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INTRODUCTION

The first four papers in Volume 5 of *Research in Labor Economics* focus on the effects of various government policies on labor market outcomes. In his opening paper, John H. Pencavel reports estimates of the effects of the Seattle–Denver Income Maintenance Experiments on the labor supply decisions of husbands and wives. Although numerous researchers have analyzed these data, Pencavel is the first to stress the relationship between one adult's labor supply and the unemployment of his or her spouse. He examines the robustness of the husband–wife labor supply relationship under alternative empirical specifications and concludes that the apparent importance of husbands' unemployment for wives' work decisions, which has been observed in studies using nonexperimental data, may result from investigators' failure to account for unobserved differences in work behavior among women.

Charles Brown's paper reviews the available evidence on the impact of federal equal employment opportunity programs, namely, those enforced by the Equal Employment Opportunity Commission and the Office

of Federal Contract Compliance Programs, on the economic well-being of nonwhites and females. Time-series studies suggest that the relative labor market positions of nonwhites have increased because of these programs. While several arguments have been advanced to show that these gains are illusory, Brown provides evidence that much of the apparent progress is real. Unfortunately, cross-sectional studies are shown by him to yield a far less conclusive picture. Brown also discusses two yet unresolved puzzles—the decline in the black male labor force participation rate that began at roughly the same time as the antidiscrimination efforts, and the fact that black females have advanced more rapidly than black males and more highly educated blacks have advanced more rapidly than less highly educated blacks since the federal antidiscrimination efforts began.

In recent years, numerous papers (including several published in earlier volumes of *Research in Labor Economics*) have analyzed the effects of particular provisions of the unemployment insurance (UI) system on unemployment. Alan L. Gustman's paper provides a more unified treatment. He decomposes unemployment into more basic elements related to the labor market flows that determine unemployment incidence and duration. His paper then proceeds systematically through these elements and considers the major behavioral explanations linking the UI system to unemployment. Utilizing this framework, he provides a critical evaluation of the existing empirical and theoretical literature relating UI to unemployment and points out areas in which major gaps in our knowledge remain.

Does retirement behavior react predictably to economic incentives, and—more specifically—how do private pensions and the social security system affect retirement decisions? These are the issues which Olivia S. Mitchell and Gary S. Fields address in their contribution. The Mitchell–Fields paper reviews the lessons and limitations of the recent economic literature on pensions, earnings, and retirement. Their review is guided by life cycle theory, and they critically evaluate the extent to which prior theoretical and empirical studies are consistent with this framework. They provide a comprehensive survey both of the state of our knowledge (as of 1982) and the major directions in which research in the retirement area should proceed.

The next three papers in the volume are rigorous theoretical treatments of particular institutional features of labor markets–treatments providing frameworks within which empirical research is then conducted. Yoram Weiss and Lee Lillard focus on the academic labor market, a market which is of great interest to many readers of *Research in Labor Economics*. They provide a model that can explain the basic features of academic

labor contracts, including lifetime employment guarantees for tenured workers, wage adjustments that occur, to a large extent, in jumps as promotions are attained, tenure and promotion standards which often are expressed as some simple index of accumulated past output (e.g., number of published papers), and the considerable variance that exists in the waiting time for tenure and promotion. Their model is based on the uncertainty inherent in the academic production process. Under certain assumptions, the distribution of waiting times to promotion and tenure is shown to be a simple function of the promotion standards and average productivity of a group. They use data obtained from the National Science Foundation and the American Council on Education on time to tenure of Ph.D.'s employed in American universities to estimate the effects of cohort, scientific field, and sex on the rate of productivity and promotion standards. The model is further tested using Israeli data.

James Brown's contribution focuses on the question, "How Close to an Auction is the Labor Market?" The model of optimal income-insuring labor contracts that he develops implies that wages should be less variable than marginal value products and that firms should rely on layoffs as a means of employment adjustment in states of sufficiently low product demand. These results do appear to be broadly consistent with the aggregate year-to-year evidence and suggest that in the short-run the labor market is not an auction market (where wages adjust continuously to clear the market). Empirical analyses of his conducted at the two-digit manufacturing industry level indicate that the estimated degree of income insurance offered varies across industries and an explanation for this result is provided. Brown also shows that over longer periods of time the labor market does function like an auction market—returning to the equality of wages and marginal products. The adjustment period involved here, however, may exceed five years.

Contracts, institutional arrangements, and work rules linking firms and workers are created in response to the goals of those firms and workers, but are conditioned by the economic environment. Dennis Epple, V. Joseph Hotz, and Allan Zelenitz focus on employment arrangements in industries like construction and dock work–industries in which the demand for labor by particular firms tends to be highly erratic leading to jobs that are short lived and workers typically working for a number of different employers during the year. These authors show that if unions have the power to stipulate the terms of a contract, then the contract that results will have workers paid more than their marginal products and that some firms, namely, those with the most stable demand schedules, will choose not to enter into the contract. That is, it may be optimal for the union *not* to try to organize the entire industry. The authors also show

that some of the predictions of their model are consistent with construction industry data, although unfortunately the data necessary to test other implications of their model are not yet available.

The eighth paper in the volume is an empirical analysis of the employment histories of noncollege young women in the period after they leave school. Stanley P. Stephenson, Jr., uses the National Longitudinal Survey (NLS) data to estimate the determinants of the transition rates of entering and leaving nonemployment status. An important finding of his is that in-school job holding affects the rate of job finding for white young women but not for nonwhites. The NLS data show that most jobs held by nonwhites in school were in the government sector, whereas white young women tended to have private sector work experience while in school. Whether these differences in the effects of in-school experience reflect the differences in the nature of the jobs held or some other factors cannot be determined from the data. It is clear, however, that the race-specific effects of prior work experience on later employment, coupled with the race differences in the types of in-school work experiences, should have important implications for youth employment policies.

The final contribution to this volume is by two British economists, David Metcalf and Steven Nickell, and focuses on occupational mobility in Great Britain. Surprisingly, rather little is currently known about the process of social mobility within generations in Great Britain. Metcalf and Nickell use retrospective data on labor market experiences for over 50,000 individuals from the 1975 National Training Survey to study this question. They find that over the 1965–1975 period there was a decline in the number of jobs in low paid occupations. Overall, however, an impression of considerable occupational stability emerges: over the 10-year period, on average, each individual changed his or her occupation only once. The authors' regression analyses also highlight the effects of age, labor market experience, marital status, and educational credentials on occupational mobility.

Volume 5 of *Research in Labor Economics* will be followed soon by a special supplement that will contain the papers from a conference on "New Approaches to Labor Unions," held in Blacksburg, Virginia, in October 1981. The papers in both this volume and the supplement each represent the highest standards of scholarship and each should generate much interest in its readers. Readers of *Research in Labor Economics* who have manuscripts that are longer than typical journal articles and that satisfy both of the above criteria are encouraged to submit them to me for consideration for possible inclusion in subsequent volumes.

Ronald G. Ehrenberg
Series Editor

CONTENTS

UNEMPLOYMENT AND THE LABOR SUPPLY EFFECTS OF THE SEATTLE–DENVER INCOME MAINTENANCE EXPERIMENTS

John H. Pencavel

I. INTRODUCTION

This paper reports estimates of the effects of the Seattle–Denver Income Maintenance Experiments (SIME/DIME) on the working decisions of husbands and wives. In measuring these effects, special attention is paid to the relationship between one adult's employment and the unemployment of his or her spouse. The role of unemployment in the operation of an income maintenance scheme has received little attention to date although the temporary relief provided to families experiencing a transitory drop in income could be one of the most important functions of a guaranteed minimum income. Moreover, the issue of within-family adjustments in labor supply has been a recurring theme in the literature on labor

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force participation (Long, 1958; Mincer, 1960, 1962; Cain, 1966; Bowen and Finegan, 1969). This paper examines the robustness of the apparent husband–wife labor supply relationship under alternative empirical specifications.

These data provide an unusually good opportunity to examine these issues since Seattle, one of the two sites for the negative income tax (NIT) experiment, experienced especially high unemployment during the years of the experiment. The unemployment rates in Seattle were high compared with those in the years before the experiment and also compared with unemployment rates in Denver, the other experimental site: according to the U.S. Department of Labor's published unemployment rate series, Seattle's unemployment rate in 1968 was 2.9% and in 1969 was 4.0% while Denver's was 3.0% and 2.9%, respectively; the unemployment rates in 1970, 1971, and 1972 were 9.7%, 12.5%, and 10.8% in Seattle while they were 3.1%, 3.0%, and 2.9% in Denver. Hence, the unemployment rate in Seattle was more than three times that in Denver when NIT payments commenced in Seattle (around November 1970), and it was some four times that in Denver when families first received payments in Denver (November 1971). Indeed, it was feared that the layoffs in Seattle's aircraft industry from 1969 to 1971 created an idiosyncratic labor market situation from which it was hazardous to extend inferences about the effects of a negative income tax to more typical labor market settings.[1] In view of this, it seems appropriate to investigate the consequences of this unemployment on the labor supply effects of the NIT experiment.

Before proceeding to the research results, some preliminary remarks are in order. In this paper, the analysis of labor supply focuses on the occurrence of market employment vs. nonemployment where the latter includes those who are unemployed and those out of the labor force as defined by the Bureau of Labor Statistics. The occurrence of these events is measured by dichotomous or polychotomous variables and, in essence, variance–covariance analysis is applied which was exactly one of the procedures Orcutt and Orcutt (1968) envisaged in their classic statement of the case for income maintenance experiments.[2] The estimates of the conditional probabilities of particular events are derived either from the application of ordinary least-squares analysis (the linear probability function) or from maximum likelihood estimation of a logistic transformation. The properties of the linear probability estimator in this context are well known: the estimator of the regression coefficients is unbiased and consistent whereas that of the standard errors is biased and inconsistent and, in particular, the variance of the disturbances is heteroskedastic; moreover, the application of Goldberger's proposed generalized least-squares

estimator to account for this heteroskedasticity runs up against the problem that the predicted values of the dependent variable may fall outside of the unit interval, in which case we are prohibited from interpretating these estimates as conditional probabilities. It was because of these defects that the logit procedure was also invoked. In fact, a comparison of the results from the ordinary least-squares estimates (not the generalized least-squares procedure) and the logit estimates when both were applied indicates negligible differences in the subsequent inferences for the working decision of both husbands and wives. Hence in the presentation of the results, we switch back and forth from maximum likelihood estimates derived from the application of the logit transformation to linear probability estimates (uncorrected for any heteroskedasticity).

The experimental environment is characterized in this paper simply in terms of an experimental–control dummy variable or in terms of separate dummy variables for the different financial programs. Of course, such an experimental–control dummy variable may be a blunt instrument for measuring the effects of the experiment since some experimental families share more features in common with control families than with other experimental families. Such may be the case for experimental families whose incomes are consistently above the break-even level and who never become eligible for regular NIT payments. In this event, the experimental–control dummy variable measures the effects of the NIT averaged over those experimental families who receive payments and over those who do not. In this sense, this procedure understates the experimental effects conditional upon being below the break-even level. On the other hand, this simple procedure delivers a straightforward story while a more elaborate modeling may be asking too much of the data. That is, where our available measures of the experimental environment contain errors in observation, the more alert the researcher to potential omitted variable bias and as a consequence the more "control variables" added to the estimated equation, the more likely we are to understate the correct experimental impact.[3]

The next section presents some descriptive statistics on employment and unemployment in Denver and Seattle before and during the experiment. Section III presents some standard estimates of the experimental impact on employment for husbands and for wives and of the role of unemployment. The part played by the husband's unemployment in the work decision of the wife is subject to further investigation in Section IV, where an interpretation of the results is offered and where the appropriateness of treating the husband's unemployment as exogenous with respect to the wife's working decision is examined. A summary of the results is contained in Section V.

II. DESCRIPTIVE STATISTICS ON EMPLOYMENT AND UNEMPLOYMENT

Participants in SIME/DIME were selected from lower income families who were considered likely to be eligible for and most affected by a national income maintenance program. The participants were divided into (i) experimental families who were enrolled in one of the 11 NIT programs and were eligible to receive payments and (ii) control families. Each NIT program is described by a support level and a tax function where the support level is the grant a family receives if it has no other income and where the tax function describes the rate at which the grant declines as other income increases. The (tax) break-even level is the level of family income at which the NIT payment is zero. A description of the 11 NIT programs tested in SIME/DIME is given in Table 1 with more detail available in Kurz and Spiegelman (1972).

The basic analysis sample consists of 1657 husband–wife families, of whom 964 are in Denver and 693 are in Seattle. Each family completed the interviews necessary for constructing three years of data consisting of one preexperimental year and of the first two years of the experiment. Since we were interested in measuring contemporaneous husband–wife interactions, the sample of families was restricted to those experiencing no marital change during this three-year period. This is an important condition since this restriction may well be removing those whose labor supply is most responsive to an NIT. The total sample of 1657 families consists of 777 control families and 880 experimental families who were eligible for NIT payments during the experiment.

Table 1. Negative Income Tax Plans Tested in SIME/DIME[a]

Plan	*Variables in Text*	*Tax Breakeven Level*
S = 3800, T = .5, R = 0	T5 = 1, S38 = 1	$10,250
S = 3800, T = .7, R = 0	T7 = 1, S38 = 1	6,350
S = 3800, T = .7, R = .025	T7R = 1, S38 = 1	10,850
S = 3800, T = .8, R = .025	T8R = 1, S38 = 1	7,800
S = 4800, T = .5, R = 0	T5 = 1, S48 = 1	13,150
S = 4800, T = .7, R = 0	T7 = 1, S48 = 1	8,520
S = 4800, T = .7, R = .025	T7R = 1, S48 = 1	19,700
S = 4800, T = .8, R = .025	T8R = 1, S48 = 1	11,510
S = 5600, T = .5, R = 0	T5 = 1	15,700
S = 5600, T = .7, R = 0	T7 = 1	9,780
S = 5600, T = .8, R = .025	T8R = 1	16,230

Notes:

[a] In this table the NIT annual support level is designated by S, the initial tax rate by T, and the rate of decline of the tax rate per thousand dollars of income by R. The figures under "Tax Breakeven Level" are for a hypothetical family of four living in Seattle (where there is no state income tax) with only one earner and with no income other than labor earnings.

The data in Table 2 provide information on the employment and unemployment experience of these 1657 families. According to the employment data, the difference between the fraction of controls and the fraction of experimentals who work at all during a year rises from 2.6% in the preexperimental year to 6.1% in the second experimental year for husbands. Correspondingly, for wives, the simple control–experimental difference in the probability of working rises from 6.7% in the preexperimental year to 18.1% in the second experimental year. Therefore, a naive estimate of the effect of the NIT experiment on employment is 3.5% (i.e., 6.1% minus 2.6%) for husbands and 11.4% (i.e., 18.1% minus 6.7%) for wives.

As for the unemployment data, about 24% of all husbands experienced a spell of unemployment during the preexperimental year whereas this figure rises to 35% and 36% during each of the experimental years. This higher incidence of unemployment during the years of the experiment is also apparent from the experience of wives. Seattle's unemployment is considerably higher than Denver's, although the gap between the two is less than that suggested by the U.S. Department of Labor's data described in Section I. The latter are, of course, an average of monthly unemployment rates while the data in Table 2 record the experience of a whole year. In addition, the sample described by the data in Table 2 are drawn from the poorer section of each city's population.

The data in Table 3 reveal a large difference between Denver and Seattle in the probability of receiving unemployment insurance: in the preexperimental year, for instance, 2.4% of husbands in Denver received unemployment insurance whereas the corresponding figure for Seattle is 24%. The rules governing the payment of unemployment insurance in the states of Colorado and Washington are quite similar and are unlikely to be the cause of this difference between Denver and Seattle.[4] On the other hand, the administration of these rules reveals Colorado to be much more strict in the denial of benefits to potential claimants. Thus, consider the accompanying tabulation, which presents figures on the denial of benefits per one thousand claimant contacts taken from various issues of the Employment and Training Administration's *Unemployment Insurance Statistics*. These are striking differences and, indeed, compared with all states, Colorado is consistently among those with the highest benefit denial rates and Washington is consistently grouped with those with the lowest benefit denial rates. So, in addition to the difference in the unemployment experience of Denver and Seattle, this contrast between Colorado and Washington in the administration of the law regarding the payment of unemployment compensation cautions against treating as confirmed the hypothesis that the two sites are the same as far as any behavioral relationship involving unemployment is concerned.

Year	*Colorado*	*Washington*
1968	60.2	16.1
1969	75.6	12.4
1970	67.7	11.6
1971	68.5	12.1
1972	70.2	16.1
1973	87.3	23.1
1974	73.0	22.8
1975	55.4	22.1
1976	52.8	27.4

III. ESTIMATES OF THE HUSBAND'S AND WIFE'S DECISION TO WORK

A. Pooling Data from All Three Years

We now turn to an analysis of the working decisions of husbands and wives. First, the observations for all 1657 families for all three years are pooled. Before turning to the logit estimates, it may be instructive to consider some linear probability equations since the major thrust of the subsequent results is evident in these aggregative estimates. Table 4 reports the linear probability estimates of the following equation fitted to all 1657 families for all three years:

$$L = \Phi[b_0 + b_1F + b_2Y1 + b_3Y2 + b_4(F \cdot Y1) + b_5(F \cdot Y2) + b_6U + b_7(F \cdot Y1 \cdot U) + b_8(F \cdot Y2 \cdot U) + b_9Z], \quad (1)$$

where L = 1 if the husband (wife) worked at all during the year,
= otherwise;
F = 1 if the family is an experimental family,
= 0 for controls;
Y1 = 1 if the observation is drawn from the first experimental year,
= 0 otherwise;
Y2 = 1 if the observation is drawn from the second experimental year,
= 0 otherwise;
U = 1 if the wife (husband) experienced any unemployment during the year,
= 0 otherwise;

and where Z is a vector of variables determining assignment of families to various programs and Φ denotes a particular distribution function—

Table 2. Fraction of Husbands and of Wives Who Each Year Experience Some Unemployment and Who Each Year Are Employed[a]

HUSBANDS

	Unemployment					*Employment*		
	Controls and Experimentals		*Denver and Seattle*					
	Denver	*Seattle*	*Controls*	*Experimentals*	*All*	*Controls*	*Experimentals*	*All*
Preexperimental year	.231	.248	.263	.217	.238	.937	.911	.923
First Experimental year	.325	.418	.342	.383	.364	.945	.911	.927
Second Experimental year	.337	.372	.327	.374	.352	.945	.884	.912

WIVES

	Unemployment					*Employment*		
	Controls and Experimentals		*Denver and Seattle*					
	Denver	*Seattle*	*Controls*	*Experimentals*	*All*	*Controls*	*Experimentals*	*All*
Preexperimental year	.128	.146	.145	.126	.135	.515	.448	.479
First Experimental year	.225	.310	.277	.247	.261	.561	.459	.507
Second Experimental year	.225	.257	.256	.258	.257	.615	.434	.519

Notes:

[a] The total sample of 1,657 families consists of 777 controls and 880 experimentals. The number of families in Denver is 964 and the number in Seattle is 693.

Table 3. Fraction of Husbands and Wives Receiving at any Time during a Year Unemployment Insurance

	Husbands			*Wives*		
	Preexperimental Year	*First Experimental Year*	*Second Experimental Year*	*Preexperimental Year*	*First Experimental Year*	*Second Experimental Year*
Seattle:						
fraction of controls and experimentals	.240	.193	.115	.029	.075	.062
fraction of controls	.263	.222	.125	.029	.088	.079
fraction of experimentals	.217	.166	.106	.029	.063	.046
Denver:						
fraction of controls and experimentals	.024	.028	.045	.010	.008	.010
fraction of controls	.023	.035	.051	.007	.009	.009
fraction of experimentals	.025	.023	.040	.013	.008	.011
Seattle and Denver combined:						
fraction of controls and experimentals	.114	.097	.074	.018	.036	.032
fraction of controls	.129	.117	.084	.017	.044	.040
fraction of experimentals	.101	.080	.066	.019	.030	.025