey methods occurs because of the difference in overall response rate. Hence, the bias in sample results is about twice as severe for the mail as for the telephone survey, although neither the one-quarter or one-half week bias is

^{1/} These three variables are also important determinants of the other labor market outcomes of interest.

TABLE III.5

EFFECT OF ADJUSTING MEAN LENGTH OF
INITIAL UNEMPLOYMENT SPELL FOR NON-RESPONSE

Variable	Adjustment in Mean Spell Length (Weeks)	
	Telephone Interview	Mail Interview
PJN80	.05	.09
BLACK	.10	.21
XPRCL	.09	.17
Total	.24	.47

NOTE: The telephone sample adjustment in mean spell length uses data reported in Chapter II for the telephone interview sample on mean spell length and on the effect on spell length of variables PJN80, BLACK, XPRCL. Effects reported for these variables on the response rate are taken from Table III.4. An overall telephone response rate of .51 was used. Comparable data on the mail sample were used for the mail interview adjustment. The response rate of .27 for the mail interview was used. These response rates are slightly higher than those reported in Table III.1 because they exclude individuals with missing CWBH data from the calculations. The effect of these missing data is discussed separately.

large relative to the mean duration (13.6 weeks) reported in Chapter II. This bias is 1.7 to 3.6 percent of the mean, and for most purposes it can probably be ignored.^{1/} Therefore, we have not adjusted our results for non-response, but we have confined the unemployment duration analysis to the telephone sample to limit non-response bias as much as possible.

An additional factor that may bias estimates made from our sample occurs because some individuals do not have CWBH data and hence cannot be used for the analysis. These individuals were selected by the states as part of the original CWBH sample and they responded to our survey. However, when we matched our sample with the federal CWBH data bank, there were no records for these individuals. Furthermore, we have not been able to determine why these CWBH data were unavailable. Data that compare labor market outcomes for this group and those for whom CWBH data were available are reported in Table III.6 for the Pennsylvania sample. (The individuals without CWBH data were all in the Pennsylvania sample). These data show that the sample without CWBH data had significantly longer unemployment spells and weeks of UI collected and they were laid-off at an earlier time period than the sample with CWBH data. Hence, our estimates of initial unemployment duration, for example, will be biased downward because of the exclusion of this part of the sample. Since individuals without CWBH data made up 11.4 percent of the completion sample, the effect on the initial spell length is substantial. If this group were included in the sample, mean duration would be 15.2 weeks instead of 12.7 for the Pennsylvania

^{1/} Estimates of bias for other labor market outcome measures are similar to those for the initial unemployment spell.

TABLE III.6

COMPARISON OF RECIPIENTS WITH AND WITHOUT CWBH DATA:
PENNSYLVANIA SAMPLE

Variable	CWBH Data		Total
	Available	Not Available	
Layoff Date			
Before January 1980	19.0%	33.9%	20.6%
January 1980	30.7	46.6	32.6
After January 1980	50.3	19.5	46.8
Total	100.0	100.0	100.0
Length of Initial Unemployment Spell (Weeks)	12.7	34.9	15.3
Percentage of Time from Layoff to Interview Employed	59.6	17.1	54.8
Weeks of UI Collected from Layoff to Interview	15.6	33.5	17.7
Sample Size ^{a/}	1251	167	1,418

^{a/} CWBH data were not available in Pennsylvania for 11.4 percent of the completion sample.

sample. For the overall two state sample this would raise mean duration by a little over a week. Although this is a fairly sizeable adjustment, we have ignored it since our primary interest is in examining the determinants of unemployment duration and not in estimating duration itself and since it should not occur once the CWBH system is fully operative. Nevertheless, this situation does indicate that if CWBH data are used to describe labor market experiences, it will be important for analysts to insure that the entire CWBH sample is available for the estimates. The absence of data may be related to the outcomes of interest, as occurred in this case.

The second possible problem that non-response may pose is that estimates of the effect of various explanatory variables on labor market outcomes may be biased. For example, we may find that females have initial unemployment spells "b" weeks longer than men but the true difference may be "b+a" weeks. To investigate this issue we have used a procedure developed by Heckman (1976) that corrects for possible sample selection bias. This procedure uses the model of non-response described above to construct a variable that is then used in the equations that explain labor market outcomes, such as the length of the initial unemployment spell. Use of this variable removes possible correlations between the outcome of interest and response to the survey, and it yields unbiased estimates of the model's coefficients.^{1/} Comparison of the

^{1/}The efficacy of this procedure is dependent on how well non-response is explained by the non-response model and the extent to which this model explains non-response using variables not used in the labor market models. The large number of significant variables in the non-response model and the fact that M is highly significant suggests that these conditions are satisfied.

results with and without this variable in the model provides evidence of the extent that non-response may bias the results. This comparison is presented in Table III.7 for the basic model used in Chapter II to explain the duration of the initial unemployment spell.^{1/} An examination of these estimates shows little difference between the adjusted and unadjusted coefficients and the coefficient of the adjustment variable was also statistically significant. These results also occurred when we examined other dependent variables (e.g., duration of UI collection, weeks unemployed during benefit year) and when we divided the sample into the mail and telephone samples and performed this analysis for each sample.

Consequently, we can conclude that non-response, despite being sizeable, does not appear to bias substantially estimates of labor market outcomes for UI recipients, nor does it bias our estimates of the determinants of those outcomes.

E. DATA QUALITY

In this section we examine the quality of the data collected on the mail and telephone interviews in order to assess more fully the relative usefulness of the three interview types. This examination proceeds first by comparing those data collected both in our benefit year end date interview and through the CWBH and ESARS data bases and second by examining other measures of relative data quality.

^{1/} The combined mail and telephone samples are used here and thus the coefficient estimates differ slightly from those presented in Chapter II.

TABLE III.7

COMPARISON OF REGRESSIONS OF INITIAL UNEMPLOYMENT SPELL
ADJUSTED AND UNADJUSTED FOR NON-RESPONSE

Variable	Adjusted for Non-Response ^{a/}		Unadjusted for Non-Response	
	Coefficient	t-statistic	Coefficient	t-statistic
NWRR	2.408*	2.05	2.359*	2.01
STATE	-0.308	-0.44	-0.425	-0.61
PJAN80	2.338*	3.42	2.433*	3.57
AJAN80	-1.154	-1.50	-1.148	-1.49
AGE	-0.018	-0.80	-0.024	-1.06
SEX	-0.795	-1.26	-0.718	-1.14
ED	-0.050	-0.30	-0.167	-1.12
BLACK	2.639*	1.88	3.166*	2.33
SPOUSE	0.460	0.82	0.285	0.52
HHSIZE	-0.082	-0.45	-0.148	-0.84
PREWAGE	0.019	0.22	0.020	0.23
XPRCL	-4.255*	-6.52	-4.411*	-6.84
CONSTANT	12.372*	3.60	16.022*	6.43
R ²		0.065		0.064
F		8.59		9.10
d.f.		13,1595		12,1596

SOURCE: Telephone and mail samples.

*Effect significantly different than zero at .05 level of significance on a one-tailed test.

^{a/}The coefficient of the adjustment variable was 2.253 with a t-statistic of 1.54.

The comparison of interview and CWBH/ESARS data provides an indication of the accuracy of the interview data and of the relative data quality produced by the three interview types. This comparison does not provide a perfect measure of interview data quality because the CWBH/ESARS data as well as the interview data contain measurement errors. However, it is likely that the CWBH/ESARS data errors are smaller than occurring for the interview data. Those CWBH data collected through an interview will have some error but they (i.e., pre-layoff earnings and hours) are collected closer to the layoff date than the interview data, and hence they are probably more accurate. The CWBH UI weekly benefit amount is collected from records, and this variable should be correct. The other data used for comparison purposes, ESARS data and UI receipt data, also come from administrative records, but because of definitional and timing problems they do not necessarily define the same variables as the corresponding interview data items. The ESARS data refer to the FY80 period (i.e., October 1979 through September 1980) and corresponding interview data to the individual Benefit Year which began during the period October 1979 to March 1980. In addition, some ESARS records which were used to define the "Use ES" variable refer to administrative actions which may not have involved the client directly. The UI receipt history data are updated periodically through the CWBH system and data on UI collection that occurred toward the end of the benefit year may be included in our interview but not in the CWBH data base.^{1/}

^{1/} CWBH data on UI receipt were not available for Pennsylvania at the time the analysis was performed.

With these caveats in mind we can examine the comparison of CWBH/ESARS data with the three types of interview data presented in Table III.8. This comparison indicates that estimates of means over the whole sample, for the variables we could observe, were quite similar for the interview and CWBH/ESARS data. For example, mean pre-layoff weekly earnings were \$246 for CWBH data and \$247 for interview data for the telephone sample. The statistical significance of this difference in means was examined by defining a variable for each individual that equalled the CWBH value minus the interview value and testing if the mean of this difference was significantly different than zero. This was the case for 40 percent of the variables we could examine, but even then the differences were not large relative to the overall means (no greater than 3.4 percent). Hence, this evidence suggests that the interview data do not produce biased results. However, for most variables there were a number of outliers in both directions suggesting that there may be substantial random errors in the data. For example, mean pre-layoff earnings were one dollar a week lower for the CWBH measure than the telephone interview measure but over \$25 a week lower for 23 percent of the telephone sample and over \$25 a week higher for another 22 percent. Similar, although generally smaller, discrepancies occurred for the other variables included in Table III.8. Given the definitional problems with these comparisons and the fact that the interview focused on events about one year before the interview, these discrepancies are probably tolerable, particularly since the means are so close to each other.

Further examination of the data in the table indicates that differences were sometimes greater for the mail than telephone interviews

TABLE III.8

COMPARISON OF CWBH, ESARS, AND INTERVIEW DATA
BY INTERVIEW TYPE

	Interview Type		
	Telephone	Detailed Mail	Abbreviated Mail
Missouri and Pennsylvania Samples			
Pre-Layoff Earnings			
Mean CWBH Earnings	\$246	\$243	\$231
Mean Interview Earnings	247	254	239
Mean Earnings Difference ^{a/} (CWBH-Interview)	-1	-9*	-9
Distribution of Earnings Difference			
Less than -\$25	23.2%	24.4%	20.6%
-\$25 to -\$10	10.1	12.2	16.0
-\$10 to \$10	34.9	41.9	40.7
\$10 to \$25	10.2	7.4	9.5
More than \$25	21.6	14.0	13.2
Total	100.0	100.0	100.0
Pre-Layoff Weekly Hours			
Mean CWBH Hours	40.0	40.3	40.0
Mean Interview Hours	40.7	41.4	40.5
Mean Hours Difference ^{a/} (CWBH-Interview)	-0.7*	-1.2*	-0.3
Distribution of Hours Difference			
Less than -5	15.1%	16.9%	13.4%
-5 to -2	6.4	10.3	9.7
-2 to 2	64.9	62.4	63.7
2 to 5	6.7	6.2	7.3
More than 5	6.8	4.1	5.9
Total	100.0	100.0	100.0
UI Weekly Benefit Amount			
Mean CWBH WBA	\$102.1	\$101.8	\$99.8
Mean Interview WBA	100.7	101.8	99.2
Mean WBA Difference ^{a/} (CWBH-Interview)	2.2*	0.7	0.7
Distribution of WBA Difference			
Less than -\$5	7.1%	6.2%	5.1%
-\$5 to \$2	11.8	17.1	16.7
-\$2 to \$2	63.3	63.2	67.7
\$2 to \$5	3.8	2.9	1.5
More than \$5	13.8	10.6	9.0
Total	100.0	100.0	100.0

Table III.8 (cont'd)

Use of the ES			
Percent with an ES Contact			
ESARS	43.5%	43.1%	45.9%
Interview	45.9	32.4	32.2
Percent with ESARS Record and no Interview ES Contact			
	12.7	17.8	19.5
Percent with an Interview ES Contact and no ESARS Record			
	15.0	7.4	5.7
Percent with a Job Referral from the ES			
ESARS	14.7%	12.0%	15.2%
Interview	14.2	10.4	10.3
Percent with ESARS Job Referral and no Interview Job Referral			
	6.9	5.8	8.6
Percent with Interview Job Referral and no ESARS Job Referral			
	6.5	4.3	3.7
Sample Size	1818	450	423
Missouri Sample			
Total Weeks of UI Collected			
Mean CWBH Weeks	14.0	13.9	13.9
Mean Interview Weeks	13.7	14.8	12.2
Mean Weeks Difference ^{a/} (CWBH-Interview)	0.3	-0.8	1.7*
Distribution of Weeks Difference			
Less than -10	7.0%	5.6%	4.5%
-10 to -5	9.1	6.9	2.9
-5 to 5	67.2	78.6	74.4
5 to 10	10.5	4.9	8.6
More than 10	6.4	4.0	9.5
Total	100.0	100.0	100.0
Exhaustion of Regular UI			
Percent Exhausting:			
CWBH	29.1%	33.5%	31.2%
Interview	25.2	31.8	21.6
Percent Exhausting on CWBH but not on Interview			
	10.4	9.2	12.1
Percent Exhausting on Interview but not on CWBH			
	6.6	7.5	2.5
Exhaustion of EB			
Percent Exhausting:			
CWBH	7.7%	11.6%	8.5%
Interview	9.5	11.6	7.5
Percent Exhausting on CWBH but not on Interview			
	4.6	7.5	5.5
Percent Exhausting on Interview			
	6.4	7.5	4.5
Sample Size	959	173	199

Table III.8 (cont'd)

^{a/} These means are the means of the variable that equals the CWBH data item minus the interview data item. It is not the CWBH mean minus the interview mean.

*Mean of difference is significantly different than zero at the .05 level of significance on a two-tailed test.

and sometimes the opposite was true. For example, differences in mean earnings were larger for the mail than telephone interviews but the opposite was true for the UI weekly benefit amount, where the telephone interview showed a larger mean discrepancy. Furthermore, for some variables (e.g., weekly earnings) the telephone survey yielded mean estimates closer to the CWBH estimate than the mail interviews, but the distribution of, in this case, the earnings difference was greater on the telephone than mail interview. Thus, these comparisons suggest that there was little discernible difference in data quality between the mail and telephone interviews, when complete data were available.

Another method of investigating data quality is to compare measured sample variances of unemployment spells data among the various survey methods. The relative size of these sample variances will reflect the relative size of measurement errors under the assumption that the measurement error associated with each interview type has a zero mean and a variance independent of the true variable.^{1/} Hence, if we find that the variance of a variable is significantly larger for, say, the detailed mail survey than the telephone survey, we can conclude that measurement error is greater in the detailed mail than in the telephone survey. The equality of variances for a given variable by interview method can be tested by noting that the ratio of the unbiased estimates of two variances has an

^{1/} Suppose $S_{m,i} = S_t + V_i$ is the measured length of an unemployment spell under method m,i , S_t is the true spell length, and V_i is the measurement error associated with method i and assumed to have mean 0 and to be independent of S_t . Then, the population variance is $\text{Var}(S_{m,i}) = \text{Var}(S_t) + \text{Var}(V_i)$ and, since $\text{Var}(S_t)$ is the same for each survey method, a comparison of $\text{Var}^i(S_{m,i})$ among methods will indicate the relative sizes of the $\text{Var}(V_i)$.

F distribution with $n-1$ and $m-1$ degrees of freedom (where n and m are the relevant sample sizes). These ratios and the results of the corresponding F-tests are reported in Table III.9. These results show that mail interview variances were greater in all but one case than the telephone variances. In addition, the mail variances were significantly greater than the telephone interview variances for over half the variables we examined. Moreover, this was the case for both mail interviews for our principal outcome variable, the length of the initial unemployment spell. Consequently, this analysis, as opposed to that presented above, suggests that it is likely that the mail interviews were subject not only to more missing data than the telephone interviews, but for interviews with complete data there was more measurement error. This conclusion was also supported by the impressions of staff involved with the data cleaning process. In particular, there appeared to be relatively more problems with dates on the mail than the telephone interviews.

F. SUMMARY AND CONCLUSIONS

The previous sections of this chapter examined non-response and data quality issues in general and more specifically in terms of differences between the telephone and mail interviews. Our general conclusions regarding non-response and data quality can be summarized as follows:

- Non-response occurred in this study both because all potential respondents did not complete an interview and because "completed" interviews sometimes contained missing information that was considered important for the analysis of unemployment spells. This non-response was large enough that study findings could be biased.

TABLE III.9

RATIO OF MAIL INTERVIEW TO TELEPHONE INTERVIEW
 VARIANCES FOR SELECTED VARIABLES

Variable	Interview Type	
	Detailed Mail	Abbreviated Mail
Initial Unemployment Spell Length	1.30*	1.37*
Weeks Unemployed	1.19*	1.02
Weeks of UI Collected	1.47*	0.96
Real Pre-UI Weekly Wage	1.09	1.51*
Degrees of Freedom for F-test	219, 1464	231, 1464

*Variances significantly different at the .05 level of significance.

- The probability of response was positively related to demographic variables such as age and education and to other variables that were positively related to the likelihood of finding a job (e.g., expectation of recall). Blacks were also less likely to respond than whites.
- The determinants of non-response and of labor market outcomes were related in such a way that the study sample underrepresented long spells of unemployment; however, the size of this bias was small and study results did not need to be adjusted for non-response.
- No evidence was found that indicated that non-response biased our estimates of the size of the effects of various variables on labor market outcomes, such as the length of the initial unemployment spell.
- Comparisons between CWBH/ESARS and interview data suggested that the interview estimates were similar to the CWBH/ESARS data at the mean, but that there was considerable noise in the interview data, (i.e., there were a number of positive and negative differences between the interviews and CWBH/ESARS data).

When we compared the three interview types we found that the two mail versions were generally quite similar and could be compared together to the telephone interview. Our findings concerning this comparison were:

- Non-response was significantly higher on the mail than the telephone interviews, the difference in response rates being 23 percentage points.
- This non-response difference resulted because of both a higher level of non-response on the mail than the telephone interview for the survey and because of missing data. Some constructed data items were missing from the mail interview for as much as 40 percent of the completion sample.
- Although non-response bias was small, overall, it was larger on the mail than the telephone interview because of the overall difference in response rate. Determinants of non-response did not differ between the two interview methods.

- Data quality on the two interview types was generally similar although there was some evidence that it was slightly worse on the mail interviews. There was more noise in the data for those interviews.

In order to choose the interview method for future studies of this nature one further piece of evidence is needed, since neither method was overwhelmingly superior. This last piece of information relates to the relative cost of the two methods. Data on actual direct interviewing costs from our study are reported in Table III.10. These data are reported in terms of interview attempts, and they show that each telephone attempt used 32.08 minutes of labor and cost \$5.50 and each mail interview attempt used 2.99 minutes of labor and cost \$2.83. These data are, of course, dependent on the way interviewing was conducted at MPR, and they may not be appropriate for other studies. In particular, two mail interview cost items deserve special mention. First, there was no specific supervisory time for the mail interviews since so small a number were done that the overall survey manager was available to handle all the work directly, and second, printing costs per attempt were quite high for the mail interviews because a special layout was used. For a larger scale survey supervisory costs would increase but printing costs would decrease. Since these two factors balance each other out, the relative difference between the methods should be reasonably accurate.

To use these numbers to compare interviewing methods, adjustments are necessary to take account of the fact that these costs are expressed in terms of attempts rather than useable interviews and to take account of overhead costs. The first adjustment can be made using the data presented in Table III.1 while the overhead adjustment will be dependent on

TABLE III.10

SUMMARY OF DIRECT INTERVIEWING TIME AND COSTS
PER ATTEMPT BY TYPE OF INTERVIEW

	Telephone		Mail	
	Minutes	Cost	Minutes	Cost
Personal Costs				
Full Time Staff				
Survey Manger	3.61	\$0.73	1.47	\$0.30
Other Professional	.28	0.03	1.30	0.16
Secretary	.75	0.07	0.11	0.01
Total	4.64	0.83	2.88	0.47
Part Time Staff				
Interviewing Supervisor	2.00	0.17	—	—
Interviewers	19.36	1.36	—	—
Clerical Support	6.08	0.47	0.11	0.51
Total	27.44	2.00	0.11	0.51
Materials and Supplies				
Telephone	—	2.22	—	—
Postage	—	0.11	—	0.68
Printing and Reproduction	—	0.12	—	1.17
Other	—	0.22	—	—
Total	—	2.67	—	1.85
Total of Direct Interviewing Time and Costs	32.08	\$5.50	2.99	\$2.83

institutional factors peculiar to each state or research organization. A rough estimate of overhead costs, however, is that full time staff overhead is 100 percent, part time staff 50 percent, and materials and supplies zero percent. Using these numbers we can compute that telephone costs would be \$14.96 per useable interview and comparable mail costs would be \$13.69. Thus, because the mail response rate was roughly half the telephone response rate, the cost advantage of the mail interviews would be quite small. Costs for the mail interview would be approximately ten percent less than the telephone interview for comparable, useable sample sizes. If no overhead is charged, the mail interview cost advantage would be less than 5 percent per useable interview.

The above summary suggests that the telephone interview data were better than the mail interview data, but that the difference was not overwhelming. This occurred because the major difference between the two interview methods was that the response rate was lower on the mail than the telephone interview, and our evidence suggests that non-response was not a serious problem for the analysis.^{1/ 2/} On the other hand, costs were slightly less for the mail interview than the telephone interview. These conclusions suggest that either method could be used by future studies of this nature, and that the choice of method will depend on whether the additional accuracy and higher response of the telephone interview is

^{1/} This does not mean that future studies can ignore non-response. An examination of potential non-response problems should be part of any study that uses interview data.

^{2/} Potential problems that may have arisen because of nonresponse to the initial CWBH interview were not addressed because no data on nonresponders were available.

needed by researchers and policy makers. If the data are to be used to examine the determinants of unemployment duration the mail interview would probably be adequate, but if the data are to be used for forecasting variables such as benefit costs or exhaustion rates non-response would affect the projections and the telephone interview should probably be chosen.

If the mail interview is chosen the detailed mail interview, as opposed to the abbreviated form, should be used, since there was little difference between the two and the detailed form provides a more precise record of labor market experience during the UI benefit year. Furthermore, a telephone follow-up could be used for mail interviews with missing or inconsistent key data items. This would improve the mail response rate and although labor costs would increase costs per useable interview might decrease. Further experience with alternative interview settings and mixed mail/telephone interviewing methods will provide additional evidence concerning the appropriate interviewing method for future studies.

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APPENDIX
INTERVIEWS

Dear Sir/Madam:

The United States Department of Labor has asked us to conduct a study to find out more about what happens to people who have filed for unemployment insurance benefits. The study is being conducted under Section 906 of the Social Security Act which directs the Secretary of Labor to establish a continuing and comprehensive program of research to evaluate the Federal-State unemployment compensation system.

Your name has been randomly selected from a list of people who filed claims for unemployment benefits in your state about a year ago. It is very important for the accuracy of this study that you fill out the enclosed questionnaire.

Under the Privacy Act of 1974, the information you give us is voluntary, and will not affect any of your past or future rights to benefits in any way. All of the information you give us will be confidential and will not be identified with your name. The information will be used only for research and the study report will be in statistical form only.

The identification number on the questionnaire is used in order to check your name off the mailing list when your questionnaire is returned.

The results of this study will be used to help improve the unemployment insurance program in the future. When you have completed the questionnaire, please use this self-addressed envelope to return it. No postage is necessary.

Thank you very much for your assistance.

Sincerely,

An Equal Opportunity Employer

Legal Citation: Section 906
Social Security Act
(42 U.S.C. 1106)
OMB #44-S-80012
MPRI #354

UNEMPLOYMENT INSURANCE STUDY

1. According to unemployment insurance records, you established a claim for benefits on _____.

THE FOLLOWING QUESTIONS ARE ABOUT THE JOB YOU HAD JUST BEFORE THE ABOVE DATE.

2. What type of company did you work for? What did they make or do?

3. What was your job title: _____

What were your main duties and activities:

4. When did you start working on this job? (If you worked there more than once, give the date you first started before applying for unemployment benefits a year ago.)

_____/_____/_____
MONTH DAY YEAR

5. How many hours did you usually work per week? (Include overtime and paid lunchtime as hours worked.)

_____ HOURS/WEEK

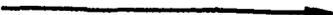
6. How much were your usual weekly earnings on this job, before taxes and other deductions? Include tips, commissions, and overtime as earnings.

_____ PER WEEK

7. What was the last day you worked on this job before you applied for unemployment insurance benefits a year ago?

_____/_____/_____
MONTH DAY YEAR

8. Did you look for work after this job ended a year ago?

1 Yes  GO TO QUESTION 10

 2 No

 9. Why not?

1 New job to start

2 Expected to get old job back

3 No job available in my line of work

Circle 4 Not enough skills, schooling, or experience

as many

as apply

5 Family responsibilities

6 Went to school

7 Ill health

8 Discrimination due to age, sex, or race

9 Retired

10 Other reason

9a. IF YOU DID NOT LOOK FOR WORK AFTER THIS JOB ENDED, GO TO QUESTION 24, PAGE 6.

10. THE QUESTIONS ON THE NEXT FEW PAGES ASK ABOUT HOW YOU LOOKED FOR JOBS AFTER THE END OF THE JOB DESCRIBED ON PAGE 1.

How many weeks were you actively looking and available for work after your job ended?

_____ WEEKS

11. On the average, how many hours each week would you say you spent looking for work during this time?

_____ HOURS PER WEEK SPENT LOOKING FOR WORK

12. Which of the following did you use to help you find a job?

1 State Employment Service or State Job Service

2 Private employment agency

3 Friends or relatives

Circle 4 Looked in newspapers

as many 5 Placed ads

as apply 6 Answered ads

7 Applied directly with possible employers

8 Union hall

9 Other (please describe) _____

13. Did you go to the State Employment Service or State Job Service?

1 Yes → **GO TO QUESTION 15**

2 No

14. Why didn't you go?

1 Didn't think it would help me get a job

2 Had a job, or awaiting recall to job

3 Too far away

Circle 4 It doesn't help union members

as many 5 Didn't think of it

as apply

6 Wages of jobs offered were too low

7 Other (please describe) _____

14a. IF YOU DID NOT GO TO THE STATE EMPLOYMENT SERVICE OR JOB SERVICE, GO TO QUESTION 23 ON PAGE 5.

15. What is the main reason you went to the State Employment Service or State Job Service?

1 I wanted help in finding a job

2 I was required to go in order to receive unemployment benefits

16. When did you go to the State Employment Service or State Job Service?

1 When I first started looking for work → **GO TO QUESTION 18**

2 Only after trying other ways of finding a job

17. Why didn't you go when you first started looking?

1 Didn't think it would help

2 Awaiting recall to job

3 Too far away

Circle

4 Didn't think it would help union members

as many

5 Didn't think of going

as apply

6 Wages of jobs offered were too low

7 Other (please describe) _____

18. When you went to the State Employment Service or State Job Service, were you referred to any employers?

1 Yes

2 No → **GO TO QUESTION 22 ON NEXT PAGE**

18a. How many employers were you referred to? _____

19. Did you get any job offers as a result of referrals from the State Employment Service or State Job Service?

1 Yes

2 No → **GO TO QUESTION 22 ON NEXT PAGE**

20. How many? _____

21. Did you accept any of these offers?

1 Yes

2 No

22. When you went to the State Employment Service or State Job Service, which of the following did they do?

- 1 Helped me fill out job applications and contact employers
- 2 Gave me information about jobs in other areas or towns
- 3 Referred me to other agencies which might help me find a job
- 4 Taught me how to apply for jobs
- 5 Gave me information to help me decide on a career or occupation
- 6 Tested me to see what jobs I am qualified or suited for
- 7 Gave me information about job training programs
- 8 Got me into a job training program
- 9 None of the above

*Circle
as many
as apply*

23. Please circle the number next to any of the reasons which might explain why you stopped looking for work during the time after the end of the job described on page 1:

- 1 Re-employed or new job to start
- 2 Expected to get old job back
- 3 Couldn't find a job
- 4 Not enough skills, schooling, or experience
- 5 Family responsibilities
- 6 Went to school
- 7 Ill health
- 8 Discrimination due to sex, age, or race
- 9 Retired
- 10 Other reason (please describe) _____

- 11 Have not stopped-still looking

*Circle
as many
as apply*

24. How many weeks did you collect unemployment benefits after the end of the job described on page 1?

_____ WEEKS

IF YOU DID NOT COLLECT ANY BENEFITS, ENTER "0" ON LINE AND GO TO QUESTION 27 BELOW.

25. How much did you collect per week in unemployment benefits?

\$ _____

26. Why did you stop collecting benefits?

1 Re-employed → **GO TO QUESTION 28**

2 Benefits exhausted

3 Stopped voluntarily

*Circle
one
number*

4 Disqualified

5 Other reason (please describe) _____

6 Did not stop--still collecting

27. Were you employed again after the date you entered in Question 7?

1 Yes

2 No → **GO TO QUESTION 69, PAGE 13**

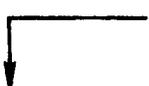
28. ANSWER THE NEXT QUESTIONS FOR THE FIRST TIME YOU WERE EMPLOYED AFTER FILING FOR UNEMPLOYMENT BENEFITS A YEAR AGO.

When did you start working at this job?

_____/_____/_____
MONTH DAY YEAR

29. Is this the same employer as the one you had on the job just before you filed for unemployment benefits a year ago?

1 Yes  GO TO QUESTION 32

2 No 

30. What type of company was this? What did they make or do?

31. What was/is your job title? _____

What were your main duties and activities?

32. How did you find this job?

- 1 Recalled by former employer
- 2 Private employment agency
- 3 State Employment Service or State Job Service
- 4 Friends or relatives
- 5 Want ads
- 6 Union halls
- 7 Applied directly with employer
- 8 Other (please describe) _____

*Circle
one
number*

33. How much were/are your usual weekly earnings on this job, before taxes and other deductions? Include tips, commissions and overtime.

\$ _____ PER WEEK

34. How many hours did you usually work per week? Include overtime and paid lunchtime as hours worked.

_____ HOURS/WEEK

35. Are you still working on this job?

1 Yes → **GO TO QUESTION 69, PAGE 13**

2 No

36. When did this job end? _____ / _____ / _____
MONTH DAY YEAR

37. Did you look for work at all after this job ended?

1 Yes

2 No → **GO TO QUESTION 39**

38. How many weeks were you actively looking and available for work after this job ended?

_____ WEEKS

39. How many weeks did you collect Unemployment Insurance benefits during this period?

_____ WEEKS

IF YOU DID NOT COLLECT BENEFITS DURING THIS PERIOD, ENTER "0" ON LINE AND GO TO QUESTION 41.

40. Why did you stop collecting benefits this time?

1 Re-employed → **GO TO QUESTION 42 ON NEXT PAGE**

2 Benefits exhausted

Circle 3 Stopped voluntarily

one 4 Disqualified

number 5 Other reason (please describe) _____

6 Have not stopped--still collecting

41. Were you employed again after the date you entered in Question 36?

1 Yes → **GO TO QUESTION 42 ON NEXT PAGE**

2 No → **GO TO QUESTION 69, PAGE 13**

42. ANSWER THESE QUESTIONS FOR THE SECOND TIME YOU WERE EMPLOYED AFTER FILING FOR UNEMPLOYMENT BENEFITS A YEAR AGO.

When did you start working at this job?

_____/_____/_____
MONTH DAY YEAR

43. Is this the same employer as the one you had on the job just before you filed for unemployment benefits a year ago?

1 Yes → **GO TO QUESTION 46**

2 No

44. What type of company was this? What did they make or do?

45. What was/is your job title? _____

What were your main duties and activities?

46. How did you find this job?

1 Recalled by former employer

2 Private employment agency

3 State Employment Service or State Job Service

Circle 4 Friends or relatives

one 5 Want ads

number 6 Union halls

7 Applied directly with employer

8 Other (please describe) _____

47. How much were/are your usual weekly earnings on this job, before taxes and other deductions? Include tips, commissions, and overtime.

\$ _____ PER WEEK

48. How many hours did you usually work per week? Include overtime and paid lunchtime as hours worked.

_____ HOURS/WEEK

49. Are you still working on this job?

1 Yes → **GO TO QUESTION 69, ON PAGE 13**

2 No

50. When did this job end?

_____/_____/_____
MONTH DAY YEAR

51. Did you look for work at all after this job ended?

1 Yes

2 No → **GO TO QUESTION 53**

52. How many weeks were you actively looking and available for work after this job ended?

_____ WEEKS

53. How many weeks did you collect Unemployment Insurance benefits during this period?

_____ WEEKS

IF YOU DID NOT COLLECT ANY BENEFITS, ENTER "0" ON LINE AND GO TO QUESTION 55.

54. Why did you stop collecting benefits this time?

1 Re-employed → **GO TO QUESTION 56 ON NEXT PAGE**

2 Benefits exhausted

3 Stopped voluntarily

Circle

4 Disqualified

*one
number*

5 Other reason (please describe) _____

6 Have not stopped--still collecting

55. Were you employed again after the date you entered in Question 50?

1 Yes

2 No → **GO TO QUESTION 69, PAGE 13**

56. ANSWER THE NEXT QUESTIONS FOR YOUR MOST RECENT JOB.

When did you start working at your most recent job?

_____/_____/_____
MONTH DAY YEAR

57. Is this the same employer as the one you had on the job just before you filed for unemployment benefits a year ago?

1 Yes → **GO TO QUESTION 60**

2 No

58. What type of company is this? What do they make or do?

59. What was/is your job title? _____

What were your main duties and activities?

60. How did you find this job?

1 Recalled by former employer

2 Private employment agency

3 State Employment Service or State Job Service

Circle

4 Friends or relatives

one

5 Want ads

number

6 Union halls

7 Applied directly with employer

8 Other (please describe) _____

61. How much were/are your usual weekly earnings on this job, before taxes and other deductions? Include tips, commissions, and overtime.

\$ _____ PER WEEK

62. How many hours did you usually work per week? Include overtime and paid lunchtime as hours worked.

_____ HOURS/WEEK

63. Are you still working on this job?

1 Yes → GO TO QUESTION 69 ON NEXT PAGE

2 No

64. When did this job end?

_____/_____/_____
MONTH DAY YEAR

65. Did you look for work at all after this job ended?

1 Yes

2 No → GO TO QUESTION 67

66. How many weeks were you actively looking and available for work after this job ended?

_____ WEEKS

67. How many weeks did you collect Unemployment Insurance benefits during this period?

_____ WEEKS

IF YOU DID NOT COLLECT ANY BENEFITS, ENTER "0" ON LINE AND GO TO QUESTION 69.

Dear Sir/Madam:

The United States Department of Labor has asked us to conduct a study to find out more about what happens to people who have filed for unemployment insurance benefits. The study is being conducted under Section 906 of the Social Security Act which directs the Secretary of Labor to establish a continuing and comprehensive program of research to evaluate the Federal-State unemployment compensation system.

Your name has been randomly selected from a list of people who filed claims for unemployment benefits in your state about a year ago. It is very important for the accuracy of this study that you fill out the enclosed questionnaire.

Under the Privacy Act of 1974, the information you give us is voluntary, and will not affect any of your past or future rights to benefits in any way. All of the information you give us will be confidential and will not be identified with your name. The information will be used only for research and the study report will be in statistical form only.

The identification number on the questionnaire is used in order to check your name off the mailing list when your questionnaire is returned.

The results of this study will be used to help improve the unemployment insurance program in the future. When you have completed the questionnaire, please use this self-addressed envelope to return it. No postage is necessary.

Thank you very much for your assistance.

Sincerely,

An Equal Opportunity Employer

Legal Citation: Section 906
Social Security Act
(42 U.S.C. 1106)

OMB #44-S-80012
MPRI #355

UNEMPLOYMENT INSURANCE STUDY

1. According to unemployment insurance records, you established a claim for benefits on _____ .

THE FOLLOWING QUESTIONS ARE ABOUT THE JOB YOU HAD JUST BEFORE THE ABOVE DATE.

2. When did you start working at that job? (job before claim)

_____/_____/_____
MONTH DAY YEAR

3. What type of company did you work for?--what did they make or do?

4. What was your job title? _____

What were your main duties and activities: _____

5. How many hours did you usually work per week? (Include overtime and paid lunchtime as hours worked.)

_____ HOURS/WEEK

6. How much were your usual weekly earnings on this job, before taxes and other deductions? Include tips, commissions, and overtime.

\$ _____ PER WEEK

7. What was the last day you worked on this job before you applied for unemployment insurance benefits a year ago?

_____/_____/_____
MONTH DAY YEAR

8. How many weeks did you collect unemployment benefits after the end of this job? If you have collected benefits more than once during this year, count only the first time you collected benefits after the end of this job.

_____ WEEKS

IF YOU DID NOT COLLECT ANY BENEFITS, ENTER "0" ON LINE AND GO TO QUESTION 11.

9. How much did you collect per week in unemployment benefits?

\$ _____ PER WEEK

10. Why did you stop collecting benefits?

1 Re-employed

2 Benefits exhausted

Circle 3 Stopped voluntarily

one 4 Disqualified

number

5 Other reason (please describe) _____

6 Did not stop--still collecting

11. Did you look for work after this job ended a year ago?

1 Yes → GO TO QUESTION 13, ON THE NEXT PAGE

2 No

12. Why not?

1 New job to start

2 Expected to get old job back

3 No job available in my line of work

4 Not enough skills, schooling, or experience

Circle as 5 Family responsibilities

many as 6 Went to school

apply 7 Ill health

8 Discrimination due to age, sex, or race

9 Retired

10 Other reason _____

12a. IF YOU DID NOT LOOK FOR WORK AFTER THIS JOB ENDED, PLEASE GO TO QUESTION 28 ON PAGE 5.

13. The questions on the next three pages are about your job search activities after the end of the job described on page 1.

How many weeks were you actively looking and available for work after your job ended?

_____ WEEKS

14. On the average, how many hours each week would you say you spent looking for work during this time?

_____ HOURS PER WEEK SPENT LOOKING FOR WORK

15. Which of the following did you use to help you find a job?

1 State Employment Service or State Job Service

2 Private employment agency

3 Friends or relatives

Circle as 4 Looked in newspapers

many as 5 Placed ads

apply 6 Answered ads

7 Applied directly with possible employers

8 Union hall

9 Other (please describe) _____

16. Did you go to the State Employment Service or Job Service?

1 Yes _____

GO TO QUESTION 18 ON THE NEXT PAGE

2 No

17. Why didn't you go?

1 Didn't think it would help me get a job

2 Had a job, or awaiting recall to job

Circle as 3 Too far away

many as 4 It doesn't help union members

apply 5 Didn't think of it

6 Wages of jobs offered were too low

7 Other (Please describe) _____

17a. IF YOU DID NOT GO TO THE STATE EMPLOYMENT SERVICE OR JOB SERVICE, PLEASE GO TO QUESTION 27, ON PAGE 5.

18. What is the main reason you went to the State Employment Service or Job Service?

- 1 I wanted help in finding a job
- 2 I was required to go in order to receive unemployment benefits

19. When did you go to the State Employment Service or State Job Service?

1 When I first started looking for work → **GO TO QUESTION 21**

2 Only after trying other ways of finding a job

20. Why didn't you go when you first started looking?

- 1 Didn't think it would help
- 2 Awaiting recall to job
- 3 Too far away
- 4 Didn't think it would help union members
- 5 Didn't think of going
- 6 Wages of jobs offered were too low
- 7 Other (please describe) _____

*Circle
as many
as apply*

21. When you went to the State Employment Service or Job Service, were you referred to any employers?

1 Yes

2 No → **GO TO QUESTION 26 ON PAGE 5**

22. How many employers were you referred to? _____

23. Did you get any job offers as a result of referrals from the State Employment Service or Job Service?

1 Yes

2 No → **GO TO QUESTION 26 ON PAGE 5**

24. How many? _____

25. Did you accept any of these offers?

1 Yes

2 No

26. When you went to the State Employment Service or Job Service, which of the following did they do?

- 1 Helped me fill out job applications and contact employers
- 2 Gave me information about jobs in other areas or towns
- 3 Referred me to other agencies which might help me find a job

*Circle
as many
as apply*

- 4 Taught me how to apply for jobs
- 5 Gave me information to help me decide on a career or occupation
- 6 Tested me to see what jobs I am qualified or suited for
- 7 Gave me information about job training programs
- 8 Got me into a job training program
- 9 None of the above

27. Please circle the number next to any of the reasons which might explain why you stopped looking for work during the time after the end of the job described on page 1:

- 1 Re-employed or new job to start
- 2 Expected to get old job back
- 3 Couldn't find a job
- 4 Not enough skills, schooling, or experience

*Circle
as many
as apply*

- 5 Family responsibilities
- 6 Went to school
- 7 Ill health
- 8 Discrimination due to sex, age, or race
- 9 Retired
- 10 Other reason (please describe) _____

11 Have not stopped--still looking

28. Have you had any job since the date you entered in Question 7?

1 Yes

2 No → GO TO QUESTION 36 ON PAGE 7

The following questions are about the first job you had after the job which ended on the date in Question 7.

29. When did this job start?

_____/_____/_____
MONTH DAY YEAR

30. What type of company is this? What do they make or do?

31. What was/is your job title? _____

What were your main duties and activities? _____

32. Is this the same employer as the one you had on the job just before you filed for unemployment benefits a year ago?

1 Yes

2 No

33. How many hours did/do you usually work per week? (Include overtime and paid lunchtime as hours worked.)

_____ HOURS/WEEK

34. How much were your usual weekly earnings on this job, before taxes and other deductions? Include tips, commissions, and overtime.

\$ _____ PER WEEK

35a. Are you still working on this job?

1 Yes

→ GO TO QUESTION 36 ON THE NEXT PAGE

2 No

↓
35b. When did this job end?

_____/_____/_____
MONTH DAY YEAR

OMB # 44-S-80012

[][][][]-[][][]-[][]

MPRI # - 356

RESPONDENT ID#

UNEMPLOYMENT SPELLS
TELEPHONE QUESTIONNAIRE

Time Began

AM...1

[][]:[][] PM...2

Hello, may I please speak to _____?

WHEN CORRECT RESPONDENT ANSWERS, SAY: My name is _____

and I'm calling from Mathematica Policy Research in Princeton, New Jersey.

We are conducting a study for the U.S. Department of Labor to find out more about the experiences of people who have collected unemployment insurance benefits.

Recently a letter was sent to you explaining a little about the study.

Did you receive it?

YES . . (CONTINUE INTRO). 1

NO 2

I'm sorry yours didn't reach you. It was a brief letter we sent so people would know we would be calling them. (CONTINUE INTRO)

We are calling a group of people who established claims for unemployment benefits just about one year ago. Under the Privacy Act of 1974, the information you furnish is voluntary, and your willingness to answer will not affect any of your past or future rights to benefits in any way. All of the information you give us will be confidential and will not be identified with your name. The information will be used only for research and the study report will be in statistical form only.

The interview takes about 15 or 20 minutes. Let's begin.

According to Unemployment Insurance records, you established a claim for unemployment benefits on (BENEFIT YEAR BEGIN DATE). I'd like to ask about the job you had just before you filed for unemployment benefits at that time.

PROBE: The job you had that made you eligible to collect unemployment insurance benefits.

IF DON'T KNOW: Then tell me about the longest job you had in the 12 months before you filed your claim a year ago.

1. What kind of company did you work for? What did they make or do?

[][]

2. What did you do there--what was your job?

[][]

3. When did you start working for that employer? If you worked there more than than once, tell me the first time you started before you applied for unemployment insurance a year ago.

INTERVIEWER: DATE MUST BE BEFORE DATE OF BENEFIT YEAR.

_____/_____/_____
MONTH DAY YEAR

4. How much were your usual weekly earnings on this job, before taxes and other deductions? Include tips, commissions and overtime.

1 \$[][], [][][] PER WEEK

2 \$[][], [][][] PER MONTH

3 \$[][], [][][] PER YEAR

IN KIND ONLY NA

5. Counting overtime and paid lunchtime, how many hours per week did you usually work on that job?

[][] HOURS PER WEEK

6. When was the last day you worked on that job before you applied for unemployment insurance benefits a year ago?

_____/_____/_____
MONTH DAY YEAR

7. I'd like to ask you about the period of time after that job ended.
Did you look for work at that time?

YES 1

NO (GO TO Q.23). 2

8. How many weeks were you actively looking and available for work after
your job ended on (DATE IN Q.6)?

[][] WEEKS

9. And about how many hours per week on the average would you say you spent
looking for work?

[][] HOURS PER WEEK

10. I'm going to read a list of a number of things people sometimes do to try
to find work, and I'd like you to tell me whether you did any of these
things.

Did you. . .

	YES	NO
a. check with the (STATE EMPLOYMENT SERVICE/ STATE JOB SERVICE)?	1	2
b. check with any private employment agency	1	2
c. ask friends or relatives about job openings?	1	2
d. look at want ads?	1	2 → (GO TO F)
e. <u>answer</u> any ads?	1	2
f. <u>place</u> any ads in newspapers or other publications?	1	2
g. apply directly with possible employers?	1	2
h. check with your union, if you are a member?	1	2
i. do anything else to try to find a job?	1	2

SPECIFY: _____

11. INTERVIEWER: DID R GO TO STATE EMPLOYMENT SERVICE OR JOB SERVICE? (SEE Q10A)

YES (GO TO Q.13). 1

NO 2

12. You said you did not go to the (STATE EMPLOYMENT SERVICE/STATE JOB SERVICE). Why didn't you go?

CIRCLE ALL THAT APPLY

- DIDN'T THINK IT WOULD HELP ME GET A JOB. 1
- AWAITING RECALL. 1
- TOO FAR AWAY 1
- WON'T HELP UNION MEMBERS 1
- DIDN'T THINK OF IT 1
- WAGES OF JOBS OFFERED TOO LOW. 1
- OTHER: (SPECIFY) 1

(GO
TO
Q21)

13. You said you went to the (STATE EMPLOYMENT SERVICE/STATE JOB SERVICE). Were you required to go to the (EMPLOYMENT SERVICE/JOB SERVICE) in order to be eligible to receive unemployment insurance benefits?

- YES 1
- NO (GO TO Q.15) 2

14. Did you go to the (EMPLOYMENT SERVICE/JOB SERVICE) mainly to get help in finding a job, or did you go mainly because you had to go in order to receive unemployment insurance benefits?

- HELP IN FINDING JOB 1
- HAD TO GO TO GET UI 2

15. Did you go to the (STATE EMPLOYMENT SERVICE/STATE JOB SERVICE) when you first started looking for work at this time, or did you go only after you had tried other ways of finding a job?

- FIRST STARTED (GO TO Q.17). . . . 1
- AFTER OTHER TRIES 2

16. Why didn't you go when you first started looking?

CIRCLE ALL THAT APPLY

- DIDN'T THINK IT WOULD HELP 1
 - AWAITING RECALL 1
 - TOO FAR AWAY. 1
 - DIDN'T THINK WOULD HELP UNION MEMBERS 1
 - DIDN'T THINK OF GOING 1
 - WAGES OF JOBS OFFERED TOO LOW 1
 - OTHER: (SPECIFY) 1
-

17. When you went to the (STATE EMPLOYMENT SERVICE/STATE JOB SERVICE), were you referred to any employers?

IF YES: How many employers were you referred to?

- YES [][]
- NO(GO TO Q.20) 0

18. Did you get any job offers as a result of referrals by the (EMPLOYMENT SERVICE/JOB SERVICE)?

IF YES: How many job offers did you get?

- YES [][]
- NO(GO TO Q.20) 0

19. Did you accept any of these offers?

- YES 1
- NO 2

20. When you went to the (STATE EMPLOYMENT SERVICE/STATE JOB SERVICE), did they . . .

	YES	NO
help you fill out job applications and contact employers?	1	2
give you information about jobs in other areas or towns?	1	2
refer you to other agencies which might help you find a job?	1	2
teach you how to apply for jobs?	1	2
give you information to help you decide on a career or occupation?	1	2
test you to see what jobs you are qualified or suited for?	1	2
give you any information about job training programs?	1	2
get you into any job training program?	1	2

21. Did you stop looking for work after this period or are you still looking?

- STOPPED LOOKING 1
- STILL LOOKING . . (GO TO Q 24). 2

22. OK, now I'd like to know why you stopped looking for a job during the time after your job ended on (DATE IN Q.6). Was it because you started working again or was there some other reason?

PROBE IF OTHER REASON: What was the reason?

CIRCLE ALL THAT APPLY

- REEMPLOYED. 1
- EXPECTED TO GET OLD JOB BACK. 1
- COULDN'T FIND ANY WORK. 1
- LACKED NECESSARY SCHOOLING, TRAINING, SKILLS OR EXPERIENCE. 1
- EMPLOYER THINKS TOO YOUNG OR TOO OLD 1
- OTHER PERSONAL HANDICAP IN FINDING JOB, INCLUDING RACIAL OR SEXUAL DISCRIMINATION 1
- COULDN'T ARRANGE CHILD CARE 1
- FAMILY RESPONSIBILITY 1
- IN SCHOOL OR OTHER TRAINING 1
- ILL HEALTH, PHYSICAL DISABILITY 1
- OTHER--SPECIFY: 1

GO TO Q.24

23. Why didn't you look for work?

CIRCLE ALL THAT APPLY

- NEW JOB TO START. 1
- EXPECTED TO GET OLD JOB BACK 1
- BELIEVED NO WORK AVAILABLE IN LINE OF WORK OR AREA . 1
- LACKED NECESSARY SCHOOLING, TRAINING, SKILLS,
EXPERIENCE. 1
- TOO YOUNG, TOO OLD. 1
- OTHER PERSONAL HANDICAP IN FINDING A JOB,
INCLUDING RACIAL OR SEXUAL DISCRIMINATION. 1
- COULDN'T ARRANGE CHILD CARE 1
- OTHER FAMILY RESPONSIBILITY 1
- IN SCHOOL OR OTHER TRAINING 1
- ILL HEALTH, PHYSICAL DISABILITY 1
- OTHER--SPECIFY: 1

24. Did you collect unemployment benefits during this time, after your job ended on (DATE IN Q.6)?

- YES 1
- NO (GO TO Q.28) 2

25. How many weeks did you collect unemployment benefits during this time?

[][] WEEKS

26. How much did you usually receive per week?

\$ [][][]/WEEK

27. Why did you stop collecting?

- REEMPLOYED. . . (GO TO Q.29). 1
- BENEFITS EXHAUSTED. 2
- STOPPED VOLUNTARILY 3
- DISQUALIFIED. 4
- OTHER (SPECIFY) _____ 5
- HAVE NOT STOPPED. 6

28. CODE WITHOUT ASKING IF KNOWN:

Have you done any work for pay since (DATE IN Q.6)?

- YES 1
- NO(GO TO END). 2

29. Who have you worked for since (DATE IN Q.6)? Tell me the names of all the companies, organizations and persons you've worked for, including any self-employed jobs you may have had since (DATE IN Q.6).

PROBE: Any others?

IF MORE THAN THREE, LIST THE FIRST TWO AND THE MOST RECENT.

FOR EACH EMPLOYER, ASK:

- a. When did you start working for (NAME OF EMPLOYER)?
PROBE FOR BEGINNING, MIDDLE OR END OF MONTH IF R CANNOT GIVE EXACT DATES.
- b. When did that job end?
- c. Did you work on that job continuously from (START DATE) to (END DATE)?

IF NO: I need to find out the dates of each time you worked for (EMPLOYER). When was the first time you stopped working there after (START DATE)? -and when were the other times you worked for (EMPLOYER) during the last year?

RECORD DATES OF ANY UNPAID INTERRUPTIONS OF ONE WEEK OR MORE, AND TREAT THESE AS SEPARATE JOBS.

NUMBER	JOB	DATES EMPLOYED	
		FROM	TO
_____	_____	____/____/____	____/____/____
_____	_____	____/____/____	____/____/____
_____	_____	____/____/____	____/____/____

NC

NUMBER JOBS ACCORDING TO START DATE FROM FIRST JOB AFTER DATE IN Q.6 TO MOST RECENT, AND ASK ABOUT JOBS IN THIS ORDER.

- JOB #1 = FIRST JOB AFTER DATE IN Q.6.
- JOB #2 = SECOND JOB AFTER DATE IN Q.6.
- JOB #3 = THIRD JOB, OR MOST RECENT IF MORE THAN 3.

I'd like to ask some questions about (this job/each one of these jobs).

	JOB #1 (FIRST JOB AFTER DATE IN Q.6)	JOB #2 (SECOND JOB AFTER DATE IN Q.6)	JOB #3 (MOST RECENT JOB IF MORE THAN 3)
30. Okay, now let's talk about the job you had at (EMPLOYER), where you worked between (DATES OF PERIOD).	FROM ___/___/___ TO ___/___/___	FROM ___/___/___ TO ___/___/___	FROM ___/___/___ TO ___/___/___
31. CODE WITHOUT ASKING IF KNOWN: Is this the same employer as the one you had on the job which ended on (DATE IN Q.6)?	YES .(GO TO Q.35) . . . 1 NO 2	YES .(GO TO Q.35) . . . 1 NO 2	YES .(GO TO Q.35) . . . 1 NO 2
32. What kind of company did you work for? What did they make or do?	_____ _____ _____ [[]]	_____ _____ _____ [[]]	_____ _____ _____ [[]]
33. What did you do there--what was your job?	_____ _____ _____ [[]]	_____ _____ _____ [[]]	_____ _____ _____ [[]]
34. How did you find this job? FOR JOB #2 OR 3, IF THIS IS SAME EMPLOYER AS JOB #1, CIRCLE 1 WITHOUT ASKING.	RECALL BY FORMER EMPLOYER. 1 PRIVATE EMPLOYMENT AGENCY 2 STATE EMPLOYMENT AGENCY/STATE JOB SERVICE 3 FRIENDS AND RELATIVES 4 WANT ADS 5 UNION HALLS 6 DIRECTLY WITH EMPLOYER. 7 OTHER: SPECIFY . . . 8 _____ NC	RECALL BY FORMER EMPLOYER. 1 PRIVATE EMPLOYMENT AGENCY 2 STATE EMPLOYMENT AGENCY/STATE JOB SERVICE 3 FRIENDS AND RELATIVES 4 WANT ADS 5 UNION HALLS 6 DIRECTLY WITH EMPLOYER. 7 OTHER: SPECIFY . . . 8 _____ NC	RECALL BY FORMER EMPLOYER. 1 PRIVATE EMPLOYMENT AGENCY 2 STATE EMPLOYMENT AGENCY/STATE JOB SERVICE 3 FRIENDS AND RELATIVES 4 WANT ADS 5 UNION HALLS 6 DIRECTLY WITH EMPLOYER. 7 OTHER: SPECIFY . . . 8 _____ NC
35. Counting overtime and paid lunchtime, how many hours per week did you usually work on that job?	[[]] HOURS	[[]] HOURS	[[]] HOURS

	JOB #1	JOB #2	JOB #3
36. How much were your usual weekly earnings on this job, before taxes and other deductions? Include tips, commissions and overtime.	1 \$ [] [] , [] [] [] PER WEEK	1 \$ [] [] , [] [] [] PER WEEK	1 \$ [] [] , [] [] [] PER WEEK
	2 \$ [] [] , [] [] [] PER MONTH	2 \$ [] [] , [] [] [] PER MONTH	2 \$ [] [] , [] [] [] PER MONTH
	3 \$ [] [] , [] [] [] PER YEAR	3 \$ [] [] , [] [] [] PER YEAR	3 \$ [] [] , [] [] [] PER YEAR
	IN-KIND ONLY NA	IN-KIND ONLY NA	IN-KIND ONLY NA
37. IF THIS IS CURRENT JOB, CODE 3 WITHOUT ASKING. Did you look for work at all after this job ended?	YES 1	YES 1	YES 1
	NO . . (GO TO Q.39) . 2	NO . . (GO TO Q.39) . 2	NO . . (GO TO Q.39) . 2
	CURRENT JOB (GO TO Q.42) . 3	CURRENT JOB (GO TO Q.42) . 3	CURRENT JOB (GO TO END) . . 3
38. How many weeks were you actively looking and available for work after this job ended?	[] [] WEEKS	[] [] WEEKS	[] [] WEEKS
	WHOLE PERIOD NA	WHOLE PERIOD NA	WHOLE PERIOD NA
39. Did you collect any unemployment benefits during this time?	YES 1	YES 1	YES 1
	NO . . (GO TO Q.42) . 2	NO . . (GO TO Q.42) . 2	NO . . (GO TO END) . 2
40. How many weeks did you receive unemployment benefits during this time?	[] [] WEEKS	[] [] WEEKS	[] [] WEEKS
	WHOLE PERIOD NA	WHOLE PERIOD NA	WHOLE PERIOD NA
41. Why did you stop collecting benefits this time?	REEMPLOYED 1	REEMPLOYED 1	REEMPLOYED 1
	EXHAUSTED BENEFITS . 2	EXHAUSTED BENEFITS . 2	EXHAUSTED BENEFITS . 2
	DISQUALIFIED 3	DISQUALIFIED 3	DISQUALIFIED 3
	STOPPED VOLUNTARILY. 4	STOPPED VOLUNTARILY. 4	STOPPED VOLUNTARILY. 4
	OTHER (SPECIFY) _____ 5	OTHER (SPECIFY) _____ 5	OTHER (SPECIFY) _____ 5
	STILL COLLECTING . . 6	STILL COLLECTING . . 6	STILL COLLECTING . . 6
42. SEE Q.29. ARE THERE MORE JOBS TO BE ASKED ABOUT?	YES . (GO TO Q30, JOB #2) 1	YES . (GO TO Q30, JOB #3) 1	
	NO 2	NO 2	

43. This is the end of the interview. Thank you very much for your participation.

AM. . . 1

TIME ENDED: [] [] : [] [] PM. . . 2