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AUTHOR Carr, Timothy James
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ABSTRACT

A number of hypotheses concerning the job search behavior of unemployed workers are tested in this study. The empirical literature on unemployment is reviewed and data from the National Longitudinal Surveys of Labor Force Behavior panels of young men (aged 14 to 24 in 1966) and middle-aged men (aged 45 to 59) are analyzed. Variables examined include duration of search; return to search; wage offer level; probability of receiving an offer; search cost; and length of horizon. The unemployment duration model and the acceptance wage model are presented and discussed in terms of these variables. Transition rate (i.e., the probability of moving from unemployed to employed status) is analyzed. Findings are summarized both from a policy perspective and as an indication of the efficacy of the hypotheses tested, and directions for future research are suggested. (Author/MC)

Center for Human Resource Research

A COMPARATIVE STUDY OF THE DURATION OF UNEMPLOYMENT OF YOUNG AND MIDDLE-AGED MEN

by

Timothy James Carr

College of Administrative Science

The Ohio State University

TABLE OF CONTENTS

Page

LIST OF TABLES

Table		Page

LIST OF FIGURES

CHAPTER I

INTRODUCTION

The optimal job search paradigm has frequently been invoked as an explanation of labor market phenomena such as turnover and unemployment. Policy-related discussions of such matters as the (alleged) unemployment-inflation tradeoff (the so-called Phillips curve)[1] and the effects of the institution of unemployment insurance[2] have been carried out using this framework. This study is an attempt to test several hypotheses implied by optimal job search theory, using a relatively unique data set.

First, a multivariate analysis of the determinants of the duration of spells of unemployment is performed, using data from national

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[1] This controversy concerns the question of whether high rates of inflation are associated with low unemployment rates, at least in the short run. A number of causal mechanisms have been proposed to explain this relation. If there is no tradeoff, then the resulting phenomenon is known as a "vertical Phillips curve" located at the "natural rate of unemployment." The voluminous research on this topic has been reviewed and criticized by Santomero and Seater (1978). The present study can be considered a direct contribution to that literature only in the sense that it sheds light on whether, say, the unemployment insurance system has increased the "natural" unemployment rate; that is to say, whether the Phillips curve has "shifted outward" (see Chapter VI for further discussion).

[2] The substantial policy concern over the economic effects of this system is evidenced by the papers persented at the Symposium on the Economics of Unemployment Insurance, most of which will be discussed in Chapter III. See Katz and Hight (1977) for a summary of the discussion. See also Hamermesh (1977).

probability samples of young men (aged 17 to 29) and middle-aged men (aged 48 to 64). Although a number of analyses of this sort have appeared in the literature, most of which focus on the effects of the liberality of unemployment benefits on unemployment duration, this study distinguishes itself by the richness of the data available, the specification of a larger and better set of "control" variables, and the attention paid to the possibility that the effects of certain variables on young workers may differ from their effects on older workers.

The analysis described above, in a sense, examines the determinants of the "average" level of unemployment duration. But hypotheses concerning the distribution of this variable can also be derived from search theory. These hypotheses have largely been ignored by researchers, but they are tested in this study. Again, explicit attention is paid to possible differences in the behavior of younger and older workers. This part of the analysis has possible policy implications in terms of such matters as the proper interpretation of unemployment statistics[3] and the effects of minimum wage laws.

Finally, a multivariate analysis is performed in which the dependent variable is the hourly rate of pay obtained by members of the samples after their unemployment experience. Again, the hypotheses that are tested are derived from optimal job search theory. In fact, most of these hypotheses are corollaries of the hypotheses tested in

[3]Cf. Salant (1977).

the preceding sections. It can hardly be said that search theory provides a unitary explanation of any given result; hence, this analysis is interesting in that it provides a test of the ability of search theory to explain a range of results. It also has policy relevance in that it provides estimates of the "benefits" of liberal unemployment insurance benefits to workers who are (presumably) enabled to "hold out" for higher remuneration. The proper measurement of returns to search for younger and older workers, respectively, is considered.

An additional respect in which this investigation has public policy relevance stems from the fact that it sheds light on the effects of aggregate demand conditions. Two researchers who studied the turnover and unemployment behavior of young workers over the 1966-69 period concluded that: "Perhaps the most significant fact about the youth labor market from a policy viewpoint is the severe disruption brought about by declining aggregate economic conditions (Antos and Mellow (1977), p. 8.3)." This study permits some conclusions as to whether that finding can be generalized to middle-aged workers, and whether it can be generalized to the 1969-71 period, a time that was even more definitely characterized by declining aggregate demand.[4] Other findings of this study with possible policy interest relate to

[4] For instance, the monthly unemployment rate rose almost monotonically from 3.4 percent in January 1969 to 6.2 percent in December 1970 and remained fairly constant at about the 6 percent level throughout 1971.

the question of racial differences in unemployment duration and wage gain and whether such differences can be explained by the optimal job search paradigm.

Plan of the study

Chapter II contains a discussion of theoretical considerations, mainly derived from search theory. Chapter III reviews the relevant empirical literature. Chapter IV describes the data and the models in detail. Chapter V contains the results of the analyses proposed in Chapter IV. Chapter VI discusses overall conclusions, policy implications, limitations of this study, and suggestions for future research.

CHAPTER II

CONCEPTUAL FRAMEWORK

Optimal job search theory

The essence of search theory, as applied to search in the labor market, is the modeling of the "optimal" reservation wage. That is, abstracting from nonpecuniary considerations, the searcher is assumed to select a rate of pay such that he/she will accept all offers greater than or equal to that rate of pay, and reject all other offers. The "optimal" reservation wage is assumed to be based on maximizing earnings, which in turn is based on the wage offer distribution, the costs of search, the span of the searcher's horizon, and other factors. Furthermore, it is assumed that the searcher will receive the rate of pay that is eventually accepted for the rest of his/her working lifetime.[1]

In this chapter we will spell out a number of predictions that can be derived from optimal job search theories, and which will be tested in this study.[2] These predictions have been derived under the

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[1]This assumption is not restrictive; in fact, it is almost tautological, so long as the relevant "lifetime" (e.g., tenure on the subsequent job) is identified.

[2]Hypotheses that will not be tested below, for lack of plausible empirical counterparts, include the searcher's subjective discount rate, his attitude toward risk, and the dispersion of the wage offer distribution; hence, there will be no theoretical discussion of these factors. Feinberg (1977a) claimed to have estimated the effects of risk attitudes and wage offer dispersion, but his proxies (especially

following assumptions:

1) The "period" is defined in such a way that the searcher can receive at most one offer per period.

2) The wage offer distribution is known to the searcher.

3) The searcher uses a sequential decision rule, in which he decides at the time of each new offer whether to continue searching or to accept the latest offer.

4) Previous offers cannot be recalled.

As Ehrenberg and Oaxaca (1976, p. 755) have noted, "the implications of these models are fairly robust and appear to be invariant to many of the assumptions." In fact, most of the hypotheses spelled out below have been derived, under fairly general conditions, from models of "batch" search processes (i.e., the number of periods of search is fixed in advance) (Stigler (1961, 1962); Schmidt (1973)); a model of search from an unknown distribution (Rothschild (1974)); and a model of "variable search intensity" in which search time can be substituted for leisure in order to generate more offers per period (Barron and Mellow (1977)).[3]

the risk attitude index) were questionable. There do not appear to be any studies that attempt to take individual differences in discount rates into account.

[3]Most of these theoretical contributions have been summarized by Burdett (1973) and Lippman and McCall (1976a).

The predictions to be tested below arise in three general areas: 1) the correlates or determinants of the duration of search; 2) the distribution of the duration of search; and 3) the correlates of the return to search, especially the correlation between the duration of search and the returns to search.

The determinants of the duration of search

The average duration of search is inversely related to the probability that a searcher will receive an "acceptable" wage offer (i.e., one that exceeds or equals the reservation wage). This, in turn, is the product of the probability that the searcher will receive <u>any</u> offer and the probability that the offer will be acceptable, given that an offer was received.[4] Symbolically,

$$P\text{ (acceptable offer)} = P\text{ (offer)} \cdot P\text{ (}w \geq w^*\text{/offer)}, \qquad (1)$$

where w^* is the reservation wage.[5] One implication that will be tested in this study follows immediately from (1):

1) <u>The duration of search varies inversely with the probability of receiving an offer in a given period.</u>[6]

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[4] See Barron (1975), Parsons (1973), and Sandell (1977).

[5] Time subscripts have been omitted from (1) for the sake of convenience. The possibility that w^* varies over time, as well as the implications thereof, is discussed starting on p. 10 below.

[6] Actually, an increase in P (offer) induces an increase in the reservation wage, and hence a (partially offsetting) increase in P ($w \geq w^*$/offer). But Feinberg (1977b) has shown that the effect of P (offer) on duration is still unambiguously negative; i.e., that the "direct" effect dominates.

Variations in search costs are expected to produce two offsetting effects on search duration. First, those searchers who have lower costs of search (or higher levels of search subsidies, such as unemployment insurance) will be better able to "hold out" for higher wages; that is, they will select more stringent reservation wage policies, *cet. par.* It is clear from (1) (and intuitively obvious) that the duration of search will vary directly with the reservation wage. Second, there is the possibility that searchers with more financial resources at their disposal may use these resources to increase the rate at which offers are received (e.g., paying the additional transportation costs necessary to visit more prospective employers per week), thus shortening the duration of search.[8] Hence the following prediction:

2) The duration of search may vary directly or inversely with the costs of search.

Burdett (1973) has shown that persons with longer horizons over which the returns to search are expected to accrue will set higher reservation wages. This principle can be made transparent by the

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[7]To the extent that searchers derive utility from time spent unemployed, the wealth effect on demand for leisure reinforces the price effect identified here.

[8]Cf. Hamermesh (1977, pp. 32-33). This second effect does not arise in models such as that of McCall (1970), where the searcher is assumed to be unable to affect the rate at which offers are received. Also, Schmidt (1973) has put forth a model in which the searcher can purchase information as well as wait "passively," and hence both effects occur. Schmidt predicted that the first effect would dominate, leading to an inverse relationship between search costs and search duration.

following simple example. Take two searchers, for both of whom the cost of another period of search is $100, and for both of whom the expected gain from "holding out" for that additional period is an income stream of one dollar per period. The first searcher expects to receive said returns for 200 periods; the discounted value (assuming a discount rate of one-tenth of a percent per period) of the income stream is $181.19, and hence that searcher will choose to "hold out." The second searcher expects to receive the returns for only 100 periods (discounted value = $95.12), and hence will not demand the higher wage and incur the concomitant longer expected duration of search. To summarize the discussion:

3) <u>The duration of search varies directly with the span of the searcher's horizon</u>.

Gronau (1974) has derived this result concerning the effect of variations in the wage offer level:

4) <u>The duration of search varies inversely with the mean of the wage offer distribution</u>.[9]

The intuitive sense of hypothesis 4) is that the foregone wage is a very important cost of search, and hence those who search from higher

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[9] A shift in the mean of the wage offer distribution obviously decreases the duration of search if the reservation wage is held constant. But Gronau demonstrated more than that; he showed that even after the reservation wage is revised, expected duration decreases in response to an upward shift in the mean of the offer distribution.

wage offer distributions will opt for less stringent (in the sense of longer expected duration) reservation wage policies, cet. par.

The distribution of the duration of search

Assume that a person searches from a stable offer density function, say $f(w)$, and that he/she selects an initial reservation wage, say w^*. Then the probability that he/she will find an "acceptable" offer in a given period (known as the transition rate)[10] is equal to

$$\Pi^* = \int_{w^*}^{\infty} f(w)\, dw \cdot P\ (\text{offer}) = (1 - F(w^*)) \cdot P\ (\text{offer}), \tag{2}$$

where $F(w^*)$ is the value of the cumulative distribution function associated with w^*. Let us assume that P (offer) does not vary over the period of search. If w^* remains constant over the period of search, then the transition rate is also a constant function of the duration of search. Similarly, if the search experience causes the job seeker to become more lenient in his/her wage demands (i.e., to select a revised reservation wage of $w^{**} < w^*$), then the transition rate would be

$$\begin{aligned} \Pi^{**} = (1 - F(w^{**})) \cdot P\ (\text{offer}) &= (F(w^*) - F(W^{**})) \cdot \\ P\ (\text{offer}) + \Pi^* &> \Pi^*. \end{aligned} \tag{3}$$

In other words, in such a case the transition rate is an increasing function of the duration of search. Of course, it can also be shown that if there is an increasing sequence of reservation wages, then the transition rate will vary inversely with search duration.

[10]The terms escape rate, hazard rate, failure rate, and mortality rate are also used in the literature.

There is not quite so much theoretical unanimity among the several variants of optimal search theory in this area as in the area discussed in the preceding section. For instance, one of the simplest and most straightforward models is that proposed (in slightly different forms) by Mortensen (1970) and McCall (1970) (hereafter referred to as the M-M model). It is based, among others, on the postulates that the marginal cost of search is constant from period to period, and that the duration of an accepted job is expected to be infinite. From these assumptions one can derive the prediction that the reservation wage that is initially selected will not be revised downward during the period of search. The following (null) hypothesis follows immediately:

5a) <u>The probability of receiving an acceptable job offer in a given period does not vary with the duration of search.</u>

Competing models of search behavior incorporate various conditions, any of which is sufficient to generate the prediction that reservation wages will be revised downward. Some of these are: the declining marginal utility of leisure (Kasper (1967)); increasing marginal costs of search, especially those resulting from declining assets and liquidity in imperfect capital markets (Holt (1970)); the termination of unemployment compensation (Kasper (1967)); and a finite horizon, which causes the period of search to "cut into" the duration of the job that is eventually accepted (Gronau (1971)). The considerations all lead to the following prediction:

5b) <u>The probability of receiving an acceptable job offer in a given period varies directly with the duration of search.</u>[11]

The distribution of the duration of search depends on whether 5a) or 5b) is true. Generally speaking, a distribution with the increasing transition rate property is less dispersed than a distribution with the same mean and a constant transition rate; a distribution with a decreasing transition rate is more dispersed. These assertions can be demonstrated formally using certain parametric assumptions; Appendix D will provide such demonstrations, along with some heuristic discussion of them.

<u>The determinants of the returns to search</u>

If a person searches from a stable wage offer distribution, say $f(w)$, and selects an (initial) reservation wage, say w^*, then the expected value of an acceptable wage offer (as opposed to the expected value of the offer distribution itself), is equal to

$$E(w_a) = \int_{w^*}^{\infty} wf(w)\,dw \Big/ \int_{w^*}^{\infty} f(w)\,dw.$$

It can easily be shown that $E(w_a)$ varies directly with w^* (see Appendix A); that is to say, those who demand higher wages receive higher wages.

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[11]Sant (1977) has argued that under certain conditions there is a theoretical basis for <u>upward</u> revisions in reservation wages (i.e., downward revisions in the probability of accepting an offer) from period to period. But he used a model of Bayesian inference in a case of search from an unknown distribution to derive this result. It appears to be a pathological case (cf. Parsons (1975)) with no intuitive basis, and it will not be considered further here.

We have already seen that search theory identifies several factors that are presumed to affect the stringency of the reservation wage policies set by job seekers. In fact, the following predictions are straightforward corollaries of footnote 5 and hypotheses 2) and 3), respectively:

6) *The return to search varies inversely with the probability of receiving an offer.*

7) *The return to search varies inversely with search costs.*

8) *The return to search varies directly with the span of the searcher's horizon.*

The intuitive discussion presented above in connection with hypotheses 1) through 3) is equally cogent here.

In addition, Gronau (1974) has derived the following (intuitively obvious) prediction:

9) *The return to search varies directly with the mean of the wage offer distribution.*

Intuitively, those who search from more lucrative distributions receive and accept more lucrative offers.

The following (competing) predictions are corollaries of hypotheses 5a) and 5b):

10a) *The return to search does not vary with the duration of search.*

10b) *The return to search varies inversely with the duration of search.*

The Role of Age

The theoretical considerations presented above suggest several ways in which the search behavior of older workers may be expected to differ from that of younger workers. First, consider the presumed effects of the span of the searcher's horizon. A job seeker with a thirty-year horizon has a horizon that is infinite, for all practical purposes; we should expect little difference in behavior between such a person and one with a forty-year horizon.[12] On the other hand, the length of a worker's expected working lifetime should make a difference among middle-aged searchers.[13] We have seen that two effects of a finite horizon are a positive correlation between the length of the horizon and the reservation wage (hypotheses 3) and 8)), and a negative correlation between the reservation wage and the duration of search (hypotheses 5b) and 10b)). The following predictions follow:

11) Hypotheses 3), 5b), 8), and 10b) are more likely to be confirmed by the behavior of middle-aged workers than that of younger workers.

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[12]This assertion can be made transparent by considering the present value of an income stream of $100 per year, discounted at a rate of 10 percent per year. The present value of a thirty-year stream is $942.69, as opposed to that of a forty-year stream ($977.91); neither is significantly different from the present value of $100 per year received in perpetuity; namely, $1000.00.

[13]To continue the example used in the preceding footnote, the discounted value of a five-year income stream is $379.08, whereas the discounted value of a ten-year stream is $614.46, a substantial difference.

The second factor combines theoretical with institutional considerations. Institutional rigidities, especially minimum wages established legally or otherwise ("social minimum"), may prevent job seekers from effectively lowering their wage demands. To the extent that this factor is important, it has its greatest impact on workers at low wage levels,[14] and relatively more young searchers would be so affected relative to middle-aged job seekers. Therefore we would expect that:

12) <u>Hypotheses 5b) and 10b) are more likely to be confirmed for middle-aged workers than for younger workers</u>.

The third source of divergence in behavior between younger and middle-aged workers arises not from optimal job search theory, but from the theory of human capital and earnings profiles.[15] Since middle-aged men have relatively flat profiles, the rate of pay of the job that is eventually accepted is a reasonably good proxy for the return to search over the span of the worker's horizon. However, the corresponding profiles for younger workers tend to diverge, depending on the extent to which they forego earnings for (formal and informal) on-the-job training. For this reason it is not clear that the

[14] Crosslin and Stevens (1977, pp. 1299-1300) have asserted that rigidities also exist at the upper end of the wage spectrum: "...there are concentrations of union members who are barred from accepting less than scale wages as a condition of membership." But it is doubtful that this factor is that important, in the absence of barriers to occupational and industrial mobility. In fact, during the 1969-71 period 62.9 percent of the young men and 44.1 percent of the middle-aged men changed one-digit occupations when changing jobs, and 70.3 percent of the young men and 43.1 percent of the middle-aged men changed one-digit industries.

[15] Cf. Mincer (1974).

acceptance wage rate is the appropriate measure of the return to search.[16] Although this consideration does not affect the hypotheses spelled out above, it should be kept in mind when constructing empirical tests of those hypotheses.

Summary

The verbal and mathematical development of hypotheses 1) through 12) in the preceding pages may have seemed rather obtuse. Tables 1 and 2 provide a concise summary, however. Table 1 sets forth the hypothesized effects of the exogenous variables on the two dependent variables (the duration of search and returns to search), as well as the effect of the one on the other. Table 2 indicates the expected effect of the elapsed duration of search on the transition rate (i.e., on the probability of terminating search).

[16] Investment in search is formally analogous to investment in schooling, in that both involve foregone earnings and perhaps explicit costs as well. Mincer (1974) has shown that returns to schooling are best measured at the "overtaking point," which is approximately equal to the inverse of the discount rate, and hence occurs several years after the investment is made. Presumably the same result holds for search investments.

Table 1: Hypothesized Partial Effects of Various Factors on Duration of Search and Gross Returns to Search

	Duration		Returns	
	Young men	Middle-aged men	Young men	Middle-aged men
Wage offer level	-	-	+	+
Probability of receiving an offer	-	-	+	+
Search costs	?	?	?	?
Horizon	0	+	0	+
Duration of search			0/-	0/-

\+ Positive effect
0 No effect
\- Negative effect
? Uncertain effect

Table 2: Hypothesized Effects of Duration of Search on Transition Rates

Young men	+/0	Middle-aged men	+/0

\+ Positive effect
0 No effect
\- Negative effect
? Uncertain effect

CHAPTER III

A REVIEW OF THE EMPIRICAL LITERATURE

Numerous empirical analyses that purport to test various aspects of search theory have appeared in recent years.[1] This chapter examines those studies which are most germane to the present study, both because they tested (or tried to test) the same hypotheses that are tested here, and because they are especially instructive in terms of empirical design.

The duration of unemployment

The studies reviewed in this section are generally multivariate analyses in which a variable representing the duration of unemployment (in a given spell, or in a given year) is the dependent variable, and various proxies for the determinants of search duration discussed in Chapter II above are the independent variables.

The first type of analysis involves regressing the duration of unemployment on a reported reservation wage. Sometimes such an equation is estimated as part of a simultaneous model of the determination of reservation wages and unemployment duration; this approach is exemplified by the work of Stephenson (1976) and Crosslin and Stevens (1977). Sometimes the equation is estimated in a single equation

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[1]As Kiefer and Neumann (1978) have noted, applications of this theory have touched on phenomena ranging from advertising expenditures to marriage and divorce.

context, as in Sandell (1977). Since these studies are not very comparable with the model proposed in Chapter IV below, which follows a "reduced form" approach in that observable proxies for the determinants of reservation wage strategy are used as explanatory variables, they will not be reviewed in detail here.[2]

The second type of study consists of models estimated from data collected on unemployment insurance claimants in selected cities or states. Marston (1975), Holen (1977), and Classen (1977) are examples of this genre.

The third type of study corresponds most closely to the present study. These analyses all utilized national samples of unemployed workers; in fact, all but one utilized the National Longitudinal Surveys data, which are used in this study.[3] The five studies in this category that will be reviewed here are Ehrenberg and Oaxaca (1976) (hereafter E-O); Antos and Mellow (1977) (hereafter A-M); Schmidt (1974); Grasso (1977); and Hills (1976).

Many of these studies were concerned with the effects of the unemployment insurance (hereafter UI) system on unemployment duration. Hence, it would be useful to bring together the estimated effects

[2]Besides, the possible problems with the use of responses to hypothetical questions (discussed below) may be applicable here.

[3]Schmidt also used the Survey of Economic Opportunity to "cross-check" the results obtained using the NLS data; however, that data set is so laden with problems (most of which Schmidt discussed) that those findings merit no space here.

derived from several studies; this has been done in Table 3.[4]

The shortcomings of studies of UI claimants such as Classen and Holen have been discussed in detail by Hamermesh (1977) and Welch (1977). The most important of these are the fact that the dependent variable is the number of weeks that benefits were collected, which is not always a good proxy for the duration of unemployment (e.g., in cases in which respondents exhaust their benefits), and the restricted sample, which limits the ability to generalize the findings to the entire population. Besides, the data used for these studies were not especially rich in variables (other than unemployment insurance-related variables) that could affect unemployment duration.

Schmidt (1974) analyzed data from the National Longitudinal Surveys (hereafter NLS) of men aged 45 to 59 for the period 1966-67. The dependent variable in his analysis was the duration of a spell of unemployment.[5] His most important findings were that the duration of unemployment is positively related to the receipt of unemployment compensation and negatively related to the rate of pay received on the previous job, as he hypothesized. But he also found that the duration of search was negatively related to the respondent's time horizon,

[4]Schmidt (1974) and Marston (1975) also found that unemployment duration is positively related to UI benefits, but their methodology did not permit the derivation of estimates comparable to those presented in Table 3.

[5]Schmidt also provided a very good explanation of why this measure is superior to others (e.g., number of weeks of unemployment in a year) as a dependent variable.

Table 3: Estimated Effect on Duration of Unemployment of an Increase in Weekly Unemployment Benefits of Ten Dollars

Study	Sample	Estimated Effect (weeks)
Classen (1977)	UI claimants, Pennsylvania	1.1
Ehrenberg and Oaxaca (1976)	Men aged 14-24, nationwide	0.41[a]
	Men, aged 45-59, nationwide	0.69[a]
Hills (1976)	Men, aged 25 and up, nationwide	0.13[a]
Holen (1977)	UI claimants, several cities	0.9

[a]Calculated assuming an hourly rate of pay on the preunemployment job of $4.00, a workweek of forty hours, and a duration of unemployment of ten weeks.

which is not consistent with search theory. It is not clear why Schmidt chose to represent the receipt of unemployment benefits by a dichotomous variable (E-O used a continuous variable in the study to be reviewed below, which used the same data for the same time period). In any event, the estimated effect of unemployment benefits is biased upward for reasons to be discussed below in connection with the work of E-O. Besides, as in other studies to be reviewed presently, data limitations led him to confine the analysis to respondents employed at both survey dates, thus censoring a number of observations, especially on the long-term unemployed, and possibly introducing biases.

E-O analyzed the duration of unemployment using data on all four NLS cohorts. Only the results for two of the cohorts will be reviewed here.[6] The analysis for the young men used data from the period between the 1966 and 1967 surveys (as had Schmidt); the model for the young men (aged 14 to 24 in 1966) was estimated using data from the 1966-69 period. E-O regressed the logarithm of the mean duration of the respondent's spell of unemployment in a year (the ratio of the number of weeks unemployed to the number of spells in a year) on the level of unemployment benefits, race, marital and dependent status, several variables representing asset levels, and the local labor market

[6]The relatively low labor force participation rates of the two female cohorts (women aged 14 to 24 in 1968 and women aged 30 to 44 in 1967) only complicates interpretation of the results. Besides, only the two male cohorts are used in the present study.

unemployment rate, among others (the list of independent variables used varied somewhat from cohort to cohort). The amount of unemployment compensation received per week was consistently found to be significantly and positively related to the log of the duration of unemployment, as expected; some of the "control" variables were statistically significant in some variants of the model.

E-O, like Schmidt, excluded data on respondents who were not employed at the survey dates defining the beginning and end of the time period under consideration. Their use of the average duration of a spell of unemployment for those experiencing more than one spell in the period may have caused heteroscedasticity in the dependent variable. Furthermore, the reason for separation (quit versus layoff or discharge) could not be ascertained unambiguously for those respondents who had more than one spell of unemployment. In fact, observations corresponding to temporary layoffs were included in the analysis; for reasons given in Chapter IV below, this is not very appropriate.

Both Schmidt's and E-O's estimates of the effects of unemployment benefits may be biased upward. The laws of most states require a waiting period of at least a week before benefits are paid to claimants (Welch (1977), p. 459); hence, a long spell of unemployment may "cause" the receipt of benefits, as well as vice versa. Furthermore, E-O's unemployment benefit variable is a "replacement ratio" (defined as the ratio of the weekly amount of benefits to the weekly rate of pay on the previous job), ostensibly because it is the policy variable that varies from jurisdiction to jurisdiction. However, this variable is difficult to interpret, since it is an amalgam of the respondent's skill level (the denominator)[7] as well as the liberality of benefits (the numerator), especially for respondents who are receiving the maximum level of weekly benefits allowed by law.[8] One does not know whether to "attribute" the observed positive relation to the numerator, the denominator, or both. It is better to treat these conceptually distinct variables separately.

A-M analyzed the NLS data on young men for the period 1966-70.[9] They regressed the number of weeks of unemployment in a year on the variables representing the reason for separation, the year in question (a proxy for cyclical fluctuations in aggregate demand), marital status, schooling, tenure, and the "market differential" (the deviation of the observed rate of pay from a "predicted" rate of pay based on the worker's characteristics). They found that aggregate demand was an important correlate of unemployment duration (a four-week differential between 1966 and 1970 corresponding to a slackening labor market), as were the reason for separation and the market

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[7] If the preunemployment rate of pay contains transient components (as A-M argue), it may not even be the best measure of the worker's skill level.

[8] Hills (1976), whose findings are reviewed below, determined that the majority of the members of his sample, taken from a national panel similar to that used by E-O, were in fact receiving the statutory maximum.

[9] Their results using data on the young women will not be discussed here, for reasons given in footnote 6.

differential. The effects of marital status, schooling, and tenure were found to be "sporadic (p. 6.6)."

Several comments on this study are in order. First, the use of the total duration of unemployment in a year as a dependent variable makes A-M's findings difficult to compare to other studies reviewed in this chapter. This variable confounds two conceptually distinct phenomena; namely, the incidence and duration of unemployment. This problem is aggravated because the sample is not limited to those who had experienced at least some unemployment. Hence, whatever conceptual or policy relevance can be ascribed to A-M's results, it is hard to interpret them as an appropriate test of search theory. Second, as in the studies by E-O and Schmidt, observations corresponding to individuals not employed at the endpoints of the period under consideration are eliminated from the analysis (but these cases are quantitatively less important here, given the longer period of time under consideration). Third, no variable representing the receipt of unemployment benefits was included; such an omission is especially surprising given the inordinate attention paid to this variable by other researchers, to the virtual exclusion of other possible determinants of unemployment duration. Hence, it is difficult to argue that search costs were adequately controlled for.

In fact, A-M gave a most peculiar interpretation to the observed positive relation between the market differential and unemployment duration. They reasoned that respondents with large market differentials were able to earn higher wages because they had had lower search costs (and hence had been able to hold out for higher pay) at the time they obtained the job they held before the period of unemployment in question. If such costs were correlated over time, then the market differential could be considered an inverse measure of search costs. But they themselves suggested an alternative (and much less tortured) explanation: "[I]f a worker's positive differential is pure economic rent...he may...initially overestimate his wage distribution and adopt an inappropriate search strategy, resulting in a positive relation between the market differential and subsequent duration (p. 6.5)."[10] Presumably only the latter interpretation would have held had A-M used available data on more direct measures of assets and costs. In fact, perhaps the simplest explanation is that A-M implicitly controlled for the receipt of UI benefits, after all. The "market differential" variable reflects high preunemployment earnings, thus entitling the respondent to high UI benefits.[11]

Grasso (1977), using data on the NLS of young men in the period between the 1970 and 1971 surveys, regressed the duration of a spell

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[10]Such a hypothesis can be generated formally only if one adopts a model of search from an unknown distribution. Even then, this result is difficult to derive rigorously (cf. Sant (1977)). Furthermore, this empirical model can be justified only if it is (heroically) assumed the previous wage is a good proxy for incorrect perceptions.

[11]Yet another explanation is that the market differential is a proxy for the reason for unemployment. People with higher market differentials are more likely to be laid off (Parsons (1972)). Since A-M could not ascertain whether respondents quit or were laid off in many cases, perhaps this variable is serving as a proxy.

of unemployment on a set of human capital variables (including the hourly rate of pay, schooling, training, experience, and tenure), variables representing financial obligations that presumably drain assets and raise marginal search costs (marital status and number of dependents), the reason for separation, two variables that presumably control for the probability of receiving an offer in a period (urbanicity--residence in an SMSA--and the local labor market unemployment rate). Most of these regressors were not consistently and significantly related to the dependent variable in the several variants of his model. The one notable exception was the reason for separation; as expected, those who had quit their previous job tended to experience shorter spells of unemployment, *ceteris paribus*; the difference amounted to 2.4 weeks for whites and 4.1 weeks for blacks.

Grasso's work exemplifies the potential that exists for exploiting the data that are available for the young men's NLS panel for the 1969-71 period (obtained from comprehensive work histories contained in the 1970 and 1971 surveys). The length of a spell of unemployment can be determined unambiguously, as can the reason for the separation that precipitated the spell; in these two respects, Grasso's work is superior to virtually all other studies reviewed here. But there are problems. The exclusion of spells in progress at the 1971 survey date, and possible biases caused thereby, could have been minimized by including data from the 1969-70 period as well as the 1970-71 period, which would also have increased the number of included observations relative to excluded observations. And Grasso's model specification could have been improved. For some reason, the weekly amount of UI benefits was not included as an explanatory variable. In addition, the hourly rate of pay was included as an independent variable along with the human capital variables that are presumed to be its determinants. The obvious simultaneity problems make the interpretation of the coefficients rather unclear.

Hills (1976) used data from the University of Michigan's Panel Study of Income Dynamics to estimate the effects of UI receipts on unemployment duration. His dependent variable was the log of the number of weeks of unemployment experienced by the respondent between 1969 and 1971. The independent variables, in addition to a replacement ratio (the ratio of UI benefits to the preunemployment weekly wage), included variables representing age, human capital, occupation, industry, the local unemployment rate, and the maximum number of weeks for which the respondent was eligible. Unemployment duration was found to vary positively with the replacement ratio, as reported in Table 3. Other significant variables were the number of years of schooling, the maximum duration of benefits, and the local unemployment rate.[12]

As Table 3 indicates, the estimated effect of UI benefits implied by Hills' study, although statistically significant, is small compared to the estimates produced by other researchers. One reason for this discrepancy is the fact that Hills used as his UI variable the level

[12]This discussion is based on the results obtained from an analysis of 236 respondents who changed employers. Hills estimated models for other samples; those results will not be discussed here, since the samples are not quite as comparable to the sample used in the present study.

of benefits for which the respondent was eligible, rather than reported weekly receipts; as we have seen above, use of the latter measure leads to estimates that are possibly upwardly biased, because of the effect of the statutory waiting period. To that extent, Hills' estimate is more accurate than the others (especially E-O). This is one advantage of that study. Another is that it is based on a sample that is representative of a large segment of the labor force (male heads of household).

On the other hand, the study by Hills has several defects. First, the dependent variable measures total unemployment suffered over a three-year period. This is a function of the number of spells suffered as well as unemployment duration, which could account for the strong explanatory power of the local unemployment,[13] a finding not replicated by any other study. Second, the replacement ratio has an ambiguous interpretation, as in the case of E-O discussed above. Furthermore, the censoring of incomplete spells at the end of the period and the inability to make a layoff/quit distinction are undesirable features of this study, as well as of the other studies reviewed above.

Reservation wage adjustments and transition rates

The empirical testing of the M-M model (see Chapter II) versus its competitors has generally involved correlating reported asking

[13]It was estimated that a respondent who lives in a community with an unemployment rate in excess of ten percent would experience 158 percent more unemployment than a respondent who resides in a locality with an unemployment rate of under 4 percent.

wages and the duration of unemployment for cross-section samples of unemployed workers. The most important such studies were those by Kasper (1967), Barnes (1975), Stephenson (1976), and Crosslin and Stevens (1977). All of these studies found a negative relation between the two variables, contrary to the prediction of the M-M model. However, the absolute magnitude of the estimated downward revision of reservation wages generally did not exceed 3.5 per month (although larger drops were occasionally estimated for peculiar subsamples).

The one important exception to this consensus is Sant (1977). He predicted that reservation wages would be positively correlated with the duration of unemployment under some circumstances, as a consequence of learning about the nature of the wage offer distribution. His analysis was confirmed using the NLS of young men. But the rationale for his model specification (especially with regard to an estimate of the mean of the wage offer distribution--predicted wage, which was crucial to his analysis) was not at all obvious.

The question of whether unemployed workers actually increase their probabilities of re-employment as a consequence of their (presumed) lowering of reservation wages has been largely neglected by researchers.[14] Katz (1975), using life table techniques often used by

[14] This is presumably more interesting for both analytical and policy purposes than hypothetical questions about asking wages (cf. Rottenberg (1956)). The only two studies that seem to correlate hypothetical and actual behavior are Stephenson (1976) and Sandell (1977). Sandell found that members of the NLS panel of middle-aged women (aged 30 to 44 in 1967) who reported higher reservation wages had longer spells of unemployment and higher postunemployment wages, as expected. However, Stephenson's analysis of young men did not confirm this finding very strongly.

demographers,[15] found that transition rates were an increasing function of the duration of unemployment for a sample of unemployed workers surveyed by the Bureau of Labor Statistics in 1961. However, that sample was limited to persons unemployed five weeks or more; besides, as Katz himself noted, the increasing probabilities of re-employment may have been a result of improving economic conditions during 1961.[16]

Carr (1977), in a preliminary investigation of re-employment probabilities using the NLS of young men for the period between the 1969 and 1971 surveys, found that the distribution of unemployment duration was roughly exponential, and hence the probability of accepting a job in a period was invariant with respect to the length of the period of joblessness already experienced up to that point. The methodology used, as well as how that study is revised and improved upon here, can be found in Chapters IV and V and Appendix D below.

The correlates of postunemployment acceptance wages

Like the studies of unemployment duration using data on claimants cited earlier in this chapter, the studies to be reviewed here fall into two categories: those based on samples of unemployment insurance claimants and those based on national samples of unemployed persons,

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[15]Related methods are used in Appendix E to estimate expected job tenure. The reference cited therein (Barclay (1958)) outlines the methods used by Katz.

[16]That is to say, in terms of equation (1) on p. II-2, what is changing is P(offer) rather than w^* (and hence $P(w \geq w^*/\text{offer})$).

claimants or otherwise. The first type of analysis is exemplified by Schmidt (1974), Ehrenberg and Oaxaca (1976), and Hills (1976). Since both types of studies are concerned, among other things, with the effects of the liberality of benefits (both in terms of weekly amounts and the maximum number of "eligible" weeks) on the dependent variable (in this case, earnings and wages after the unemployment experience), it is once more useful to juxtapose the findings of these diverse studies; this has been done in Table 4.[17]

Burgess and Kingston (hereafter B-K) analyzed data from three cities. They found that an increase of a dollar in the weekly amount of benefits increase subsequent annual earnings by 25 dollars, consistent with their hypothesis (and consistent with job search theory). But, as Welch (1977) has noted, their work is defective in a number of crucial respects. First, the subsequent earnings data are from Social Security records and are truncated at $7,800. Second, unemployment duration was introduced as an explanatory variable, despite the obvious simultaneity problems. Third, those who exhaust benefits are excluded from the sample. Welch's overall comment was: "In sum, I find it difficult to be persuaded that the B-K estimates contain important lessons concerning effects of UI (p. 455)."

Holen (1977) and Classen (1977) used data sets similar to B-K. Holen reported that a $10 increase in weekly unemployment benefits

[17] Schmidt (1974) represented the level of UI benefits by a dichotomous variable (receipt/non-receipt), so that it is hard to conpare his findings to those reported in Table 4.

Table 4: Estimated Effects on Postunemployment Hourly Rates of Pay of an Increase in Weekly Unemployment Benefits of Ten Dollars

Study	Sample	Estimated Effect (dollars)
Burgess and Kingston (1977)	UI claimants, several cities	.12
Classen (1977)	UI claimants, Pennsylvania	.01
Ehrenberg and Oaxaca (1976)	Men, aged 14-24, nationwide	.02[a]
	Men, aged 45-59, nationwide	.11[a]
Hills (1976)	Men, nationwide	.24[a]
Holen (1977)	UI claimants, several cities	.17

[a]Calculated assuming an hourly rate of pay on the preunemployment job of $4.00 and a workweek of forty hours.

led to an increase in subsequent annual earnings of about $350. This figure is surely biased upward because the least successful searchers (exhaustees) are selected out.[18] Classen reported a statistically insignificant estimated effect of weekly benefit amounts on subsequent earnings.

The analyses of the postunemployment wages of the unemployed using surveys with national coverage, with the exception of Hills (1976), are all based on the NLS data; in fact, all were published together with analyses of the duration of unemployment reviewed earlier in this chapter.[19] Schmidt (1974) analyzed the 1966-67 data for middle-aged men, as mentioned above, and found that the difference in weekly earnings between the survey week jobs reported before and after unemployment was positively related to the receipt of unemployment benefits and the duration of unemployment; his explanation of the latter result is not at all obvious or clear, especially given that unemployment duration and acceptance wages are simultaneously determined. The other variables (local labor market size, time horizon, etc.) were generally insignificant.

E-O regressed the logarithm of the ratio of postunemployment rate of pay to the preunemployment rate of pay on essentially the same set

[18]Even Holen (p. 449) noted that by her calculations, an additional week of unemployment would cost only fifty dollars (foregone earnings minus benefits), and it would yield $350 a year in higher earnings, an implausible finding indeed.

[19]Many of the limitations of these studies were discussed above, and are also applicable here.

of independent variables used to explain the duration of unemployment. They found that the level of unemployment benefits (their principal variable of interest) was significantly and positively related to the dependent variable, as expected, for the sample of middle-aged men, but not for young men. They advanced one hypothesis for the latter finding:

> [Y]ounger recipients of UI benefits may search for jobs offering better opportunities for on-the-job training. To the extent that this is true, we would expect them to accept jobs with low postunemployment wages because of the investment options offered. Consequently...their returns to search would be more appropriately measured by examining changes in their lifetime earnings streams (p. 765n.).

This is perhaps all quite true. But they continued: "Unfortunately, the data do not permit us to test this hypothesis." In fact, it is precisely an advantage of a longitudinal data set such as the NLS that one can follow a sample of respondents (such as those experiencing unemployment in a given period) and see how much they are earning at given points "down the road," and not just immediately after the unemployment experience. A design for doing just that is elaborated in Chapter IV and carried out in Chapter V of this study.

Hills (1976) estimated an effect of UI on postunemployment weekly earnings that is even larger than that found by Holen. But due to severe data limitations, this estimate is based on only 16 sample cases; for that reason alone, no further discussion is warranted.

CHAPTER IV

EMPIRICAL DESIGN

The data base

The data used to test the hypotheses spelled out in Chapter II come from the National Longitudinal Surveys of Labor Force Behavior (hereafter NLS). Specifically, data from the panels of young men (aged 14-24 in 1966) and middle-aged men (aged 45-59 in 1966) are analyzed.[1] The analysis focuses on the 1969-71 period, since the data for that period are very detailed.[2] The ability of the researcher to generalize to other periods of time on the basis of the results presented in Chapter V may be limited because of peculiar features of this two-year period such as the state of the business cycle (although variations in aggregate demand within the period are controlled for).

[1]For a description of the data for young men, see Kohen et al. (1977), especially the paper by Grasso (1977). For a description of the data for middle-aged men, see Parnes et al. (1974).

[2]The 1970 wave of interviews of the young men (conducted between October 1970 and February 1971) and the 1971 round of interviews (which took place between October 1971 and February 1972) provided information on the characteristics (occupation, industry, rate of pay, etc.) of every job held by a respondent in the previous year, as well as all spells of not working between said jobs. The 1971 survey of the older panel (conducted between January and November of 1971) provided a two-year retrospective work history (there was no 1970 survey of the middle-aged men).

During the period under consideration, there were 4739 instances of employer changes among members of the younger cohort who were not enrolled in school, of which 1821 resulted in periods of unemployment of at least one week. There were 1016 instances of employer changes among members of the older group, of which 378 involved spells of unemployment.[3,4]

The variables

The hypothesized relationships posited in Chapter II and summarized in Tables 1 and 2 all involve abstract concepts. To find the appropriate empirical representations ("proxies") of the concepts requires some care.[5] This section discusses the variables that are used in the empirical analysis of Chapter V, organized by the theoretical concepts to which they correspond.[6]

The duration of search. The studies reviewed in Chapter III above almost invariably take the duration of unemployment (DUR) as the measure of the duration of search. The present study follows

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[3]Only periods of unemployment resulting from employer changes (as opposed to temporary layoffs) will be analyzed in this study, for reasons given below.

[4]In this study, an "observation" is an instance of employer change or unemployment, not a respondent. The two are not equivalent in cases where a respondent changed jobs (was unemployed) more than once during the period under consideration.

[5]This point has been made most lucidly by Schmidt (1974).

[6]The Glossary (Appendix B) describes the variables in more detail.

suit.[7] However, this assumption is tenable only under certain circumstances. First, it is well documented that employees on temporary layoff generally expect to be recalled and seldom pursue other employment opportunities.[8] Hence, we use only observations based on instances of separations (i.e., employer changes). Second, spells of unemployment experienced while a respondent is enrolled in school are excluded from the analysis, since it can plausibly be assumed that because of the time devoted to school, an unemployed student searches less "intensively" than a comparable nonstudent. Finally, the NLS data permit the researcher to ascertain not only the number of "idle" weeks between jobs, but also the number of those weeks during which the respondent was actively looking for work. The latter measure is used, as it appears to correspond more closely to the theoretical notion of search behavior.[9]

The return to search. The log of the hourly rate of pay on the first job[10] (LNPOSTWAGE) landed by the respondent after the spell of

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[7]Two variants of this variable are also employed at certain points in the analyses reported below. These variables are LNDUR (the natural logarithm of DUR) and LNDURRES (the "residual" of LNDUR). These variables are defined in Appendix B, and the rationale for using them rather than DUR will be discussed in context.

[8]Both Feldstein (1976) and Barron and Mellow (1977) have documented this assertion.

[9]Of course, the two variables are very highly correlated (r = .85 for the young men; r = .91 for the middle-aged men).

[10]In fact, the "first job" is defined here as the first job of at least one month's duration. There is a certain amount of very casual employment, especially among the young men; it would be inappropriate to gauge the returns to search by such brief experiences.

unemployment is used to measure the outcome of the search process. This procedure is consistent with the studies reviewed in Chapter III. In the case of the middle-aged men, the use of this variable does not appear to involve any heroic assumptions, since wage profiles tend to be rather flat (or even slightly downward sloping) in that age range. But for reasons explained on pp. 15-16 above (and "confirmed" empirically by E-O's anomalous results mentioned in Chapter III), it is not clear that the initial rate of pay is the most appropriate measure of "success" in search for young men. Hence, a variable measuring the annual gain in the log of the hourly rate of pay (DLNWAGE) will also be used to measure returns to search for this cohort.

Wage offer level. The mean of the (unobservable) offer distribution is represented by two variants of predicted log hourly rate of pay, SKILL1 and SKILL2, the derivation of which is discussed in detail in Appendix C.

Probability of receiving an offer. A vector of three variables serves as proxies for this concept: namely, the local unemployment rate (LOCUR); the local labor market size (LMSIZE); and the reason for separation from the previous employer (QUIT). The justification for using each of these variables is as follows. The local unemployment rate is expected to be negatively correlated with the probability of discovering an offer, because it reflects cyclical as well as structural influences on aggregate demand.[11] Labor market size is

[11]Cf. Fearn (1975). Fleisher and Rhodes (1976) have argued, in a somewhat different context, that cross-sectional variation in unemployment rates reflects primarily differences in the demographic

included on the supposition that the greater concentration of employers in urban areas makes searching large numbers of prospective employers more efficient.[12] Quitters are presumed to be more likely to have anticipated their separations, and hence to have conducted some search while still employed (cf. Schmidt (1974); Antos and Mellow (197 ,). We know that much job search takes place while people are employed, if only because a substantial proportion of employer changes occur without spells of unemployment.[13]

<u>Search costs</u>. The variables that fall under this rubric include the amount of unemployment compensation received per week (UC), assets (ASSETS), nonlabor income (EXOINC), the number of dependents excluding the wife (DEP), and the wife's income (WFIN).[14] The level of unemployment benefits is hypothesized to subsidize search, and its

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composition of local labor markets. Whatever the merit of such an argument, the variable used here is not purely "cross-sectional" in that it is measured as of the year in which the spell occurred, and hence there is variation in it within local labor markets.

[12]Of course, this is at least partially offset by the fact that there are more persons looking for jobs in urban areas.

[13]In our two samples. 61.6 percent of the young men and 62.8 percent of the middle-aged men who changed jobs reported no unemployment associated with the change. See also Mattila (1974).

[14]Due to various data limitations, the variable WFIN could only be defined for the sample of young men. The questions asked of the respondents in the panel of middle-aged men generally do not permit the researcher to differentiate between the wife's income and the income of other members of the household.

empirical importance was documented in Chapter III. In the presence of imperfect capital markets, it is presumed to be less costly to finance search from one's own resources than to borrow (Schmidt (1973)); hence the inclusion of the asset and income variables. Finally, the number of dependents is presumed to vary positively with search costs, since said dependents presumably lead to a faster depletion of financial resources.[15]

Horizon. As we have seen, it has been customary to assume that the job that is accepted to terminate the period of unemployment will last the rest of the worker's career. Consistent with this convention, a variable (HORIZ1) was constructed to measure the time remaining to the expected age of retirement.[16, 17]

But a more plausible assumption, especially in the case of younger workers, is that the expected tenure on the postunemployment job is the relevant horizon span. For this reason, an expected tenure variable (HORIZ2) was created from data on interfirm mobility in the manner described in Appendix E; it is used as an alternative measure of the length of the searcher's horizon.

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[15]As Holt (1970, p. 61n) put it: "We do not observe a worker holding out permanently for an offer that conforms to his supply curve while his children starve and his wife pleads with him (emphasis in original)."

[16]Retirement at age sixty-five was assumed for those middle-aged men who did not report an expected retirement age, as well as all of the young men (who were not asked about their retirement plans).

[17]Although the retirement decision may be endogenous (i.e., conditioned by the unemployment experience), this variable is not, since it refers to expected retirement age as of 1969, the beginning of the period under consideration.

Summary. Table 5 summarizes the above discussion. It indicates how the empirical variables are posited to be related to the theoretical concepts, and should be self-explanatory.

The unemployment duration model

In this section a multivariate model is presented that constitutes a test of the hypotheses set forth in the first two columns of Table 1 on p. 15. The log of the duration of unemployment is regressed on the variables listed in Table 6.[18, 19] The expected signs of the coefficients of these variables are listed next to them; the predictions follow directly from Tables 1 and 5. For instance, Table 1 indicates that the duration of search is inversely related to the probability of receiving an offer for both the young men and the middle-aged men. Table 5 indicates that the duration of unemployment is used to represent the duration of search, and that the local unemployment rate measures (inversely) the probability of receiving an offer. Hence, we expect that the (log) duration of unemployment will vary directly with the local unemployment rate.

[18] The theoretical literature reviewed in Chapter II seldom gives any guidance as to the specific functional form relating the duration of search to its determinants. Hence, both the linear and semilogarithmic versions of the model are estimated, to ascertain whether the conclusions drawn are robust with respect to functional assumptions. The semilogarithmic form seems more reasonable on the basis of the obvious positive skewness of the dependent variable. E-O attempted a "theoretical justification," but it was rather cryptic.

[19] The variable SKILL1 is used rather than SKILL2 in order to abstract from wage level differences resulting from cross-sectional (e.g., South vs. nonSouth) price variation.

Table 5: Postulated Relations Between Theoretical Variables and Empirical Variables

<u>Duration of search</u>	
DUR	+
LNDUR	+
<u>Returns to search</u>	
LNPOSTWAGE	+
DLNWAGE	+
<u>Wage offer level</u>	
SKILL1	+
SKILL2	+
<u>Probability of receiving an offer</u>	
LOCUR	−
LMSIZE	+
QUIT	+
<u>Search costs</u>	
UC	−
DEP	+
ASSETS	−
EXOINC	−
WFIN	−
<u>Length of horizon</u>	
HORIZ1	+
HORIZ2	+

Table 6: Independent Variables Hypothesized to Affect the (Log of the) Duration of Unemployment

	Young men	Middle-aged men
Wage offer level		
SKILL1	–	–
Probability of receiving an offer		
LOCUR	+	+
LMSIZE	–	–
QUIT	–	–
Search costs		
UC	?	?
DEP	?	?
ASSETS	?	?
EXOINC	?	?
WFIN	?	?
Length of horizon		
HORIZ1[a]	0	+
BLACK	?	?

[a]HORIZ2 will be used alternatively.

An additional variable is included that is not derived from search theory. A binary variable indicating the race of the respondent (BLACK) is entered, to test for black-white differences in unemployment duration, controlling for other variables. At least two studies have concluded that there are no racial differences, but they are not definitive.[20]

A few remarks comparing this model to the studies reviewed in Chapter III are in order. First, the data set is unusually rich in variables that are plausible proxies for search theoretic variables. In particular, the opportunity to use the actual duration of unemployment in a spell (as opposed to duration of receipt of unemployment benefits or the amount of unemployment experienced in a year) is welcome. Also, it is very helpful to know whether the instance of unemployment resulted in an employer change, and whether it resulted from a quit or layoff.[21] Finally, the degree to which observations on unsuccessful searchers (the long-term unemployed) are censored is minimized.[22]

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[20] Cf. Hall (1972); Smith and Holt (1971). Ralph Smith has advised the author that the latter study suffered from possible censorship bias in that spells of unemployment in progress at the end of the period were not included in the analysis; since blacks were more likely to be unemployed at the endpoint of the period, the black-white differential may have been understated.

[21] The small number of cases of discharge, conscription, and imprisonment are excluded from the analysis.

[22] The censored cases in this study represent instances of spells of unemployment in progress at the time of the 1971 survey; the duration of those spells is not ascertainable. There were 43 nonenrolled young men who were unemployed in the 1971 survey week and for whom

The analysis of transition rates

Recall that we are interested in the question of whether the transition rate (i.e., the probability of moving from unemployed to employed status) is invariant with respect to the elapsed duration of unemployment. It can be shown that the Weibull distribution has the property of a monotonically increasing, constant, or decreasing transition rate, depending on the value of a single parameter. That is to say, the hypotheses stated in Table 2 can be evaluated by estimating the scale parameter of the Weibull distribution. The details of estimation are spelled out in Appendix D.

The Weibull parameters are estimated separately for the samples of young men and middle-aged men. However, there may still be a problem in that it may be heroic to assume that all observations are

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data on the independent variables listed in Table 9 could be ascertained. About half of these young men (according to Feldstein (1975)) would have returned to their original employers and hence not been included in the sample used in Table 9; the remainder, about 22 respondents would have been added to the sample of 688 observations used in Table 9. By similar reasoning, there were about 21 censored observations on middle-aged men compared to 163 included observations (the longer duration of unemployment for this cohort makes censoring quantitatively more important, although even here, only 11.4 percent of the potential observations were excluded).

Suppose that we were to impose the requirement that all included respondents be employed at both the beginning and end of the two-year period, even though the spell in question fell entirely between the two points, as was done in several of the studies reviewed in Chapter III. Then 273 of the 688 observations on young men and 30 of the 163 observations on middle-aged men would have been excluded, over and above the excluded cases referred to above.

generated by the same distribution. This problem of heterogeneity may lead to a bias toward the conclusion that transition rates are decreasing.[23] Therefore, the samples are further stratified (to the extent sample sizes permit) by variables found to be important correlates of unemployment duration in the analysis performed in the previous section.[24] The goodness of fit of the estimated distributions is tested using the Kolmogorov goodness of fit statistic.[25]

The acceptance wage model

In this section the hypotheses summarized in the third and fourth columns of Table 1 are tested. Table 7 lists the variables that are hypothesized to affect the log acceptance rate of pay (LNPOSTWAGE), the dependent variable. The expected signs of the coefficients should be self-explanatory, in light of Tables 1 and 5. The model is estimated separately for the young and middle-aged samples. In addition,

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[23] Kaitz (1970) found that for the labor force as a whole, the long-term unemployed were least likely to become re-employed in a given week. But he cautioned that this result may be due to heterogeneity. The potential for bias has been demonstrated more formally by Proschan (1963).

[24] The fact that we are dealing with two specific age-sex groups results in a certain degree of "homogeneity" without further stratification. In support of this assertion, examine the means for the variables DUR and LNDUR reported in Table 8.

[25] The test essentially involves comparing the actual and fitted values of the cumulative distribution function. It is one of a class of statistics known generically as statistics of the Kolmogorov-Smirnov type (cf. Conover (1971), pp. 293-326). It is generally considered superior to chi-square tests, which are also used in situations such as this (Conover (1971), p. 295).

for the young men, the model set forth in Table 7 is run with DLNWAGE, the annual gain in the log hourly rate of pay,[26] as the dependent variable, in order to assess the possible distorting effects of on-the-job investments (cf. pp. 15-16). This procedure is similar to the one used by Lazear (1976).

Note that one of the hypotheses which are tested here concerns the relation between unemployment duration and acceptance rates of pay. The variable used to represent unemployment duration is LNDURRES, the deviation of the actual value of LNDUR from the "predicted" value generated by the model proposed in Table 6 above and estimated in Table 9 below.[27] Recall from Chapter II that search theory postulates that essentially the same set of exogenous variables affects both log duration and log acceptance rate of pay. Clearly, LNDURRES is "purged" of the effect of these exogenous variables.[28] Hence, the simultaneity problems (i.e., spurious correlation between the unemployment measure and subsequent wages caused by common determinants) that plague, say, Burgess and Kingston (1976) are done away with.

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[26] This variable is defined more precisely in the Glossary (Appendix B).

[27] A more formal definition is given in Appendix B.

[28] More formally, LNDURRES is the residual of the regression estimated in Table 9. As such, it is orthogonal to (i.e., strictly uncorrelated with) the exogenous variables for that sample (Theil (1971), p. 113). Since the sample used for the acceptance wage model is somewhat smaller, the condition of strict orthogonality does not hold, although LNDURRES is uncorrelated with the independent variables in the limit.

Table 7: Independent Variables Hypothesized to Affect the Log of the Acceptance Rate of Pay

	Young men	Middle-aged men
<u>Wage offer level</u>		
SKILL2	+	–
<u>Probability of receiving an offer</u>		
LOCUR	–	–
LMSIZE	+	+
QUIT	+	+
<u>Search costs</u>		
UC	+	+
DEP	–	–
ASSETS	+	+
EYOINC	+	+
WPIN	+	
<u>Length of horizon</u>		
HORIZN	0	+
<u>Duration of search</u>		
LNDURRES	0/–	0/–
BLACK	?	?

This model compares favorably with the studies reviewed in Chapter III for the same reasons as the unemployment duration model specified above in terms of richer and more accurate data, and in terms of minimizing censorship biases.

CHAPTER V

EMPIRICAL RESULTS

The unemployment duration model

The model specified in Table 6 in Chapter IV was estimated for samples of 688 observations for the young men and 163 observations for the middle-aged men. The summary statistics are presented in Table 8; the regression results are presented in Table 9. Additional results for the sample of young men and subsamples thereof are presented in Table 10; additional results for the middle-aged men are presented in Table 11. The results will be discussed according to the theoretical concepts that the variables represent, as outlined in Table 6.

Wage offer level. The coefficient of SKILL1 is never significant, and it has the expected negative sign only in the case of the linear specification of the model for the middle-aged men. This is consistent with the empirical literature reviewed in Chapter III, where the estimated effects of various proxies for the wage offer level (e.g., number of years of schooling) were found to be rather sporadic.

Probability of receiving an offer. The coefficient of LOCUR is positive for both young and middle-aged men in both the linear and semilog specifications, but somewhat surprisingly, it is not statistically significant. We can better understand this finding by considering layoffs and quits separately. The regression results for these

Table 8: Summary Statistics for Young Men and Middle-Aged Men

Variables[a]	Young men		Middle-aged men	
	Mean	Std. dev.	Mean	Std. dev.
LNDUR	1.310	0.947	1.941	1.130
DUR	5.983	6.714	11.865	11.254
SKILL1	5.725	0.178	5.902	0.270
LOCUR	5.375	2.134	5.381	2.691
LMSIZE	0.550	1.016	0.450	0.843
QUIT	0.535	0.499	0.276	0.448
UC	5.452	16.731	23.873	30.304
DEP	0.603	1.033	1.491	2.023
ASSETS	0.853	4.451	12.052	17.527
EXOINC	0.031	0.192	0.162	0.662
HORIZ1	43.503	3.055	9.552	4.295
BLACK	0.356	0.479	0.301	0.460
Number of observations	688[b]		163[b]	

[a]All variables are defined in Appendix B.

[b]For descriptions of the samples, see Table 9, footnote d.

Table 9: Determinants of the Duration of Unemployment for Young Men and Middle-Aged Men: Regression Results[a]

Explanatory Variables	Young men		Middle-aged men	
	Total[b]	Total[c]	Total[b]	Total[c]
SKILL1	.2341 (0.88)	1.5350 (0.82)	.0095 (0.02)	-5.2245 (-1.27)
LOCUR	.0087 (0.51)	.0156 (0.13)	.0012 (0.36)	.0017 (0.05)
LMSIZE	.0598 (1.68)	.4735 (1.89)	.1084 (0.91)	1.3945 (1.18)
QUIT	-.1138 (-1.47)	-.9398 (-1.72)	-.1006 (-0.43)	-.8518 (-0.37)
UC	.0012 (0.50)	.0038 (0.23)	.0076 (2.05)	.0877 (2.40)
DEP	.0659 (1.71)	.4980 (1.83)	-.0050 (-0.10)	-.0756 (-0.16)
ASSETS	-.0116 (-1.37)	-.0882 (-1.48)	-.0042 (-0.71)	.0212 (0.37)
EXOINC	-.1597 (-0.84)	-.4740 (-0.35)	.0853 (0.63)	1.9330 (1.43)
HORIZ1	.0210 (1.31)	.1455 (1.28)	-.0342 (-1.50)	-1.549 (-0.68)
BLACK	.1293 (1.67)	1.0690 (1.95)	.0146 (0.06)	.9371 (0.42)
CONSTANT	-1.0394 (-0.51)	-9.6431 (-0.67)	1.9841 (0.83)	40.8640 (1.72)
R^2 (adjusted)	.011	.015	.024	.030
F ratio	1.79	2.02	1.40	1.51
Number of observations	688[d]	688[d]	163[d]	163[d]

Table 9 (continued)

[a]t-statistics are in parentheses.

[b]Dependent variable is LNDUR.

[c]Dependent variable is DUR.

[d]Sample consists of all instances of unemployment of at least one week's duration between the 1969 and 1971 surveys for which data on all relevant variables were ascertainable.

Table 10: Determinants of the Duration of Unemployment for Young Men: Additional Regression Results[a]

Explanatory Variables	Samples			
	Layoffs[b]	Quits[b]	Total[b]	Total[b]
SKILL1	.3558 (0.82)	.0816 (0.24)	.2259 (0.82)	.0462 (0.19)
LOCUR	.0438 (1.70)	-.0200 (-0.87)	.0064 (0.36)	-.0060 (-0.34)
LMSIZE	.0736 (1.32)	.0487 (1.05)	.0569 (1.59)	.0463 (1.25)
QUIT			-.1184 (-1.51)	-.1067 (-1.33)
UC	.0010 (0.38)		.0009 (0.37)	.0014 (0.57)
DEP	.1109 (2.03)	.0191 (0.34)	.0601 (1.53)	.0511 (1.34)
WFIN			-.2934 (-1.44)	
ASSETS	-.0244 (-1.58)	-.0050 (-0.51)	-.0119 (-1.30)	-.0180 (-1.88)
EXOINC	.2288 (0.76)	-.4833 (-2.00)	-.2934 (-1.44)	-.2605 (-1.32)
HORIZ1	.0220 (0.87)	.0137 (0.66)	.0161 (0.98)	
HORIZ2				.0333 (0.71)
BLACK	.0836 (0.70)	.1764 (1.73)	.1396 (1.76)	.1285 (1.50)
CONSTANT	-1.9958 (-0.60)	.2089 (0.08)	-.7570 (-0.36)	.9006 (0.68)
R^2 (adjusted)	.010	.007	.012	.011
F ratio	1.36	1.33	1.74	1.66
Number of observations	320[c]	368[d]	672[e]	611[f]

Table 10 (continued)

[a]t-statistics are in parentheses.

[b]Dependent variable is DUR.

[c]Sample is the same as described in Table 9, footnote d, except that it is further restricted to instances of layoffs.

[d]Sample is the same as described in Table 9, footnote d, except that it is further restricted to instances of quits.

[e]Sample is the same as described in Table 9, footnote d, except that it is further restricted to observations for which the variable WFIN is ascertainable.

[f]Sample is the same as described in Table 9, footnote d, except that it is further restricted to observations for which the variable HORIZ2 is ascertainable.

Table 11: Determinants of the Duration of Unemployment for Middle-Aged Men: Additional Regression Results[a]

Explanatory Variables	Samples			
	Layoffs[b]	Quits[b]	Total[b]	Total[b]
SKILL1	-.3613 (-0.73)	1.0333 (1.36)	.0649 (0.16)	.1682 (0.29)
LOCUR	.0415 (1.15)	-.1458 (-1.48)	.0111 (0.32)	.0487 (1.09)
LMSIZE	.2394 (1.87)	-.6180 (-1.87)	.1094 (0.92)	.2436 (1.68)
QUIT			-.1303 (-0.55)	.2780 (0.89)
UC	.0066 (1.78)		.0076 (2.06)	.0074 (1.61)
DEP	-.0384 (-0.60)	.0069 (0.09)	-.0067 (-0.14)	-.0917 (-1.23)
ASSETS	.0004 (0.06)	-.0234 (-1.63)	-.0057 (-0.95)	.0004 (0.06)
EXOINC	.4219 (1.67)	.0127 (0.08)	.0960 (0.70)	.8277 (2.76)
HORIZ1	-.0227 (-0.81)	-.0496 (-1.23)	-.1403 (-1.45)	
HORIZ1SQ			.0056 (1.12)	
HORIZ2				-.0048 (-0.14)
BLACK	.0270 (0.10)	-.1496 (-0.35)	.0433 (0.19)	.3992 (1.26)
CONSTANT	3.8116 (1.32)	-2.7356 (-0.61)	2.0756 (0.87)	.3331 (0.10)
R^2 (adjusted)	.032	.062	.026	.064
F ratio	1.43	1.36	1.39	1.56
Number of observations	118[c]	45[d]	163[e]	84[f]

Table 11 (continued)

[a] t-statistics are in parentheses.

[b] Dependent variable is DUR.

[c] Sample is the same as described in Table 9, footnote d, except that it is further restricted to instances of layoffs.

[d] Sample is the same as described in Table 9, footnote d, except that it is further restricted to instances of quits.

[e] Sample is the same as described in Table 9, footnote d.

[f] Sample is the same as described in Table 9, footnote d, except that it is further restricted to observations for which the variable HORIZ2 is ascertainable.

two types of turnover can be found in the first two columns of Table 10 for the young men, and the first two columns of Table 11 for the middle-aged men. We see that the effect of the local labor market unemployment rate on the log of duration is positive as expected in cases of layoffs for both cohorts; in the case of the young men, the coefficient implies that a one percentage point increase in the local unemployment rate produces an increase in unemployment duration of about 4.4 percent, or about 0.3 weeks in the neighborhood of six weeks (the sample mean), and it is statistically significant at the five percent level. But the coefficient of LOCUR is of the "wrong" sign, albeit quite insignificant, for quitters in both age groups. This may reflect a selectivity phenomenon. We know that the incidence of voluntary turnover is countercyclical; i.e., workers hold on to their jobs rather than quit when aggregate demand is weak (Parsons (1975)). This phenomenon would tend to "dampen" the observed adverse effects of labor market conditions on quitters. Furthermore, it may very well be the case that the effect of LOCUR on unemployment duration is even greater for those workers who are laid off and do not change employers (who are excluded from this analysis) than for those who are laid off and do change employers (who are included).[1]

[1]As was noted in Chapter IV above, temporary layoffs ending in recall are not included in this analysis because such instances of unemployment do not seem to provide an appropriate "experiment" for testing search theory. It should also be noted that the NLS data simply do not permit an analysis of that type of unemployment comparable to the analysis presented here, for reasons too numerous to discuss in detail here.

The effect of urbanicity (LMSIZE) is positive for both age groups and statistically significant for the young men. Thus the hypothesis that residence in an area that (presumably) contains more potential employers leads to more "efficient" search does not appear to hold water. But again, an interesting quit-layoff distinction is apparent from the first two columns of Tables 10 and 11, respectively. The effect of LMSIZE is positive for layoff victims and negative for quitters. This phenomenon can be explained if we assume that unemployment benefits are more "liberal" in jurisdictions with large labor markets.[2] In that case, LMSIZE would be a proxy for "potential" benefits, and according to an argument set forth in Chapter VI below, quitters (who are generally ineligible to receive benefits) have a greater incentive to become re-employed as benefits increase.

Quitters experience spells of unemployment that are 11 percent shorter than those experienced by layoff victims, in the case of young men. Among middle-aged men, the difference is 10 percent. These findings are, of course, quite consistent with our *a priori* hypotheses.

Search costs. The level of weekly unemployment compensation (UC) is positively related to unemployment duration, as expected, but the coefficient of UC is significant only for the sample of middle-aged men. One possible reason for the small effect of UC in the younger

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[2] This conjecture is confirmed by the correlation between the variables LMSIZE and UC in the middle-aged sample (r = .35). But the correlation is very weak (r = .03) in the younger cohort, mainly since so few respondents receive benefits, even in instances of layoffs.

sample is essentially demand-related.[3] The probability of recall to the previous employer is presumably greater for younger workers than older workers who, due to their seniority, are "immune" to the waves of temporary layoffs that occur from time to time. That is to say, older workers who are laid off are usually laid off permanently, with little hope for recall. The effect of the unemployment insurance system is to increase the probability of recall and decrease unemployment duration for temporary layoffs, since experience rating makes unemployment costly to the employer. So even though the younger workers in our sample did change employers *ex post*, the *ex ante* possibility of recall may have affected their unemployment duration.

Further analysis of the estimated effects of UC and their policy implications may be found in Chapter VI.

The other variables presumed to affect search costs (DEP, ASSETS, EXOINC) are all insignificant correlates of unemployment duration among members of the middle-aged cohort. But the results for young men are somewhat different. The coefficient of DEP is positive and significant in both the linear and semilog specifications, consistent with the hypothesis that those young men with more financial resources at their disposal (in this case, those with fewer dependents) use said resources to purchase information about job opportunities, as opposed to using them to subsidize a longer wait for a suitable job. The negative coefficients of ASSETS and EXOINC, although not so

[3]The author is indebted to Daniel S. Hamermesh for the following scenario.

significant statistically, support the same hypothesis.[4] So does the negative coefficient of WFIN in the model reported in the third column of Table 10. For that matter, the relatively small positive coefficient of UC for young men, discussed in the previous paragraph, may be explained by the apparent tendency of young searchers to use their weekly benefits to purchase information. But there is no obvious *a priori* explanation for this difference in behavior between the two cohorts.

Expected horizon. The *a priori* expectation was that those respondents with longer horizons over which returns to search would accrue would have an incentive to "hold out" for a better job. This hypothesis is not borne out. The effect of the variable HORIZ1 is positive and significant at the ten percent level for young men in both linear and semilog variants of the model. But the coefficient of HORIZ1 is of the theoretically inappropriate sign (i.e., negative) for the older sample. It was precisely for this group that the effect of impending retirement was supposed to be most important. A variant of the model in which LNDUR is postulated to be a quadratic function of HORIZ1 was estimated for the middle-aged men;[5] it is presented in the third column of Table 11. The *a priori* expectation was that the

[4]It may also be that the ability to accumulate assets and to generate nonlabor income flows is a positive function of one's human capital (i.e., ASSETS and EXOINC are proxies for the wage offer level).

[5]Due to collinearity, this model could not be estimated for the younger cohort.

effect of HORIZ1 would be positive but decreasing with HORIZ1 (i.e., the coefficient of HORIZ1 would be positive but the coefficient of HORIZ1SQ would be negative).[6] In fact, the estimated coefficients are not of the expected signs (although neither is highly significant).

On the supposition that it is expected tenure on the first post-unemployment job, rather than expected tenure in the labor force as a whole, that is relevant to an unemployed job seeker's behavior, the model was re-estimated with HORIZ2 in place of HORIZ1; the results are presented in the last columns of Tables 10 and 11 for the young men and middle-aged men, respectively. Expected tenure does not appear to have any explanatory power. Of course, this variable is somewhat crude in any event. First, HORIZ2 is an objective measure of expected tenure derived from turnover data. What is really required is a measure of the respondent's <u>subjectively perceived</u> expected tenure.[7] Second, HORIZ2 is constructed using information on the occupation in which the respondent accepted the job that terminated unemployment; i.e., <u>ex post</u> information on the market in which the unemployed person was searching. In light of the healthy amount of occupational mobility of unemployed persons,[8] it is likely that many people search more than one occupational market.

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semi-intuitive justification for this expectation arises from the numerical examples presented in footnotes 9 and 10 of Chapter [illegible].

[7]However, Stephenson (1976) did have just such a measure available in his study of unemployed youth, and it was not significantly correlated with unemployment duration, <u>cet. par</u>.

[8]See p. 15, footnote [illegible].

Additional remarks. Young blacks experience spells of unemployment that are about 13 percent longer than those of young whites, *cet. par*. This is a statistically significant "residual" that search theory has yet to explain. The estimated racial difference for middle-aged men is much smaller (less than two percent). More than anything else, these figures reflect the fact that there are gross racial differences in unemployment duration among young men, but not middle-aged men. The mean of LNDUR is 1.00 for whites and 1.19 for blacks in the younger cohort. Among older men, the direction of the racial differences is actually reversed; the mean of LNDUR is 1.96 for whites and 1.90 for blacks.[9]

Table 9 shows at a glance that virtually every conclusion drawn above is robust with respect to the functional form chosen. Hence, the supplemental results presented in Tables 10 and 11 all are based on the semilogarithmic variant (which makes more sense, among other reasons, because the dependent variable can only take positive values).

Although certain observed differences between the behavior of quitters and layoff victims were noted in the discussion above, the Chow test for differences in the estimated vectors of coefficients as a whole[10] showed no significant difference beween the two groups in

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[9]These figures imply geometric means of 2.71 weeks for white young men, 3.22 weeks for black young men, 7.08 weeks for white middle-aged men, and 6.71 weeks for black middle-aged men.

[10]Cf. Fisher (1970).

in either the younger or middle-aged cohort.[11]

The reported F ratios show that none of the vectors of coefficients reported in Tables 9 through 11 were significantly different from the null vector. This is not unusual for models of this sort using microdata.[12]

The distribution of unemployment duration

The object of this part of the analysis is to see whether the probability of re-employment varies over the course of a spell of unemployment, presumably because of revisions in the job seeker's search policy. The results are presented in Table 12. As Appendix D shows, a value of b that is greater than unity indicates that the probability of finding a job in the (t + 1)th week, given that a worker has been unable to find an acceptable job for t weeks, is an increasing function of t. Table 12 shows that the probability of re-employment is essentially constant, confirming the preliminary findings presented in Carr (1977). Those results were based on an analysis of data for young men only. It was expected that older workers would be more likely to exhibit an increasing transition rate (i.e., probability of re-employment), because they were less encumbered by minimum wage laws and such, but that is not the case. It

[11]Of course, as has been noted by other researchers (cf. Kalachek (1969)), relatively few older men enter unemployment except through layoff. The sample sizes reported in Table 11 support that conclusion. Unfortunately for the researcher, the small number of middle-aged quitters makes statistical tests of this sort relatively meaningless.

[12]See, for instance, Ehrenberg and Oaxaca (1976).

Table 12: Estimated Weibull Parameters of the Unemployment Duration Distribution for Young and Middle-Aged Men, by Race, Wage Level, Reason for Separation, and Level of Unemployment Benefits

	Number of observations	a	b	(t)[a]	K
Young men					
Whites					
Low SKILL1[b]					
Layoff	117	5.5331	1.0869	(0.94)	.1088
Quit	145	4.2707	1.1189	(1.43)	.1609###
High SKILL1[b]					
Layoff	147	5.3592	1.0114	(0.14)	.1739###
Quit	173	3.8317	1.2185	(2.87)***	.1788###
Blacks					
Layoff	144	5.8080	1.0567	(0.68)	.1193##
Quit	171	4.8457	1.0881	(1.15)	.1593###
Middle-aged men					
Layoff					
Low UC[c]	72	9.1982	1.0457	(0.37)	.1115
High UC[c]	72	14.7183	1.0380	(0.31)	.0992
Quit	59	8.7491	0.9221	(-0.57)	.1096

*** Significantly greater than one at the one percent level.

Significant at the five percent level.
Significant at the one percent level.

a Indicates whether b is significantly greater than one.

b A value of SKILL1 greater than ln(300) is considered "high."

c A value of UC greater than 25 is considered "high."

was also expected that young men searching from higher wage offer distributions would likewise be more likely to make downward revisions in wage demands and hence to increase their chances of finding a job, but that hypothesis is also not confirmed.[13]

Table 12 does indicate that for young men at least, quitters are more likely than those who are laid off to exhibit increasing transition rates. There is nothing in search theory that would yield such an a priori hypothesis. Perhaps the layoff victims who were included in this analysis originally expected to be recalled, as most layoff victims do.[14] If that were the case, their behavior would be "passive," and there would be no necessary reason for their probabilities of re-employment to increase systematically from week to week. At some point they would have had to search for alternative employers, and to adopt (and perhaps revise) a reservation wage strategy; otherwise they would not have changed employers, and thus have been included in this sample. The statistical technique employed here cannot "pick up" such a change in behavior; it posits that transition rates are either increasing, constant, or decreasing throughout.

The Kolmogorov goodness of fit statistic (K) was generally significant for the younger men, indicating that the "reasonableness" of the assumption that the duration of unemployment followed a Weibull

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[13] There is a good deal of evidence that minimum wage laws cause higher levels of youth unemployment (for instance, see Ragan (1977)). The findings presented here do not contradict those studies, but they do tend to rule out one mechanism whereby minimum wage laws have an effect.

[14] Cf. Feldstein (1975).

distribution could be questioned.[15]

As was mentioned in Chapter II above, a constant transition rate from unemployment to employment is not inconsistent with the search paradigm under certain assumptions. But there are other explanations for such a phenomenon which do not require the assumption that workers adopt explicit search strategies. These will be discussed in Chapter VI.

The acceptance wage model

The multivariate model outlined in Table 7 in Chapter IV was estimated for samples of 565 observations for the young men and 119 observations for the middle-aged men. Summary statistics for these groups are given in Table 13.[16] The regression results are presented in Table 14. Additional regression results are presented in Tables 15 and 16.

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[15] It would be a mistake to assert that the analysis outlined in this section would be "invalid" if the Kolmogorov test indicated that the goodness of fit was unacceptably poor. For instance, Heckman and Willis (1977) estimated a (most novel) model of labor force participation, and then proceeded to demonstrate via chi-square tests (which are, if anything, less powerful than the Kolmogorov test used here) that there were statistically significant discrepancies between actual and predicted behavior. As Conover (1971, p. 187) has noted: "We may always be quite sure that the true distribution function is never exactly the same as the hypothesized distribution function. We realize that in any goodness of fit test (the null hypothesis) will be rejected if the sample size is large enough."

[16] The universes used in Table 13 are subsets of the Table 8 universes. Those for whom the log acceptance rate of pay was not ascertainable were excluded from the former sample. By juxtaposing Tables 8 and 13 one can see that the restricted sample differs little from the larger sample in terms of relevant characteristics.

Table 13: Summary Statistics for Young Men and Middle-Aged Men

Variables[a]	Young men		Middle-aged men	
	Mean	Std. dev.	Mean	Std. dev.
LNPOSTWAGE	5.526	0.421	5.659	0.944
SKILL2	5.609	0.242	5.866	0.347
LOCUR	5.244	2.143	5.449	2.964
LMSIZE	0.535	1.006	0.475	0.875
QUIT	0.538	0.499	0.269	0.445
UC	5.001	15.898	25.993	31.495
DEP	0.565	0.944	1.420	2.015
ASSETS	0.697	3.514	12.482	15.581
EXOINC	0.034	0.201	0.182	0.748
HORIZON1	13.487	3.130	9.840	4.150
BLACK	0.333	0.472	0.227	0.421
LNDURRES	0.018	0.917	0.005	1.060
Number of Observations	565[b]		119[b]	

[a] All variables are defined in Appendix B.

[b] For descriptions of the samples see Table 14, footnote c.

Table 14: Determinants of the Acceptance Rate of Pay for Young Men and Middle-Aged Men: Regression Results[a]

Explanatory Variables	Young men Total[b]	Middle-aged men Total[b]
SKILL2	.6848 (7.53)	.9232 (2.47)
LOCUR	.0127 (1.75)	.0016 (0.51)
LMSIZE	-.0178 (-1.06)	-.0199 (-0.17)
QUIT	-.0073 (-0.22)	-.0313 (0.14)
UC	.0047 (4.39)	.0043 (1.26)
DEP	.0253 (1.38)	-.0113 (-0.24)
ASSETS	.0132 (2.80)	.0037 (0.58)
EXOINC	-.0759 (-0.96)	.0195 (0.17)
HORIZON1	.0051 (0.81)	.0037 (0.17)
BLACK	-.0543 (-1.42)	.1966 (0.77)
LNDURRES	-.0425 (-2.51)	.0513 (0.64)
CONSTANT	1.3857 (1.98)	-.0680 (-0.03)
R^2 (adjusted)	.242	.058
F ratio	17.33	1.66
Number of observations	565[c]	119[c]

Table 14 (continued)

[a] t-statistics are in parentheses.

[b] Dependent variable is LNPOSTWAGE.

[c] Sample consists of all instances of unemployment of at least one week's duration between the 1969 and 1971 survey for which data on all relevant variables are ascertainable.

Table 15: Determinants of the Acceptance Rate of Pay of Young Men: Additional Regression Results[a]

Explanatory Variables	Samples: Layoffs[b]	Quits[b]	Total[b]	Total[b]
SKILL2	.6096 (4.51)	.7841 (6.25)	.6619 (7.12)	.0409 (1.05)
LOCUR	.0092 (0.88)	.0151 (1.47)	.0128 (1.70)	-.0006 (-0.20)
LMSIZE	-.0093 (-0.38)	-.0352 (-1.48)	-.0167 (-0.99)	-.0025 (-0.32)
QUIT			-.0104 (-0.31)	-.0015 (-0.10)
UC	.0047 (4.40)		.0043 (3.94)	-.0001 (-0.24)
DEP	.0292 (1.25)	.0173 (0.58)	.0210 (1.13)	-.0125 (-1.58)
ASSETS	.0191 (3.31)	.0020 (0.24)	.0118 (2.46)	-.0262 (-0.74)
EXOINC	-.1293 (-1.03)	-.0214 (-0.21)	-.0958 (-1.[illegible]0)	.0019 (0.99)
WFIN			.0212 (1.93)	
HORIZ1	.0115 (1.21)	-.0010 (-0.12)	.0063 (0.99)	.0018 (0.67)
BLACK	-.0957 (-1.74)	-.0122 (-0.23)	-.0473 (-1.22)	-.0179 (-1.05)
LNDURRES	-.0362 (-1.53)	-.0487 (-1.96)	-.0421 (-2.46)	.0064 (0.86)
CONSTANT	1.5512 (1.46)	1.0800 (1.15)	1.4506 (2.04)	-.1839 (-0.62)
R^2 (adjusted)	.292	.174	.24[illegible]	.002
F ratio	11.72	8.09	15.69	1.09
Number of observations	261[d]	304[c]	555[f]	385[g]

Table 15 (continued)

[a]t-statistics are in parentheses.

[b]Dependent variable is LNPOSTWAGE.

[c]Dependent variable is DLNWAGE.

[d]Sample is the same as described in Table 14, footnote c, except that it is further restricted to instances of layoffs.

[e]Sample is the same as described in Table 14, footnote c, except that it is further restricted to instances of quits.

[f]Sample is the same as described in Table 14, footnote c, except that it is further restricted to observations for which the variable WFIN is ascertainable.

[g]Sample is the same as described in Table 14, footnote c, except that it is further restricted to observations for which the variable DLNWAGE is ascertainable.

Table 16: Determinants of the Acceptance Rate of Pay for Middle-Aged Men: Additional Regression Results[a]

Explanatory Variables	Samples	
	Layoffs[b]	Quits[b]
SKILL2	1.0495 (1.93)	.9403 (2.63)
LOCUR	.0216 (0.58)	-.0311 (-0.64)
LMSIZE	-.0623 (-0.41)	.0387 (0.22)
UC	.0049 (1.21)	
DEP	-.0418 (-0.55)	.0162 (0.41)
ASSETS	.0051 (0.62)	-.0002 (-0.02)
EXOINC	.1939 (0.70)	-.0308 (-0.41)
HORIZON1	-.0044 (-0.14)	.0051 (0.23)
BLACK	.3435 (0.96)	.1196 (0.45)
LNDURRES	.1070 (0.97)	-.1282 (-1.30)
CONSTANT	-.7965 (-0.25)	.0816 (0.04)
R^2	.031	.202
F ratio	1.274	1.870
Number of observations	87[c]	32[d]

Table 16 (continued)

[a] t-statistics are in parentheses.

[b] Dependent variable is LNPOSTWAGE.

[c] Sample is the same as describe in Table 14, footnote c, except that it is further restricted to instances of layoffs.

[d] Sample is the same as described in Table 14, footnote c, except that it is further restricted to instances of quits.

Wage offer level. Those workers who searched from higher wage offer distributions (as represented by the variable SKILL2[17]) garnered better-paying jobs, as expected.

Probability of receiving an offer. The variables LOCUR, LMSIZE, and QUIT had generally statistically insignificant effects on acceptance wages. The estimated effect of the local unemployment rate was positive and significant for the young men (it was expected to be negative). Although this is not consistent with our *a priori* hypothesis derived from the search paradigm, it does shed light on the validity of a certain alternative to that paradigm. This matter is discussed further in Chapter VI.

Search costs. The level of unemployment compensation was positively related to postunemployment wages. The estimated effects for both cohorts were similar (between four and five percent for an increase in weekly benefits of ten dollars), but the coefficient for the middle-aged men was not statistically significant, presumably due to the small sample size. Chapter VI presents certain extensions of these results and a discussion of their relevance for public policy.

The level of assets and wife's income had a significant positive effect for the younger sample, as predicted by search theory. But again, an alternative explanation is that those workers with greater human capital stocks are more successful at both accumulating assets (a function of preunemployment earnings) and achieving high

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[17]Of course SKILL2 also incorporates the effects of geographic location and hence cross-sectional price variation (presumably).

postunemployment rates of pay. Otherwise, the variables representing search costs were generally statistically insignificant.

Expected horizon. This variable had the expected positive estimated effect on acceptance wages fo[illegible] cohorts, but neither coefficient was statistically signifi[illegible]

The duration of search. The variable LNDURRES (the log duration of unemp[illegible] "purged" of systematic components) was significantly negatively [illegible]ed to acceptance rates of pay. The explanation consistent with search theory is that those who are unemployed longer revise their wage demands downward. But the evidence in Table 14 does not so strongly support that conclusion, since an obvious corrollary is that searchers who relaxed their wage demands would increase their chances of finding a job thereby, cet. par. Chapter VI contains further discussion of how these findings might be reconciled.

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[18] The equations in which HORIZ1 is replaced by HORIZ2 are not reported here, because the ex post nature of HORIZ2 (see p. 63) causes spurious positive correlation between acceptance wages and expected tenure. That is to say, random influences that cause a searcher to be more successful in terms of higher postunemployment wages are also likely, by the same token, to land him in an occupation characterized by low turnover, unless the searcher limits himself to seeking jobs in a single occupation from the start. Very little is known about this aspect of the search behavior of the unemployed, not nearly enough to make sound interpretations of an observed relationship between acceptance wages and expected tenure, as defined here.

Additional remarks. The hypothesis that the behavior of unemployed workers differs significantly by whether they quit or were laid off was tested by estimating the parameters of the basic model for the two groups; the results for the young men appear in the first two columns of Table 15, and the corresponding results for middle-aged men appear in Table 16. A Chow test showed no significant differences between the sets of coefficients as a whole for either cohort; furthermore, there were no significant differences in the coefficients of any individual variable.[19]

There was no significant racial differential in initial post-unemployment rates of pay.

Recall from Chapter III that Ehrenberg and Oaxaca (1976) found that for younger workers, the level of unemployment compensation did not significantly affect initial acceptance rates of pay, cet. par., and that they conjectured that the effects of the benefits were masked by the propensity of at least some young searchers to seek jobs with better prospects for high earnings over the life cycle, which are not necessarily the jobs with the highest starting rates of pay. That is to say, the effects of unemployment benefits on wage gain may be "delayed." To test this hypothesis, the basic model of Table [illegible] was estimated with [illegible], the annual change in the log of the hourly rate of pay, as the dependent variable. The results are presented in the last column of Table 15. The E-O hypothesis is not confirmed.

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[19]See footnote [illegible] above.

insofar as the estimated coefficient of UC remains at about the same level in that equation as in the equation reported in the first column of Table 14. Of course, our estimate of the impact of unemployment compensation of initial acceptance wages was higher than E-O's to begin with, thus partially obviating the need for this exercise.

Table 15 does reveal at least one other interesting pattern, though. Those workers who have more dependents tend to earn lower wages, cet. par., consistent with search theory, whereas the analysis of initial acceptance wages yields the opposite finding. That is, persons who have more dependents appear to opt for a wage profile that calls for relatively high initial earnings, and hence the usual analysis of acceptance rates of pay is somewhat misleading. This points out the advantages of longitudinal data for the purposes of this sort of analysis.

CHAPTER VI

SUMMARY AND CONCLUSIONS

Summary of findings

A number of hypotheses concerning the job search behavior of unemployed workers have been tested in this study, with the following results: the duration of unemployment of young men (aged 17 to 29) varies directly with the local labor market size and the number of dependents. Black young men tend to be unemployed longer than white young men, cet. par., and layoff victims are unemployed longer than quitters. The duration of unemployment of middle-aged men (aged 48 to 64) varies positively with the weekly amount of unemployment benefits, but is generally uncorrelated with any other variable. In particular, there are no statistically significant differences by race or degree of volition (layoff versus quit).

The distribution of unemployment duration is approximately exponential for both young and middle-aged men. This implies that the probability of leaving unemployment in a given week is the same for the short-term unemployed as for the long-term unemployed.

Postunemployment hourly rates of pay vary positively with human capital stocks and the level of unemployment benefits for both young and middle-aged men. In addition, the postunemployment wage of young men varies inversely with unemployment duration and directly with the local unemployment rate, cet. par.

How good is optimal job search theory?

In this section we will evaluate the efficacy of the optimal job search paradigm that was used to generate most of the hypotheses tested herein. In so doing, we will examine empirical findings previously reported as well as those reported here in order to get the "big picture" of how labor markets operate.

The results of the two multivariate analyses presented in Tables 9 and 14 show that the majority of the hypotheses set forth in Tables 6 and 7 in Chapter IV have not been confirmed. In this respect, this study does not differ much from most earlier studies. However, one such study (that by E-O) elicited the following comment from Welch (1977, p. 453):

> I am sympathetic with the multiple regression technique of introducing control variables for particling out "true" relationships, but this paper must represent something of a record in ineffective...control. Virtually all of the "action" is in the variable of interest [the unemployment insurance variable]....

Furthermore, the robustness of the theory (in the sense that diverse phenomena are explained by the theory) is not always evident. For instance, search theory predicts a positive effect of nonlabor income on the duration of unemployment; that is certainly confirmed by Table 9, especially for the sample of middle-aged men. But the search theoretic explanation is that this [illegible] relation is a result of the more stringent wage demands [illegible] from non-work related [illegible] that were the case, then the postunemployment wages earned by those with higher nonlabor income would tend to be higher, cet. par. Table 14 does not bear out this corollary hypothesis at all.

The finding that the probability of locating and accepting a job is largely invariant with respect to the amount of time the worker has been searching is not necessarily inconsistent with search theory under certain conditions. The search theoretic explanation is that workers do not revise their asking wages. But that is not the only possible explanation of the phenomenon. It could just as well be that workers do not adopt an asking wage "strategy" that divides wage offers into acceptable and unacceptable offers, but that the probability of accepting an offer is unity, for all practical purposes. Sometimes this notion is expressed as follows: "Unemployed workers don't search for the best offer, they search for an offer, period." Hence, the major determinant of the duration of unemployment is the probability of receiving an offer (i.e., demand side considerations rather than supply side considerations).

But now consider our finding that acceptance wages are negatively correlated with the duration of unemployment, cet. par. This finding is not consistent with the apparent lack of correlation between transition rates and the duration of unemployment under the assumption of a stable wage offer distribution, an almost ubiquitous feature of search models that are currently [illegible] suggests that it might be appropriate for search theorists [illegible] devote their energies to developing models of search and [illegible] employment that incorporate the possibility of "signalling." That is to say, the event of unemployment itself, especially a spell [illegible] long duration, could serve as a (negative) "signal" to prospective employers about the candidate's quality, causing the level of wage offers to deteriorate.[2,3] Such a model would not only be consistent with the findings [illegible] here, but with most of the studies of [illegible] cited in Chapter III above.

Another puzzling [illegible] young men, at least, [illegible] communities with high unemployment rates [illegible] was [illegible] not. [illegible] The standard search theoretic [illegible] is that [illegible] presumably would

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[1] Of course, one could always [illegible] by claiming that one result or [illegible] is "wrong," i.e., by impeaching the empirical analysis on technical grounds. [illegible] for purposes of this discussion [illegible] such empirical [illegible] in its own right, so that we can see what [illegible] theories of labor markets. Possible technical limits [illegible] discussed in the next section.

[2] In a similar vein, [illegible] theory [illegible] in which employers discriminate against [illegible] groups (e.g., blacks and women) on the basis of [illegible] adverse perception [illegible] of [illegible] members' productivity.

[3] Welch (1977) has [illegible] would only reinforce such an effect, [illegible] the extent that employers perceive certain job applicants as "[illegible]" and hence [illegible] from the standpoint of [illegible] system.

aggregate demand in those localities would lead to less stringent wage demands. There may be a plausible explanation in Hall's (1972) argument that the wage offer level is higher in high unemployment markets because of demand side considerations: "High wages could be paid in cities with high unemployment rates precisely because the high rates discourage quits, and a work force with a low quit rate is more productive (p. 726)." Hall also presents some empirical evidence in support of his assertion. Once more, though, the search paradigm would have to be modified to incorporate this effect.[4]

Finally, it should be noted that none of our hypotheses concerning the effect of the length of the searcher's horizon on search behavior was confirmed. The first "horizon" proxy (expected working lifetime) did not perform as expected, just as it did not have the expected effect in the studies reviewed in Chapter III. But that proxy was somewhat suspect to begin with, since it could not explain differences in unemployment duration <u>between</u> cohorts (i.e., if time remaining until retirement were the relevant "horizon," older workers would experience shorter spells than younger workers, contrary to fact). On the other hand, the longer expected job tenure of older men does explain their longer spells of unemployment; however, our proxy for that variable failed to explain variations in unemployment

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[4]Search theorists are beginning to develop models that explain how the wage offer distribution that the suppliers of labor face is generated, instead of introducing it as a deus ex machina. Burdett (1973) and Rothschild (1973) have reviewed some of these models.

duration <u>within</u> cohorts. Perhaps the [illegible] in the role of risk aversion; i.e., although searchers with longer expected tenure have an incentive to search longer, [illegible] individuals will opt for both short spells of unemployment and [illegible]-turnover occupations and industries, producing an inverse relation between unemployment duration and expected job tenure.[5] Further research is clearly called for.

Policy implications: [illegible]

Perhaps the [illegible] study from a policy perspective are the [illegible] estimates [illegible] of the [illegible] of the unemployment insurance [illegible], in [illegible] results [illegible], in Tables [illegible] and 4. We derive the estimated changes in unemployment [illegible] and acceptance wages associated with a ten dollar [illegible] weekly [illegible] amount.[6] Such an increase in [illegible] would increase [illegible] duration of spells by 0.12 weeks [illegible] young men [illegible] the [illegible] of the middle-aged men. The hourly acceptance rate of pay [illegible] estimated to rise by nineteen cents for the younger [illegible] and [illegible] cents for the older cohort. These [illegible] as those presented in Tables 3 and [illegible], although the acceptance [illegible] effects are higher than most previous [illegible].

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[5] Cf. Feinberg (19[illegible]) [illegible] of this point.

[6] Just as in Tables [illegible], [illegible] weeks and a postunemployment [illegible] per hour is assumed.

Perhaps the import of these numbers would be even more transparent if we were to calculate the predicted duration of unemployment and acceptance wage for our samples given the mean value of weekly benefits versus the predicted duration in the absence of benefits.[7] The predicted duration drops by 0.02 weeks for young men and by 1.15 weeks for middle-aged men.[8,9] It is well known that in a steady state, the unemployment rate is equal to the product of the probability of entering unemployment and the duration of unemployment. Let us assume for the moment that the UI system does not affect flows into unemployment. Then the figures presented in the preceding paragraph imply that the November 1971 unemployment rate of 10.7% for males aged 20 to 24 would have been 10.6% in the total absence of unemployment benefits; the 3.2% unemployment rate for males aged 55 to 64 would have dropped to 2.7%. These are not spectacular changes by any means, especially when one considers that the impact of the system is greatest on older workers, whose propensity to become unemployed is relatively low.

Calculations of this sort have implications for aggregate economic policy. As Classen (1977, p. 442) has noted,

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[7] The procedure used to derive these numbers is spelled out in Appendix F.

[8] E-O performed comparable calculations and estimated decreases of 0.1 weeks for young men and 0.9 for middle-aged men.

[9] Of course, the differential impact by cohort reflects both the higher level of benefits and the greater impact per dollar among older men.

> [I]f UI is responsible for a [illegible] of the recent increases in unemployment, then the "natural" or non-inflationary rate of unemployment may have increased. Fiscal and monetary policies that are designed to deal with aggregate demand failures may lead to unacceptable rates of inflation when they are used to reduce UI-induced unemployment.

This conjecture does not appear to be borne out, although no firm conclusions can be drawn for the following reasons.

1) The NLS data [illegible] age [illegible] in the labor force.

2) Unemployment resulting from layoffs [illegible] is not covered by these data. The impact of the [illegible] duration in this group is probably [illegible] for job changers for reasons [illegible] above.

3) The level of benefits may affect [illegible] unemployment as well as the duration of unemployment, although the direction of such an impact is unclear. This issue will be discussed further below in the section on directions for future research.

4) There may be a [illegible] impact on duration among members of a segment of the labor force that is not included in our sample: namely, workers (especially recent labor market entrants) who do not qualify for benefits because they have insufficient experience in jobs that are covered by the system.[10] An increase in the level of benefits is expected to cause such workers to [illegible] [illegible]ion wages so

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[10] See Hamermesh (1977) for a discussion of eligibility requirements in the several states.

that they can become re-employed and qualify for benefits, just the opposite of the effect predicted above.[11] Since there are presumably more unqualified workers among young men than middle-aged men, this is yet another theoretical basis for expecting differences in behavior between the two age groups. Unfortunately, the data do not permit us to investigate this hypothesis, since we cannot observe "potential" benefits for the unqualified, but only actual benefits for the qualified.

5) There is an implicit assumption that the aggregate demand for labor is extremely elastic.[12] To see this, assume that all benefits were cut off and that the formerly insured lowered their reservation wages accordingly. The the formerly uninsured would be "squeezed out" of job opportunities; i.e., there would be a tradeoff between unemployment among the formerly insured and the formerly uninsured, unless there were enough vacancies to go around.

6) There is a possible upward bias caused by the fact that our UI variable reflects actual receipt of benefits rather than potential benefits, just as in other studies (see p. 23 above).

On the whole, then, even the modest impact of the UI system implied by our results is probably overstated on balance.

Similarly, our results imply that in the absence of UI benefits, the post unemployment wages of young men would be 2.3 percent lower

[11]Cf. Mortensen (1977).

[12]The following argument has been put forth by Marston (1975).

than they are, and the wages of middle-aged men would drop by 10.6 percent.[13] These estimates must also be qualified by several caveats:

1) Employers may shift UI taxes onto workers through lower wages (Welch (1977)).

2) The impact of UI on future earnings is overstated (understated) to the extent that subsequent turnover and unemployment is encouraged (discouraged).

3) The subset of the unemployed examined here (recipients who change employers) are more likely to benefit in the way of higher wages from i[illegible] UI benefits than non-recipients (the "unqualified", [illegible] temporary layoff victims.

4) Again, [illegible] of all age-sex groups would be desirable.

Although [illegible] cannot say for sure, the aggregate impact of UI on workers' wages is probably smaller than what is implied by our results.

Policy implications: other issues

The expected effect of worsening labor market conditions (as measured by the local unemployment rate)[14] was not in evidence to

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[13] E-O estimated a decrease in wages of 13.9 percent for the middle-aged (they failed to perform a similar calculation for young men because of the statistical insignificance of the estimated impact of UI benefits).

[14] Among other reasons, the impact of this variable is interesting from a policy viewpoint because it "triggers" extended UI benefits. Cf. Hamermesh (1977, p. 6).

any great degree in the findings presented in this study, either among young workers (who presumably would be most affected) or among workers in the pre-retirement years. However, the labor market problems of youth may be manifested not through the duration of unemployment and wage changes among job changers (the objects of this study) but by, say, increased flows into the unemployed status and by increased duration of unemployment among layoff victims who are recalled (which are beyond the scope of this study). This conjecture receives some support from the respective findings of E-O and Grasso (1977), both of whom used the NLS data, and both of whose models included the local unemployment rate as a hypothetical determinant of unemployment duration. E-O's sample of young men included both job changers and others; they found that a one percentage point increase in the local unemployment rate produced a 9.7 percent increase in the length of a spell, or about 0.6 weeks, evaluated at six weeks, which is roughly the sample mean for this group (see Table 8 above). On the other hand, Grasso found an impact of only 0.1 weeks for each percentage point increase in the local unemployment rate for a sample limited to job changers.

The analysis of the distribution of unemployment duration is relevant to the correct interpretation of statistics on the mean length of the incomplete spell (i.e., duration up to the survey date) for those unemployed as of a monthly CPS survey, and their relation to the length of completed spells (i.e., duration up to the date of reemployment). Salant (1977) has shown that under certain assumptions about the probability of re-employment and whether it rises or falls with time unemployed, the length of the incomplete spell can be a poor proxy for the length of the *completed* spell of unemployment, and hence that one cannot casually use CPS statistics as a measure of the impact of unemployment on various subgroups of the population.[15] The findings presented here (that the probability of finding a job is relatively constant) suggest that the one variable is in fact a workable proxy for the other, at least for certain purposes.

Finally, policy makers as well as scholars may wish to know whether the sweeping changes in the labor market and fertility behavior of women in recent years have affected the experiences of unemployed men.[16] This study sheds some light on these questions. First, we note that there is no significant relation between household size and either of our dependent variables in the case of the middle-aged sample.[17] But for the young men, an increase in the number of dependents tends to increase unemployment duration without increasing postunemployment wages, *cet. par.*, and an increase in the wife's income leads to shorter spells and higher postunemployment wages. These findings suggest that current trends toward smaller families and

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[15] Salant analyzed unemployment data by occupation. He found, for instance, that sales workers had the second longest incomplete spells on average (8.7 weeks) but the shortest expected completed spell lengths (3.8 weeks).

[16] The author is indebted to Kristen A. Moore for suggesting the policy relevance of these results.

[17] Recall that the wife's income could not be ascertained for the middle-aged respondents.

higher female labor market activity may tend to ameliorate the unemployment experiences of young men. However, we must withhold final judgment, since we cannot tell from this study whether the increased labor supply of women has decreased the demand for male labor, leading to a lower wage offer level and to concomitant longer unemployment spells and lower acceptance wages.

Caveats and limitations

As was emphasized in Chapter IV above, no test of the predictions of a theory is any better than the data used to represent the relevant theoretical concepts. The NLS data are of almost unparalleled richness in providing data on the personal characteristics of respondents. But the data are deficient in some respects, and the conclusions of this study must be considered with these limitations in mind.

The data on unemployment insurance-related variables are not ideal. First, the data reflect receipt of benefits, not legal eligibility; the problems caused by that fact have already been noted. Second, the data refer to average weekly benefits over a year's period; there may be measurement errors introduced when such information is imputed to an individual spell.[18] Third, if the evidence

[18] As Appendix B indicates, that problem was minimized in this study by imputing a zero value to the unemployment benefit variable in cases of quits, since quitters are generally ineligible for benefits (Hammermesh (1977), p. 5). Apparently no other user of the NLS data has used information on reason for separation to correct the UI data.

presented by Hills (1976) and Holen (1977) is sound, the number of weeks for which the respondent can receive benefits, as well as the weekly amount for which he is eligible, is of material importance. This information is also not ascertainable from the NLS data.

Also, certain environmental variables (e.g., local labor market size and the local unemployment rate) are imputed to the respondent based on his survey date residence. To the extent that there is geographic mobility between surveys, these factors are measured with error, with unclear implications.

Of course, the use of the duration of unemployment as a proxy for search "inputs" is not perfect in this study any more than it is in the numerous other studies in the literature. In particular, data on two factors that might affect the number of offers generated per period would be useful: namely, the division of time between search and leisure[19] and the purchase of information.

Directions for future research

Some potentially fruitful areas for future research have been identified in the previous two sections, in connection with the the theoretical and empirical discussions contained therein. In this section we identify other questions that should be explored.

[19] Barron and Mellow (1977) developed and tested (with favorable results) several hypotheses concerning the search-leisure tradeoff of the unemployed, but they did not really address the interesting question of whether more "intensive" search actually generates more offers and shortens unemployment duration.

First, the eligibility of workers for extended benefits when the unemployment rate in a given state exceeds a "critical" level (Hamermesh (1977), p. 6), a relatively new program not in effect during most of the period of time covered by this study or any of the other studies reviewed in Chapter III,[20] seems likely to increase the cross-sectional variation in the maximum number of eligible weeks, and hence makes that variable potentially more important. Its effects should be investigated fully.

Also, longitudinal surveys as the NLS should be used to study the correlates of tenure on the postunemployment job. If, say, the provision of unemployment benefits fosters "productive" search and "good" employer-employee matches, then the unemployed should stay longer at the jobs that they eventually accept, and the effect of the UI system would be at least partially offset by decreased turnover and the concomitant flows into unemployment. But if UI merely causes unemployed job seekers to hold out for wages that are "unusually" high compared to their skill level, then it may very well cause subsequent layoffs.[21] Reinforcing this latter effect is the effect of imperfect experience rating. Experience rating is designed to assess a firm's contribution toward financing the UI system on the basis of its past layoff practices. Such rating is invariably imperfect,

[20]Hills (1976) has noted that the Federal law establishing extended benefits was enacted in October 1970, but unemployment was still generally low then.

[21]The relation between layoffs and the level of wages relative to skill level was examined by Parsons (1972).

causing a tendency to subsidize firms and industries with unstable labor demand.[22] The data needed to test these hypotheses would have to come from a follow-up study covering several years after the unemployment experience, to avoid the obvious possibilities for censorship biases.

Another avenue for potentially fruitful research is the analysis of the determinants of unemployment duration for various subgroups of the unemployed. For instance, it would be interesting to investigate the effect of UI benefit levels on those not yet qualified to receive benefits, to see whether their search behavior is affected by "potential" benefits. Also, a comparative study of the search and unemployment behavior of those who presumably are seeking alternative employment and those who are not would shed much light on the phenomena uncovered in this study. Previous studies have generally failed to distinguish between job changers and others. This study has made the distinction between the two groups, but the data permit an analysis based on the former group only. Even that distinction is based on whether or not the searcher actually changed employers; i.e., it is based on an ex post distinction. If somehow workers (expecially layoff victims) could be classified on the basis of their ex ante perceptions of whether they expect to be recalled to their old employers, an analysis that exploits that information would be most enlightening.

[22] Cf. Katz and Hight (1977).

Finally, the implications of censoring observations on persons who are unemployed at the end of the period under consideration should be explored fully. The direction of biases caused by this generally unavoidable defect in empirical design is usually unclear, to say nothing of their magnitude. It has been argued above that this study is less afflicted by censorship problems than other studies,[23] but that is no substitute for knowing how our conclusions should be qualified by any remaining problems.

[23] See pp. 45-46, footnote 22.

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APPENDIX A

PROOF OF EQUATION (4)

Assume a distribution with density function $f(w)$ such that $f(w) \geq 0$, $0 < w < \infty$, and $f(w) = 0$ elsewhere. If the distribution is truncated from below at $w^* > 0$, then the mean of the truncated distribution is

$$E_1 = A/B, \tag{A1}$$

where

$$A = \int_{w^*}^{\infty} w\, f(w)\, dw > 0. \tag{A2}$$

and

$$B = \int_{w^*}^{\infty} f(w)\, dw > 0. \tag{A3}$$

If the distribution is truncated from above at w^* and from below at $0 < w^{**} < w^*$, then the mean of the truncated distribution is

$$E_2 = C/D, \tag{A4}$$

where

$$C = \int_{w^{**}}^{w^*} w\, f(w)\, dw > 0 \tag{A5}$$

and

$$D = \int_{w^{**}}^{w^*} f(w)\, dw > 0. \tag{A6}$$

If the distribution is truncated from below at w^{**}, the resulting mean is

$$E_3 = \int_{w^{**}}^{\infty} w\, f(w)\, dw / \int_{w^{**}}^{\infty} f(w)\, dw = (A + C)/(B + D). \tag{A7}$$

We wish to show that the mean of the truncated distribution varies positively with the lower truncation point; i.e., that

$$E_1 = A/B > E_3 = (A + C)/(B + D). \tag{A8}$$

Since the mean of a distribution, if such exists (and we assume it does) cannot lie outside the range of values for which the density function is positive, we have

$$w^* \leq E_1 = A/B, \tag{A9}$$

and

$$w^{**} \leq E_2 = C/D \leq w^*. \tag{A10}$$

Hence

$$C/D \leq A/B, \tag{A11}$$

with the strict inequality holding except when $E_1 = E_2 = w^*$, a rather trivial case that we can assume away. Equation (A11) implies

$$BC < AD \tag{A12}$$

$$B\,(A + C) < A\,(B + D) \tag{A13}$$

$$(A + C)/(B + D) < A/B, \tag{A14}$$

Q.E.D.

APPENDIX B

GLOSSARY

AGE

The age of the respondent in years, as of the start of the spell of unemployment.

ASSETS

Net family assets, as of the survey date immediately preceding start of the spell of unemployment (in thousands of 1971 dollars).

BLACK

Equals one if the respondent is black, and zero otherwise.

DEP

Number of dependents other than the respondent's wife, as of the survey closest to the start of the spell of unemployment.

DLNWAGE

Equals the natural logarithm of the 1973 survey week hourly rate of pay (in 1971 cents) minus LNPOSTWAGE (q.v.), divided by the time (in years) between the start of the postunemployment job and the 1973 survey week.

DUR

The number of weeks the respondent reported looking for work between jobs.

EXOINC

Income from interest, dividends, etc., reported at the survey closest to the start of the spell of unemployment (in thousands of 1971 dollars).

HEALTH

Equals one if the respondent reported a work-limiting health problem as of the survey date closest to the start of the spell of unemployment, and zero otherwise.

HORIZ1

Equals the difference between the respondent's expected retirement age (reported at the 1969 survey date) and his current age, for those middle-aged men who reported an expected retirement age; for all other middle-aged men and all young men, equals sixty-five minus AGE (q.v.).

HORIZ1SQ

Equals the square of HORIZ1 (q.v.).

HORIZ2

An estimate of the respondent's expected tenure on the subsequent job, derived by the method described in Appendix E.

LMSIZE

The 1960 size of the labor force in the labor market in which the respondent resided, as of the survey closest to the spell of unemployment (in millions).

LNDUR

The natural logarithm of DUR (q.v.).

LNDURRES

Calculated according to the formula LNDURRES = LNDUR $- \sum_j b_j X_j$, where the X_j are the independent variables listed in Table 9 and the bj are the estimated coefficients given in the first (third) column of Table 9 for the young (middle-aged) men.

LNPOSTWAGE

The natural logarithm of the hourly rate of pay (in 1971 cents) earned by the respondent on the first job of at least one month's duration after the spell of unemployment.

LNWAGE71

The natural logarithm of the hourly rate of pay (in 1971 cents) earned by the respondent on the 1971 survey week job.

LOCUR

The unemployment rate in the labor market in which the respondent resided, as of the survey closest to the beginning of the spell of unemployment (in percent).

POTX

For young men, equals the number of months between the time the respondent stopped attending regular school and the start of the spell of unemployment (divided by twelve so that it is expressed in years).

POTXSQ

Equals the square of POTX (q.v.).

QUIT

Equals one if the respondent quit the last job of at least one month's duration preceding the spell of unemployment, and zero if he was laid off.

SCHL

The highest grade of "regular" school completed by the respondent as of the spell of unemployment.

SKILL1

The natural logarithm of the hourly rate of pay (in 1971 cents), calculated from the equations presented in Appendix C under the assumption that the respondent is a resident of a non-Southern labor market with a labor force of 500,000 persons.

SKILL2

The natural logarithm of the hourly rate of pay (in 1971 cents), calculated from the equations presented in Appendix C under the assumption that the respondent lives in the labor market in which he lived at the survey date closest to the start of the spell of unemployment.

SOUTH

Equals one if the respondent lived in the South at the survey closest to the start of the spell of unemployment, and zero otherwise.

UC

In instances of layoffs, equals the average weekly unemployment compensation per week reported by the respondent for the year in which the layoff occurred; equals zero otherwise.

WFIN

For married respondents, equals total income of the wife reported at the survey closest to the start of the spell of unemployment (in 1971 dollars). For all other respondents, equals zero.

APPENDIX C

DERIVATION OF MEASURES OF THE WAGE OFFER LEVEL

There is an extensive literature dealing with the analysis of wage determination using human capital models. In particular, the specific functional form which relates the worker's wage to its determinants such as schooling and labor market experience has been studied by a number of researchers. The theoretical and empirical justification of the specific functional form used in this appendix can be found, for instance, in Mincer (1974).

The procedure used to define the variables SKILL1 and SKILL2 is as follows. First, wage structures for whites and blacks from both cohorts were estimated using data from the 1971 surveys. The results are reported in Table 17; summary statistics are given in Table 18. Then the variable SKILL1 was defined for each observation by imputing a value of 0 for the variable SOUTH and a value of .500 for LMSIZE, regardless of the actual geographic location of the respondent, and then computing, for the t-th respondent

$$SKILL1_t = \sum_j b_j X^*_{jt}, \tag{C1}$$

where the X^*_{jt}, $j = 1, \ldots, K$, are the (modified) values of the independent variables listed in Table 18, and the b_j are the coefficients for the relevant age-race group. Then SKILL 2 was computed using the formula

$$SKILL2_t = \sum_j b_j X_{jt}, \tag{C2}$$

Table 17: Summary Statistics

Variables[a]	Young white men		Young black men	
	Mean	Std. dev.	Mean	Std. dev.
LNWAGE71	5.845	0.428	5.569	0.413
SCHL	12.476	2.377	10.890	2.436
POTX	5.240	3.797	5.651	3.817
POTXSQ	41.864	52.954	46.476	57.417
LMSIZE	0.666	1.136	0.727	1.217
SOUTH	0.293	0.455	0.638	0.481
HEALTH	0.068	0.251	0.055	0.228
Number of observations	1570[b]		600[b]	

Variables[a]	Middle-aged white men		Middle-aged black men	
	Mean	Std. dev.	Mean	Std. dev.
LNWAGE71	6.041	0.550	5.638	0.518
SCHL	10.458	3.311	7.302	3.826
POTX	39.230	5.564	42.469	5.877
POTXSQ	1569.922	442.883	1838.079	500.374
LMSIZE	0.720	1.184	0.693	1.054
SOUTH	0.249	0.433	0.613	0.487
HEALTH	0.192	0.394	0.159	0.366
Number of observations	1832[b]		749[b]	

[a] All variables are defined in Appendix B.

[b] For a description of the samples, see Table 18, footnote c.

Table 18: Predicted Log Hourly Rate of Pay Equations, by Cohort and Race[a]

Explanatory Variables	Young white men[b]	Young black men[b]	Middle-aged white men[b]	Middle-aged black men[b]
SCHL	.0801 (17.30)	.0759 (11.81)	.0606 (12.70)	.0179 (2.66)
POTX	.0797 (9.77)	.0272 (2.33)	.0001 (0.00)	.0046 (0.12)
POTXSQ	-.0029 (-4.97)	-.0002 (-0.30)	-.00005 (-0.15)	-.0002 (-0.57)
LMSIZE	.0454 (5.28)	.0092 (0.67)	.0500 (4.96)	.0471 (2.43)
SOUTH	-.1406 (-6.51)	-.3191 (-9.11)	-.0899 (-3.25)	-.3154 (-7.56)
HEALTH	-.0931 (-2.49)	-.1246 (-2.09)	-.1467 (-5.00)	.0178 (0.40)
CONSTANT	4.5657 (65.18)	4.8029 (49.65)	5.4913 (10.73)	5.9279 (7.18)
R^2 (adjusted)	.252	.360	.202	.275
F ratio	89.15	57.23	78.10	48.35
Number of observations	1570[c]	600[c]	1932[c]	749[c]

[a] t-statistics are in parentheses.

[b] Dependent variable is LNWAGE71.

[c] Samples consist of respondents in the relevant age-race group who were not enrolled in school and who were employed at the 1971 survey, and for whom data on all relevant variables are ascertainable.

where the X_{jt} are the values of the independent variables, including the "raw" values of SOUTH and LMSIZE.

The interpretation of these two variables is as follows. It is assumed that variation in wages due to the two geographic variables represents primarily geographical variation in price levels, rather than "real" (human capital and health) factors. Hence SKILL1, an estimate of the hourly rate of pay a respondent could earn, on average, in a Northern community with a labor force size of 500,000, is "purged" of such variation in nominal wages, and is the most appropriate proxy for the wage offer level in the analysis of unemployment duration. SKILL2 is used for the analysis of post-unemployment wage determination because the dependent variable in that analysis is also affected by nominal wage variation.[1]

---

[1] Alternatively, SKILL1 could have been used and the dependent variable transformed to reflect what that (log) wage would have been in the hypothetical community. It can easily be shown that none of the coefficients in Tables 14 through 16 would have been affected, except for the constants.

APPENDIX D

PROPERTIES OF THE WEIBULL DISTRIBUTION AND ESTIMATION PROCEDURES

Properties

The (two-parameter) Weibull distribution has the density function

$$f(t) = (\beta/\alpha)\ (t/\alpha)^{-1} \exp\left(-(t/\alpha)^{\beta}\right),\ 0 < t < \infty. \tag{D1}$$

The mean of the distribution is $\Gamma(\beta^{-1} + 1)$, which is approximately equal to α in the neighborhood of $\beta = 1$ (which is the case for the estimated distributions reported in Table 12 in Chapter V). The variance is equal to $\alpha^2(\Gamma(2\,\beta^{-1} + 1) - (\Gamma(\beta^{-1} + 1))^2)$, a decreasing function of β (Johnson and Kotz (1970a), pp. 252-253). Figure 1 displays three density functions, each with a mean of unity, but with differing values of β. The figure should make it clear that the distribution becomes less dispersed as β increases.

The instantaneous hazard rate is

$$H(t) = (\beta/\alpha)\ (t/\alpha)^{\beta-1}, \tag{D2}$$

an increasing, constant, or decreasing function of t as β is greater than, equal to, or less than unity. This property is interesting when the Weibull distribution is used as a waiting time distribution because it determines whether the probability of an event occuring in the period between and $t + \Delta t$ is an increasing, constant, or decreasing function of the elapsed time t (Carr (1977)).

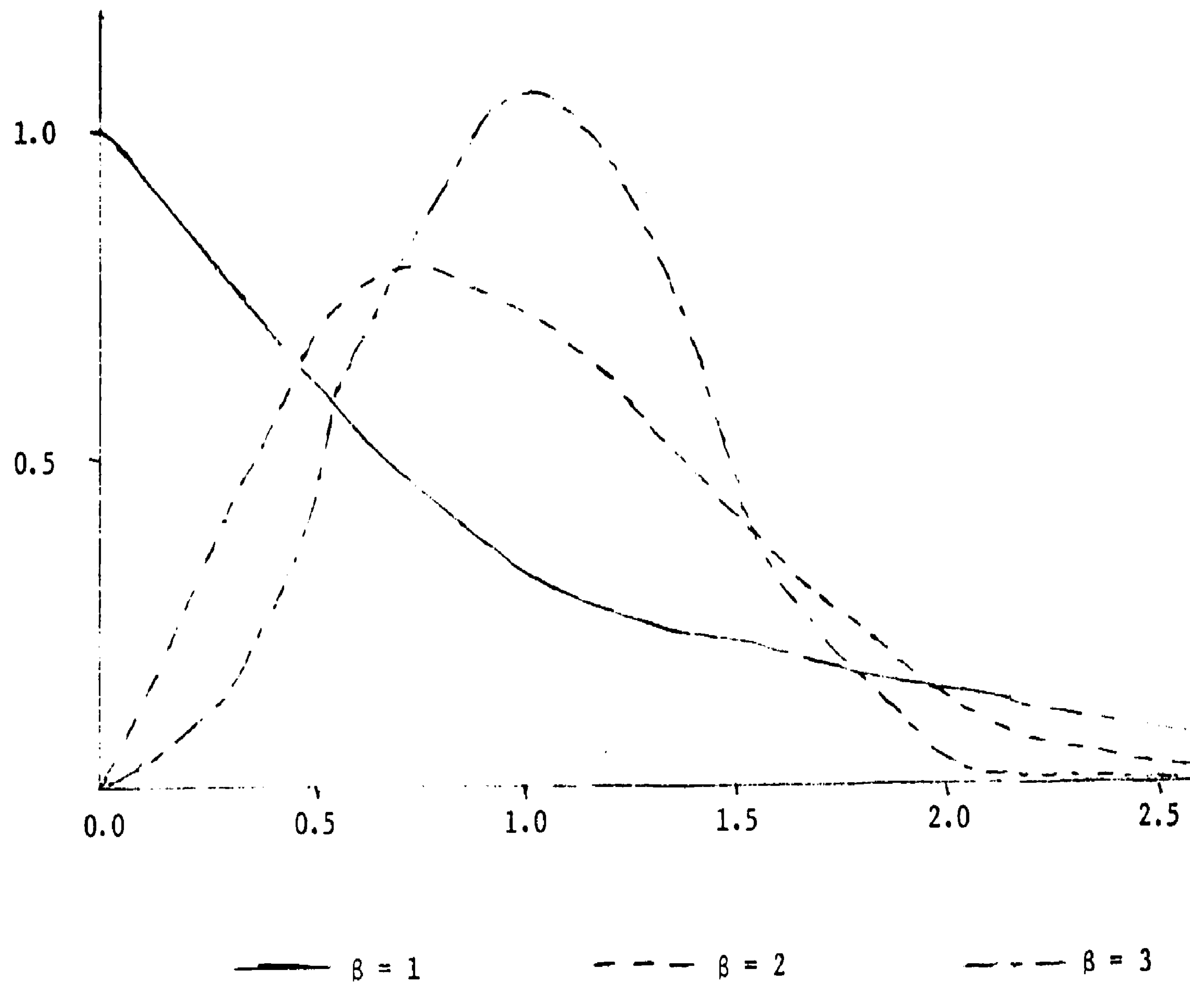

Figure 1: Weibull Density Functions for Different Values of β

Estimation

Let $t_1, \ldots, t_N$ be a random sample from a Weibull distribution. Menon (1963) showed that

$$b^{-1} = \sqrt{6}/\Pi \text{ (standard deviation of } \ln t_1, \ldots, t_N) \qquad \text{(D3)}$$

is a consistent estimator of β^{-1}; it is asymptotically normally distributed with asymptotic variance $1.1/(N\beta^2)$. An estimator of α is

$$a = \exp \text{ (mean of } (\ln t_1, \ldots, t_N) + \gamma b^{-1}), \qquad \text{(D4)}$$

where γ is Euler's constant.

When a discrete distribution $f(t)$ which only takes on integer values is approximated by a continuous distribution $g(t)$, the probability that $t = t_0$ is approximated by $G(t_o + 0.5) - G(t_o - 0.5)$, where $G(t)$ is the cumulative distribution function associated with $g(t)$. Since here $Pr(t < 1) = 0$, the fitted distribution $g(t)$ should have a range from 0.5 to infinity, as opposed to a range of zero to infinity as in the usual two-parameter Weibull case. This is accomplished by setting $t_i^* = t_i - 0.5$, $i = 1, \ldots, N$, and evaluating the estimators given by equations (D3) and (D4) using the transformed data.

APPENDIX E

ESTIMATION OF EXPECTED JOB TENURE

If a member of a population has a constant probability p of leaving that population in a given period, his life expectancy (i.e., expected length of membership) can easily be shown to equal $1/p$. But if the escape probability varies according to the length of time that person has belonged to the population, his life expectancy at "birth" (entry into the population) must be calculated in a manner such as the one described below, which is commonly used by demographers.[1]

Assume that the probability of separation is p_1 in the first t periods and p_2 thereafter.[2] Then, assuming that separations are evenly spaced over the interval,[3] the number of "survivors" after t periods is $(1-p_1t)N$, where N is the size of the original population, and the average number of periods lived in the first t periods equals $(1-\frac{1}{2}p_1t)Nt$. Since the number of people who survive the first t periods is $(1-p_1t)N$ and each has a remaining life expectancy of $\frac{1}{p_2}$,

[1]For a more detailed exposition, see Barclay (1958, Chapter 4).

[2]If the escape rate rises or falls more or less continuously with tenure (as is the case in most applications), separate probabilities for each of a large number of short intervals would have to be used in a more complicated formula.

[3]This is, strictly speaking, inconsistent with the assumption that p_1 is constant over the interval. It is a very good approximation for short intervals.

the total number of periods lived after the first t periods is $(1-p_1t)N/p_2$. After summing over the two intervals and dividing through by N,

$$\text{life expectancy at entry} = (1-\tfrac{1}{2}p_1t)t + 1/p_2 \qquad \text{(E1)}$$

For the purpose of this study, expected tenure was estimated for a number of age-occupation groups,[4,5] using data from the 1970 and 1971 surveys of young men and the 1969 and 1971 surveys of middle-aged men. It is very well documented that turnover is very high among employees who have been with their employers only a few months.[6] Accordingly, within each age-occupation group, the probability that respondents who were employed in 1970 (1969) were not employed with the same firm in 1971 was estimated separately for "low tenure" and "high tenure" groups. For the young men, "low tenure" was defined as not more than nine months' service with the respondent's 1970 survey week employer (i.e., $t = 0.75$); it is at about this point that turnover rates begin to level off.[7] It would have been desirable to do

[4]Sample sizes permitted blue-collar workers to be divided into two groups by race.

[5]Parnes (1971) and Kohen (1974) have investigated the determinants of turnover among young men; Parnes et al. (1973) and Parnes and Nestel (1974) have performed similar analyses for the middle-aged men. The consensus of these studies is that age and occupation are important determinants of turnover. Sample sizes do not permit further stratification on the basis of any other variable.

[6]See the references cited in footnote 5.

[7]It should go without saying that separation probabilities do not drop abruptly at the end of the first nine months. In fact, they drop continuously over that period, and they drop slightly thereafter. If sample sizes had permitted, a more complicated procedure alluded to in footnote 1 would have been used.

likewise with the middle-aged men; unfortunately, the distribution of tenure of currently employed members of that cohort at the 1969 survey was such that there were few respondents who had only recently begun to work with their current employers. Hence, "low tenure" was defined as less than five years' service.[8,9]

Expected tenure was then calculated by formula (E1); the results are presented in Table 19. The value HORIZ2 was created by imputing these values to respondents in the two cohorts according to their age,[10] subsequent occupation, and race. For those middle-aged men who reported an expected retirement age and whose expected tenure, as calculated by equation (E1), was greater than the time remaining until that age, expected tenure (HORIZ2) was revised downward so as not to exceed the time remaining until retirement.

---

[8] The cutoff at five years' service is largely arbitrary; it does ensure that there are sufficient sample sizes to estimate separation probabilities in both groups. The undesirable consequence of this procedure is that for the middle-aged men, there is little advantage to be gained by using formula (E1), compared to the crude method of taking the reciprocal of a separation probability estimated for all members of the sample, regardless of length of service.

[9] Since there was no 1970 survey of middle-aged men, we can only ascertain whether there was an employer change over a two-year period between 1969 and 1971. However, this presents no major problems. The "period" is simply redefined as two years for purposes of equation (E1); the expected number of periods is then doubled to get the expected number of years. See Barclay (1958) for further details.

[10] The analysis presented in Table 19 is based on data from the 1970 (1969) wave of surveys, when the young men (middle-aged men) were 18 to 28 (48 to 62) years of age. Unemployed workers in our sample could range in age from 17 to 29 (48 to 64). Those respondents who fell outside the age range covered by Table 19 were simply assigned to the nearest age group.

Table 19: Expected Tenure by Age, Occupation, and Race

Young men			Middle-aged men		
Age-occupation-race group	Number of Observations	Expected tenure (in years)	Age-occupation-race group	Number of Observations	Expected tenure (in years)
Age 18-20			Age 48-52		
White collar	113	2.86	White collar	334	16.70
Craftsmen	95	2.64	Craftsmen	273	10.92
Blue collar[a]			Blue collar[a]		
Whites	185	2.64	Whites	140	14.74
Blacks	96	2.20	Blacks	71	12.91
			Service and Farm	83	11.07
Age 21-24			Age 53-57		
White collar	258	3.97	White collar	264	12.88
Craftsmen	148	2.92	Craftsmen	203	8.29
Blue collar[a]			Blue collar[a]		
Whites	189	3.55	Whites	122	13.20
Blacks	119	2.95	Blacks	84	8.30
			Service and Farm	116	11.05
Age 25-28			Age 58-63		
White collar	315	4.76	White collar	189	8.40
Craftsmen	181	7.30	Craftsmen	121	5.04
Blue collar[a]			Blue collar[a,c]	112	5.58
Whites	175	3.34	Service and Farm	110	7.33
Blacks	103	3.62			
Service and Farm[b]	170	2.72			

[a]Here "blue collar" is defined so as to exclude craftsmen.

[b]There were an insufficient number of cases to stratify this occupation group by age.

[c]There were an insufficient number of cases to stratify this occupation group by race.

APPENDIX F

CALCULATION OF EFFECTS OF UNEMPLOYMENT BENEFITS ON UNEMPLOYMENT DURATION AND POSTUNEMPLOYMENT RATES OF PAY

This procedure follows that outlined in an unpublished appendix to Ehrenberg and Oaxaca (1976).

The predicted value of the log of the duration of unemployment associated with a given set of values of the independent variables $x_1^*, \ldots, x_k^*$, is

$$\text{LNDUR}^* = \sum_{j=1}^{k} b_j x_j, \tag{F1}$$

where $b_1, \ldots, b_k$ are the estimated coefficients reported in the first column of Table 9 for the young men, and in the third column of Table 9 for the middle-aged men.

It can easily be shown (Theil (1971), p. 113) that the predicted value associated with the sample means of the independent variables is the sample mean of the dependent variable, i.e.,

$$\overline{\text{LNDUR}} = \sum_{j=1}^{k} b_j \bar{x}_j \tag{F2}$$

Now consider the predicted value implied by a vector of values of the independent variables where one variable (in this case, the level of unemployment benefits (UC)) is equal to zero, and all of the others assume their mean values. This predicted value is

$$\text{LNDUR}^* = \sum_{j=2}^{k} b_j \bar{x}_j = \sum_{j=1}^{k} b_j \bar{x}_j - b_1 \bar{x}_1 = \overline{\text{LNDUR}} - b_1 \bar{x}_1 \tag{F3}$$

The values of the predicted duration of unemployment associated with $\overline{LNDUR}$ and LNDUR* are found by taking their antilogs. The difference between these two values; i.e.,

$$D = e^{LNDUR^*} - e^{\overline{LNDUR}}, \tag{F4}$$

is the number reported on p. 65 of Chapter VI.

The impact of unemployment benefits on acceptance wages (p. is derived by applying equations (F2) and (F3), replacing LNDUR by LNPOSTWAGE, and using the estimated coefficients reported in Table 14. Then, taking antilogs, we get the predicted wages with and without unemployment benefits; namely, $e^{\overline{LNPOSTWAGE}}$ and $e^{LNPOSTWAGE^*}$, respectively. Then the percentage change in wages caused by setting UC equal to zero is

$$PD = \frac{e^{LNPOSTWAGE^*} - e^{\overline{LNPOSTWAGE}}}{e^{\overline{LNPOSTWAGE}}}. \tag{F5}$$

The Center for Human Resource Research

The Center for Human Resource Research is a policy-oriented research unit based in the College of Administrative Science of The Ohio State University. Established in 1965, the Center is concerned with a wide range of contemporary problems associated with human resource development, conservation and utilization. The personnel include approximately twenty senior staff members drawn from the disciplines of economics, education, health sciences, industrial relations, management science, psychology, public administration, social work and sociology. This multidisciplinary team is supported by approximately 50 graduate research associates, full-time research assistants, computer programmers and other personnel.

The Center has acquired pre-eminence in the fields of labor market research and manpower planning. The National Longitudinal Surveys of Labor Force Behavior have been the responsibility of the Center since 1965 under continuing support from the United States Department of Labor. Staff have been called upon for human resource planning assistance throughout the world with major studies conducted in Bolivia, Ecuador and Venezuela, and recently the National Science Foundation requested a review of the state of the art in human resource planning. Senior personnel are also engaged in several other areas of research including collective bargaining and labor relations, evaluation and monitoring of the operation of government employment and training programs and the projection of health education and facility needs.

The Center for Human Resource Research has received over one million dollars annually from government agencies and private foundations to support its research in recent years. Providing support have been the U.S. Departments of Labor, State, and Health, Education and Welfare; Ohio's Health and Education Departments and Bureau of Employment Services; the Ohio cities of Columbus and Springfield; the Ohio AFL-CIO; and the George Gund Foundation. The breadth of research interests may be seen by examining a few of the present projects.

The largest of the current projects is the National Longitudinal Surveys of Labor Force Behavior. This project involves repeated interviews over a fifteen year period with four groups of the United State population; older men, middle-aged women, and young men and women. The data are collected for 20,000 individuals by the U.S. Bureau of the Census, and the Center is responsible for data anlysis. To date dozens of research monographs and special reports have been prepared by the staff. Responsibilities also include the preparation and distribution of data tapes for public use. Beginning in 1979, an additional cohort of 12,000 young men and women between the ages of 14 and 21 will be studied on an annual basis for the following five years. Again the Center will provide analysis and public use tapes for this cohort.

The Quality of Working Life Project is another ongoing study operated in conjunction with the cities of Springfield and Columbus, in an attempt to improve both the productivity and the meaningfulness of work for public employees in these two municipalities. Center staff serve as third party advisors, as well as researchers, to explore new techniques for attaining management-worker cooperation.

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A third area of research in which the Center has been active is manpower planning both in the U.S. and in developing countries. A current project for the Ohio Advisory Council for Vocational Education seeks to identify and inventory the highly fragmented institutions and agencies responsible for supplying vocational and technical training in Ohio. These data will subsequently be integrated into a comprehensive model for forecasting the State's supply of vocational and technical skills.

Another focus of research is collective bargaining. In a project for the U.S. Department of Labor, staff members are evaluating several current experiments for "expedited grievance procedures," working with unions and management in a variety of industries. The procedural adequacies, safeguards for due process, cost and timing of the new procedure are being weighed against traditional arbitration techniques.

Senior staff also serve as consultants to many boards and commissions at the national and state level. Recent papers have been written for the Joint Economic Committee of Congress, The National Commission for Employment and Unemployment Statistics, The National Commission for Manpower Policy, The White House Conference on the Family, the Ohio Board of Regents, the Ohio Governor's Task Force on Health, and the Ohio Governor's Task Force on Welfare.

The Center maintains a working library of approximately 9,000 titles which includes a wide range of reference works and current periodicals. Also provided are computer facilities linked with those of the University and staffed by approximately a dozen computer programmers. They serve the needs of in-house researchers and users of the National Longitudinal Survey tapes.

For more information on specific Center activities or for a copy of the Publications List, write: Director, Center for Human Resource Research, Suite 585, 1375 Perry Street, Columbus, Ohio 43201.