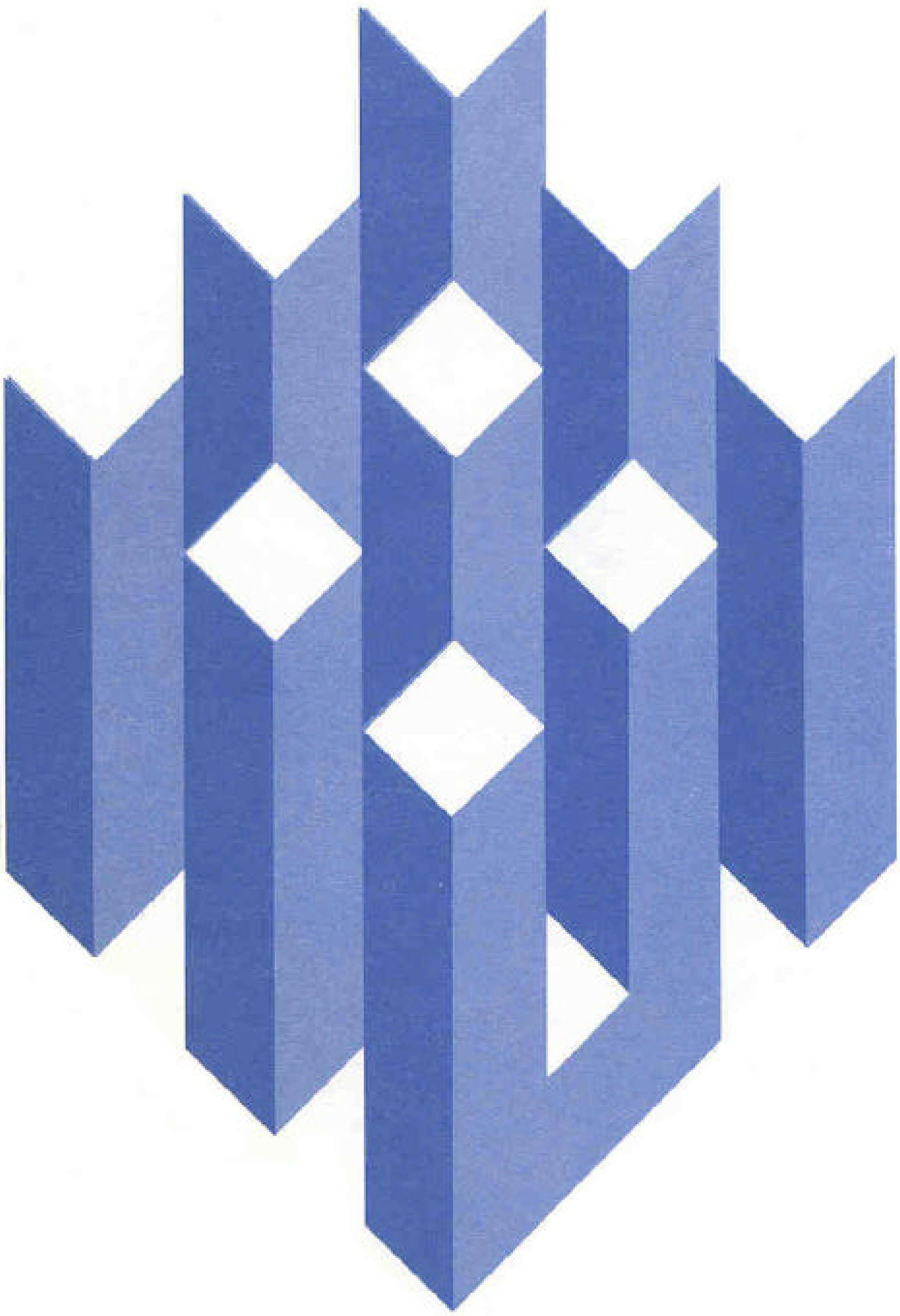
The Effect of the Duration of Unemployment Benefits on Work Incentives: An Analysis of Four Data Sets

Unemployment Insurance
Occasional Paper 85-4

Department of Labor
Employment and Training Administration



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U.S. Department of Labor
William E. Brock, Secretary

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Essential support for this project was provided by the project director at MPR, Walter Corson, who in addition to normal project-director duties provided advice and assistance at all stages of the project, from the analysis design to the data set construction to the analysis. The assistance of several individuals who provided us with data for the project is also gratefully acknowledged. These individuals are Dr. Stephen Hills of the Center for Human Resource Research at Ohio State University, Professor Arnold Katz of the University of Pittsburgh, and Professor Harvey Rosen of Princeton University. Excellent research assistance on the project was provided by Carl Campbell of Princeton University and by Meg Beardslee, Steve Fillebrown, and Lee Ratzan of MPR. Joan Mattei of MPR also made the JSARP extract with characteristic efficiency. Finally, Annette Kasprack produced the final report with her usual speed and accuracy.

Robert Moffitt

EXECUTIVE SUMMARY

This report is concerned with the effect of changes in the potential duration of unemployment insurance benefits on the length of spells of unemployment and nonwork. An unemployment spell is a period of time during which the individual is looking for work, and a nonwork spell is a period of time during which the individual is either looking for work or out of the labor force. The report is particularly concerned with the effect of benefit extensions, such as the Extended Benefit (EB) program, during recessionary periods. During recessions, exhaustion rates under regular UI rise and unemployment spells lengthen. This study is concerned with whether benefit extensions during such periods increase the unemployment spell beyond what it would be otherwise, beyond what is necessary to find suitable employment. Benefit extensions provide an incentive for individuals to lengthen their spells, but whether they act on it or not is an empirical question.

Extensions of benefits have been under active consideration for several years, as evidenced by the changing nature of the EB program, the enactment of the Federal Supplemental Benefit (FSB) program in 1975, and the current Federal Supplemental Compensation (FSC) program. The issue of whether these extensions of benefits have work disincentives has always been important. Unfortunately, the current state of knowledge regarding the existence and magnitude of any such disincentives is very far from what is needed for solid policy decisions. Several estimates are available, but they do not fall into a narrow range and are subject to many criticisms.

The aim of this study is to narrow the range of estimates of work disincentives of benefit extensions. To this end, several sets of data—some used in past studies and some new ones—were collected and analyzed. Identical statistical methodologies were used on all different data sets to ensure uniformity of treatment (many past studies used different statistical methodologies).

The results of the study are shown in Table 1 for the three data sets for which we could obtain reliable estimates. From the results it appears that the effect of a one-week extension in potential UI duration increases the spells of males by .17-.45 weeks and increases the unemployment spells of females by .10-.37 weeks. The nonwork spell is increased by more, by .52 weeks for males and .66 weeks for females (also this was obtained only for one data set). These ranges are considerably narrower than those obtained from past studies. However, the ranges are still larger than is desirable, and in the conclusion to our study we suggest further research that could narrow the ranges further.

Several other findings were obtained:

1. No significant effects of increases in potential duration on the reemployment wage rate were found. This is similar to the findings from past research.

2. No significant pattern of effects of increases in potential duration on the work effort of other members of the household (other than the recipient of UI itself) was found. This is a new finding which has not been examined in prior research.

3. Some evidence was obtained that the effects shown in Table 1 increase when the unemployment rate is higher. This evidence is weak because the statistical significance of the coefficients upon which this conclusion is based was very low.

4. The effect of a sudden unexpected increase in potential duration on mean unemployment spells is smaller than that shown in Table 1. The effects shown in Table 1 are only applicable if the increase in duration occurs at the beginning of the spell, or before the spell begins.

TABLE 1

SUMMARY OF ESTIMATES OF EFFECT OF ONE-WEEK
INCREASE IN POTENTIAL DURATION

Data Set	On Length of Unemployment Spell		On Length of Nonwork Spell	
	Males	Females	Males	Females
Continuous Wage and Benefit History[a]	.17	.10	—	—
Job Search Assistance Research Project	.45	.28	.52	.66
Newton-Rosen	.17	.37	—	—

[a] Whites only.

I. INTRODUCTION

This report is concerned with the effect of changes in the potential duration of unemployment insurance benefits on the length of spells of unemployment and "nonwork." Nonwork is defined as time spent either unemployed or out of the labor force. We will be particularly concerned with the effect of benefit extensions on these two variables during recessionary periods. During recessions, exhaustion rates under regular UI benefits rise and unemployment spells lengthen, reflecting the increasing difficulty with which the unemployed can find new jobs. Extension of benefits during recessions is intended to provide income to unemployed workers for a longer period of time, presumably allowing them more time to find suitable work. However, the extension of benefits at the same time may provide an incentive to increase the unemployment spell beyond what it would be otherwise, beyond what is necessary to find suitable employment. To the extent these incentives are acted upon, the extension of benefits has the unfavorable and unwanted effects of increasing the length of unemployment spells, of keeping the unemployment rate above what it would be otherwise, and of lowering productivity and output in the economy.

Extensions of UI benefits beyond regular state durations have been under active policy consideration for many years. The federal government first provided support for the extension of benefits during a recession in the late 1950s. Subsequently, extensions were provided again in 1961 and, after several years of Congressional activity in the later 1960s, Extended Benefits (EB) were provided on a permanent basis (under conditions of high unemployment) in 1970. The EB program provides federal support for state programs extending benefits 50 percent beyond regular state durations, up to a maximum of 39 weeks. In 1974 and 1975, an additional program of Federal Supplemental Benefits (FSB) was enacted in response to rising exhaustion rates and lengthening unemployment spells of the 1974-1975 recession. It eventually provided up to 65 weeks of unemployment benefits. As the recession subsided, FSB benefits and durations were subsequently reduced or otherwise restricted, and the program was finally allowed to expire in 1978.

The issue of whether these extensions of benefits have work disincentive effects by lengthening unemployment spells has always been recognized to be important. At issue in most discussions is whether these work disincentives occur in any substantial magnitude, and how they vary with the state of the economy, with the potential duration of benefits, and with other factors. It is generally presumed that, at some point, extensions of benefits to very long durations will generate substantial work disincentives in full-employment periods. For example, potential durations of 65 weeks are no doubt far more than necessary in such periods. But whether this duration is overly generous in recessions such as that in 1974-1975, or that from which the U.S. is currently recovering (unemployment rates rivaled, and occasionally exceeded, those of 1974-1975), is a more difficult question. Some evidence suggests that FSB was overly generous in 1974-1975. For example, the fact that exhaustion rates (of FSB) fell during that recession after the enactment of FSB suggests that the 65-week maximum was more than was necessary (Corson and Nicholson, 1982). If this is correct, it raises the question of what the potential duration should have been at that time. Would the 39-week maximum provided by EB alone have been sufficient? If not, what duration between 39 and 65 would have been "best?"

It should be stressed at the outset that there is unlikely to be a single "answer" to the work-disincentive question, for the magnitude and importance of the disincentives are likely to vary substantially in different circumstances. It is important for the policymakers' sense of the tradeoffs involved to know whether, say, the 13-week extension provided by the EB program lengthens unemployment spells by 1 week or 6 weeks, for example. The former value may be acceptable while the latter may not. In addition, the implications for the budgetary costs of the extension are very different in the two cases. But these work incentives may also be different if the unemployment rate is 6 percent or 9 percent. Plus, it may vary depending upon whether the unemployment is regionally concentrated or dispersed evenly across the country; on the levels of the benefits involved; on the characteristics of the unemployed; and on other factors. The business cycle may also exert a more subtle influence on the magnitude of the work disincentives if the magnitude varies with past and future expected trends in the unemployment rate. The absolute <u>level</u> of the unemployment rate may have less impact on work disincentives than whether the economy is going into or coming out of a recession. The decision of the individual is based largely upon expectations of the future, and, consequently, the effect of the extension of benefits on decisions will vary according to those expectations.

Ideally, what is needed for informed policy discussion is an entire menu of options with associated work-disincentive magnitudes. Each item on the menu would consist of the expected work disincentive effects if benefits are extended from a maximum of "x" to a maximum of "y;" if the unemployment rate is "z;" if we are at a certain stage of the business cycle; and so on. Policy choice from the menu could then be made on an informed basis regarding the tradeoffs between work disincentives and unemployment compensation. Also, for example, such information could provide the basis for setting the trigger conditions for an automatic program like EB--the trigger conditions could be set so that the extension would kick in only when the expected work disincentives fall to acceptable levels (there are other considerations, of course, that could go into setting triggers). The estimates will, of course, also be informative concerning the disincentive effects of the regular UI program, and they will be useful for policy discussions concerning potential duration changes in that program. In addition, although the effect of potential duration on reemployment wages has not been emphasized, we should note that a second menu giving such wage effects under all the same circumstances should also be of considerable importance in any policy decision. If the lengthening of unemployment spells induced by benefit extension were accompanied by higher reemployment wages, the extension would presumably be judged more favorably.

Unfortunately, the current state of knowledge regarding the effects of benefit extension on spell lengths and reemployment wages is very far from what is required to construct the necessary menu. While there have been a handful of studies of the effects of potential UI duration, which we review below, they do not provide results in sufficient detail or reliability for policy purposes. Some studies examine the effects of variation in existing durations under the regular state UI program, while others examine the effects of EB and/or FSB. Some studies are performed on data taken from recessionary periods, and some are not. For these reasons, and perhaps others, their results are extremely varied, indicating that one extra week of potential benefit duration lengthens the unemployment spell by somewhere between .8 weeks and zero weeks. Studies of reemployment-wage effects are even more scarce and the results more varied.

In this report we present the results of analyzing several different data bases, some of which have been used in prior studies and some of which have not. The motivation behind our study is to estimate the same model on several different data bases. In this way, we can determine whether the differences across results obtained by previous analysts were caused by different model specifications or different data bases. We, therefore, first specify a simple model that can be estimated on all our data bases, then we estimate it on all of them. Additional tests are then performed, varying somewhat from data base to data base depending upon the type of data being used. Our aim is to narrow the range of estimates from prior studies, and therefore to give a more reliable and solid base for the formulation of policy. We also examine business-cycle effects and intra-household UI effects in our studies, again varying from data set to data set depending upon the type of data being used. Our data sets also cover a range of different types: two are from household surveys and two are from UI administrative records. One of the household surveys includes selected UI administrative data, the other does not. Two of the studies are from the 1974-1975 recession period, while two cover the period from 1979 to the present, thus ranging across a varying macroeconomic environment. Thus, again our interest is in comparing the results across data sets. We also use a variety of different econometric models—ordinary least squares, Tobit analysis, exponential fits to unemployment spell distributions, and a nonparametic approach not used before in the UI literature.

Outline of Report. In the next chapter following this one we review previous research on the effects of duration extensions and show that estimates on a common specification across several data sets is warranted. We then review the methodological issues in estimating the effects of potential duration on the lengths of unemployment and nonwork spells. In the subsequent four chapters we discuss the results of our analyses of our four data sets—the Continuous Wage and Benefit History, the Job Search Assistance Research Project, the Federal Supplemental Benefit data, and the Newton-Rosen Georgia UI data set. In each chapter we decribe the data and show the results of estimating a common specification. Additional tests of several hypotheses are also presented in each chapter. Finally, in Chapter VII, we summarize our results and suggest avenues for future research.

II. PREVIOUS RESEARCH AND MODELING ISSUES

A. REVIEW OF PAST STUDIES

To begin, we shall briefly review past studies on the effect of benefit duration on the length of unemployment spells and reemployment wages. Table II.1 shows the results of most of the studies to date. In the eight studies reporting an effect of extending benefits on the length of the unemployment spell, the estimates range from zero to .80. The five studies reporting the effect of extending benefits on reemployment wages show estimates ranging from zero to $30 per quarter. As discussed above, these ranges are far too wide for use in policy analysis. The range must be narrowed substantially before these results can be useful.

A common approach to narrowing the range of estimates in this type of situation is to attempt to explain the differences by post-hoc reasoning. This approach has been taken many times in the labor-supply literature, where wide ranges of estimates of the effect of wage rates, nonwage income, and tax rates on hours of work have been obtained (Borjas and Heckman, 1978; Cain and Watts, 1973; Danziger et al., 1981; Moffitt and Kehrer, 1981). We have made a preliminary examination of this type for the studies of duration effects, and our results are shown in Table II.2. The table offers a comparison of the studies across many dimensions--time periods covered, equations estimated, variables specified, and so on. Consider the first column, showing the different time periods covered by the studies. Although the time periods are very different in different studies, there is no simple correlation between the time period used and the size of the estimate in Table II.1. For example, the two lowest

TABLE II.1

PAST STUDIES OF BENEFIT DURATION

	Data Set	Effect of One Additional Week of Potential Duration	
		On Weeks Unemployed	On Earnings Per Quarter
Ehrenberg-Oaxaca (1976)	National Longitudinal Survey	0	0
Holen (1977)	Special Five-City Survey	0.8	$2.50
Brewster et al. (1978)	FSB Recipients	0.4-0.6	0
Newton-Rosen (1979)	Georgia UI Recipients	0.4-0.5	--
Solon (1979)	New York UI Exhaustees	0.3	--
Kiefer and Neumann (1979, 1981)	Special Survey of Unemployed	--	0
Fishe and Maddala (1980), Fishe (1982)	Florida CWBH Sample	0.8	$30.00
Katz and Ochs (1980)	CPS	0.2	--
Moffitt and Nicholson (1982)	FSB Recipients	0.1	--
Nicholson and Corson (1978) and Nicholson (1981)	Aggregate Exhaustion Rates	Positive	--

TABLE II.2

COMPARISON OF DURATION STUDIES

	Time Period	States	Spell Lengths Included[a]	Sample Size	Exact U* Measure?[b]	Estimating Technique	Specification of U*
Ehrenberg-Oaxaca	1966-1967	All	ST	200-400	No	OLS	Linear
Holen	1969-1970	5 Cities	All	19000	Yes	OLS	Linear
Brewster Et al.	1974-1977	15	LT	700-800	Yes	OLS	Linear
Newton-Rosen	1974-1976	Georgia	ST,MT	600	Yes	Tobit	Quadratic
Solon	1972-1974	New York	MT	2200	No	OLS	EB Dummy
Kiefer-Neumann	Not Given	14	LT	500	No	Max. Like.	Linear
Fishe-Maddala	1971-1974	Florida	All	200	Yes	Max. Like.	Linear
Katz-Ochs	1968-1970 1973-1977	26	All	2900	No	Max. Like.	Piecewise-Linear
Moffitt-Nicholson	1974-1977	15	LT	400-600	Yes	Max. Like.	Nonlinear
Nicholson-Corson	1965-1974	All	All	50	No	OLS	Linear

Table II.2 (continued)

	Data on Compensated Weeks Only?	Wage Net of Taxes	Benefit Variable(s)[c]	Special Equation Characteristics	Special Sample Characteristics
Ehrenberg-Oaxaca	No	Yes	B/W	--	Estimated State of Residence Truncated Spell Lengths
Holen	Yes	No	B,W	--	Atypical sample
Brewster et al.	No	Yes	B/W	Interacted U* and B/W	--
Newton-Rosen	Yes	Yes	W(1-r)	Interacted U* and W(1-r)	--
Solon	No	No Wage	B	--	Atypical Sample
Kiefer-Neumann	No	No	B	--	Atypical Sample
Fishe-Maddala	Yes	No	B	(U*-U) entered[d]	--
Katz-Ochs	No	No	B,W	--	Movers Excluded CPS Retrospective
Moffitt-Nicholson	No	Yes	W(1-r)	--	--
Nicholson-Corson	--	No	B/W	--	--

[a] ST = short term, MT = medium term, LT = long term.

[b] U* = weeks of potential duration.

[c] B = benefit, W = wage, r = B/W = replacement wage.

[d] U = length of unemployment.

estimates (Ehrenberg-Oaxaca and Moffitt-Nicholson) were obtained from the 1966-1967 and 1974-1977 periods, one a period of very low unemployment and one a period of very high unemployment.

The studies often include different states, and the samples often differ in their relative proportions of short-term and long-term unemployed. In addition, not all data sets had precise measures of each individual's potential duration—many were estimated. These attributes of the studies are unchangeable characteristics of the underlying data. However, many of the other differences in the studies are related to choice of analysis technique. The estimating techniques used were ordinary least squares, generalized least squares, maximum likelihood methods of various types, and Tobit analysis (itself a maximum-likelihood method). In addition, the potential duration variable (U*) was often entered differently into the equations, as was the UI benefit variable. The wage variable was sometimes interacted with the benefit variable; the wage was not always net of taxes; and the benefit and potential duration variables were sometimes interacted and sometimes not. Some data sets had only compensated weeks available, requiring a different estimating technique than OLS (see Newton and Rosen, for example). Not shown in the table are the differences in the sets of independent variables included in the studies, for the sets were frequently very dissimilar. If such variables are correlated with UI variables for the benefit and potential duration, as they often are, then different sets of such variables will generate different UI coefficients.

The basic model underlying the different studies is also often very dissimilar. About half the studies employed a basically similar model in which the length of the unemployment spell is assumed to be a linear function of a set of variables including maximum potential duration. But the other studies differ, often in significant ways. The Katz-Ochs model is a continuous-time model assuming exponentially-distributed unemployment spells. The Kiefer-Neumann and Fishe-Maddala models are discrete-time reservation-wage models of job search. The Moffitt-Nicholson model uniquely models the nonlinearity of the effect of potential duration on the unemployment spells of differently-situated individuals.

Our conclusion from this brief comparison is that much of the difference in the estimates across studies may be a result of the differing models estimated and differing methods of handling the data. There is only one way of determining how much of the variance in the estimates can be explained by these differences, and that is by estimating a common set of models with common variable specifications and common econometric techniques across several of them.

In our study we employ four different data sets. The first is the Continuous Wage and Benefit History (CWBH) data set collected by DOL from state UI records. An early CWBH sample was used by Fishe and Maddala, and Fishe, but the new CWBH is considerably more comprehensive. Other studies in the past have used UI administrative records and the CWBH should be similar to those in its type. The second data set is the data from the Job Search Assistance Research Project (JSARP), an experimental program designed to test the effects of a subsidized search and employment program on the earnings and labor market success of enrollees. This data set covers the period 1979-1981 and comes from a household survey base, making it quite different from the administrative data sources. It also has the exact length of unemployment and nonwork spells of a random sample of the population. These first two data sets are our primary ones: the first an important UI administrative data source, the second a survey data source. A comparison of results across these two data sets should be instructive in learning the relative merits of the two types of data. The third data set we use is the FSB follow-up data set, one used in two of the previous UI studies and available at MPR. This data set covers the 1975-1978 period and consists of a number of former recipients of FSB and EB. Most of the data come from household survey sources. The fourth data set is that utilized by Newton and Rosen, which we simply term the "Newton-Rosen" data set. It covers one state, Georgia, in 1974-1975 and was drawn from UI records, unlike the previous two data sets.

We should also mention what data bases we did not consider and those which we considered but, for varying reasons, did not utilize. We rejected, on a prior basis, the data sets used by Holen, Solon, and Kiefer-Newman as being too idiosyncratic. They are highly specialized data sets conducted on narrowly defined populations, populations that may be very different than the general population of UI recipients. For this reason, if we were to analyze these data bases and determine that they give different results from the bulk of the others, we would never be able to determine how much of the difference was due to the specialized subpopulation. We also investigated several other data sets. The Ehrenberg-Oaxaça data set is no longer available, but we did obtain a data set used to replicate the Ehrenberg-Oaxaca results from Dr. Stephen Hills of Ohio State University. We did not employ that data set, however, because it was dominated by the JSARP data set (that is, JSARP has all the advantages of the NLS, plus some in addition, and no more disadvantages). The latter is a household survey, like the NLS, but is broader in scope (the NLS only included individuals employed at two points a year apart). The Fishe-Maddala set is also no longer available. The Katz-Ochs CPS data set was also obtained but was not used because, again, the JSARP data set dominated it. The CPS data come from a household survey but do not contain actual spells of nonwork and unemployment, only the number of weeks not working over a calendar year (plus the lengths of spells in progress as of the March survey date). Such data are difficult to analyze in a spell model. The aggregate data set used by Nicholson, and Corson and Nicholson was also not used because it is an aggregate data set and thus requires a different set of analytic techniques than the four microdata sets we use. In addition, only exhaustion rates can be analyzed with that data base, not spell lengths. Exhaustion rates are an important dependent variable, but not very useful for a study such as ours whose primary goal is to compare similar equations across data sets. Finally, several data sets not used by past analysts were investigated. The Survey of Income and Program Participation, a household survey data set covering 1979, was investigated but was found not to be ready for analysis at this time. The SIME-DIME data base was also investigated, which comes from an income maintenance experiment in Seattle and Denver over the period 1970-1974. This data set was also rejected because it is quite old, because it only covers two cities, and because it contains only a low-income sample of the population.

B. MODELING ISSUES

There are many econometric and modeling issues that arise in the estimation of the effect of potential duration of benefits on the length of unemployment and nonwork spells. In our study, we address what are some of the most important ones. The first set of issues we discuss all arise within the context of the most common model used in past studies, the linear spell model:

$$(1) \qquad U = a + bP + cB + dW + eX + \text{error term}$$

where the following variable definitions are used in the equation:

U = length of spell
P = potential duration of benefits from UI system
B = UI benefit
W = individual's pre-UI wage level
X = other variables such as sex, race, etc.

In this model the coefficient "b" measures the effect of increasing potential duration by 1 week on the length of the spell. The coefficient "c" measures the effect of increasing the UI benefit on the length of the spell. We also control for the level of the pre-UI wage in the regression, because both P and B are partly determined by W, yet W probably has independent effects on U.

(1) The first modeling issue we address in the report is the appropriate dependent variable to use—the length of the unemployment spell, the length of the nonwork spell, or the number of weeks of UI receipt. The length of the unemployment spell is the most common conceptual variable thought to be appropriate, but the other two variables have some advantages as well. The nonwork spell is by definition longer than unemployment spells because nonwork periods include periods in which the individual is not "looking for work." Such periods may be affected by

UI for several reasons. First, since the definition of "looking for work" is often obtained from a survey instrument not using the same definition of the UI offices, it is possible for individuals to be receiving benefits who are "not looking for work" by the survey definition. The survey definitions are often very strict, classifying individuals as unemployed only if numerous specific search actions have been taken (more than required by the UI offices, that is). Second, although benefits are not received during periods of nonsearch if the appropriate definitions are used, the receipt of UI benefits can have an effect on periods of nonwork if the individual uses UI benefits he or she has saved in the past to finance consumption during periods of nonwork. Finally, it is an empirical question whether there are any effects of UI on periods of nonwork above and beyond those on the unemployment spell, an empirical question which we examine with those of our data sets that have data on nonwork spells.

The weeks of receipt of UI benefits is the dependent variable that is used with UI administrative data by definition. This variable differs from the other two for several reasons. First, since UI benefits are truncated at the exhaustion point, the survey spells will often be longer than the weeks of benefits received. Second, since the weeks of UI benefits may be interspersed with weeks of either employment, unemployment, or nonwork, the lengths of the survey spells may again seem longer if the intervening weeks are weeks of unemployment or nonwork, and shorter if the intervening weeks are weeks of employment. Third, as noted above, the definition of required job search in the UI system is often different from that used in survey questionnaires, giving rise to some differences in spells from that source.

(2) A second equation specification issue is the proper variable to use for the benefit, B. A number of authors have tried different specifications of this variable, with three main alternatives: (a) the use of B as in the equation written in (1); (b) the use of the replacement rate, B/W, in place of B; and (c) the use of the "net wage" in the equation, replacing the variables B and W with the variable $W(1-r)=W(1-B/W)=W-B$. In this last specification, it is the difference between the wage and the benefit that is entered, on the theory that only the difference determines the net cost of not going back to work in each week. Economic theory in its usual form leads to a specification of the third alternative, with W-B entered in the equation. But this has the potential of being rather restrictive, for it forces the coefficient on B to be the opposite sign and magnitude of that on W. In fact, the magnitudes of the two may be different for a number of reasons, since the UI benefit may have many other effects on U other than that working strictly through the cash benefit (for example, there are search requirements for UI), and since the wage W is not the sole determinant of going back to work. If the two effects are different, then it is dangerous to constrain the two coefficients to be the same because one may be really significant and the other not, yet the common coefficient will be either significant or insignificant. For example, the variable W usually has very strong effects on U in most unemployment regressions. If the coefficient on (W-B) comes out strong and significant, it is unclear whether the variable B is really exerting any significant influence on U.

The use of the replacement rate is perhaps the most common method of specification but it has the disadvantage of being fairly restrictive.

Since the variable is a ratio of B and W, there is again the problem that its coefficient reflects both the influence of B and W, just as in the last example. The difference here is that W is also entered elsewhere in the equation, so its effect is controlled. But since W is entered nonlinearly in the replacement ratio (i.e., 1/W), it is still possible for W to exert an independent influence through the replacement rate variable. For example, it is quite possible that the two variables W and 1/W would both be significant in the equation; this would be a sign of an inverse quadratic relationship between W and U. Consequently, we again cannot be positive that the replacement rate coefficient is fully reflecting the influence of B and not W. The idea behind the replacement ratio is, of course, that the effect of B should be in some sense relative to the individual's value of W. This makes the replacement ratio a useful summary index of the generosity of UI benefits in general, but it is not necessary in a regression equation where W is elsewhere in the equation, for in that case the B coefficient is already implicitly measured relative to W.

(3) A related specification issue concerns whether the variable W should be pre-tax or post-tax, and, if the latter, how the post-tax wage should be calculated. Most analysts agree that in theory the post-tax wage should be used, and that it should be used not only in the W variable in equation (1) but also in the (B-W) variable or the replacement ratio if either of those two variables is also in the equation. The question is whether empirically it makes much difference, which is a question that can be answered only with empirical work. We will explore this in some of our work below. A more complex issue, one we have not explored in depth, is whether to use a "marginal tax rate" concept, an "average tax rate"

concept, and when to measure the rate. The marginal tax rate concept is most favored by economists because economic theory suggests that most behavior is determined on the margin. But the correct marginal tax rate to use in such a model is not the marginal tax rate on the pre-UI wage or pre-UI income, but rather the marginal tax incurred if one were to return to work in a particular week. Since the federal tax system is on an annual basis, this marginal tax rate will be determined by the amount of earnings and income incurred prior in the year, possibly before the spell began, and by the remaining period in the year, assuming the job is held throughout the rest of the year. Further, since the federal income tax is progressive, the marginal tax rate changes over the length of the spell, and it should be the whole tax schedule that affects an individual's decision. Clearly, this is a fairly complex matter, and one we leave to further research.

(4) An issue we have discussed before is the proper set of variables to include in X and how to define them. We do not do a great deal of sensitivity testing in this report to this choice of variables because of the nature of our study. Since we are using four different data sets and wish to estimate a common specification across all of them, we are constrained to adopt a set of X variables which can be obtained on all four data sets. As it turned out, there were very few. The Newton-Rosen data set, which only contained about 40 variables, played the most important role in restricting our choice of X variables. In the end, our "basic" specification has only four variables: age, race, sex, and the unemployment rate. For some of the data sets we test additional variables

as well, but these are not included in our baseline, across-data-set specification.

(5) A more intrinsically econometric issue is the issue of truncation of the sample on the basis of the dependent variable. All four of our data sets have truncation present, but the most severe and the most related to our variable of interest (P) is that in the two UI administrative data bases, the CWBH data and the Newton-Rosen data. As mentioned previously, by definition, UI administrative data, which measure the length of the spell by the number of UI weeks for which benefits were paid, do not go beyond the exhaustion point. This results in biased estimates of the coefficient on P in equation (1) if ordinary least squares (OLS) is used. The reason is as follows. Suppose one has two groups of individuals, one of whom has a uniform duration of 25 weeks and the other has a uniform duration of 26 weeks. Anyone who exhausts benefits is observed to have exactly 25 or 26, depending upon which group he is in. Now suppose that all individuals in both groups have exactly the same unemployment-spell lengths: 27 weeks. Then, we know that in these data the true effect of the extra week of benefits is zero, since both groups have the same spell lengths, but the data set we have will show that the effect of an extra week of benefits lengthens the spell by one week. All individuals in the first group will have 25 weeks of benefits and all individuals in the second group will have 26 weeks of benefits, so it will appear as if the second group has been induced to stay out of work an extra week.

We pay considerable attention to this truncation problem in our report. Obviously, it has a potentially large bias. The example just given was constructed intentionally to indicate that the bias can be severe (the coefficient on P is generally thought to be between zero and one, so in the example the bias was at its maximum). Unfortunately, the issue is not an easy one to address econometrically. The most common approach to the issue adopted in the literature (by Newton and Rosen, and by Katz-Ochs) is to assume a specific parametric form for the spell distribution and to fit the spells as if they come from a truncated form of that distribution. Newton and Rosen assume the distribution of spells is normal, and Katz and Ochs assume that it is exponential. The Newton-Rosen assumption has the obvious disadvantage of implying that there are some spells with negative values, although this is not too important if the tail of the normal is small to the left of zero.

It is possible to get around the truncation problem with this approach because the truncation of the spells is modeled as part of the estimation. Take the exponential case, for example. If the sample consists of two groups, one with potential weeks at 30 and the other with potential weeks at 31, the estimation technique assumes that the shape of the first spell distribution will be exponential truncated at 30 and the shape of the second will be exponential trucated at 31. More important, the technique assumes that the mean will be a truncated mean--that is, a weighted average of the U's less than the truncation point and the value of U (=P) at the truncation point. Therefore, the technique will assume that if the truncation point is increased by, say, one week, the truncated mean will increase in part simply because the truncation point has changed--but the untruncated mean of the entire distribution (with which we are really concerned) may not have changed.

This technique for addressing the truncation problem was adapated from econometric techniques developed in the early 1970s in the labor economics literature. In the last few years, the analysts in that literature have come to the realization that these techniques have several problems. Perhaps the most important is that they rely upon a specific, parametric form of the unemployment distribution that is very difficult to test but may make a significant difference in the estimated coefficients. For example, the two studies mentioned above make different distributional assumptions, one assuming unemployment spells to be normally distributed and one assuming them to be exponentially distributed. The shapes of these distributions are very different: the exponential distribution declines monotonically as U increases, while the normal increases then falls. If the distribution assumed for the estimation is an "incorrect" distribution, then the coefficient estimates may be quite far off. If the distribution assumed is incorrect, then the change in the implied truncated mean when P increases by 1 may be quite far off. In our work below on the FSB and Newton-Rosen data, we do some testing of this issue. We find, as other analysts have in other areas, that the results are quite sensitive to the distribution assumed.

(5) Another issue of importance in the analysis of the effect of extended durations is the effect of a change in P in equation (1) on the duration of unemployment, U. The equation in (1) is usually based upon the assumption that the value of P at the beginning of the unemployment spell stays constant throughout the spell. It may be at regular-UI level (e.g., 26 weeks) or it may be at an EB level (e.g., 39 weeks) but it stays constant throughout. Yet, we know this is not the case for anyone in a

state which switches on or off EB, or for someone unemployed during the recent FSC legislation, which changed fairly frequently. In addition, beyond the narrow econometric problems we are discussing here, the issue is important because the effect of ad-hoc changes in federal duration legislation probably have different effects on behavior than a built-in program like EB, and the latter probably has a different effect on behavior than the regular UI program, which is in place permanently.

The econometric problem created by a changing value of P is immediately seen by realizing that, to estimate equation (1), some value or values of P must be entered. The easiest solution, which unfortunately also probably creates one of the most serious biases, is to enter a new value of P in equation (1) only for those still unemployed at the time of the change in P. This may cause a fairly serious bias because a certain definitional relationship is set up between the independent variable P and the dependent variable U that has nothing to do with behavior, but only reflects the fact that only those with long spells are assumed to be receiving the different value of P. For example, if EB switches on, only those with long spells and high values of U will face the higher value of P, so a positive correlation is set up that will bias the coefficient on P in a positive direction. For example, assume that the increase in P has no true effect on behavior--i.e., that the distribution of unemployment spells is the same regardless of P. Then, the technique under discussion will still give a positive coefficient on P, because those with higher values of U will, in the regression, have higher values of P.

The statement of the problem in these terms suggests as a solution some method of holding P constant in the regression. One solution would be to simply enter the P value at the beginning of everyone's spell and leave it at that. This is a misspecification of course, because some individuals will eventually be affected by a different value of P. Another solution is to enter all values of P for everyone. Here, for example, it would be important to enter new, higher values of P in the regression even for those whose spells were not long enough to experience the new values of P. Here, however, the different coefficients on the different values of P would be mish-mashes of different individuals' responses, some of whom would have a zero response (e.g., if their spell ended early) and some would have a nonzero response. This approach also does not control for the lengths of time the different values of P are in effect. A more sophisticated version of this approach that would eliminate this second problem would be to enter a single value which is a weighted average of the different P's over a fixed length of time, such as a benefit year, where the weights are the fractions of the period in which each value of P was in effect.

The Kaplan-Meier and Cox Models. The three problems of (1) truncation, (2) distributional assumption, and (3) varying P values can all be solved by an application of statistical models developed by Kaplan and Meier (1958) and Cox (1972). (See Kalbfleisch and Prentice (1977) for an exposition of these models.) These techniques have been used for some time in the biomedical sciences in the testing of laboratory animals and in clinical trials and experiments, but have only recently come to the attention of economists and other social scientists.

In the Kaplan-Meier (hereafter the KM) model, the truncated distribution of spells can be fit nonparametrically. This solves the truncation problem and does so in a way that does not require the assumption of a specific parametric distribution—essentially, the technique lets the data themselves say what the distribution is. The KM technique essentially fits what is called the "empirical distribution function" of the data. Ordinarily, this would only require, say, plotting the data and seeing what the distribution of spells actually looks like. It is more complicated because truncation is present, so the actual distribution of truncated spells in the data will not be equal to the true distribution of spells. The KM model works instead off what is known in reliability theory as the "hazard" rate—the probability that an individual will become reemployed at each point in time, given that he or she is still unemployed. If one knows the hazard rate at each point in time, then one can calculate the entire distribution function of spells. This is because the probability that, say, an individual has a spell less than 10 weeks long is the probability that he was unemployed only one week and left the second, plus the probability that he stayed unemployed two weeks and left the third, and so on—these probabilities are just the above-defined hazard rates. Alternatively, the probability that an individual's spell is greater than 10 weeks is the product of the probabilities that he or she did not leave at each of the preceding 10 week points, again, just a product of the hazards. Now, the KM technique accounts for truncation by calculating the hazard at each week only on those who have a possibility of exiting the following week. Suppose, for example, that one wishes to calculate the probability of becoming reemployed given that one has been unemployed for 5 weeks. To calculate the probability, KM takes all those who are still unemployed after 5 weeks of their spell, then excludes from the sample all those who have a potential duration of 5 weeks (because they have no "chance" of being observed to be reemployed the next week), and then calculates the fraction of the remaining sample that becomes reemployed in the 6th week. Thus, the probability of becoming reemployed is calculated only on the untruncated sample at each point in time (the sample changes of course, depending upon potential duration). Given an estimate of these correct hazard rates, a true distribution function can then be derived.

The disadvantage of the KM model, if it may be called that, is that it only provides an estimate of part of the distribution function—only up to the maximum potential duration. If the maximum duration in the sample is 39 weeks, for example, no hazard can be calculated for beyond 39 weeks so we do not know what the distribution looks like beyond that point. In the parametric techniques, on the other hand, an assumption is made about the entire distribution, even including that beyond 39 weeks. The estimates from those models could be used, in fact, to predict values of U in excess of 39 weeks, essentially by extrapolation. It is in some sense not a disadvantage of the KM model that it cannot do this, for the data intrinsically contain no information about the shape of the distribution in excess of 39 weeks anyway.

For present purposes, the KM model has the disadvantage of not having any independent variables, and our goal is to estimate the effect of P on U. One way to use the KM technique to obtain estimates of such effects is to apply the technique separately to different subsamples in which the value of P is different. This is a satisfactory first analytic technique, one which we employ below. But to control more generally for the other variables in the equation, the Cox model mentioned above must be used. The Cox model is similar to the KM model in that it makes no parametric assumption about the distribution of the data. But it incorporates any number of independent variables, albeit in a specific way—it assumes that a change in a right-hand-side variable affects all hazard rates at all points in time proportionately. This proportionality assumption is for analytic convenience, for if one allowed every independent variable to have a different effect on every different hazard at every different point in time, a very large and messy model would result indeed. We, therefore, use the Cox model in our work below.

Time-Varying Variables. The Cox model also has the advantage of allowing time-varying variables to be used as independent variables. This solves the problem of changing P above. In the technique, the hazard at each point in time is assumed to be a function of the values of the right-hand-side variables at that point in time, including the value of P then in effect. Then, the probability of, say, being unemployed for more than 10 weeks is equal to the product of the probabilities of not becoming reemployed in the first nine weeks—and each of these probabilities could have a different value of P or some other variable.

This approach to the problem of changing P values is not only econometrically satisfactory but more satisfactory conceptually as well. Essentially, the implicit model in equation (1) must be jettisoned—it assumes that an individual faces unchanging values of all variables, in a stable and stationary environment, and makes a U decision. Instead, individuals make decisions as they go along and in response to the events that are occurring at the time. Many of these decisions are unplanned because the changing nature of events cannot be forseen. The fact that a person is then observed to be unemployed after 10 weeks can be explained only by the entire history of events that has occurred in each of the past 10 weeks, i.e., by all that has gone before.

Other Truncation Problems. The discussion up to now has concerned the truncation problems created by the UI exhaustion point, a problem that arises with the UI administrative data sources we use. But the survey data also have truncation problems because individuals are interviewed at certain points in time, and many individuals will still be in the midst of their spells at the time of interview. Consequently, we do not observe their completed spell lengths—we only know that they will be equal to or longer than that elapsed at the time of the interview. This problem is present in the JSARP data and to a lesser extent in the FSB data (because most individuals had completed by the second interview data in the latter). Because the truncation in this case is not directly related to the value of P, it is of less concern than the prior truncation problem. But ignoring it will lead to biased coefficients, including that on P. We, therefore, do deal with this truncation problem as well. To do so, all the analyses we have already discussed are appropriate—the KM model, the Cox model, and so on can be applied to this truncation problem as well as any truncation problem.

The FSB data set has an additional truncation problem which, we discovered in the course of the analysis, is potentially quite severe. It is discussed at length in the chapter below on FSB, but it can be succinctly stated here since truncation problems are already under discussion. The FSB data set contains only individuals who were on EB or FSB. Consequently, it is truncated from the opposite direction as the UI administrative data--those with spells less than the EB or FSB qualifying date are not in the data set. As a result, those with greater values of potential weeks, including FSB--50 to 65, for example--are also those with longer regular UI durations. When comparing, for example, those with 60 and 65 weeks of benefits, one is also comparing a group with 24 vs. 26 weeks of regular UI benefits. Even if potential duration has no effect on U, the group with 65 potential weeks will have the higher average value of U--because anyone in that group who had 25 weeks of benefits will not be in the sample (they have been excluded because they did not make it to EB), whereas anyone with 25 weeks will be in the duration-60 sample because 25 weeks put them on EB.

In principle, the KM and Cox models discussed above could be modified to account for truncation from this direction. However, this was beyond the scope of the project, for it would require new basic statistical work and development of new techniques. Instead, the truncation was addressed by the more conventional method of assuming a specific parametric distribution and modeling the truncation in that fashion. The results are unsatisfactory, leaving room for future research.

III. ANALYSIS OF THE CONTINUOUS WAGE AND BENEFIT HISTORY DATA

The Continuous Wage and Benefit History (CWBH) data contain administrative and survey data from the UI systems in several states. Most of the data is administrative, containing data on benefit levels and receipt, but there is also a survey administered to the CWBH sample members which is also included in the data. The data cover the period from 1978 to the present (the present data set goes up to April 1983), although most states entered into the system at some data later than 1978. Most were in by 1979.

The advantage of the CWBH data relative to other data sets lies in its administrative source. Data on benefits and potential duration, and on weeks compensated by the UI system, are much more accurate than those obtainable directly from questions in household surveys. The disadvantages of the data also lie in its administrative nature, particularly in the fact that only the number of weeks compensated by the UI system are available. First, any behavior after the exhaustion point is not observed. Hence, the total length of the unemployment spell cannot be ascertained for exhaustees. This creates econometric problems that must be dealt with in the estimation procedure, and ignoring them creates incorrect estimates of the effect of extensions of benefits on unemployment spell lengths. Second, many individuals do not receive benefits continuously, but rather skip weeks during which benefits are not received. Since the data set is an administrative one, no information on the missing weeks is availab_e. Whether the individuals are not in the labor force, working, or for some reason do not wish to or cannot collect benefits is not known.

Consequently, the dependent variable in the analysis, the number of weeks compensated by the UI system, is an underestimate of the lengths of unemployment spells. Whether this problem causes bias is more difficult to determine, for it depends upon whether individuals who face a benefit extension will change the number of weeks they do not collect benefits as well as the number of weeks they do. Without knowing their activities during the intervening weeks, it is difficult to speculate on the sign and magnitude of any bias that might occur.

A. CONSTRUCTION OF THE SAMPLE

Over a million records are currently available from the CWBH, covering all the states involved for all their time periods up to April 1983. Given this large number of records, only a random sample need be used for the present analysis. In addition, several sample exclusions are made before an analysis sample is constructed. First, only individuals with a valid first payment date are included in the data. Second, weeks compensated must be positive. Third, the individual must have valid data on the basic variables needed for the analysis—sex, race, the weekly benefit amount, potential duration, and weeks compensated. Fourth, individuals who change states are deleted from the sample, inasmuch as their behavior is more difficult to specify. Fifth, individuals who have several months' gap between the first payment date and the first checkdate are deleted. Presumably for these individuals there was some delay in getting their first check, so the official first payment date does not accurately approximate the beginning of the payment of benefits. Finally, the individual must have a valid last payment date and the number of weeks between the first payment date and the last payment date must not diverge from the number of weeks compensated by the system by more than 10 weeks.

The random sample of the cases is a simple 1-in-10 random sample. On top of this analysis sample, a second group of cases is added for reasons that will become apparent below. This second subsample is a "constant-potential-duration" subsample, or, using the notation to be defined below, a "constant-P" subsample. It consists of individuals for whom the value of potential duration did not change over the first 26 weeks after their first payment dates, or over the first 39 weeks, if EB was in effect at the first payment date. Since there turn out to be very few people in this category—for most, potential duration changes at least once during the spell—a one-hundred percent sample of such individuals was drawn. This subsample is then added to the random 1-in-10 subsample. The resulting total analysis sample contains 5,167 men and 2,902 women.

The means of the main variables used in the analysis are shown in Table III.1. Men have an average of 13 compensated weeks and women have an average of 12 compensated weeks. The mean weekly benefit for men is $103 and that for women is $75. Potential weeks average 35 for men and 33 for women, considerably larger than 26 both because EB was in effect for many of the periods and because of FSC. Pre-UI weekly earnings (after-tax) average $168 for men and $106 for women. The net (after-tax) replacement rate is .50 for men and .79 for women.

B. ANALYSIS OF THE CONSTANT-P SAMPLE

Most of the analyses performed on these data are on the constant-P subsample. As was discussed in the previous chapter, changing values of P require new techniques and prevent simple estimation of the linear spells

TABLE III.1

MEANS OF VARIABLES USED IN THE ANALYSIS

	Males	Females
Number of Observations	5,167	2,902
Weeks Compensated (U)	13.3	12.1
Weekly UI Benefit (B)	$103.07	$74.68
Potential Duration at Start of Spell (P)	34.55	32.78
Net Pre-UI Weekly Earnings (W)	$168.25	$105.94
Years of Education	11.66	11.59
Age	36.70	38.09
Unemployment Rate at Start of Spell	8.79	8.29
Net Replacement Rate	0.50	0.79
Race (1=White)	0.80	0.75
Marital Status (1=MSP)	0.67	0.58
Number of Observations in Constant-P Sample	2,315	1,418

equation with a single P variable on the right-hand-side. We, therefore, deal with the varying-P sample separately and concentrate first on the constant-P subsample.

Table III.2 shows the basic set of regressions on this sample. The results for males show that the net wage has a significantly negative effect on spell lengths, the UI benefit has a significantly positive effect on spell lengths, and potential duration has a significantly positive effect. The latter implies that an extra week of benefits increases the unemployment spell by a little over one-tenth of a week. The other variables suggest that male spells are longer for nonwhites and the unemployment rate is higher. For females, the results show no significant net-wage effect but a significantly positive effect of benefits on weeks compensated. The coefficient on potential duration, P, indicates again a significantly positive effect, in this case implying that an extra week of benefits will increase the spell by about .19 weeks. Older women have shorter spells, though the races do not differ significantly in this case. Again, spells are longer when the unemployment rate is higher.

Table III.3 shows some sensitivity tests to the specification of the benefit and wage variables in the equation. This issue was discussed in the prior chapter, where it was pointed out that many analysts use the replacement rate or a "net cost" variable instead of the benefit in the regressions. Later in this report we will do similar sorts of sensitivity tests on the Newton-Rosen data (where the the authors employed the net-cost approach) and on the FSB data (where prior authors have used either the net-cost approach or the replacement-rate approach). The tests shown in the table were only performed for males. As the results show, the

TABLE III.2

OLS REGRESSIONS ON CONSTANT-P
SAMPLE (DEPENDENT VARIABLE = U)

	Males	Females
W	-.032*	-.019
	(.005)	(.015)
B	.066*	.055*
	(.013)	(.025)
P	.112*	.194*
	(.044)	(.050)
Age	-.004	-.046*
	(.245)	(.023)
Race	-2.200**	-.808
	(.541)	(.638)
Unemp.	.681**	.816*
	(.195)	(.261)
Intercept	5.976	2.139
R^2	.042	.035

NOTES: Standard errors in parentheses.
*: Significant at 10 percent level.

TABLE III.3

ALTERNATIVE SPECIFICATIONS OF UI BENEFIT - MALES (Dependent Variable = U)

W	-.029* (.005)	—	.006 (.005)
W(1-r)[a]	—	-.026*	—
B	.061* (.011)	—	—
r[a]	—	—	10.788* (2.006)
P	.146* (.036)	.175* (.034)	.146* (.036)

NOTES: Standard errors in parentheses.
*: Significant at 10 percent level.

[a] r = net replacement rate = B/W.

different specifications give quite different results. When a net-cost variable equal to (W-B), or W(1-r), is entered instead of W and B separately, the coefficient is significant and negative. Effectively, this specification constrains the coefficients on W and B to be of the same magnitude but opposite in sign. The coefficient on the net cost variable is close to the net-wage coefficient, but is quite far off from the UI benefit coefficient. This suggests that the two variables should be left separate rather than being combined. The use of the replacement rate instead of the benefit also changes the results. First, the net-wage coefficient alone becomes insignificant in this case. Second, the implied effect of the benefit (equal to the coefficient on the replacement rate divided by W) is about .10, considerably higher than the correct .06 coefficient. Here, it seems that the negative effect of the wage on spell lengths is partly being picked up in the replacement rate coefficient, so the coefficient does not wholly represent the effect of UI. Again, then, the separate, unconstrained specification seems superior.

Table III.4 shows the effect of additional estimations with different specifications. Estimates for both males and females are shown. First, a quadratic specification of P is introduced. The results show significantly quadratic effects for males but not for females. The second set of tests enters the logarithm of P instead of the absolute value of P in the equation. Again, both coefficients are significant, just as they were in the previous analysis. Next, the table shows the effect of interacting the P variable with the unemployment rate. The interaction coefficient is insignificant for males, but positive and significant for females. For the latter, then, the results imply that the disincentive

TABLE III.4

ADDITIONAL ESTIMATES

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
P	1.21* (.38)	—	.26* (.16)	.08* (.04)	.60 (.41)	—	-.16 (.20)	.18* (.05)
P^2	-.02* (.01)	—	—	—	-.01 (.01)	—	—	—
Log P	—	4.10* (1.07)	—	—	—	5.12* (1.24)	—	—
P * Unemp	—	—	-.02 (.02)	—	—	—	.05* (.03)	—
No Recall[a]	—	—	—	3.03* (.50)	—	—	—	4.97* (.61)

NOTES: Standard errors in parentheses.
*: Significant at 10 percent level.

[a] Variable = 2 if do not expect recall, = 1 if do expect recall.

effect of increasing P is greater when the unemploymnt rate is high. Finally, the effect of controlling for whether the individual is expecting to be recalled from his or her job is tested. As the results show, the effect of P is lowered by a small amount from what it was before.

C. CONTROLLING FOR TRUNCATION

As discussed in the previous chapter, the problem of truncation occurs in administrative UI data sets such as the GWBH. The potential bias from OLS is in an upward direction: since many individuals are exhaustees, an increase in P may cause a one-week increase in weeks compensated but no (or less of an) effect on true unemployment spell lengths. The implication is that the effects of duration estimated above may be too large.

The first approach to addressing the truncation problem is the assumption of a parametric distribution for unemployment spells. In other work in economics with truncated distributions, the normal distribution is most commonly assumed for this purpose. The technique which uses the normal distribution is called Tobit analysis. Tobit analysis controls for truncation in the data by assuming that the increase in weeks compensated when P increases will be composed of two parts: one part is the mechanical part resulting from the truncation of spells, and one part is the true effect. The former is implicitly estimated in the technique by the assumption of normally-distributed spells, for the implicit assumption is that the mechanical increase in mean spells is calculable from the truncated mean of a normal distribution. If unemployment spells are not in fact normally distributed, the allocation of the increase in weeks compensated between the true effect and the mechanical effect will be in error.

Table III.5 shows estimates of the Tobit model on the CWBH sample. Virtually all of the coefficients that were significant in the OLS regressions are also significant in the Tobit regressions. However, the coefficients on P are indeed somewhat lower than those in OLS. For men, the coefficient on P is .085 and for women it is .12. Recall that these coefficients were .11 and .19 in the OLS regressions, respectively. Thus, there are signs of truncation bias in the data, although these differences are not large.

The assumption of a normal distribution for the data is, however, not a good one. The normal assumes that there are negative as well as positive spells, and it assumes that the distribution of positive spells is normally shaped--that is, the density of observations rises from zero and then later falls, and the density is symmetric around the mean. Most unemployment spells do not have this distribution. A more common distribution to assume for unemployment is the exponential distribution. In this distribution, the density of observations falls continuously from zero, and there are no negative spell lengths.

Estimates of the equation assuming the exponential distribution are also shown in Table III.5. In these results, the effect of P is very much different. For men, there is no significant effect of P on spell lengths and, in fact, the sign of the coefficient is negative. For women, the coefficient is still positive but lower in magnitude and has a t-statistic of only 1.2. Thus, if the exponential distribution is the true distribution, the effect of P is much smaller than the OLS results indicated. Indeed, the effect of P in the male OLS regressions is, by implication, wholy the result of truncation bias.

TABLE III.5

PARAMETRIC MAXIMUM LIKELIHOOD ESTIMATES

	Males		Females	
	Normal	Exponential	Normal	Exponential
W	-.028* (6.453)	-0.17* (5.175)	-.009 (.638)	.012 .956)
B	.066* (5.604)	.073* (5.432)	.038 (1.553)	.001 (.040
P	.085* (2.286)	-.008 (.126)	.116* (2.384)	.078 (1.157)
Age	.029 (1.579)	.049 (1.578)	-.063* 2.500)	-.075* (2.383)
Race	-1.397* (2.501)	-2.354* (2.227)	-.127 (.179)	-.502 .519)
Unemp.	.698* (4.751)	.859* (.3450)	.536* (2.575)	.466 (1.524)
Constant	4.159	6.415	6.721	(0.23)

NOTES: T-statistics in parentheses.
*: Significant at 10 percent level.

The difficulty in discriminating between these models lies in their parametric nature. Whether the true distribution is normal, exponential, or some third distribution is not known. Since it appears that the results are in fact sensitive to which distribution is assumed, it would be more useful to conduct an analysis without having to make distributional assumptions at all. The Kaplan-Meier and Cox models discussed in the previous chapter are of this type.

D. NONPARAMETRIC MODELS

The Kaplan-Meier estimation technique is a nonparametric technique in which the distribution of unemployment spells is determined by direct examination of the data, instead of by a priori assumption. In its simplest form the KM technique merely involves plotting the data, thereby obtaining what is generally termed the "empirical distribution function." The complication that the KM technique addresses is the complication introduced by the fact that the data are truncated, and that different individuals are truncated at different points. A simple plot of the data on weeks compensated would therefore give an incorrect picture of the true distribution of spell lengths. The KM technique gets around this problem by deriving the distribution from what is termed the hazard rate, h. The hazard rate at time t, h_t, is defined as the fraction of those who are unemployed at time t who leave unemployment at that time. If one knows the hazard rate at each time t, then the probability that an individual will be unemployed exactly x weeks can be calculated. The probability of being unemployed exactly one week is h_1—that is, the probability of leaving unemployment in the first week. The probability of being unemployed exactly two weeks is $(1-h_1)h_2$; that is, the probability of not leaving unemployment the first week but leaving it the second. In general, the probability of having a spell which is completed in exactly x weeks is: $(1-h_1)(1-h_2)(1-h_3)\ldots(1-h_{x-1})h_x$. Thus, if all the hazards are known, the probability of each spell length can be calculated. Therefore, the mean spell length, which is a weighted average of all x's, can also be calculated using the probabilities as weights. In addition, the cumulative distribution function—the probability of being unemployed less than x weeks, or the probability of being unemployed more than x weeks—can also be calculated. Hence, the entire distribution is obtainable if the hazard rates are known.

The purpose of introducing the hazard rate into the analysis is that the truncation problem can be readily avoided by using it. This can be achieved by calculating the hazard rate from the data using only observations that are not truncated at that point. If an observation is truncated at that point, one cannot determine whether it does or does not leave unemployment; therefore, that observation is useless in determining the hazard and, by extension, useless in telling us anything about what the distribution of spells looks like beyond that point. Therefore, only the untruncated observations are used in calculating the hazards at each point. This means that the sample upon which the hazards are calculated will fall as the time considered increases—at greater time periods, more individuals have been truncated and cannot be used in the calculation. In the constant-P sample with which we are currently working, all individuals are truncated at 39 weeks or less. Hence, the hazards cannot be calculated beyond that point and we cannot say anything about what the distribution looks like beyond 39 weeks.

Figure 1 and 2 show a plot of the hazard rates for men and for women in the constant-P sample. Figure 1 shows that for males the probability of leaving unemployment in the first week is about .085, fairly high. The probability then drops, on average, until about the 10th week. Subsequently, the hazard begins to rise and, hence, the probability of returning to work increases. The hazard continues to rise and takes a sharp jump at 26 weeks. This suggests that the exhaustion of benefits induces many individuals to leave unemployment (recall that the truncated observations are not included in this calculation). After 26 weeks, the hazard continues to rise and, apart from a sudden dip in the 34th week, rises up to 37-39 weeks. Again, the fact that a fraction of the individuals in the sample have potential weeks of 39 suggests that this is affecting their decisions. Figure 2, which shows a similar graph for women, shows a generally similar picture. Hazard rates initially fall but then rise, with a peak at 26. Then they fall to a much greater extent than for males, but rise again in the 30's.

The average hazard rate can be determined either by simply averaging the hazards or by regression analysis. In the latter approach, one first calculates what is called the "survivor function"—namely, the probability at each time t that an individual will be unemployed for longer than that t. In the present data set, we have 38 observations on this survivor function, one for t=1 to t=28. When the logarithms of these survivor functions are regressed upon t, it can be shown that the coefficient is the negative of the hazard rate. The results of such regressions are shown in Table III.6. For males, the results show an average hazard of .08 and for females it is .072. If the distribution is

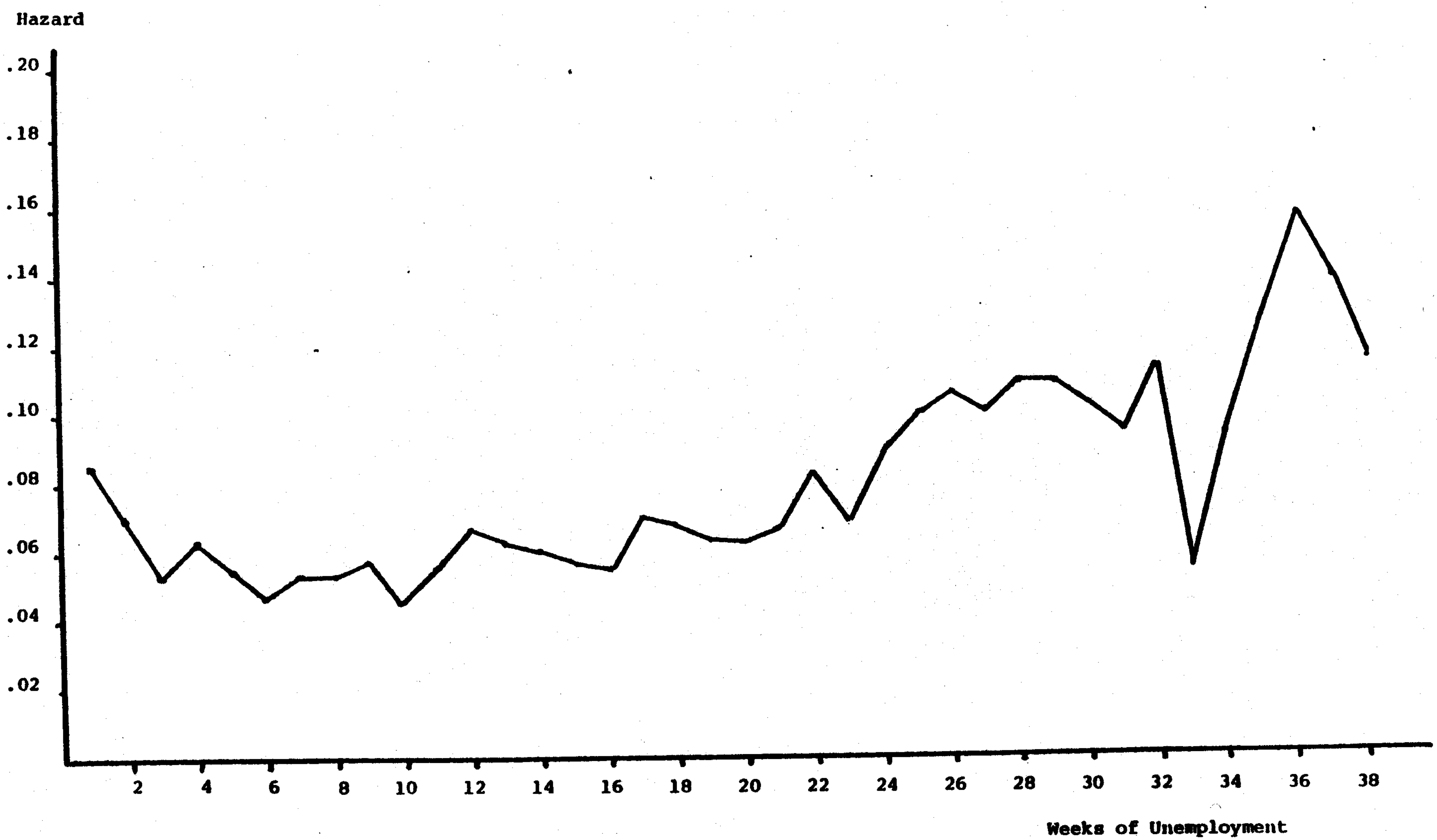

Figure 1: Male Hazard Rates

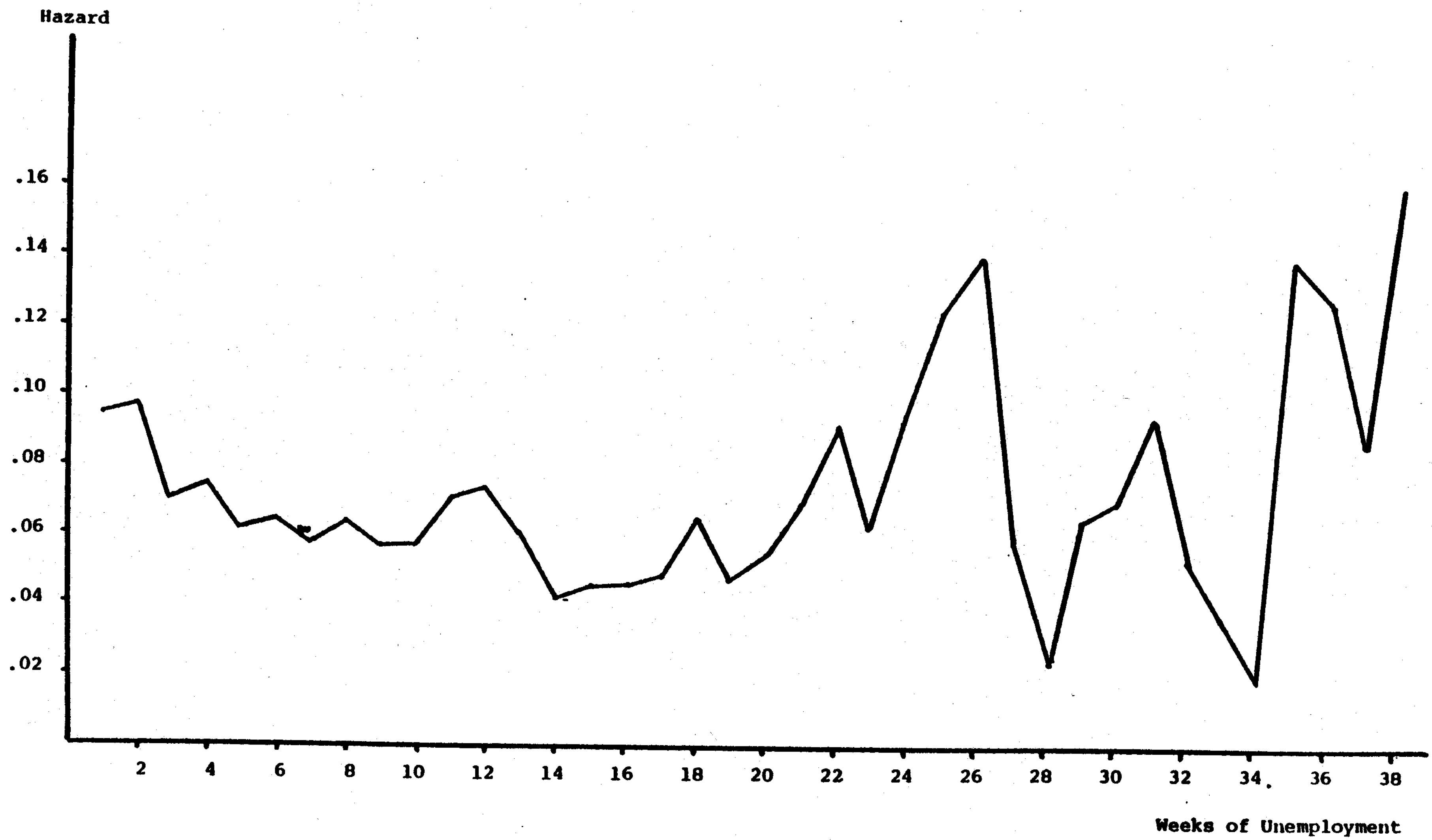

Figure 2: Female Hazard Rates

TABLE III.6

TESTS OF EXPONENTIAL DISTRIBUTION

	Males	Females
T	-.080*	-.072*
Constant	.264	.071
R^2	.977	.992

NOTES: Dependent variable = log of survivor function.
T = time (week)
Standard errors in parentheses.
*: Significant at 10 percent level.

truly an exponential one, the hazard rate is constant at this value. However, it is clear from the figures that the hazard rate is not constant.

The KM technique allows no formal provision for estimating the effect of a regressor variable on the distribution of unemployment spells. The Cox model to be discussed below does that. But the KM technique can be used in a simple way to do so, and that is by calculating the hazards and the distribution separately for subgroups of the sample who have different values for some variable. In particular, we are most interested in subsamples with different values of P. The graphs in Figure 1 and 2 are very suggestive inasmuch as it appears that individuals are taking the value of P into account in their behavior, but the effect of P on mean unemployment spells and on the distribution cannot be calculated from those figures. Instead, for the present analysis, the sample was subdivided into two groups: those with a P less than or equal to 26 weeks, and those with a P between 27 and 39 weeks. The hazards were then calculated separately for the two groups.

Figures 3 and 4 show the plots of the hazard rates for the two groups, for males and females. For both high P and low P groups, the hazard dips a bit in the beginning, but rises thereafter. The hazard is a bit higher for those with a larger P, but the difference does not become large until the 26-week point approaches. There, the hazard rate of those truncated at 26 weeks takes a large jump, clearly much higher than the hazard of those truncated at more than 26 weeks. Beyond 26 weeks the two groups cannot be compared.

Table III.7 shows the results of comparing these distributions by regression analysis. Again, the survivor function was calculated at each

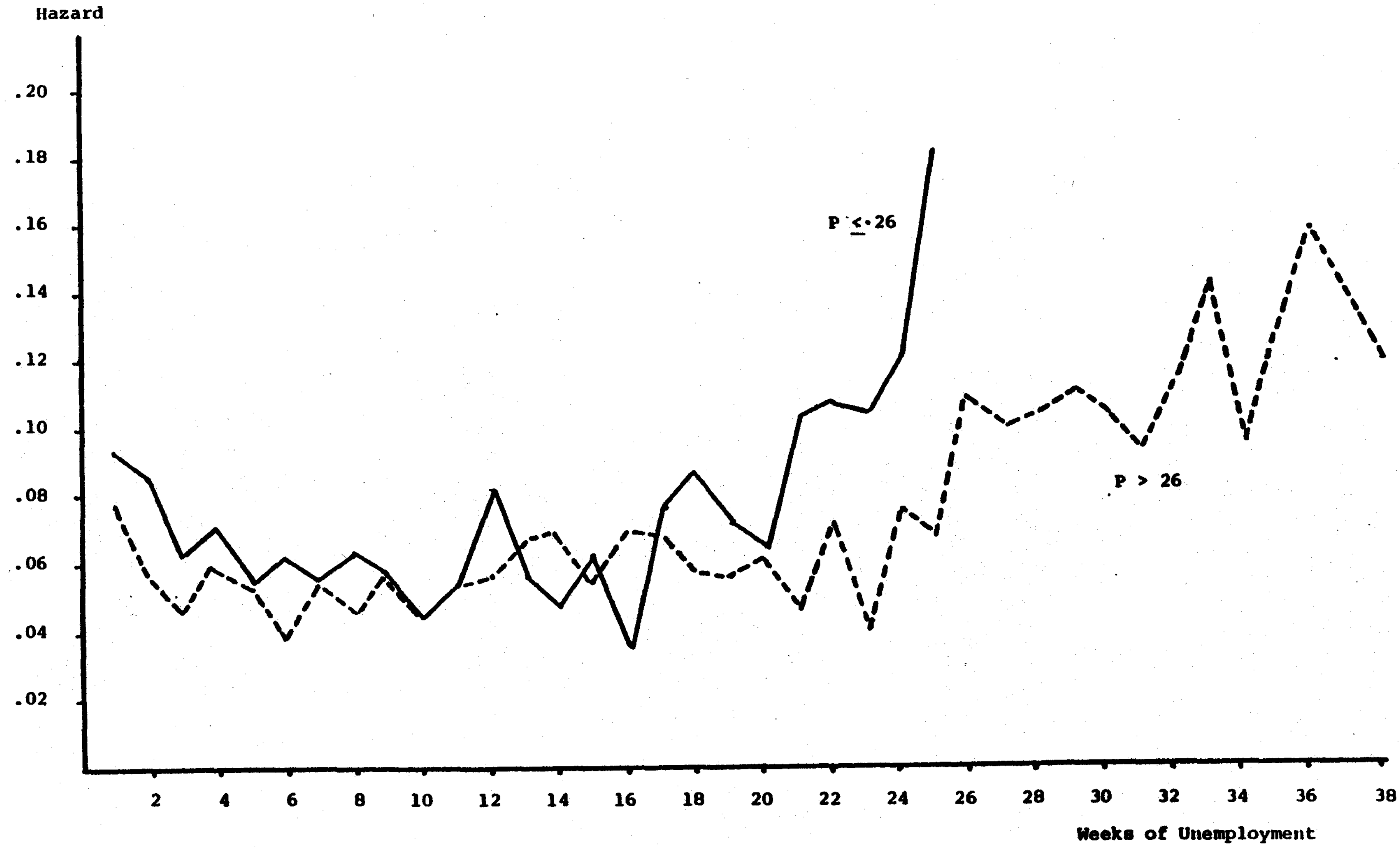

Figure 3: Male Hazard Rates by P

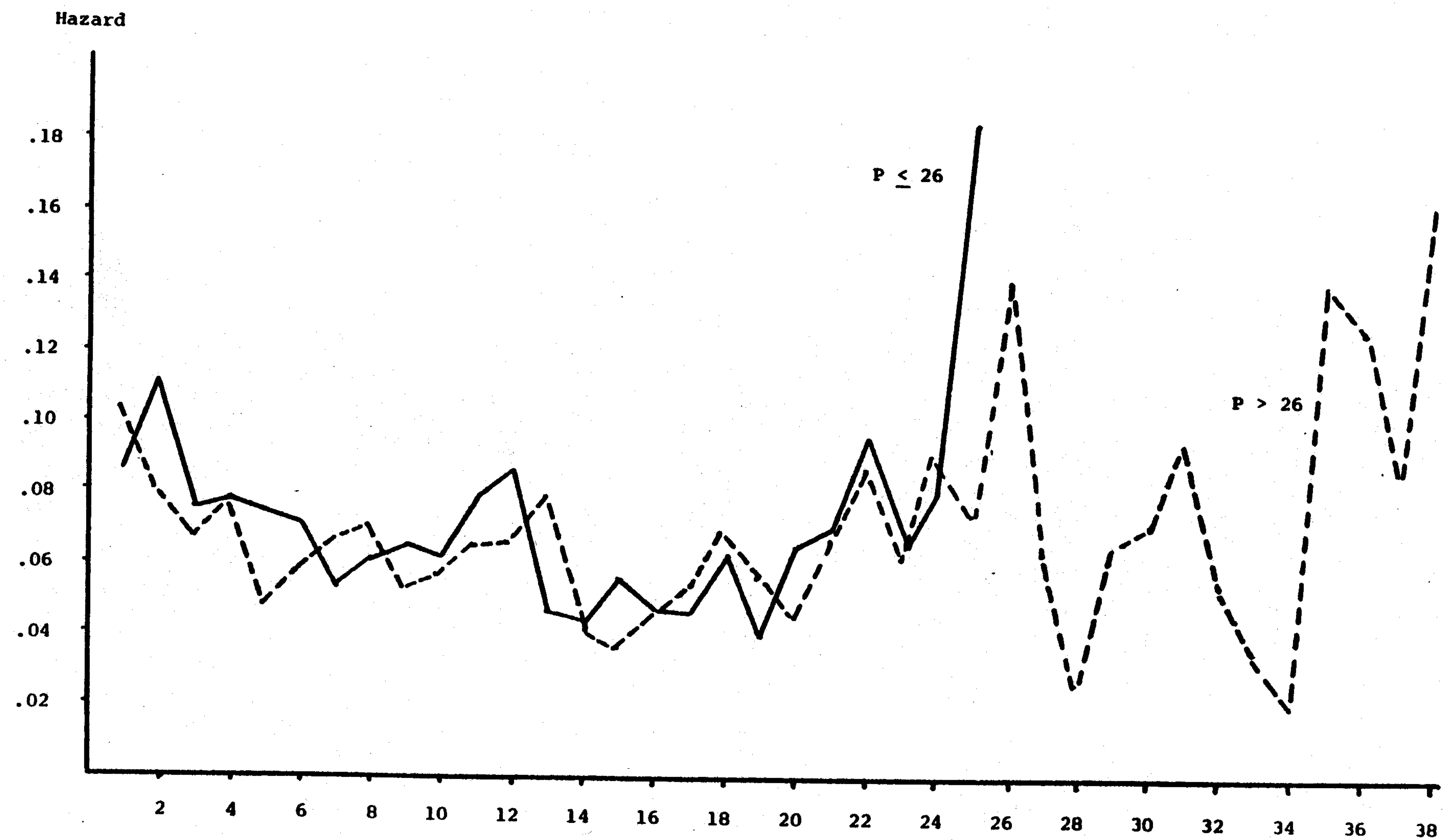

Figure 4: Female Hazard Rates by P

TABLE III.7

EFFECT OF P ON FITS OF EXPONENTIAL DISTRIBUTIONS

	Males				Females			
			Pooled				Pooled	
	$P \leq 26$	$P > 26$	(1)	(2)	$P \leq 26$	$P > 26$	(1)	(2)
T	-.071* (.002)	-.073* (.002)	-.076* (.002)	-.074* (.004)	-.066* (.001)	-.069* (.001)	-.069* (.001)	-.064* (.002)
D^a	—	—	.163* (.033)	.220* (.085)	—	—	.092* (.019)	.189* (.045)
TD	—	—	—	-.003 (.005)	—	—	—	-.006* (.002)
Constant	.078	.252	.177	.134	-.031	.086	.013	-.060
R^2	.981	.972	.975	.975	.992	.992	.990	.991

NOTES: Standard errors in parentheses.
*: Significant at 10 percent level.

[a] D=1 if in $P > 26$ sample, D=0 if in $P \leq 26$ sample.

point. The first two columns for each sex show the separate regressions for the two P groups. In both cases, the hazard is a bit higher for the higher-P group, although the difference is very small. More important, the next columns to the right show the effect of pooling the two samples and entering a dummy variable for the two. The dummy variable is significantly positive and quite large in magnitude. For males, for example, its coefficient is .163, indicating that the probability of leaving unemployment is 16 percent higher in the high-P group than in the low-P group at each point in time (on average). For females, the comparable percentage is 9.2 percent. The next regressions in the table test whether the hazards for the two groups are significantly different. The results show that they are not. Thus, the two groups have the same average hazard, but the high-P group has higher hazards at higher time periods.

These results suggest quite strongly that there are disincentive effects of P in the table. It is fairly clear from the plots in the figures above as well as from the regressions. The fact that the exponential ML results above did not show any effect now appears to be a reflection of the inaccuracy of the assumption of an exponential distribution.

A more general technique is that used in the Cox model. In the Cox model, the hazard at each point, h_t, is assumed to be:

$$h_t = H_t \exp(X\beta)$$

where H_t is an unknown constant at each time t, X is a set of regressor variables, and β is its coefficient vector. Thus, an increase in an X variable will affect the hazard rate and hence will affect the distribution and mean of unemployment spells. The important restriction in the Cox model is that the effect of all X variables is proportional over the entire time period. An increase in X has the same proportionate effect regardless of t. The Cox model can be used to control for truncation in a similar fashion to that used in the KM model, but the coefficient β can be estimated in the process.

Table III.8 shows the results of estimating the Cox model on the constant-P CWBH sample. It should be noted that the signs of the coefficients in this model will be opposite to those in the OLS equations, for an increase in X in the Cox model will, if β is positive, increase the probability of leaving unemployment; hence, the mean unemployment spell will drop. The results of the estimation show the expected signs on all the coefficients, but the significance levels and magnitudes of the coefficients are quite small. The P coefficient, in particular, is insignificant for both males and females. Its magnitude is also low. In the Cox model, the effect of an increase in an X variable on the mean unemployment spell length is approximately $-\beta\bar{U}$, where $\bar{U}$ is the mean of the spell distribution. Taking mean lengths of 13 and 12 weeks from Table III.1 for males and females, respectively, and multiplying them by the Cox P coefficients shows that a one-week increase in P has negligible effects on spell lengths.

The conflict between these results and those in the KM analysis probably lies in the proportionality assumption in the Cox model. It is clear from Figures 3 and 4 that the hazard rate is not proportionately affected by the difference in P: some hazards are higher, some are lower,

TABLE III.8

COX REGRESSIONS

	Males	Females
W	.0029	.0003
	(.0046)	(.0014)
B	-.0069*	-.0027
	(.0012)	(.0024)
P	-.0036	-.0047
	(.0041)	(.0051)
Age	-.0033*	.0055*
	(.0019)	(.0022)
Race	.1515*	.0191*
	(.0598)	(.0022)
Unemp.	-.0506*	-.0308
	(.0150)	(.02010

NOTE: Standard errors in parentheses.
*: Significant at 10 percent level.

and many are virtually the same. As the regressions above indicated, there is no difference in the average hazard rate between the two groups. Thus, the P variable seems to affect the shape of the distribution in more complex ways, ways that the Cox model in its present form does not accommodate. In principle, however, the Cox model could incorporate such factors if the effect of an X variable were allowed to vary with t. This could be done either by stratifying the X variables on t or by entering a variable for t into the regressor set. This would seem to be a useful area for future work.

E. VARYING-P SAMPLE

Given the extensiveness of the analysis that has been done with the constant-P sample, relatively little analysis was performed with the varying-P sample. However, the Cox model was estimated on the larger sample. About 54 percent of the sample had at least one change in P during their spells, and 18 percent had two or more. A virtue of the Cox model is that the X variables can be allowed to change over time. If an X variable, such as P, does change during a spell, the hazard rate will change at that point and at all points subsequent. Once the Cox model is employed, incorporation of this changing-P feature is relatively easy. Recall that it is very difficult, if not impossible, to incorporate changing P's into an OLS specification.

Table III.9 shows the results of estimating the Cox model on the full analy·is sample. The results were found to be quite different by race as well as sex, so separate estimates are provided by race and sex. For present purposes, the most important coefficient is that on P. For white males and white females, the coefficient is significantly negative, as

TABLE III.9

COX REGRESSIONS - VARYING P

	Males		Females	
	White	Nonwhite	White	Nonwhite
W	-.0002 (.0002)	.0022** (.0008)	-.0018* (.0007)	-.0014 (.0014)
B	-.0005 (.0006)	-.0121* (.0017)	.0018 (.0012)	.0044* (.0022)
P	-.0124* (.0018)	.0114* (.0042	-.0083* (.0022	.0020 (.0043)
Age	.0036* (.0001)	.0068 (.0024)	.0081* (.0011)	.0079* (.0023)
Unemp.	.0059 (.0091)	-.012 (.021)	.0344* (.0114)	.0694* (.0239)

NOTES: Standard errors in parentheses.
*: Significant at 10 percent level.

expected. For nonwhite males, it is surprisingly positive, and for nonwhite females it is insignificant. No ready explanation comes to mind for these results. But for whites, it appears that the changing value of P induces behavioral changes that the constant-P sample did not pick up. The assumption behind these regressions is still that P has a proportionate effect on the hazard, but in this case more of the variance in the P variable is a result of actual changes in P over time rather than differences in P across individuals or across states.

The magnitude of the effect of P on the unemployment spell can be calculated as discussed above. Using mean spell lengths, the coefficients imply that an increase in P of one week would increase white male spell lengths by about .17 weeks, and by about .10 weeks for white females. The model can also be used to estimate the effect of a change in P in the middle of the spells of a group of people who begin receiving benefits at the same point in time. Table III.10 shows the results of such a calculation. If P rises by one week at the very start of the spells of these individuals, U will increase by .17 and .10 for males and females, respectively. In other words, the effect will be the same as noted above, for the new value of P is known from the beginning. If the increase in P occurs at t=10, the effect on mean spell lengths in the sample is only .146 weeks for males and .007 for females. The drop occurs for two reasons. First, many individuals have become reemployed prior to t=10 and hence their spell lengths are not affected. Second, of those still unemployed at t=10, their cumulative probability of being reemployed at each subsequent time will still be lower than it would have been if the increase in P had occurred at t=1, for they are only affected by having an increased hazard

TABLE III.10

EFFECTS OF AN EXTRA WEEK OF BENEFITS
ON MEAN WEEKS OF UNEMPLOYMENT

	Males	Females
If at Beginning of Spell	.165	.101
If in Middle of Spell:		
At 10th week	.146	.007
At 20th week	.093	.004
At 26th week	.067	.003
At 30th week	.053	.002
At 39th week	.026	.001

at each t subsequent to t=10. The lower hazards for this group at all t less than 10 still apply. Thus, both factors work to lower the effect on spell lengths. The table shows further that if the increase in P occurs at yet later time points, the effect is still smaller. If the increase in P occurs at the 39th week or beyond, it will have little effect.

F. SUMMARY

The analysis of the CWBH data set suggests that a one-week increase in potential duration, if in effect at the beginning of an individual's spell, increases the length of the unemployment spell by about .17 weeks for white males and .10 weeks for white females. The effects for nonwhite males and females are lower and insignificant or negative. If the increase in potential duration occurs in the middle of the spell, smaller effects occur.

Several qualifications must surround these estimates. The most general one is that relating to the truncation of spells in the data by the exhaustion point. It was found that the method of controlling for the truncation, the distributional assumptions made, whether the effect is assumed to be proportional or not, and other assumptions, make a difference in the estimate of the potential duration effect. The best technique (the nonparametric one) gives different results when the sample is restricted to those with unchanging potential duration. Both models have the disadvantage of restricting the effects of potential duration to be proportional on the probability of leaving unemployment. In future work, this proportionality assumption should be relaxed and the difference between the two samples should be reexamined in that light.

IV. ANALYSIS OF THE JOB SEARCH ASSISTANCE RESEARCH PROJECT DATA

The Job Search Assistance Research Project (JSARP), also known as the Employment Opportunity Pilot Project (EOPP), was an experimental test of employment and job search assistance to disadvantaged workers. Conducted and analyzed by MPR, the project took place in 20 areas of the country, 10 of which were "experimental" sites in which the residents were offered the JSARP program and 10 of which were "comparison" sites selected to match the experimental sites. The primary objective of the project was to analyze the effects of the program on earnings, job search, and labor market performance, but much of the data need not be used for this purpose. In particular, a household survey was administered to a random sample of the entire population in each of the areas around the time of the beginning of the project, and thus includes eligibles and noneligibles, potential participants and nonparticipants, and so on. The survey asked retrospective questions about spells of nonwork, unemployment, and employment, as well as the receipt of UI. These retrospective data cover a period prior to the beginning of the program and thus the behavior exhibited by the individuals in the sample over this period should be unaffected by the program. It is these data we analyze here.

In its usefulness for a study of the effects of UI on unemployment and employment, the data have all the strengths and weaknesses of survey data. In some respects, these strengths and weaknesses complement those of the CWBH data, for the JSARP data are strong where the CWBH data are weak but weak in some areas where the CWBH data are strong. Perhaps the primary strength of the JSARP data is that the spells of unemployment and nonwork are not truncated by the potential duration of UI benefits, as CWBH data are. Thus, behavior after exhaustion can be observed, and one of the main truncation problems that played so important a role in the previous chapter is avoided. There are still many spells that are truncated in household data—those that were still in progress at the interview date—but this is less severe than the truncation present in the CWBH data. A second advantage of many household data sets, and JSARP in particular, is that an entire sample of the population is covered, not just UI recipients. Although UI recipients are naturally the greatest source of interest, non-UI recipients can be useful as an explicit or implicit comparison group with UI recipients. The difference between no benefits and positive benefits, for example, may be a stronger indication of the effects of UI than the difference (often quite small) between different levels of positive benefits. A third advantage of most household data sets is that behavior while not receiving UI benefits is observed. As will be recalled from the previous chapter, there are considerable periods of nonreceipt of UI benefits over an individual's benefit year, and the UI data by definition do not yield any information on those periods. With retrospective questions asked regarding activity during all weeks, such information can be obtained. A fourth advantage of the JSARP data in particular is that all individuals at the time of the interview were asked the retrospective questions, not just those unemployed at the time of the interview. One of the main problems with many household surveys, the Current Population Survey being the most well-known in this regard, is that the questions on the duration of unemployment spells are asked only of those unemployed at the time of the survey. Economists and statisticians have learned when studying such data that biased estimates of the lengths of unemployment spells are obtained, for those with particularly long spells of unemployment are more likely to be sampled than those with short spells in such a survey. The exact direction of bias this would cause in a study such as ours is not clear, but it is clear that it is better to have a random sample of all unemployment spells for analysis.

The disadvantage of the JSARP data are also shared by most household surveys. First, there is some amount of underreporting and misreporting of income and other variables in such surveys, especially if they are retrospective, creating a source of error that is not present in administrative data sources such as the CWBH. Of particular concern for this study is the underreporting or misreporting of UI benefits. Second, and somewhat related, the UI data from such surveys are not obtained directly from UI records. This affects not only the UI benefit but also the potential duration of UI weeks, which is not asked in household surveys. Such potential durations have to be imputed to individuals in such surveys by the analysts, and this is subject to error. Thus, the main disadvantage of the JSARP data is its lack of administrative UI data, which is the main strength of the CWBH data. The analysis of such data sets is useful because it will show, to some extent, how much empirical difference these relative advantages and disadvantages make.

A. DESCRIPTION OF THE SAMPLE

As noted above, the sample is based upon a household survey administered to a random sample of the entire population of 20 areas in the U.S. The survey was administered in calendar 1980, with the bulk of the interviews occurring in the summer of that year but with some occurring both in the spring and in the fall. A follow-up household survey was conducted sometime later, but only the low-income subsample was interviewed; therefore, these data are not used. The 1980 survey asked retrospective questions on whether an individual was employed, unemployed, or not in the labor force at each point in time from January 1, 1979 to the interview date. Thus, a year or more of retrospective labor force history data are available for the individuals in the sample.

Table IV.1 shows the construction of the analysis sample from these data. Over fifty thousand individuals were interviewed in the 1980 survey. Of these, over twenty-two thousand had no spells of either nonwork or unemployment. They were naturally deleted from the sample, leaving about thirty-one thousand observations. This sample was then reduced considerably further by a number of restrictions. First, it was decided to examine only individuals whose spells began in or after February 1, 1979. A large fraction of the sample was unemployed or not in the labor force in January 1979, the first period of the time frame, and were in the middle of unemployment or nonwork spells. Since the data do not give us the starting date of those spells, we cannot measure their duration. Consequently, only spell beginnings in February 1979 or after can be identified and included in the analysis sample. Second, it was discovered that a number of individuals were enrolled in the JSARP program fairly early, and therefore their unemployment spells could have been affected by the program. It was decided that the spells for these individuals would be truncated at the date of enrollment and wo..ld be treated as truncated spells, just as if the interview itself had been conducted at that date. A formal "truncation date" was then constructed equal to the minimum of the enrollment date of

TABLE IV.1

CONSTRUCTION OF JSARP ANALYSIS SAMPLE

Original Sample Size	53,250
Number With No Unemployment Spell and No Nonwork Spell	22,199
Remaining Sample	31,051
Number with unemployment spell in progress in January 1979	1,779
Number with nonwork spell in progress in January 1979	25,152
Number with unemployment spell beginning after October 1979	3,146
Number with nonwork spell beginning after October 1979	2,728
Number with truncation date on or after October 1979	31,027
Remaining Sample—Total	
Males	1,701
Females	2,397
Analysis Sample—Total	
Males	1,563
Females	2,230

the individual and the interview date, whichever came first. Examination of the distribution of the truncation date revealed that a number were in 1979; for example, about 1 percent were before October 1979. It was decided to exclude these individuals from the sample as well, for anyone with a truncation date that early could not possibly be observed for very long from February 1979. In addition, for an analysis of the type we wish to conduct here, it is undesirable to have such individuals in the sample because their presence will bias the distribution of the dates of the beginning of spells in the population. By definition, if an individual's truncation date is in June 1979, for example, there is no possibility of observing his or her first spell beginning in September 1979 (assuming the individual had been employed from January to August). Therefore, an analysis sample ignoring this problem would have "too few" spells beginning in September 1979 compared to, for example, April 1979. Furthermore, since truncation dates after October 1979 are allowed in the sample, it is undesirable to include anyone whose first spell began after October 1979, for since there are a significant number of truncation dates after that point, again the count of the number of spells starting in months subsequent to October will be incorrect relative to the number starting prior to October. Consequently, any individual who had no spell starting before October 1979 was also excluded from the sample. The remaining sample, then, consists of individuals who had a spell of either unemployment or nonwork that began between the months of February 1979 and October 1979, and who were either interviewed or enrolled in JSARP after October 1979. Thus, the sample is a random one of the entire population of spells beginning between February and October.

The sample remaining after these exclusions is naturally much smaller than the original sample, and is equal to 1,701 males and 2,397 females, as shown in Table IV.1. By far, the major source of exclusion is the exclusion of those in the middle of spells in January 1979. Remaining exclusions were then imposed for the requirement that all variables used in the regression be nonmissing, leaving final analysis samples of 1,563 for males and 2,230 for females.

The means of the variables used in the analysis are shown in Table IV.2. Males had weekly wages (prior to unemployment) of about $167 and females had weekly wages of about $87. About 70 percent of the sample was white. Unemployment rates (measured at the beginning of the first spell of nonwork or unemployment) were about 6 percent, fairly low by the standards of the last decade. Both males and females were in their early 30's on average. The mean potential duration of benefits in the sample—assigned as the mean duration for the state in which the individual was located—was about 23 weeks for both males and females. This potential duration is at the beginning of the first spell. Mean UI benefits (for those with benefits) were $96 for males and $70 for females.

The analysis sample was based upon the first spell of either unemployment or nonwork in the sample. (On average, there were 1.3 nonwork spells per person.) As the table indicates, the mean unemployment spell length was 15 weeks for men and 14 weeks for women, while the mean nonwork spell lengths were 26 weeks for men and 33 weeks for women. About 13 percent of the unemployment spells and 26-35 percent of the nonwork spells were truncated by either the interview date or the JSARP enrollment date.

TABLE IV.2

MEANS OF VARIABLES IN REGRESSIONS

	Males	Females
Weekly Wages	166.73	86.60
Race[a]	0.70	0.68
Unemployment Rate	5.88	5.93
Age	31.19	30.20
P[b]	22.60	22.58
Weekly UI Benefit	$96.40	$70.48
Mean Durations of First Spells (Weeks)		
Unemployment	14.65	13.60
Nonwork	26.48	33.5
Percent of First Spells That Are Truncated		
Unemployment	13.28	14.07
Nonwork	26.38	35.23

[a] 1 = white, 0 = nonwhite.

[b] Mean in state at beginning of spell.

B. RESULTS OF THE ANALYSIS

Table IV.3 shows the results of OLS regressions estimated for both unemployment spells and nonwork spells, for both men and women. These OLS regressions ignore the truncation of spells in the data which will be addressed by Cox regressions to be presented shortly. The results for the unemployment spells show several significant coefficients. First, the weekly UI benefit has a significant and positive effect on the length of the spell. Second, the potential duration in the state also has a significant and positive effect on the spell length. The magnitude of the coefficient is about .32-.38. The results also show insignificant effects of the wage on unemployment spell lengths, significantly negative effects on spell lengths for whites, and significantly positive effects of age on spell lengths. The unemployment rate is, somewhat surprisingly, insignificant.

The results for nonwork spells are a bit different but similar in most respects. The UI benefit is insignificant on nonwork spell lengths, in contrast to the unemployment spell lengths. But potential duration has a positive and significant effect on nonwork spell lengths. Here the effect is considerably larger than the effect on unemployment spells, equal to about .58 for both males and females. Before considering behavioral hypotheses to explain these results, we shall wait to consider the Cox-model adjustments for truncation bias (for recall that truncation was more severe in the nonwork spells). There are significantly negative effects of the wage and race on nonwork spell lengths and significantly positive effects of age on spell lengths. Again, the unemployment rate is insignificant.

TABLE IV.3

OLS REGRESSIONS ON JSARP SAMPLE

	Unemployment Spell		Nonwork Spell	
	Males	Females	Males	Females
Weekly UI Benefit/100	4.96*	9.90*	0.68	-1.26
	(4.31)	(5.36)	(0.50)	(0.55)
P	0.38*	0.32*	0.58*	0.58*
	(2.37)	(2.24)	(3.18)	(3.39)
Weekly Wage/100	0.16	0.10	-2.92*	-5.50*
	(0.43)	(0.20)	(7.45)	(10.43)
Race	-3.12*	-4.64*	-5.26*	-7.56*
	(2.96)	(5.27)	(4.20)	(6.95)
Age	0.12*	0.08*	0.40*	0.08*
	(2.44)	(2.02)	(9.83)	(1.73)
Unemployment Rate	-0.74	-0.42	-0.08	-0.72
	(1.53)	(0.97)	(0.15)	(1.46)
Intercept	8.54*	8.84*	9.94*	32.46*
	(2.52)	(3.08)	(2.65)	(9.63)
R^2	0.05	0.05	0.09	0.08
F-value	7.77	11.34	24.13	30.90
Sample Size	994	1,322	1,471	2,183

NOTES: Unsigned t-statistic in parentheses.
*: Significance at 10 percent level.

Table IV.4 shows the results of estimating the Cox model discussed in Chapter II and in the previous chapter. This model, it will be recalled, adjusts for the truncation in the data and does so non-parametrically, without assuming any particular functional form for the distribution of unemployment and nonwork spells. The results are in most respects similar to the OLS results, at least in the signs of the coefficient and in their magnitudes. (Recall that the signs of the coefficients are to be interpreted as the effects of the regressors on the probability of returning to work, and hence are opposite to those of the effect of the regressors on the length of unemployment spells.) The UI benefit has a significantly positive effect on unemployment spell lengths and an insignificant effect on nonwork spell lengths, as the OLS results also indicated. The potential duration variable has a positive and significant effect again on both unemployment and nonwork spell lengths. However, the results show that the magnitude of the effects are approximately the same for both types of spells. Consequently, it appears that the earlier larger effect for nonwork spells was partly a result of the greater truncation of the nonwork spells. The pattern of signs and significance levels of the other variables is again very close to that of OLS.

In order to compare the potential duration effects in these Cox regressions to those from OLS, it will be recalled from the previous chapter that the Cox coefficients must be reversed in sign and multiplied by the mean spell lengths. Performing the calculation for the coefficients in Table IV.4 and using the mean spell lengths in Table IV.2, we find that the effects of a one-week increase in P on mean unemployment spell lengths

TABLE IV.4

COX REGRESSIONS ON JSARP SAMPLE

	Unemployment Spell		Nonwork Spell	
	Males	Females	Males	Females
Weekly UI Benefit/100	−0.31*	−0.50*	−0.05	0.15
	(3.34)	(3.58)	(0.57)	(1.22)
P	−0.03*	−0.02*	0.02*	−0.02*
	(2.45)	(2.00)	(2.31)	(2.58
Weekly Wage/100	0.03	−0.06	0.10*	0.13*
	(0.98)	(1.52)	(4.91)	(4.38)
Race	0.26*	0.32*	0.25*	0.39*
	(3.17)	(4.84)	(3.41)	(6.19)
Age	−0.01*	−0.01	−0.03*	−0.01*
	(2.78)	(1.59)	(10.19)	(3.80)
Unemployment Rate	0.04	0.003	−0.001	0.03
	(0.96)	(0.11)	(0.03)	(1.04)
Chi-square	45.44	53.39	135.26	78.71
Sample Size	994	1,322	1,471	2,183

NOTES: Unsigned t-statistic in parentheses.
*: Significance at 10 percent level.

are .45 and .28 for males and females, respectively; and the effects on mean nonwork spell lengths are .52 and .66 for males and females, respectively. Thus, the effects are slightly larger than the OLS results suggested. This is probably a result of the fact that truncation, in general, biases OLS coefficients towards zero. (This is because an increase in an X variable that increases the true length of spells will be observed to have zero effect in the data if the individual is at the truncation point.) Note, too, that the estimated effects now indicate, once more, that effects on nonwork spells are greater than those on unemployment spells. The reason is now clear. The rates of leaving unemployment and nonwork are the same at the same point in the spells, but because nonwork spells are simply longer than unemployment spells, the cumulative effect is larger for the latter.

C. SUMMARY

The results of analyzing the JSARP sample show nonzero effects of potential duration. A one-week increase in potential duration increases the mean unemployment-spell length by about .45 weeks for males and .28 weeks for females. Nonwork spell effects are larger, about .52 for males and .66 for females.

V. ANALYSIS OF THE FSB FOLLOW-UP SURVEY

The FSB follow-up data set is a data set collected and analyzed by MPR in several projects. It is one of our household survey data sets, and thus has the advantage of having interview data. It is also a data set that was explicitly collected to analyze the effects of one particular extended benefit experience (the FSB program) and thus benefits in several ways from that motivation of collection.

The FSB data were collected by MPR in 1975-1977, and consist of FSB and EB recipients drawn from state UI files in 1975 from fifteen states. The individuals were interviewed once in 1976, and a smaller subsample was reinterviewed in 1977. Only the latter group is included in this analysis, the same group analyzed in the FSB follow-up study conducted by MPR (Brewster et al., 1977). Relative to other data sets, the FSB data have the advantages of (1) containing benefit and potential duration data drawn directly from UI records, rather than being imputed data; (2) having interview data on labor-force behavior <u>after</u> the exhaustion point of exhaustees, thus avoiding the common truncation of data drawn solely from UI administrative records; (3) having covered a long time period (about three years); (4) having more socioeconomic characteristics than UI administrative data, again because personal interviews were conducted; and (5) having large sample sizes (about 2,000 total) of EB and FSB recipients, unlike most other data sets available. The main disadvantage of the data set is that it consists exclusively of exhaustees of regular UI benefits. The implication of this sample exclusion will be discussed below. A related problem with the EB portion of the data set is that the EB recipients were sampled at different rates than the FSB recipients and in a fairly complex fashion, making it difficult to use the data on the EB individuals. Finally, a minor disadvantage of the data is that compensated weeks of benefits are available only from the personal interviews and are thus subject to normal response error, unlike data sets wherein such compensated weeks are obtained from UI records directly.

There have been two analyses of the FSB follow-up data set: Brewster et al. (1978) and Moffitt and Nicholson (1982). These studies both indicated that the FSB extension of benefits had positive effects on unemployment duration. The order of the topics discussed below are: (1) a description of the sample, (2) the replication of the Brewster et al. study, (3) a presentation of some new results, (4) a discussion of potential truncation bias in the data, (5) a report of the effect of duration on post-unemployment wages, and (6) a report of the effect of duration on the earnings of others in the household.

A. DESCRIPTION OF THE SAMPLE

As noted above, the data set consists of FSB recipients and EB recipients drawn from the UI records of fifteen states in 1975. Since individuals had to be on EB or FSB sometime in 1975, most unemployment spells of these individuals began in 1974. The FSB sample is a random sample of FSB recipients in 1975, but the EB sample is a sample only of those EB recipients who had not received FSB in 1975. The data contain labor-force histories from the date of the beginning of the spell through the second interview, which could be more than three years later. Data drawn from UI records include the weekly benefit amount and potential duration, while the dependent variables as well as all socioeconomic conditions are drawn from the personal interviews.

The sample analyzed here includes all individuals who have usable data on the variables in the analysis. These variables are shown in Tables V.1 and V.2. Mean nonwork spell lengths in the sample are fairly long, of course, equal to about 65 weeks for men and 78 for women. Unemployment-spell lengths are by definition somewhat shorter, since nonwork spells include periods of not being in the labor force. At this point, two features of the data related to these variables should be noted. First, these two variables, particularly the latter, are not necessarily the same as weeks of UI compensation even for nonexhaustees. The distinction between unemployment and not-in-the-labor force in the interviews was made on the basis of search-effort questions asked at the time of the interview. Since both the timing and nature of these questions differs from those used to determine the eligibility of UI claimants, some individuals in our data set are classified as not in the labor force but receiving UI benefits.[1] Second, the exact end date of the initial unemployment spell is not known, but is only approximated. The data contain the total number of weeks unemployed within each spell of nonwork, and the variable denoted here as the "initial unemployment spell" is obtained by allocating all of the unemployed weeks to the beginning of the nonwork spell. For both these reasons as well as for the reason that unemployment not-in-the-labor-force distinctions are difficult to make in

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[1] To be classified as unemployed an individual must be actively seeking work unless he or she expects recall, whereas to be eligible for UI an individual must state that he or she is able and available for work. In some but not all states evidence of active searching is also required.

TABLE V.1

DESCRIPTIVE STATISTICS, FSB SAMPLE: MALES

	Mean	Standard Deviation	Minimum	Maximum
Length of Initial Nonwork Spell in Weeks (N)	64.59	49.38	0.00	202.00
Length of Initial Unemployment Spell in Weeks (U)	58.00	40.64	1.00	202.00
Total Weeks of Nonwork Over the Period (NT)	88.65	45.76	0.00	202.00
Total Weeks of Unemployment Over the Period (UT)	71.27	43.26	0.00	202.00
Total Weeks of Employment Over the Period (ET)	67.06	44.12	0.00	176.00
After-Tax Pre-Unemployment Weekly Wage (W)[a]	199.88	96.77	16.51	1175.50
Weekly UI Benefit Amount (B)	75.55	20.66	13.00	171.00
Net Replacement Ratio (r=B/w)	0.42	0.15	0.03	0.99
Potential UI Duration (P)	56.58	12.68	20.60	65.00
Total Assets (thousands)	3.09	7.86	0.00	100.00
Spouse Annual Earnings in 1974 (thousands)	1.23	2.58	0.00	19.00
Education	11.42	2.73	1.00	22.00
Race (=1 if black)	0.17	0.38	0.00	1.00
Married (=1 if married)	0.64	0.48	0.00	1.00
Union (=1 if in union)	0.43	0.50	0.00	1.00
Health (=1 if in poor health)	0.15	0.36	0.00	1.00
Experience (percentage of adult years worked)	0.95	0.17	0.00	1.00
Insured Unemployment Rate in 1975 (IUR)	5.96	1.23	2.13	7.78
Number of Children	0.71	1.19	0.00	8.00
Age Dummies:				
<25	0.27	0.44	0.00	1.00
26-34	0.24	0.43	0.00	1.00
45-54	0.13	0.34	0.00	1.00
55-64	0.13	0.34	0.00	1.00
65+	0.07	0.26	0.00	1.00
Weekly Wage on First Post-Post Spell	181.41	94.95	3.00	600.00
Average Weekly Wage in 1977	183.81	97.27	2.30	575.22

Table V.1 (Continued)

	Mean	Standard Deviation	Minimum	Maximum
Earnings of Spouse at Second Interview	62.38	85.58	0.00	575.00
Earnings of Spouse in 1976	2194.67	3993.66	0.00	29900.00
Earnings of Spouse in 1977	2572.78	4186.45	0.00	30359.98
Earnings of Others at Second Interview	12.49	50.48	0.00	400.00

Number of observations = 853.

[a] Adjusted for marginal tax rate on family income.

TABLE V.2

DESCRIPTIVE STATISTICS, FSB SAMPLE: FEMALES

	Mean	Standard Deviation	Minimum	Maximum
Length of Initial Nonwork Spell in Weeks (N)	77.98	55.56	1.00	204.00
Length of Initial Unemployment Spell in Weeks (U)	66.64	43.08	1.00	204.00
Total Weeks of Nonwork Over the Period (NT)	102.65	47.29	0.00	204.00
Total Weeks of Unemployment Over the Period (UT)	71.70	46.62	0.00	204.00
Total Weeks of Employment Over the Period (ET)	54.93	44.14	0.00	163.00
After-Tax Pre-Unemployment Weekly Wage (W)[a]	120.16	55.04	12.80	727.60
Weekly UI Benefit Amount (B)	55.85	18.27	13.00	129.00
Net Replacement Ratio (r=B/W)	0.49	0.14	0.07	0.99
Potential UI Duration (P)	55.43	12.63	20.00	65.00
Total Assets (thousands)	2.17	5.77	0.00	84.50
Spouse Annual Earnings in 1974 (thousands)	6.32	5.94	0.00	30.00
Education	11.41	2.32	0.00	21.00
Race (=1 if black)	0.19	0.39	0.00	1.00
Married (=1 if married)	0.72	0.45	0.00	1.00
Union (=1 if in union)	0.27	0.44	0.00	1.00
Health (=1 if in poor health)	0.09	0.29	0.00	1.00
Experience (percentage of adult years worked)	0.83	0.24	0.00	1.00
Insured Unemployment Rate in 1975 (IUR)	5.86	1.30	2.13	7.78
Number of Children	1.02	1.29	0.00	8.00
Age Dummies:				
<25	0.17	0.37	0.00	1.00
26-34	0.27	0.44	0.00	1.00
45-54	0.20	0.40	0.00	1.00
55-64	0.11	0.31	0.00	1.00
65+	0.05	0.22	0.00	1.00
Weekly Wage on First Post-Spell Job	123.24	87.66	5.00	600.00

Table 2 (continued)

	Mean	Standard Deviation	Minimum	Maximum
Average Weekly Wage in 1977	114.39	69.04	4.42	593.16
Earnings of Spouse at Second Interview	208.79	137.88	0.00	600.00
Earnings of Spouse in 1976	8979.48	7552.08	0.00	31200.00
Earnings of Spouse in 1977	9269.10	7551.10	0.00	3059.98
Earnings of Others at Second Interview	19.88	58.46	0.00	500.00

Number of observations = 772.

[a] Adjusted for marginal tax rate on family income.

general, the initial spell of nonwork (N) will be given more emphasis than that of unemployment (U) in the analysis reported below.

Total weeks of employment, unemployment, and nonwork are also reported in Tables V.1 and V.2, as are the UI variables. The net replacement ratio is .42 for men and .49 for women, and potential weeks are 57 for men and 55 for women. The other demographic variables shown in the tables will be used in the regression analysis. There are about 850 men and 770 women in the sample.

B. BREWSTER ET AL. REPLICATION

A large number of regressions were run on these data by Brewster et al. (1978) in the FSB Follow-Up Final Report. Although most of these regressions were also run here, only three will be presented for brevity. They are shown in Table V.3. In their study, Brewster et al. estimated unemployment and employment regressions with OLS, including a large set of socioeconomic characteristics on the right-hand-side as well as three UI variables: the replacement ratio, potential duration, and an interaction between the two. For purposes of the replication, an identical set of right-hand-side variables were constructed and observations missing any of these variables were deleted. The sample sizes are not quite the same, however, and it is probable that different criteria for missing or outlying data were used.

The results shown in Table V.3 are unfortunately far from identical in the replication and the Brewster et al. study. Most coefficients are quite a bit different in magnitude, and a few are different in sign. However, for the most part, this seems to be a result of the general insignificance of the coefficients. Those coefficients that change sign

TABLE V.3

BREWSTER ET AL. REPLICATION[a]

	Males		Females	
	Brewster et al.[b]	Replication	Brewster et al.[c]	Replication
Dependent Variable: ET				
r	-36.56 (1.34)	-21.11 (0.50)	-19.04 (1.21)	18.03 (0.71)
P	-0.69* (2.63)	-0.44 (1.54)	-0.42 (1.57)	0.09 (0.38)
Pxr	0.70 (1.54)	0.33 (0.50)	0.47 (1.51)	-0.30 (0.71)
Dependent Variable: UT				
r	8.48 (0.28)	-13.95 (0.30)	5.92 (0.34)	-21.42 (0.75)
P	0.45 (1.54)	0.28 (0.89)	0.52* (1.74)	0.24 (0.89)
Pxr	-0.19 (0.38)	0.22 (0.30)	-0.21 (0.61)	0.28 (0.47)
Dependent Variable: U				
r	19.81 (1.24)	-13.97 (0.36)	7.40 (0.63)	-4.36 (0.15)
P	0.74* (4.33)	0.33 (1.25)	0.63* (3.40)	0.49 (1.53)
Pxr	-0.28 (1.00)	0.31 (0.51)	-0.13 (0.62)	0.08 (0.13)

NOTES: Unsigned t-statistics in parentheses.
*: Significant at the 10 percent level.

[a] Control variables the same as those in Table 4 with following exceptions: (1) marital status and number of children are excluded, and (2) three variables for the number of children less than 6, 6-12, and over 12 included. See p. 22 of Brewster et al. Also, r is not adjusted for taxes in these regressions.

[b] See p. 44 of Brewster et al.

[c] See p. 63 of Brewster et al.

have extremely low t-values, and in most of those cases, the standard errors are sufficiently large that 90 percent confidence intervals for the two coefficients would overlap. It is not surprising that coefficients that are so imprecisely estimated could change considerably in magnitude with small changes in sample definition. In addition, of the four Brewster et al. coefficients that are significant, those in the replication are of the same sign, fairly large, and usually on the borderline of significance at the 10 percent level. Thus, it appears that the main difference in the original and in the replication is that somewhat weaker and less significant coefficients are found in the latter.

Perhaps most important is the result that the effects of replacement ratios and potential durations are not far off at the means. Replacement ratios have little effect and are generally insignificant, while duration has a positive effect in both estimates. Moreover, when evaluated at the mean r, the duration effects in the two sets of regressions are closer than the two separate coefficients appear, for the differences in the two sets of coefficients move in opposite directions. Thus, a part of the difference in the original and in the replication appears to be a somewhat different allocation of the duration effect between the "pure" portion and that portion that varies with the level of the replacement ratio. Thus, on the basic issue of whether replacement ratios and potential durations have any effect and in what direction, the two sets of regressions are in broad agreement.

C. FURTHER EXPLORATIONS

To explore the nature of the UI effects in the data, additional sets of regressions were estimated. These differ from the Brewster et al.

regressions in several ways. First, there is a slight change of control variables included on the right-hand-side. It appeared from testing that this made little difference (the set included here is, instead, that used in the Moffitt-Nicholson study). Second the two UI variables were not interacted. This is a simpler specification and allows a more direct reading of the mean effect from the regression coefficients. Third, in addition to the three dependent variables shown by Brewster et al., the initial spell of nonwork was used for the reasons discussed above.

A specimen regression showing the coefficients on all the control variables is shown in Table V.4. In this particular regression the dependent variable is N and the UI benefit, B, is entered instead of the replacement ratio. The replacement ratio is insignificant for males and females, while the benefit is significant and postive for females. Potential duration has a significant and large effect for both groups, upwards of .70 week of additional nonwork for every extra week of potential benefits. Assets have a significantly positive effect for males; this is to be expected inasmuch as higher levels of assets should allow the individual to stay out of work for a longer period. Also, the husband's earnings has a significantly positive effect on the nonwork spells of women, presumably for the same reason. In addition, the men's regression indicates that longer spells are experienced by the more educated, by blacks, those in poor health, those with less experience, and those who are older. The women's regression indicates that longer spells are had by those who are married, those not in a union, those in poor health, those with fewer children, and the more aged. In both regressions it also appears that spells are longer when the unemployment rate is higher, as

TABLE V.4

SPECIMEN REGRESSIONS FOR MALES AND FEMALES

	Dependent Variable: N	
	Males	Females
W	-0.01 (0.02)	-0.05 (0.04)
B	-0.08 (0.10)	0.24* 0.13)
P	0.70* (0.13)	0.72* (0.16)
Assets	0.72* (3.40)	0.14 (0.37)
Spouse Earnings	-0.98 (0.63)	1.03* (0.42)
Education	1.34* (0.61)	0.28 (0.90)
Race	8.40* (4.04)	8.06 (5.16)
Married	-2.06 (4.00)	9.14* (5.27)
Union	-3.07 (3.35)	-13.27* (4.59)
Health	11.88* (4.44)	14.36* (6.56)
Experience	-17.31* (9.22)	-8.59 (8.41)
IUR	2.41* (1.31)	4.14* (1.51)
Number of Children	-0.34 (1.59)	-3.37* (1.86)
Age Dummies		
≤ 25	-12.54* (5.37)	2.32 (6.85)

Table V.4 (continued)

	Dependent Variable: N	
	Males	Females
26-34	-11.76* (4.99)	12.92* (5.58)
45-54	5.12 (5.99)	4.41 (6.37)
55-64	27.38* (6.40)	30.22* (7.84)
65+	65.26* (7.66)	79.64* (10.32)
Constant	16.31 (16.08)	-11.63 (17.91)
R-Squared	0.29	0.20

NOTES: Standard errors in parentheses.
*: Significant at the 10 percent level.

should be expected (but recall that this was not found in the Newton-Rosen data).

Table V.5 shows the results of testing several variants of these equations on all four dependent variables. As columns (1) and (4) indicate, the insignificance of the UI benefit holds up for all four male dependent variables, but its significance in the female regressions disappears in the equations for total weeks of nonwork and unemployment. Likewise, while the strong duration effect drops somewhat in the U regressions from the N regressions, it drops much further in the total weeks regressions. This pattern is probably a simple result of the fact that the total weeks variables include long time periods after the potential duration point and after the spells have ended. It should not be expected that the UI benefit should have effects whose strength continues indefinitely into the future. For this rather obvious reason, these dependent variables will be deemphasized in the subsequent work.

The regressions in columns (2)-(3) and (5)-(6) show the effect of specifying the UI benefit in different ways. Columns (2) and (5) show the effect of entering the replacement ratio instead of the benefit, a common specification in the literature. For males this has no effect on the previous results, but for females the replacement ratio is insignificant (whereas the benefit was significant). This is an interesting result, for it implies that the insignificance of the replacement ratio is not an indication of no effect of the UI benefit, but is instead a result of the insignificance of the wage, which appears in its denominator. This suggests caution in the use of the replacement ratio in these sorts of equations, even when the wage is controlled for elsewhere in the equation.

TABLE V.5

BASIC UNEMPLOYMENT REGRESSIONS,
ALTERNATIVE SPECIFICATIONS

	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable: N						
W(1-r)	--	--	-0.01 (0.02)	--	--	-0.04 (0.04)
W	-0.01 (0.02)	-0.00[a] (0.03)	--	-0.05 (0.04)	-0.01 (0.04)	--
B	-0.08 (0.10)	--	--	0.24* (0.13)	--	--
r	--	11.86 (15.65)	--	--	-0.31 (16.56)	--
P	0.70* (0.13)	0.64* (0.13)	0.66* (0.12)	0.72* (0.16)	0.76* (0.16)	0.75* (0.16)
Dependent Variable: U						
W(1-r)	--	--	-0.00[a] (0.02)	--	--	-0.03 (0.04)
W	-0.01 (0.02)	0.01 (0.03)	--	-0.04 (0.04)	0.00[a] (0.04)	--
B	-0.02 (0.09)	--	--	0.21* 0.12)	--	--
r	--	14.18 (15.10)	--	--	5.00 (14.84)	--
P	0.58* (0.12)	0.54* (0.12)	0.57* (0.12)	0.66* (0.14)	0.69* (0.14)	0.70* (0.14)
Dependent Variable: NT						
W(1-r)	--	--	0.03 (0.02)	--	--	-0.01 (0.04)
W	0.03 (0.02)	0.03 (0.03)	--	-0.00[a] (0.04)	-0.02 (0.04)	--
B	-0.12 (0.09)	--	--	0.02 (0.11)	--	--
r	--	7.56 (14.66)	--	--	-12.72 (13.91)	--
P	0.39* (0.12)	0.32* (0.12)	0.35* (0.11)	0.28* (0.13)	0.29* (0.13)	0.27* (0.13)
Dependent Variable: UT						
W(1-r)	--	--	0.05* (0.02)	--	--	0.03 (0.04)
W	0.04 (0.02)	0.04 (0.03)	--	0.04 (0.04)	0.00[a] (0.04)	--

Table V.5 (continued)

	Male (1)	Male (2)	Male (3)	Female (4)	Female (5)	Female (6)
B	-0.15 (0.10)	--	--	-0.06 (0.12)	--	--
r	--	5.26 (15.48)	--	--	-18.56 (14.88)	--
P	0.41* (0.13)	0.34* (0.13)	0.36* (0.12)	0.41* (0.14)	0.43* (0.14)	0.41* (0.14)

NOTES: Standard errors in parentheses.
*: Significant at the 10 percent level.

[a] Less than 0.005 in absolute value.

Columns (3) and (6) show the effect of constraining W and B to have the same coefficient (but opposite in sign), by entering the net wage W(1-r). This is the specification suggested by simple economic theory, and has been used in some studies (e.g., Newton and Rosen, Moffitt and Nicholson). Again, this restriction does not alter the insignificance of the benefit effect for males, but it does make the female benefit effect disappear once more. Thus, this restriction seems also to be unwise to impose.

Table V.6 shows the results of several sensitivity tests for the N equation. The equations were also run for the U variable, but are not shown (the relative effects are the same). In all cases the UI benefit is specified separately from the wage for reasons just discussed. In the first two columns regressions are shown that are estimated only on those who separated from their pre-UI job because of a layoff. This test is for comparability with the well-known study of Ehrenberg and Oaxaca (1976), who found significant UI effects only for this group. The results indicate that, although the coefficient changes somewhat in magnitude and significance (the female benefit loses significance but not magnitude), no major difference from the previous results appears. In the next two columns the two UI variables are interacted with the unemployment rate to see if their effect varies with the business cycle. The signs of the interaction variables suggest that the disincentive effect of the UI benefit is lower and the disincentive effect of potential duration is higher when the unemployment rate is higher. However, only one of the four coefficients is significant, so these results should be treated cautiously. The last two columns show the effect of using gross wages and replacement

TABLE V.6

SENSITIVITY TESTS TO BASIC REGRESSIONS
(Dependent Variable: N)

	Layoffs Only		IUR Interaction		Non-Tax Specification[a]	
	Males	Females	Males	Females	Males	Females
W	-0.04 (0.02)	-0.05 (0.06)	-0.02 (0.02)	-0.05 (0.05)	-0.01 (0.03)	0.01 (0.05)
B	-0.05 (0.10)	0.22 (0.16)	0.57 (0.47)	1.44* (0.64)	--	--
r	--	--	--	--	8.66 (15.70)	1.80 (16.77)
P	0.67* (0.14)	0.87* (0.19)	0.06 (0.58)	-0.20 (0.72)	0.64* (0.13)	0.75* (0.16)
B x IUR	--	--	-0.10 (0.07)	-0.19* (0.10)	--	--
P x IUR	--	--	0.10 (0.09)	0.15 (0.12)	--	--

NOTES: Standard errors in parentheses.
*: Significant at the 10 percent level.

[a] W, r in gross terms.

ratios, unadjusted for taxes, instead of their tax-adjusted counterparts used thus far. The results change little except for females, where the benefit variable loses significance when taxes are ignored. This is consistent with the view that the effect of marginal taxes are most important for women, for the tax rates vary more in the female population than in the male population and because, on average, women are more likely to be secondary earners than men are. Thus, the results suggest that marginal tax rates should be adjusted for, at least for women.

Table V.7 shows the result of tests of the Newton-Rosen (1979) specification. The NR specification differs from the present one primarily because (1) NR had available only a small set of control variables--sex, race, and the unemployment rate--and (2) NR entered the net wage and potential duration in quadratic and interacted form. Males and females were also pooled in the NR specification (although they were also run separately), so that they are pooled here as well. Columns (1) and (3) in the table show the effect of reducing the set of control variables down to the three used in the NR study, but leaving the UI variables (net wage and potential duration, in this case) in linear form. The results change very little from those shown in the (sex-specific) equations in Table V.6. These results, as well as others run but not reported here, indicate that the general nature of the UI coefficients in the FSB data is not altered by inclusion or exclusion of any of the large number of control variables we have been using. Columns (2) and (4) show the result of using the NR specification. While the five coefficients are almost always of the same sign as the NR coefficients (which is, actually, quite remarkable), the coefficients here are considerably less significant, particularly those

TABLE V.7

NEWTON-ROSEN SPECIFICATION

	Dependent Variable: N		Dependent Variable: U	
	(1)	(2)	(3)	(4)
W(1-r)	-0.02 (0.03)	-0.02 (0.12)	-0.01 (0.02)	0.06 (0.10)
P	0.81* (0.10)	-1.86* (1.10)	0.71* (0.10)	-1.43 (0.95)
$[W(1-r)]^2$	--	0.0002 (0.0001)	--	0.0002* (0.0001)
P^2	--	0.03* (0.01)	--	0.02* (0.01)
$P_x[W(1-r)]$	--	-0.001 (0.002)	--	-0.002 (0.002)
Sex[a]	14.78* (2.86)	14.22* (2.93)	10.36* (2.46)	9.64* (2.52)
Race	0.72 (3.41)	0.77 (3.40)	6.18* (2.87)	6.03* (2.87)
IUR	2.85* (1.04)	2.48* (1.05)	2.99* (0.90)	2.60* (0.91)

NOTES: Standard errors in parentheses.
*: Significant at the 10 percent level.

[a] Variable = 1 if female, 0 if male.

involving the net wage. This appears to be a basic difference in the samples, for the equations with linear net wage and potential duration variables comparable to those in columns (1) and (3) have, when estimated on the NR data, significantly negative net wage coefficients (see Chapter VI). However, as indicated in Chapter VI, that result disappears when the net wage variable is decomposed into W and B. Thus, the two studies offer broadly similar results: insignificant benefit effects (if not negative, as in the NR data) but significant and positive duration effects.

D. TRUNCATION PROBLEMS IN THE DATA

The estimated effects of potential duration in the results thus far are not only significant but quite large in magnitude. The effects are never less than .50 and are as high as .87. The implied disincentive effects of extensions of UI benefits are considerably larger than most other estimates and would be of concern for public policy if correct. However, a possible truncation bias in the data could be partly responsible for these results. Such a bias could have arisen because of the restriction of the sample to exhaustees of regular UI benefits (recall that only EB and FSB recipients are in the sample). Since those with longer regular UI durations--and hence, longer FSB durations on average--will, by definition, have to be out of work for longer periods to make it into the sample, a spurious positive correlation will be set up between potential duration and the length of the spell. For example, suppose one had drawn FSB recipients from two states with uniform, but different, potential regular UI durations. Assume too that the difference in potential duration has no effect at all on unemployment duration, so that the average length of the individual unemployment spell is the same in both states (exhaustees and nonexhaustees together). Then, <u>in the data selected</u>, the average spell length in the state with the higher potential duration would be larger than that in the other state, since to be an FSB recipient in the first place one had to be unemployed longer.

A graphical illustration of the truncation bias is illustrated in Figure 1. The 45 degree line in the figure has slope equal to one, and thus delineates exhaustees from nonexhaustees: any data point above the line represents an exhaustee. A hypothetical "true" regression line is drawn in with slope less than one. The data points will be randomly distributed above and below this line with, on average, a deviation from the line of zero. But if nonexhaustees are excluded from the sample, then only the "o" data points will be included in the data--the "x" data points will be excluded. An OLS regression line will go through the "o" points only, generating a fitted line such as that shown in the Figure. The slope of the fitted line is obviously biased upwards. Because individuals with higher potential durations (P) can make it into the sample only if they have a large positive residual (higher-than-average N or U), a spurious positive relationship will be induced.

One implication of the diagram in Figure 1 is that the bias will be greater at higher levels of potential duration, P, than at lower levels. At low levels of P, little truncation takes place and a fitted OLS line may come close to the true line; at high levels of P, the truncation bias would be most severe. To see if this pattern is true in the FSB data, the regressions were run separately for three subsamples, defined to include individuals with low, medium, and high values of P*, the potential duration for EB (this is the minimum spell length required to become an FSB

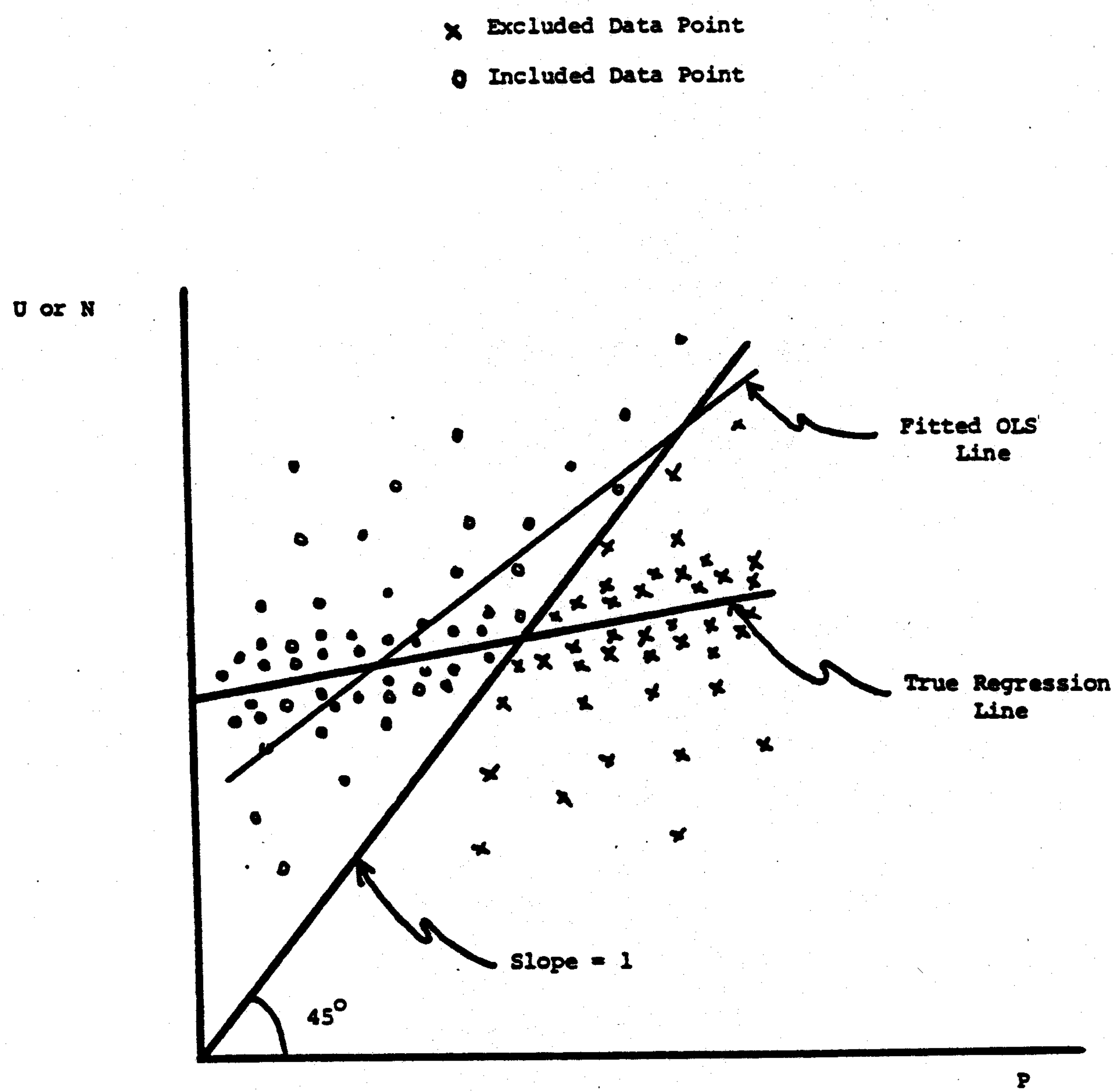

Figure 1: OLS Bias in a Truncated Sample

recipient). The results are shown in Table V.8. Although the coefficients on P bounce around considerably as higher levels of P* are considered, there is an unmistakable positive trend in the coefficient. Indeed, at the highest category of P* (as well as at lower levels for some regressions) the coefficient on P is greater than one. Of course, it should be noted that, in principle, this test cannot separate trunction bias from a true nonlinear relationship (the straight-line true relationship drawn in Figure 1 may not hold, that is), but the magnitudes of the coefficients here are implausible.

A somewhat different examination of this issue is shown in Table V.9. The first section of the table repeats the full results for the N and U regressions. The second section of the table shows the result of deleting the EB subsample. Deleting the EB subsample effectively truncates the data at a yet higher level of P, and thus should make the truncation problem worse. Indeed, as the results show, the coefficient on P registers a sizable jump when this group is omitted. The third and fourth sections of the table show the effect of truncating the sample further by imposing the restriction that the lengths of the initial spells of N and U be greater than the exhaustion point P* for those in the FSB subsample. The data do in fact contain individuals (approximately 25 percent of the sample) whose N and U are less than P* even though they received FSB; this is presumably because they had multiple spells of nonwork and unemployment. The results in the table show that the duration effects again trend upward, providing further evidence of truncation effects.

There is unfortunately no simple solution to truncation problems in general or to this one in particular (the same holds, it should be

TABLE V.8

ESTIMATES ON THREE SUBSAMPLES[a]

	Males		Females	
	N	U	N	U
$P^* < 25$				
W	0.04 (0.07)	0.08 (0.06)	0.23* (0.12)	0.21* (0.11)
r	21.70 (32.93)	30.42 (31.18)	53.39 (36.29)	57.12* (32.40)
P	0.90 (0.62)	1.17* (0.60)	0.61 (0.90)	0.59 (0.79)
$25 < P^* < 35$				
W	−0.10 (0.08)	−0.01 (0.08)	−0.11 (0.13)	0.00[b] (0.11)
r	−44.87 (37.69)	−28.93 (32.61)	−15.89 (33.53)	4.23 (28.25)
P	1.00 (0.80)	0.64 (0.75)	2.41* (0.86)	1.92* (0.73)
$35 < P^*$				
W	−0.03 (0.04)	−0.03 (0.04)	−0.02 (0.06)	−0.01 (0.05)
r	7.72 (21.07)	9.67 (20.77)	−12.22 (24.50)	−2.53 (23.13)
P	3.21* (1.70)	2.44 (1.55)	1.47 (1.92)	1.60 (1.80)

NOTES: Standard errors in parentheses.
*: Significant at the 10 percent level.

[a] P^* = EB exhaustion point.

[b] Less than 0.005.

TABLE V.9

ESTIMATES ON ADDITIONAL SUBSAMPLES

	Males		Females	
	N	U	N	U
Full Sample				
W	-0.00[a] (0.03)	0.01 (0.03)	-0.01 (0.04)	0.00[a] (0.04)
r	11.86 (15.65)	14.18 (15.10)	-0.31 (16.56)	5.00 (14.84)
P	0.64* (0.13)	0.54* (0.12)	0.76* (0.16)	0.69* (0.14)
"EB-Onlies" Omitted				
W	-0.02 (0.03)	-0.01 (0.03)	-0.02 (0.05)	0.01 (0.40)
r	11.05 (18.17)	9.66 (17.59)	-1.37 (19.58)	11.17 (17.37)
P	0.81* (0.14)	0.74* (0.14)	0.82* (0.18)	0.73* (0.16)
Individuals with $N < P^*$ Also Omitted				
W	0.00[a] (0.03)	0.00[a] (0.03)	-0.02 (0.05)	-0.01 (0.04)
r	9.43 (18.24)	9.95 (18.12)	9.47 (19.30)	1.42 (17.05)
P	0.88* (0.14)	0.81* (0.14)	1.00* (0.18)	0.86* (0.16)
Individuals with $U < P^*$ Also Omitted				
W	0.01 (0.03)	0.01 (0.03)	-0.01 (0.05)	-0.01 (0.04)
r	23.42 (19.41)	13.94 (17.90)	14.57 (19.92)	1.69 (17.00)
P	0.93* (0.15)	0.79* (0.14)	0.95* (0.19)	0.88* (0.16)

NOTES: Standard errors in parentheses.
*: Significant at the 10 percent level.

[a] Less than 0.005 in absolute value.

mentioned, for samples drawn solely from UI data). This is because the data needed to relax the truncation problem are by definition not in the data set and hence are not available. However, there are some types of solutions available from maximum likelihood estimation. If a specific distributional assumption is made for the dependent variable--for example, the normal--then a maximum-likelihood procedure analogous to Tobit analysis can be used to correct for the truncation. If the distribution of spell lengths is assumed to be normal, then the observations in the truncated data set can be fit to a truncated normal distribution. The essence of the procedure, however, is that it is assumed that the missing sections of the data are also (truncated) normal, an assumption that even in principle cannot be tested with the available data set. There are two sorts of general approaches to take to this problem of the nonobservability of the missing sections of truncated populations. One is to simply fit the truncated data to a variety of distributions--normal, log-normal, exponential, and so on--to see if the results vary with the distributional assumption. If they do not, one may safely assume that the shape of the distribution in the unobserved range does not greatly affect the coefficients of interest. However, if they do vary, little can be concluded about the true effect. A second approach is to altogether forgo the attempt to fit the entire distribution, and attempt instead to fit only the distribution in the observed, sampled population. In an obvious and fundamental sense, if the data only contain individuals with potential durations greater than 26 weeks, it should be surprising if one could come up with estimates of the effect of potential durations of less than 26 weeks. Yet, the method of assuming a specific distribution does precisely this. If one instead fits only the truncated distribution, one is obviously on safer ground; but the result is that estimates can only be made for the effect of altering potential durations from the minimum P in the data to higher levels of P.

As discussed in Chapter II above, implementing the latter, non-parametric approach on the FSB data is rather difficult because the data are truncated on both the upper and the lower ends of the distribution. To apply the Kaplan-Meier or the Cox techniques to this data would therefore require some new statistical development. Instead, what was attempted here was a fit of one parametric distribution. The exponential distribution was chosen on a priori grounds as being the closest single distribution to the true distribution. The FSB data were then used in a maximum likelihood procedure to fit the data to a doubly-truncated distribution, at the upper end by the interview date and at the lower end by the FSB eligibility date. The results are shown in Table V.10. As the table indicates, wage effects are insignificant for males and significantly negative for females. The effect of the benefit is significantly negative for males and significantly positive for females. But, most important, the effect of duration on U does not fall in these estimates; indeed, it rises a bit. This unexpected result has two very different explanations. First, it could be that the exponential distribution is incorrect. The usual criticism of the exponential distribution is that it fits the unemployment spell distribution poorly in the upper tail, usually that of 30 or so weeks of unemployment and beyond. Here we are dealing with a very unusual tail of the distribution where the mean U is over 60 weeks. There is little guidance to choose a distribution in this case. Second, it could be that

TABLE V.10

TWO-LIMIT EXPONENTIAL ML RESULTS

	Males	Females
W	.014 (.035)	-0.34* (0.10)
B	-.45* (.17)	0.60* (0.32)
P	1.18* (0.17)	0.88* (0.30)
Unemp. Rate	-1.03 (2.63)	5.04* (2.96)
Race	7.17 (7.21)	-35.46* (8.92)
Intercept	38.15	38.0

Standard errors in parentheses.

*: Significant at 10 percent level.

the exponential distribution is correct and the truncation problem is not as severe as one might think.

Given the diametric opposite conclusions that are drawn from these two alternative interpretations of the results, little can be said with confidence about the FSB results. Although the results in Table V.10 have been obtained from the most sophisticated econometric technique used in this chapter, there are strong intuitive reasons for considering them with caution. First, the coefficients are implausibly large. Virtually no prior study has obtained coefficients larger than one, for which it is difficult to derive an explanation. Second, the sensitivity tests to the truncation point discussed above and illustrated in prior tables suggested the presence of a truncation bias of the type discussed; this evidence cannot be ignored. Third, the estimates here are very much out of line with those in the other studies in this report, and therefore deserve less weight.

Another possibility for explaining the difference between the results obtained on this data set and those on the other data sets, at least the CWBH and the JSARP data set, is that the spells here are very long. It is possible that the disincentive effects of UI become large when potential duration reaches FSB levels, and when the unemployment rate is high. This is mere speculation at this point, for none of the evidence in this report suggests such a result. The unemployment rate has not been one of the more significant variables in the analyses, and the P coefficient was not that large in the Newton-Rosen data (see Chapter VI), where durations and spell lengths approach those of this data set. The conclusion to be drawn is that the FSB data are providing intrinsically outlier estimates. In the future, estimates of the KM model or the Cox model may provide better estimates.

E. WAGE EFFECTS

An additional issue in the literature on UI concerns the effect of UI on subsequent wages. Part of the rationale for the UI program is to allow individuals to search longer or merely to wait longer for jobs more suitable for their skills and with higher wages than jobs that might be immediately available. In their prior work on this data set, Brewster et al. found no significant effects of UI on subsequent wages. Table V.11 shows the estimates obtained here. For males, most effects are weak and insignificant, but the effect of the replacement rate is positive and also significant in one case. Thus, a very weak indication of positive effects on wages is suggested. For females, however, replacement-rate effects are insignificant and duration effects significantly negative. This result could be related to the truncation problem discussed above. If those with higher potential durations are the longer-term unemployed, and if the longer-term unemployed are those who have failed to obtain large or even positive wage gains (over their pre-UI wage), then the negative coefficient in the table could result. This is of course only a speculative explanation.

F. INTRA-HOUSEHOLD EFFECTS

One of the possible effects of the UI system is an effect on the work effort of members of the household other than the UI recipient. The added income provided by UI benefits may allow other household members to

TABLE V.11

POST-UNEMPLOYMENT WAGE REGRESSIONS[a]

	Weekly Wage on First Post-Spell Job		Average Weekly Wage in 1977	
	Males	Females	Males	Females
W	0.52* (0.07)	0.26* (0.10)	0.45* (0.07)	0.42* (0.07)
r	65.14* (35.92)	-28.48 (34.87)	50.27 (35.05)	-17.92 (24.56)
P	-0.23 (0.27)	-0.54* (0.32)	0.13 (0.27)	-0.44* (0.22)

NOTES: Standard errors in parentheses.
*: Significant at the 10 percent level.

[a] Those without a wage (i.e., not working) omitted. Also, W and r in gross terms, not adjusted for taxes.

not seek new jobs, for example. Likewise, an extension of benefits may allow other household members to postpone obtaining new jobs.

Respondents in the initial FSB interview were asked some questions related to the response of other members of the household to their unemployment. In response to the question, "Because you lost that job, did anyone in your household go to work who wasn't working?", 5.3 percent of the men and 1.8 percent of the women said "yes." Thus, at least for men, there seemed to be some intra-household effects. Respondents were also asked if anyone in the household began to work longer hours on a job they already had, to which 2.6 percent of the men and 3.6 percent of the women responded "yes," not very large figures. Finally, when asked if anyone in the household picked up a second job, less than 1 percent of the men and 3.1 percent of the women said yes. Thus, although there seems to be some evidence of intra-household effects, they do not seem large.

The more pertinent question for present purposes is whether the level of the UI benefit or potential duration of the UI recipient affected the earnings of others in the household. Since some family earnings data were collected, this question can be addressed with regression analysis. The results are shown in Table V.12. As the table indicates, there are almost no detectable effects of r and P on the earnings of the spouse or others in the household. The single coefficient that is significant at conventional levels suggests that higher UI benefits led to smaller earnings of tertiary household members. However, given the widespread insignificance of the coefficients and the consequent likelihood that this effect could have occurred by chance, the evidence for intra-household effects in general appears to be weak.

TABLE 12

REGRESSION ESTIMATES OF EFFECT OF RESPONDENT UI ON EARNINGS OF SPOUSE AND OTHERS

	Dependent Variables			
	Earnings of Spouse at Second Interview (Weekly)	Earnings of Spouse in 1976 (Annual)	Earnings of Spouse in 1977 (Annual)	Earnings of Others at Second Interview (Weekly)
Males				
r	17.82 (35.76)	1269.68 (1650.94)	500.27 (1729.60)	-4.79 (23.77)
P	0.28 (0.29)	9.83 (13.42)	6.30 (14.14)	0.08 (0.19)
Females				
r	68.28 (44.57)	557.63 (2539.33)	-245.93 (2558.58)	-43.16* (21.71)
P	-0.06 (0.42)	16.60 (23.94)	-31.46 (24.08)	0.31 (0.20)

NOTES: Standard errors in parentheses
*: Significant at the 10 percent level.

[a] Other variables in equation are those in the standard set (which includes spouses' pre-earnings) and W. W and r are not adjusted for taxes.

G. SUMMARY

The analysis of the effect of potential duration in the FSB data set on weeks of unemployment and nonwork has yielded few solid conclusions. In the course of the analysis, a truncation bias present in the sample design was discovered which, it is suspected, led to implausible results for the coefficient magnitudes. Some attempt to control for the bias with maximum likelihood was attempted, but this appeared to be unsuccessful. As a result of the analysis, therefore, very little has been learned that can be generalized or upon which policy could be reliably based. Future work should apply the Kaplan-Meier or Cox model to the data to control for the truncation bias in a superior fashion.

The data set also provided some evidence on the effect of UI and duration on post-unemployment wages and on other members of the family. The results of the analysis suggest very little such effects. Post-unemployment wages were generally insignificantly affected by potential duration, and for intra-household effects, insignificance was the rule rather than the exception. More work on other data sets on these issues would be of interest.

VI. ANALYSIS OF THE NEWTON-ROSEN DATA SET

The data of Newton and Rosen (Newton and Rosen, 1979) is our second UI administrative data set. The Newton-Rosen data (NR) are drawn from the UI records in Georgia over the period 1974-1976. In their study Newton and Rosen stated that their study's contributions were (1) to provide an additional set of estimates of the effect of UI; (2) to use benefit and potential duration data drawn directly from UI records instead of having to be imputed onto survey data; (3) to impose a theoretically correct specification of the UI benefit (in the form of a net wage, or net cost of unemployment, rather than as a replacement ratio); and (4) to deal with the truncation of spells inherent in UI administrative data arising from the fact that weeks of unemployment of exhaustees are not fully observed with a correct econometric procedure (Tobit analysis). As NR note in their study, a disadvantage of their study is that it covers only one state.

Professor Rosen kindly made available the NR data set for this project. The data set includes 24 variables for each individual in the sample, the variables being those used in the regressions reported in the NR article plus a handful of others used in additional regressions. As noted above, the data were drawn from administrative records in the UI offices in Georgia over the period 1974-1976, and contain information on the number of weeks of UI compensation, the weekly benefit amount, potential duration of UI, race, sex, pre-unemployment wages, and so on. In addition, NR added a variable for the local unemployment rate to the UI record data. The data set includes 627 observations.

The means of the main variables in the NR data are shown in Table VI.1. The individuals in the sample were unemployed an average of about 19 weeks, had a mean benefit of $62, and were eligible for an average of 44 weeks of benefits. Note that potential duration has a maximum of 65 weeks. Since EB was in effect for much of this period and since FSB began in 1975, extended and supplementary benefits were available to many of the individuals in the sample. The mean (net) replacement ratio is .52. Note too that 8 percent of the sample were exhaustees (of regular, EB, or FSB--we do not know which). It is this 8 percent that necessitates the use of Tobit analysis, which corrects for the truncation of the dependent variable (unemployment spell length) at the potential duration point.

Table V.2 shows the basic set of runs using the NR data. The first column is a replication of the results reported in the NR article. The coefficients on the variables shown are close to those reported in the article (the differences, though small, are probably the result of using a different Tobit package). The notable features of the NR equation are (1) that the net cost of an additional week of unemployment, denoted W(1-r) here, is entered rather than the replacement ratio, r, and (2) both the net cost and potential duration are entered in quadratic form and interacted. As NR note in their article, when the coefficients on the five UI variables are evaluated at their means, net cost has a negative effect on unemployment-spell length and potential duration has a positive effect, as is generally expected. Blacks appear to have shorter spells, women have longer spells, but the unemployment rate appears, strangely, to have no effect on mean spell length (more on this below).

TABLE VI.1

DESCRIPTIVE STATISTICS OF NEWTON-ROSEN (NR) DATA

Variable	Mean	Standard Deviation	Minimum	Maximum
Unemployment Duration	18.8	15.2	1.0	68.0
Weekly Benefit Amount (B)	62.1	17.3	12.0	90.0
Potential Duration (P)	44.2	16.5	4.0	65.0
Weekly Earnings After Tax (W)[a]	129.2	61.3	22.2	455.2
Net Replacement Ratio (r=B/W)	0.52	0.10	0.17	0.75
Net Wage [W(1-r)]	67.1	50.2	10.2	365.2
Race (1 if black, 0 if not)	0.35	0.48	0.0	1.0
Sex (1 if female, 0 if non)	0.37	0.48	0.0	1.0
Unemp. (Unemployment Rate)	8.2	0.8	5.2	9.5
Exhaustion (1 if exhaust, 0 if not)	0.08	0.28	0.0	1.0

[a] Computed from variables on file: W = B/r.

TABLE VI.2

UNEMPLOYMENT DURATION EQUATIONS ESTIMATED ON NR DATA SET

	NR Replication (Tobit)	Simplified Equation (Tobit)	OLS	
			Quadratic	Linear
W(1-r)	-0.142* (2.56)	-0.052* (3.59)	-0.135* (2.62)	-0.053* (3.98)
P	-0.078 (.37)	0.37* (8.16)	0.16 (0.87)	0.41* (10.6)
$[W(1-r)]^2$	0.0007* (4.84)	—	0.0007* (5.13)	—
P^2	0.0077* (2.84)	—	0.0054* (2.21)	—
P[W(1-r)]	-.0021* (2.05)	—	-0.0021* (2.23)	—
Race	-5.15* (4.06)	-5.18* (3.99)	-5.11* (4.39)	-5.15* (4.34)
Sex	0.33 (0.25)	1.48 (1.11)	0.17 (0.14)	1.31 (1.08)
Unemp.	-0.18 (0.02)	0.15 (0.18)	-0.53 (0.70)	-0.36 (0.47)
Constant	19.11* (2.46)	6.89 (1.02)	16.93* (2.36)	8.28 (1.33)

NOTES: Asymptotic t-statistics in parentheses (unsigned).

*Significant at the 10 percent level.

To make the results easier to read directly from the coefficients, the simpler specification shown in the second column of the table was estimated. This equation contains no interactions and only linear terms for the net cost and potential duration variables. The results show clearly a negative effect of net cost--hence, a positive effect of the UI benefit, since "net cost" equals the net wage minus the UI benefit--and a positive effect of potential duration on unemployment spells. Both coefficients are highly significant. The coefficient on P is .37, indicating that one extra week of benefits increases the mean unemployment spell by a little more than a third of a week.

The third and fourth columns show the results of estimating the equations with ordinary least squares (OLS) instead of Tobit. As the table indicates, those coefficients that are significant in the Tobit equations are quite close in magnitude to the OLS coefficients, sometimes almost exactly the same. This is probably a result of the fact noted above that only 8 percent of the sample is truncated. When such a small fraction of the sample is at the limit, Tobit and OLS estimates are frequently quite close. As a result of this finding, many of the subsequent runs done in the analysis below were done with OLS.

Table VI.3 shows the results of several alternative specifications of the equations. The first column shows the result of disaggregating the net cost, or net wage, variable, into its two components, W and B. Recall in our discussion in Chapter II that one of the specification issues in the estimation of the basic spells regression is whether the UI benefit should be combined in one way or another with the net wage in the equation, or whether the benefit should stand separately. The results in the table show

TABLE VI.3

ALTERNATIVE ESTIMATES USING NR DATA

	Disaggregated Net Wage (Tobit)		Separate Estimates by Sex (OLS)					
	(1)	(2)	(3)		(4)		(5)	
			M	F	M	F	M	F
W	-0.039*	-0.053*	--	--	-0.035*	-0.064*	-0.041*	-0.085*
	(2.63)	(2.93)			(2.34)	(1.28)	(1.97)	(2.61)
B(=rW)	-0.106*	--	--	--	-0.115*	-0.059	--	--
	(2.03)				(1.92)	(0.56)		
r	--	1.45	--	--	--	--	5.04	0.28
		(0.16)					(0.48)	(0.02)
W(1-r)	--	--	-0.044*	-0.101*	--	--	--	--
			(3.11)	(2.25)				
P	0.43*	0.40*	0.38*	0.49*	0.44*	0.54*	0.41*	0.53*
	(8.73)	(8.41)	(7.96)	(7.08)	(8.44)	(7.18)	(8.12)	(7.14)
Race	-5.54*	-5.43*	-5.80*	-3.26	-6.14*	-3.56*	-6.01*	-3.54*
	(4.29)	(4.18)	(4.10)	(1.47)	(4.36)	(1.60)	(4.23)	(1.59)
Sex	0.54	0.90	--	--	--	--	--	--
	(0.41)	(0.67)						
Unemp.	-0.66	-0.27	0.19	-1.28	-0.79	-1.80	-0.20	-1.66
	(0.77)	(0.31)	(0.20)	(0.99)	(0.78)	(1.36)	(0.20)	(1.26)
Constant	19.42	11.76	4.89	15.76	19.28*	23.17*	6.65	21.16
	2.50)	(1.15)	(0.63)	(1.52)	(2.07)	(2.06)	(0.55)	(1.38)

NOTES: T-statistics in parentheses.

*Significant at the 10 percent level.

that the coefficient on W is negative, as expected, but that on B is also negative. Furthermore, the latter is quite significant. No ready explanation for this finding comes to mind, with the exception of a speculative explanation to be mentioned below. But, it does highlight the importance of disaggregation. In the second column the replacement rate is substituted for the benefit. Although this specification does not correspond to a linear decomposition of the net cost variable, it is a specification that has been estimated frequently in other UI studies and hence is of interest. As the table indicates, the coefficient on the replacement rate is positive but insignificant. Note, of course, that since the replacement rate contains W in its denominator, this finding is not inconsistent with that in the first column--it appears that the W variable is driving the coefficient on r.

The last six columns in Table VI.3 show the results of disaggregating by sex (OLS is used). Since most of the other equations that have been estimated are sex-specific, these estimates are for comparability. Estimates are shown for the constrained net-cost specification as well as for the two unconstrained specifications. The results are quite similar to those obtained for the pooled sample: while net cost has a negative effect in the constrained case, the benefit and replacement ratio have either the wrong sign or are insignificant in the unconstrained equations. Thus, the puzzling result obtained above remains.

A final set of explorations were conducted on the unemployment rate variable. As noted by NR and as shown in the results thus far, the unemployment rate coefficient is negative and insignificant in the equations. This is particularly surprising in light of the time period of the analysis, for the unemployment rate rose sharply from 1974 to 1975 (from 5.6 to 8.5 nationally and from 5.2 to 8.6 in Georgia), reaching what was then the highest level since the Depression. The unemployment rate fell in 1976(down to 7.7 nationally and to 8.1 in Georgia), though not back to its 1974 level. Although it is possible that some individuals in certain circumstances (such as those on temporary layoff) might have shorter spells in a recession, it is rather surprising that the mean spell length would fall in a recession of such severity.

To examine the unemployment-rate effect in more detail, the effect was allowed to differ by the level of the unemployment rate. Specifically, the range of the unemployment rate (from 5.2 to 9.5, as shown in Table VI.1) was divided into the three ranges of (1) less than 7.5, (2) between 7.5 and 8.5, and (3) greater than 8.5. This particular division was chosen so as to result in approximately equal sample sizes in the three intervals. In Table VI.4, column 1, the results of allowing the coefficient on the unemployment rate to differ in these three intervals are shown. As the results indicate, the unemployment rate coefficient is quite positive and significant over the low range, positive but less significant in the middle range, and extremely negative and significant in the third and highest range. The equations shown in columns (2)-(4) are those that result when the sample is stratified into the three subsamples (effectively interacting all other variables in the equation with the unemployment rate interval). The coefficients on the unemployment rate show the same pattern. This pattern is quadratic, implying that unemployment spell lengths initially rise but eventually fall as the unemployment rate rises.

TABLE VI.4

ALTERNATIVE SPECIFICATIONS OF THE UNEMPLOYMENT RATE
(Tobit)

	(1) Piecewise Linear	(2) Unemp. $\leq$ 7.5	(3) 7.5 < Unemp. $\leq$ 8.5	(4) 8.5 $\leq$ Unemp.
W	-0.030*	-0.026*	-0.041*	-0.020
	(2.14)	(1.88)	(1.69)	(0.76)
B	-0.081	-0.019	-0.18*	0.047
	(1.63)	(0.38)	(2.22)	(0.48)
P	0.387*	0.227*	0.549*	0.220*
	(8.30)	(4.64)	(7.31)	(2.41)
Race	-5.45*	-6.89*	-7.87*	-1.80
	(4.53)	(4.74)	(3.93)	(0.86)
Sex	1.17	-0.20	1.03	2.04
	(0.94)	(0.12)	(0.49)	(0.98)
Unemp.	--	14.70*	2.27	-24.54*
		(7.80)	(0.58)	(7.22)
(D1)(Unemp.)	13.66*	--	--	--
	(4.02)			
(D2)(Unemp.)	5.74	--	--	--
	(1.61)			
(D3)(Unemp.)	-24.37*	--	--	--
	(8.11)			
D2	51.33	--	--	--
	(1.40)			
D3	310.88*	--	--	--
	(8.70)			
Intercept	-83.19*	-87.76*	-1.85	227.32*
	(3.57)	(6.83)	(0.06)	(7.56)

NOTES: T-statistics in parentheses

*Significant at 10 percent level.

D1 = 1 if Unemp. < 7.5
D2 = 1 if 7.5 $\leq$ Unemp. < 8.5
D3 = 1 if 8.5 $\leq$ Unemp., 0 if not

Although this could of course be a correct finding from the data, it is natural to consider alternative explanations for this result. One possible explanation could lie in an inadvertent truncation of spells in the collection of the data. A common difficulty in the collection of unemployment spell data arises if some individuals have not completed their spells at the time the data are collected. If, for example, all individuals have the same expected spell length, those who are in the middle of their spell lengths will by definition have shorter mean spell lengths than those who have completed their spells. Since the unemployment rate was rising over the 1974-1975 period, then if there were an inadvertent truncation of the spells of those who began their spells later in the period, a negative spell-length-unemployment-rate relationship of the quadratic type found above could be generated. In addition, this could provide an explanation of the negative benefit effect. NR note in their article that Georgia raised the benefit schedule in 1975; hence, a truncation of the spells in the later parts of the period could induce a negative spell-length-benefit relationship. Indeed, Table VI.4 shows that the negative benefit coefficient drops greatly in significance in the low-unemployment-rate range.

It should be pointed out immediately, however, that NR explicitly state that those with incomplete spells were not included in the data set. NR also state that only 7 observations (relative to 627 in the data set) were not included for this reason. This seems small, but could simply be a result of collecting the data at a sufficiently late date that most of those whose spells began in 1974-1976 had completed them. On the other hand, since FSB was in effect over part of this period, some spells may have appeared to be complete when they were not. Whereas the maximum of 26 weeks of regular UI benefits must be collected within the individual's benefit year--that is, within 52 weeks of the time he or she qualifies--extended benefits and FSB are not under this restriction. If an individual has reached the end of his or her benefit year and cannot requalify for regular UI benefits, an extra 13 weeks of EB and then possibly 26 weeks of FSB can be collected more or less indefinitely, with no limit on the latest date of receipt. Thus, individuals with several interruptions in their spells who repeatedly returned to continue collecting benefits could have collected for up to two years, or more.

It should also be noted that this issue is not unconnected with the effect of potential duration estimated in the equation. For example, Table VI.5 shows the distribution of potential duration in the NR data. As the distribution indicates, it appears that there is a slight clustering of the data around 30-35 weeks of benefits, approximately the length of potential duration of EB alone. There is also a marked clustering around the 60-65 FSB range. Since FSB went into effect in 1975 at the same time that unemployment rates were rising, potential duration and spell lengths should be positively correlated (as they are in the data--the correlation coefficient is .38). Although the unemployment rate variable should, ordinarily, control for this effect, its negative coefficients in the main regression equations suggest that it may not have done so. Indeed, in Table VI.4, the coefficient on potential duration is somewhat small (.23) in the low-unemployment-rate subsample (i.e., possibly the early, 1974, EB only subsample), although its coefficient jumps around considerably as well in the other equations.

TABLE VI.5

DISTRIBUTION OF POTENTIAL DURATION IN NR DATA

Range	Percent of Sample
0–5	0[a]
5–10	2
10–15	3
15–20	4
20–25	7
25–30	8
30–35	10
35–40	8
40–45	5
45–50	12
50–55	4
55–60	15
60–65	22
	100

[a] Less than 1 percent.

A more fundamental issue should be noted in this connection, related to the change in potential duration over the 1974-1975 period. A question that must arise when potential duration changes is what value of "P" to give to individuals who are in the middle of their spells at the time the change occurs. Giving such individuals the "old" P value would seem incorrect, since they now face a different potential duration. But if they are given the "new" P, an upward bias is induced in the spell-length-potential-duration relationship, for an individual's value of P would be a positive function of spell length for definitional reasons (i.e., a positive correlation would be observed if even if there were no true effect of potential duration on spell length). This bias, is clearly, unrelated to the business cycle and the unemployment rate. Although it is not known how NR assigned P values for such individuals, this could have biased the P coefficient upwards.

<u>Sensitivity to Distributional Assumptions</u>. As discussed in Chapter II, one of the issues in the current literature on truncation is the importance of the distributional assumption made in the analysis. The most common distributional assumption made by economists and other analysts and statisticians is the normal distribution, made loosely upon the basis of the Central Limit Theorem. As an empirical matter, however, many distributions are far from the normal. The distribution of unemployment spells would seem to be one of these. First of all, unemployment spells cannot be negative. Thus, the normal distribution, which ranges from plus infinity to minus infinity, would seem to be appropriate. But also, even if a truncated normal were used, the symmetry of the distribution does not seem to correspond well to the distribution of spells. As shown above in

the discussion of the CWBH data, hazard rates vary a great deal and the distribution has no mode of the type suggested by the normal.

Nevertheless, it is an empirical question as to how much the parametric distribution assumption makes. In Table VI.6 some evidence on this question is reported, where we show the effects of assuming the distribution is lognormal instead of normal, and exponential as well. The first column in the table repeats the OLS results from above. The second column shows the Tobit estimates (i.e., those assuming a normal distribution). These differ from those presented previously only by virtue of being completely stratified on sex. As noted before, the OLS results and the Tobit results are fairly close and show no large differences. The third column shows the results of assuming the distribution is exponential, a distribution more often used for unemployment spells. As the table shows, the results change dramatically. But the net wage and the benefit lose their significance for males, and become even less significant than previously for females. Furthermore, the coefficient on duration is markedly reduced for males (by over 50 percent) and by a smaller amount for females (about 25 percent). Another interesting result is that the coefficient on the unemployment rate is, for the first time, positive and significant (also quite large in absolute magnitude). The main implication of these results is that it appears that the results can be quite sensitive to the distribution that is assumed for the data. In the case under consideration, if one were to have to choose one of the distributions, the exponential case would be the one to choose because unemployment spells are clearly distributed closer to the exponential than the normal.

TABLE VI.6

PARAMETRIC MAXIMUM LIKELIHOOD ESTIMATION ON NR DATA

	Dependent Variable: U			Dependent Variable: logU	
	OLS	Normal ML (Tobit)	Exponential ML	OLS	Lognormal ML (Tobit)
Males:					
W	-0.035*	-0.033*	-0.006	-0.003*	-0.003*
	(2.34)	(2.13)	(0.02)	(2.50)	(1.92)
B	-0.115*	-0.11*	-0.007	-0.01*	-.01*
	(1.95)	(1.77)	(0.09)	(4.58)	(1.80)
P	0.44*	0.40*	0.17*	0.026*	.022*
	(8.44)	(6.70)	(2.33)	(6.14)	(4.43)
Race	-6.14*	-6.03*	-4.44*	-.55*	-.55*
	(4.36)	(4.00)	(2.35)	(4.50)	(4.26)
Unemp	-0.79	-0.37	4.70*	-.0001	.037
	(0.78)	(.34)	(1.49)	(.08)	(.41)
Constant	19.28	18.14	-23.62	2.48	2.38
Females:					
W	-0.064*	-0.069	-0.03	-.001	-.001
	(1.28)	(1.25)	(0.14)	(.004)	(.27)
B	-0.059	-0.063	0.018	-.011	-.011
	(0.56)	(0.53)	(0.25)	(1.35)	(1.22)
P	0.54*	0.49*	0.37*	.026*	.023*
	(7.18)	(5.49)	(2.50)	(6.51)	(3.46)
Race	-3.56*	-3.96*	-4.58*	-.30*	-.34*
	(1.60)	(1.59)	(3.54)	(2.61)	(1.8)
Unemp	-1.80	-1.11	3.59*	-.25*	-.19*
	(1.36)	(0.75)	(1.87)	(2.50)	(1.70)
Constant	23.17	21.40	-18.39	4.18	4.05

NOTES: Unsigned t-statistics in parentheses.
*: Significant at 10 percent level.

The last two columns show the results of assuming lognormality for the distribution of unemployment spells. The fourth column shows the OLS results for an equation with log(U) as the dependent variable, while the last column shows the results of estimating Tobit with lognormality instead of normality assumed for the dependent variable. Clearly, lognormality is to be preferred to normality because the distribution is constrained to lie in the positive-U range. The results show, first, that again OLS and normal ML are quite close. Comparing the coefficients on the logarithmic equations with the others, observe first the significance levels follow the same pattern as those on the regular Tobit. The lognormal coefficients themselves are of a different magnitude because they give the percentage change in U for a one-unit change in each right-hand-side variable. For P, the coefficient of .022 implies that a one-week increase in P increases U (at the mean) by about .41 weeks. This is fairly close to the regular normal ML. Thus, we find that the lognormal ML and the normal ML give one set of results and the exponential give another. Again, however, the exponential is to be preferred on a priori grounds. The lognormal forces the density of the spell of observations to increase from zero, which is not necessary the case.

Summary. The Newton-Rosen data set gives estimates of the effect of duration on unemployment spells of about somewhere in the range of .17-.44 weeks for males and from .37-.54 for females. The effect for females is a bit larger than that for males, as has been found in a number of other studies. The range for males is uncomfortably large, and arises from differences in the assumptions made about the underlying distribution of unemployment spells. A main methodological discovery in the analyses of these data was the sensitivity of coefficient estimates in general to the distributional assumption. On a priori grounds, the estimates from the exponential distribution seem to be the best. These provide the lower estimates at the lower end of the range, .17 for males and .37 for females. Thus, the lower part of the range should be given more weight than the upper.

VII. SUMMARY AND CONCLUSIONS

In this report we have analyzed four different data sets on unemployment insurance. These are the Continuous Wage and Benefit History (CWBH) data, the Job Search Assistance Research Project (JSARP) data, data from the Federal Supplemental Benefit (FSB) Follow-Up Survey, and the Newton-Rosen (NR) Georgia UI data set. The first and last are drawn primarily from UI administrative records, while the second and third size are primarily drawn from household survey data.

The results of estimating the effect of potential duration on the lengths of unemployment and nonwork spells using a common set of right-hand-side variables and a common specification yields a range of estimates. Our preferred estimates of these effects for three of the data sets are shown in Table VII.1. The CWBH data set shows an increase of .17 weeks for white males and .10 for white females from a one-week increase in potential duration. The JSARP data show a .45-week effect for males and a .28-week effect for females. The NR data set shows a .17-week effect for males and a .37-week effect for females. Thus, our findings suggest that the male effect lies in the range .17-.45 and the female effect lies in the range .10-.37. The JSARP data set also allowed us to estimate the effect of benefit extensions on the lengths of nonwork spells, which include time out of the labor force as well as time in the labor force but unemployed. The estimates for that data set show a .52-week effect for males and a .66-week effect for females. Thus, the effect on nonwork spells is somewhat larger than that on unemployment spells. These estimates all include the same right-hand-side variables and all control for the truncation of

TABLE VII.1

SUMMARY OF ESTIMATES OF EFFECT OF ONE-WEEK INCREASE IN POTENTIAL DURATION

	On Length of Unemployment Spell		On Length of Nonwork Spell	
	Males	Females	Males	Females
CWBH[a]	.17	.10	—	—
JSARP	.45	.28	.52	.66
NR	.17	.37	—	—

[a] Whites only.

spells, either at the exhaustion point of UI (for the first and third data sets) or for the interview date (the second data set).

The results of our analysis of the fourth data set, that from the FSB Follow-Up Survey, are not shown in the table. The results from this analysis showed effects which were implausibly large, often in excess of 1.0. Our analysis of the data set revealed that a sample selection bias was present in the fact that only EB and FSB recipients were included. A series of tests for the presence of this bias were conducted which suggested that it could be quite severe. An attempt was made to control for it by modeling the sample selection parametrically (with an exponential distribution) but this attempt was unsuccessful. Given the problems with this data set and given the large differences between its estimates and those of the other three data sets, we do not present them in the table.

A number of other findings were obtained from the analyses on the data sets:

1. Potential duration appears to have little effect on the wages received by those who become reemployed. This analysis was only conducted on the FSB data set, which has the problems just noted, and should be considered provisional.

2. Potential duration appears to have relatively little effect on the earnings and work effort of other members of the household. An analysis was again conducted only on the FSB data set, which has the problems just noted. It appeared that relatively few other members of the household went out to work or changed their work effort, either by working more or less or changing their labor supply, in response to the unemployment of the UI recipient. Also, regression analysis revealed little significant effect of potential duration on the employment status or the earnings of other members of the household.

3. Weak evidence was obtained that the effect of potential duration on unemployment-spell lengths is greater when the unemployment rate is high. This evidence was obtained from the FSB data and from the CWBH data. In both data sets this finding was obtained. However, the coefficients involved were very weak in statistical significance and hence this finding must be considered tentative.

4. The effect of a sudden introduction of a benefit extension on average spell lengths is smaller than that of a benefit extension that has been in place for some time. This is because some individuals are already in the middle of their spells when the new extension is introduced, or have become reemployed already. It was also found that the statistical estimates obtained in the study are stronger in significance and more precisely determined when the effects of duration changes over time are examined than when solely cross-state or cross-individual differences are examined.

5. Several methodological findings were obtained. It was found that tax rates do have a small effect on UI estimates. Also, it was found that the wage rate has different effects than the UI benefit, and that using the replacement rate in the regressions gives often incorrect estimates of UI effects. The importance of controlling for truncation in the data was also a major theme of the methodological work.

Several methodological advances were also made in the report. The most important is the last just referred to, relating to the truncation of the spells in the data. For the first time in this literature a technique was applied to the analysis which dealt with several problems simultaneously: (1) the truncation of the data common to all data sets, (2) the problem of the assumption of a particular distribution for unemployment spells, and (3) the problem of a changing value for potential duration as the spell progresses. One of the methodological findings in the report is the importance of these issues--controlling for truncation in the data makes a significant difference in the estimated effects of duration; the distributional assumption made can also have a major effect on the estimates; and accounting for the changing value of P over the spell gives different estimates than one obtains by ignoring the changing value. Estimates from previous studies in the literature should be reinterpreted in this light.

Future Research. The estimates of duration effects shown in Table VII.1 above fall into a narrower range than those discussed in Chapter II above, when prior studies in this literature were reviewed. Thus, we have been able to reduce the range of the estimates somewhat from what it was previously. But we believe that the range can be narrowed even further with further research, and that the means to do so are suggested by some of the analyses in this report.

The most broadly-defined avenue for future research suggested by this report is the importance of controlling for the dynamic decisions made by individuals over their unemployment spells. Most of the work done in this literature in the past has assumed that the environment surrounding individual decisions is stable--that potential duration is not changing, that the unemployment rate is constant, and so on. These assumptions have been made in the past for convenience, primarily because past analysts have not had available the statistical techniques necessary to model the complex way in which a constantly-changing unemployment rate or potential duration affects the individual as the unemployment spell progresses. By necessity, it has had to be assumed that such changes will have little effect on the estimates of duration. In this report, because we have been able to develop the statistical techniques necessary to examine this issue, we have been able to examine this question in a preliminary way. Specifically, we have found that the estimated effects obtained by examining changing values of P are quite a bit different than those obtained when ignoring such changes. We have not obtained any estimates of the effect of changing unemployment rates on spell lengths--here we have made the usual assumption that the level of the unemployment rate at the beginning of the spell, or the average over the length of the spell, is a sufficient control for the labor market--but the technique we have developed is applicable to such an examination, for the technique allows us to estimate any number of time-varying variables over the spell.

The importance of these issues for policy is considerable. The fact that sudden changes in P have a different effect than permanent, or expected changes in P means that the effect of a permanent change in regular UI duration will have a different effect than the periodic extensions provided by EB, and that the periodic extensions provided by EB will have a different effect than the ad hoc extensions provided by federal legislation such as FSB or FSC. Also, if the individual's search decisions are affected in a major way by the changes in the unemployment rate over time--or, perhaps, by how the changes in the individual's <u>expectation</u> of what the unemployment rate will be in the future--the effect of the business cycle on unemployment could change quite a bit from what policymakers currently think it is. This is important for potential duration policy for two reasons. First, if spells change in a systematic way in response to changes in the unemployment rate, this will affect the need for a benefit extension in the first place as well as the best timing for implementing a benefit extension. Second, if the effect of benefit extensions on the length of the unemployment spell <u>interacts</u> with the manner in which the unemployment rate is changing over time, then again the best timing and level of benefit extensions will be affected. Our knowledge of these effects could be increased by an in-depth examination of the data sets we have collected here in regards to the effect of changing potential duration and unemployment rates on spell lengths.

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