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THE IMPACT OF INCOME MAINTENANCE AND MANPOWER SUBSIDIES ON THE DECISION TO INVEST IN HUMAN CAPITAL: INTERIM RESULTS

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SEATTLE INCOME MAINTENANCE EXPERIMENT • DENVER INCOME MAINTENANCE EXPERIMENT

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SUMMARY OF FINDINGS

This study is an econometric evaluation of the impact of income maintenance, manpower counseling, and training subsidy options on the decision to invest in formal schooling. The treatments form the basis for the Seattle and Denver Income Maintenance Experiments (SIME/DIME), which involve a stratified random assignment of families to a series of financial and manpower treatments. In this study, the major groups analyzed are male heads (husbands), female heads (wives) of two-parent households and the female heads of single-parent households. The sample sizes are 1,825, 1,833, and 1,465 for husbands, wives, and single female heads, respectively.

The only prior evaluations of the impact of income maintenance on school or training attendance were done by Mallar (1973) for the New Jersey Income Maintenance Experiment and by McDonald and Stephenson (1976) for the Gary Income Maintenance Experiment. The present study is unique because it evaluates the schooling decisions of heads of households, rather than of teenagers as in the two previous studies. Additionally, SIME/DIME contained a manpower treatment whereby randomly selected individuals could receive one of three treatments: free job and career planning counseling, counseling plus 50% of all direct training costs (including tuition, books, transportation, and child care), or counseling plus 100% of all direct training costs. Finally, the experimental design includes a statistically matched control group (no treatments) to permit a more precise measurement of the impact.

Design of the Study

The basic purpose of this study is to evaluate changes in school attendance of individuals who received various financial and manpower treatments. We view such school attendance as investment in human capital. To test hypotheses about changes in human capital investments, a two-period model of experimental and postexperimental school and labor

supply behavior was developed. From this theoretical model we derived equations that related the demand for human capital investments to a series of control and treatment variables. For analysis, the measure of investment in human capital was the number of experimental quarters during which the individual was in a formal schooling program.

The independent variables used in the analysis were divided into a control set, a treatment set, and control-treatment interactions. Control variables such as age, race, and preexperimental education level are included because they are not necessarily randomly distributed by treatment level. In addition, they are expected to influence human capital formation but do not represent experimental treatments. All control variables were measured for the preexperimental period. To determine treatment effects, we used dummy variables for the manpower treatments and parameterized financial treatment variables that captured income and substitution effects. We interacted the manpower and the financial treatment variables with a dummy variable representing those heads who were under 26 years old at the start of the experiment and with a dummy variable representing those who were attending school during the preexperimental quarters. These interactions helped to identify differential responses to the treatments among those subgroups.

The observed variable, the number of quarters of school attendance, suffers from a double truncation. On the one hand, we do not observe the total school attendance period for those individuals who attended school for more than 8 quarters. In addition, the amount of schooling is bounded below by zero. To adjust for these truncations, we specified a tobit distribution for the quantity of schooling equation and estimated the parameters of the model using a maximum likelihood procedure. The impact of the treatments is defined as differences in the number of quarters of school taken between the controls and those with various treatments.

Empirical Findings

Average Response to Treatments

In Table S1, we show the predicted average quantities of schooling taken by the controls and by those who had various manpower and financial

Table S1

PREDICTED QUARTERS OF SCHOOLING TAKEN DURING THE FIRST EIGHT EXPERIMENTAL QUARTERS

		Manpower Treatment			Financial Treatment		
	Controls	Counseling Only	Counseling and 50% Subsidy	Counseling and 100% Subsidy	$3,800 Support, .50 Tax Rate	$4,800 Support, .50 Tax Rate	$5,600 Support, .50 Tax Rate
Husbands	.381	.521	.662***	.872***	.308**	.321	.316
Wives	.272	.254	.551***	1.213***	.223*	.233	.230
Single female heads	.401	.589	.977***	1.337***	.420	.420	.414

*Difference from the controls is significant at the 10% level.
**Difference from the controls is significant at the 5% level.
***Difference from the controls is significant at the 1% level.

treatments. These predictions are made for each of the three groups--husbands, wives, and female heads. For all groups, the amount of school taken by the controls during the first two years of the experimental period is very small. We found that the counseling-only option was significant only for the female heads. The pure price subsidies of 50% or 100% of the training costs led to significant increases in the quantity of schooling taken by all groups. When the impacts are averaged across all husbands, wives, or single heads, it was found that the greatest response to the 100% subsidy was made by wives. For wives the average quantity of schooling taken went from .272 quarters to 1.213 quarters--an increase of almost 250%--when the full subsidy was available. For female heads the full subsidy increased the quantity of school from .401 quarters to 1.337 quarters, an increase of 133%. Husbands increased their schooling by 128% (from .381 to .872 quarters) when given the full subsidy. These results show that a pure price subsidy--one that covers all or part of the direct costs of a formal school program--can have substantial short-run effects.

The financial treatments led to significant reductions in the quantity of school taken by husbands and wives whose treatment provided a support level of $3,800 (for a family of four) and a marginal tax rate of .50. Those programs that provided a more lucrative support level (while holding the marginal tax rate constant) had an insignificant impact on the average response by group.

The Response Among Subgroups

The impact of the manpower treatments varied according to the age of the individual and whether the individual was in school during the preexperimental period.* Those individuals who were in school previously attended school for more quarters, on average, than did those who were not in school. For example, we found that husbands aged 26 to 45 years

*Enrollment in the experiment began in September 1970 in Seattle and September 1971 in Denver. It took approximately 14 months in each site to enroll the entire sample. For each individual in the sample, the preexperimental period consists of the 4 quarters before the enrollment date.

who had been in school previously took an average of 5.144 quarters of school when they received the counseling plus 100% training subsidy option. Those husbands in the same age group who had not been in school during the preexperimental period averaged only .683 quarters of school. The manpower options that provided training subsidies significantly increased the school attendance of all subgroups of husbands older than 25 years.

For wives and single female heads, the greatest impact of the manpower treatments occurred among those who had not been in school during the preexperimental period. The counseling plus 100% subsidy option caused schooling attendance for female heads aged 16 to 25 to increase from .449 quarters to 1.480 quarters. Increases of similar magnitude were observed for all subgroups among females who received this option. The findings suggest that the treatments which provided subsidy of training costs significantly induced wives and female heads to return to school but had little effect on those who were in school during the preexperimental period.

The financial treatments led to a reduction in the quantity of school attendance among every subgroup in which the impact was significant. In the subgroups, the responses to the financial treatments were concentrated more among the husbands and single female heads than among the wives. Those husbands who were in school during the preexperimental period were more likely to reduce their school attendance than husbands who were not in school during the preexperimental period. When the marginal tax rate was held constant, the reduction in school attendance among husbands increased with the size of the subsidy. Few of the subgroups among wives and female heads had significant reactions to the financial treatments. When the responses were significant, they were uniformly negative.

The results of the analysis strongly suggest that training subsidies lead to increased demand for schooling. The subsidies lower the price of schooling relative to the price of other goods, and the demand for schooling increases. The financial subsidies, on the other hand, increase the family income and reduce the returns to schooling. The result is a decrease in the demand for schooling. The analysis also suggests that individuals respond more to the training subsidies than they do to the financial support levels. These responses vary across subgroups in the sample.

I INTRODUCTION AND SCOPE OF THE STUDY

In this study we seek to examine the impacts of an income maintenance subsidy and an option to receive a manpower treatment on the decision to invest in human capital. The human capital investment we consider is investment in a formal schooling program, defined to be any program in an accredited college or university leading to an A.A., B.A., or higher degree.

The decision to invest in human capital is just one of many choices facing the individual, and an analysis of the determinants of these investments is one of the more important aspects of the impact of income maintenance. This study complements the study of the impact of income maintenance on labor supply since it provides additional information about the alternative choices. For example, an analysis of the impact of income maintenance on labor supply may reveal that individuals reduce their work effort as a result of the experimental treatment. One explanation for such a reduction would be that individuals make greater human capital investments so that future work effort may be more beneficial to the individual. An alternative hypothesis is that income maintenance increases the demand for leisure and reduces the demand for schooling as well as the supply of labor. This study helps test the hypothesis that income maintenance decreases the demand for schooling while the manpower treatments increase the demand for schooling.

This study also has implications for policies designed to increase the level of human capital investments made by the groups in the experiment. In later reports we shall examine the returns to the human capital investments, and we shall seek to integrate the schooling decision with the labor supply decision.

A Brief Survey of the Literature

Most of the empirical literature on the investment in human capital focuses on the costs and returns of such an investment. There is very meager evidence on the central question for our study, namely, the sensitivity of investment decisions to changes in monetary costs or returns. Nevertheless, it should be of use to summarize some of the earlier evidence on returns from schooling.

Because the investment in schooling* is easily observable, it has been subject to many empirical studies. Typically, the cost and returns are estimated from groups with different levels of schooling. G. Hanoch (1967), for instance, estimates the rate of return for high school to be about 16% and for college to be 7%. The figures indicate that schooling is probably at least as profitable as most alternative lines of investment in physical capital. It should be pointed out that such estimates are marred by severe conceptual difficulties [see Rosen (1975)]. The first difficulty is that the reason the two compared groups chose to invest differently is left unexplained. Thus, the returns from schooling may, in fact, capture returns from some other characteristic by which the groups differ, such as ability. Another major difficulty is that the returns from schooling accrue in the future and are uncertain. Estimates such as Hanoch's are based on an arbitrary expectations hypothesis. Typically, it is assumed that the economy is perfectly static and that future earnings can be derived from current cross-section comparisons.

The mere fact that schooling increases earnings need not imply that increases in the profitability of schooling, due either to changing demand conditions or to an income maintenance program, will increase the investment in schooling. Freeman (1975), in a number of studies, concludes that there is a high degree of responsiveness to changes in market conditions.

Two studies more related to our present attempt are by Mallar (1973) and by McDonald and Stephenson (1976). Both estimated the effects of

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*Measured by the number of years in a formal schooling program.

income maintenance programs on the schooling choices of young adults (16 to 19 years old). Mallar, using data from the New Jersey Income Maintenance Experiment, reports positive experimental effects in only some special cases, e.g., 18 years old at the fifth quarter, but was unable to identify significant effects on school enrollment at other ages or quarters. McDonald and Stephenson, using data from the Gary Income Maintenance Experiment, found positive treatment effects for males but no effects for females.

The present study has some distinct advantages over prior empirical studies--both those that use nonexperimental data and those that use experimental data. Compared with nonexperimental work, the existence of a well-defined control group allows us to separate better the effects of changes in the costs of schooling or training from other exogenous factors that may vary across individuals or over time. Compared with previous experimental work, the wider range of treatments allows us to obtain better estimates of the effects of changes in the costs of schooling. In particular, in addition to the financial treatment, this study evaluates the effects of a manpower option that permits subsidization of all schooling costs.

Plan of the Report

The plan of the report is as follows. In Chapter II, we discuss the design of the study, with special attention to the manpower option and how it operates in the two experimental sites. In Chapter III, we present a theoretical model of the relationship between school and work and from this model derive hypotheses about the demand for schooling. In Chapter IV, we specify the empirical model and the empirical methods used in the evaluation. Chapter V presents the results of the estimation of the demand equations.

II DESIGN OF THE FINANCIAL AND MANPOWER COMPONENTS OF THE EXPERIMENT

Because an understanding of the experimental structure is essential to an understanding of the analysis that follows, we present a brief description of the experimental design. A more detailed description is available in Kurz and Spiegelman (1972).

Assignment to the Experiment

A major consideration in a controlled experiment is the nature of the sample population to which various combinations of the treatment are given. In addition, there must be a statistically matched group to act as the control sample. The experimental design, therefore, involved the assignment of various treatments to well-defined subgroups, or cells, in the sample. The needs of the experiment dictated that the sample selection be random not across the entire population of interest but only within these cells. The cells were created by the complete interaction of certain variables, as follows:

Variable	Cells	Definition
Race	2	Black, white*
Site	2	Seattle, Denver
Family status	2	Households with single head, households with two heads
Normal income†	6	

The complete interaction of these exogenous variables produced 48 possible cells. The cells were the basic units within which each individual was randomly assigned to one of the treatments. For a variety of reasons some cells remained empty.‡

*In Denver, Chicanos were a third racial group, but they are not part of the analysis.

†Normal income levels are described in Table 2 (Chapter 4).

‡See Conlisk and Kurz (1972) for the mathematical assignment model actually used.

The experimental treatments consisted of 11 possible financial subsidy and tax treatment levels, three manpower options, and two durations (either 3 or 5 assigned years on the program). In addition, there were the financial controls and the manpower controls. The complete interaction of the experimental treatments and controls produces 96 variations in treatments; but only 84 treatment configurations were used, since the highest manpower option, M3, was not available to those with an assigned experimental period of 5 years.

Participation in the experiment was limited to families who were likely to be eligible for a national program if one were initiated. The following were the eligibility requirements for experimental families:

(1) The family unit had to contain a bona fide family of at least two members consisting either of a husband and wife or of an adult and a dependent child.

(2) The male head of a two-parent family or the head of a one-parent family had to be at least 18 years old and not more than 58 years old.

(3) The 1970 earnings of the family had to be less than \$9,000 for a family of four with one working head and less than \$11,000 for a family of four with two working heads. The maximum permissible income for families with other than four members was obtained through an adjustment using standard living cost differentials related to family size.

(4) The family heads could not be permanently disabled.

The decision was made to allocate families to financial treatments in such a way that 75% of the total predicted payment costs would go to those on the 3-year program, the remaining 25% would go to those on the 5-year program, and the total predicted payments would be proportional to the size of each of the three racial groups. Payment costs were predicted on the basis of family type, race, normal earnings of the family, and the generosity of the financial plan.

The Financial Treatments

The financial treatments consisted of a support level, sometimes called the "Guarantee," and a tax function. The support level is the annual amount guaranteed to the family if it has no income at all. The tax function tells how the payment to the family declines in response to other earnings received by the family. If G is the amount of the grant, S the level of support, Y the amount of taxable income received by the family, t(Y) the function that describes the tax rate on income Y, t_0 the initial tax rate on the first dollar of income, and r the rate of decline of the tax rate with increases in income, then the equations describing the financial treatment are:

$$G = S - y(Y) \cdot Y$$

$$t(Y) = t_0 - rY \quad .$$

This is a generalized system that allows for the use of the two types of tax system included in the experiment. There were three basic support levels--$3,800 per year, $4,800 per year, and $5,600 per year--for a family of four.* These levels were adjusted to account for family size and status in the household. There were four different tax systems: two constant tax systems at 50% and 70%, two declining-rate systems with initial taxes at 70% and 80% and both declining at an average rate of 2.5% per $1,000 of income (equivalent to a marginal rate of 5% per $1,000 of income). Table 1 shows the various combinations of support levels and tax systems used in the experiment. Further details about the operation of the negative income tax may be found in Keeley et al. (1976).

The Manpower Treatments

Operation of the Program

The manpower treatments consisted of options to receive counseling and a training subsidy. The treatment was graduated, and the full range of options is summarized as:

M0	No counseling or training
M1	Counseling only
M2	Counseling + 50% of training costs
M3	Counseling + 100% of training costs

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*The support levels are defined for constant 1971 dollars. The actual support levels were adjusted annually to reflect increases due to inflation.

Table 1

ALTERNATIVE FINANCIAL PROGRAMS

Treatment Designation	Support Level (Family of 4 Persons)	Initial Tax Rate	Average Rate of Tax Decline per $1
F0	Control		
F1	$3,800	.50	0
F2	3,800	.70	0
F3	3,800	.70	.000025
F4	3,800	.80	.000025
F5	4,800	.50	0
F6	4,800	.70	0
F7	4,800	.70	.000025
F8	4,800	.80	.000025
F9	5,600	.50	0
F10	5,600	.70	0
F11	5,600	.80	.000025

Source: Kurz and Spiegelman (1972), Table 2

Training costs were defined as the direct out-of-pocket costs of the training or schooling program and included tuition and fees, books and supplies, transportation, and child care costs.

If a family was assigned a manpower treatment of M1, M2, or M3, this entitled each member of that family 16 years or over to the same treatment. The treatment was not diminished for one family member if another family member also used it. Those who received a positive manpower treatment were entitled to receive counseling services from a group of trained counselors employed by the Seattle Community College for the SIME participants and by the Community College of Denver for the DIME participants. To as large an extent as possible, the counselors for the two sites were chosen using the same basic criteria with regard to education, background, and experience. Most had at least a bachelor's degree, and a few had a counseling-specific advanced degree. For the most part, however, the counselors were not trained career employment counselors. There was a mixture of male and female counselors; they were black and white in Seattle and black, white, and Chicano in Denver. Although the racial composition of the counselors matched the racial composition of the participating sample, there was no explicit attempt to match participants and counselors by race or sex. The counselors from the two sites were encouraged to exchange ideas freely with each other; the result was a minimizing of any counselor-induced differences between the two sites. It was hoped that the counseling input from the two sites was largely the same, and that "counseling" could be regarded as a single good without reference to site.

Those families with the manpower option were contacted at the earliest possible time following their enrollment in the experiment. At that time the manpower treatment available to them was explained fully, and information concerning the operation of the counseling center was provided. It was made clear that the services were available for the entire duration of the experiment* and that refusal to participate

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*The duration of the manpower option was 3 or 5 years to the month in which the family was contacted by the counseling center. In some instances the financial treatment was concluded before the manpower immediately did not affect the financial treatment (if one was being received) nor the opportunity to participate in the manpower program later in the experimental period. One year after the initial contact, the family was recontacted and the options available to them were again made clear. Extensive efforts were made to ensure that every eligible member (those 16 years or older at the time of enrollment and those who became 16 during the experiment, and without physical or mental impediments to labor market participation) of every family with the manpower option was fully aware of the nature of the program and the options available to the family.

Counseling and Training Program

The counseling sought to be nondirective--that is, the counselors did not seek to direct the participants into schooling, job search, or any other labor-market-related activity. Instead, the counseling sought to let the participant decide such matters as the type of labor market activity the individual wished to undertake, the nature of the training or schooling program, and the institution in which the training was to be received. The only major constraint on individual choice was that the schooling or training had to be related to some definable job-oriented goal.* Payments for training expenses were made sometimes by vouchers to the school or training institution and sometimes by reimbursement or direct payments to the individual.

An important consequence of the assignment to the experiments and the structure of the experiments is that families varying in type, race, and normal earnings were not randomly assigned to different financial

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treatment came to an end since the financial grant and the manpower subsidy were initiated by separate groups. For the most part, the two treatments were no more than one calendar month apart in starting and ending.

*Early in the program this requirement was largely relaxed, so that schooling or training with large consumption components was permitted.

or manpower plans. For this reason, the impacts of the income maintenance experiments cannot be accurately assessed through direct comparison of control and treatment families, but must be analyzed through multivariate techniques that take into account the stratification of the sample. In the next chapter we develop a model that becomes the basis for empirical evaluation of the demand for schooling.

III THEORETICAL CONSIDERATIONS

Assumptions of the Model

Let us consider two types of treatment:

- A negative income tax program.
- A manpower program of training subsidies.

We wish to determine the effects of these treatments on school/work decisions.

We assume that the total remaining life of the individual, T, is measured from the point of entry to the program and is divided into two exogenously fixed intervals: an experimental period, E, and a post-experimental period, N. Thus, $T = E + N$. These periods may vary across individuals, subject to the total time constraint. To simplify, we shall examine a two-period model as in Metcalf (1973). There are several ways to justify such an aggregation. One may impose restrictions on preferences, on price variations, or on feasible choices. We adopt the simplifying assumption that within each period the flow of wages, consumption, work, leisure, and schooling are fixed; the only variation is between periods. When we discuss a permanent program, rather than an unexpected experiment of limited duration, the two periods must be reinterpreted. In such context E should be viewed as the investment period, i.e., the segment of life with positive investment in human capital, and N should be interpreted as the noninvestment period, i.e., the segment with zero investment in human capital.

For a consistent analysis, one further assumption is necessary: schooling affects earning capacity with a lag. That is, only wages in the postexperimental period are affected by the additional schooling or training obtained during the experimental period. We assume that future earnings are determined by the relation:

$$w_1 = f(w_0, Es_0) \quad , \tag{1}$$

for which

$$\frac{\partial f}{\partial Es_0} > 0, \qquad \frac{\partial f}{\partial w_0} > 0, \qquad \frac{\partial^2 f}{\partial (Es_0)^2} < 0 \qquad ,$$

where $w_i (i = 0, 1)$ is the wage rate in each of the two periods and Es_0 is the total amount of time spent in school or training during the experiment. The appearance of w_0 in equation (1) reflects past investments as well as individual differences in ability.

The maximization problem of each individual may be written as

$$\underset{c_i, h_i, \ell_i, s_i}{\text{maximize:}} \qquad u(c_0, \ell_0, c_1, \ell_1) \qquad , \tag{2}$$

subject to the budget constraints:

$$R_0 c_0 + R_1 c_1 = R_0 w_0 h_0 + R_1 w_1 h_1 + A \qquad , \tag{3a}$$

the time constraint:

$$h_0 + s_0 + \ell_0 = 1 \tag{3b}$$

$$h_1 + \ell_1 = 1 \qquad , \tag{3c}$$

and the nonnegativity constraints:

$$c_i, h_i, \ell_i, s_i \geq 0, \qquad \text{for } i = 0, 1 \qquad . \tag{3d}$$

In these equations,

A = initial wealth at the beginning of the experimental period;

c_i = the rate of consumption of goods per unit of time in the experimental and postexperimental periods (i = 0 and 1, respectively);

ℓ_i = the rate of consumption of leisure per unit of time in each period;

h_i = the rate of work per unit of time in each period;

s_0 = the rate of schooling per unit of time in the experimental period (in the present two-period context, there is no loss of generality if we assume $s_1 = 0$);

and

$$R_0 = \int_0^E e^{-rt} dt \quad \text{and} \quad R_1 = \int_E^{E+N} e^{-rt} dt$$

are the appropriate discount factors for the two periods. Prices of consumer goods are assumed to be the same in the two periods and have been normalized to zero.

In specifying the utility function, we assume that school and work are perfect substitutes and that they are equally unpleasant relative to leisure. We also assume that school has no direct consumption or home production value; its only effect is on future market opportunities. Finally, note that the particular form of the utility function implicitly depends on the exogenous variables E and N.

Equilibrium Conditions of the Model

It is convenient to reduce the optimization problem into a standard consumer demand problem and write it as:

$$\text{maximize:} \quad u(c_0, \ell_0, c_1, \ell_1) \quad , \tag{4}$$

subject to:

$$R_0c_0 + R_1c_1 + R_0w_0\ell_0 + R_1w_1\ell_1 = A + R_0w_0(1 - s_0) + R_1w_1$$

$$= A + H(s_0) \quad ,$$

where $H(s_0)$ denotes lifetime potential earnings, i.e., human capital. There are also the added inequality constraints, $s_0 \leq 1$, $\ell_1 \geqq 0$, and $\ell_0 + s_0 \leqq 1$. If the inequality constraints are ineffective, the problem may be solved by defining, for every s_0, regular demand functions for ℓ_0, ℓ_1, c_0, and c_1. The demand functions may be written as:

$$\ell_0 = \ell_0[w_0, w_1(s_0), A + H(s_0); R_0, R_1] \tag{5a}$$

$$\ell_1 = \ell_1[w_0, w_1(s_0), A + H(s_0); R_0, R_1] \tag{5b}$$

$$c_0 = c_0[w_0, w_1(s_0), A + H(s_0); R_0, R_1] \tag{5c}$$

$$c_1 = c_1[w_0, w_1(s_0), A + H(s_0); R_0, R_1] \quad . \tag{5d}$$

We can then write utility as an indirect function of s and maximize it with respect to s_0. The basic point is that s_0 does not appear in the demand functions, except in its effect on prices and income. This reflects the fact that s_0 does not appear in the utility function, and that h_0 serves as a slack variable (changes in s_0 that affect wages and wealth in a compensating fashion will have no effect on leisure). Furthermore, as long as h_0 is positive, a necessary condition for the choice s_0 is simply that the marginal rate of return for investment, conditioned on the choice of ℓ_1, is equated to the interest rate. In this case, the choice of s_0 can be determined from a system of two equations with only s_0 and ℓ_1 as variables. This is the system we shall use for the analysis of the comparative statics.

The conditions for the choice of s_0 and ℓ_1 are (1) Condition 1--the demand for future leisure condition:

$$\ell_1 = \ell_1[w_0, w_1(s_0), A + H(s_0); R_0, R_1] \quad ,$$

and (2) Condition 2--the rate of return condition:

$$(1 - \ell_1) R_1 \frac{\partial w_1}{\partial s_0} = R_0 w_0 \quad .$$

Condition 2 reveals a rather surprising aspect of the model: the demand for schooling depends only on future work! It is independent of the current choice of leisure or of consumption decisions. The reason is that as long as $h_0 > 0$, an individual can increase his schooling while holding current leisure constant. Condition 2 states that at the optimum such reallocation of current time (holding future leisure constant) cannot increase wealth and thus consumption. Note, that if the constraint $h_0 = 0$ is effective, then w_0 in Condition 2 must be replaced by the shadow price of time, λu_{ℓ_0}, and more equations are needed to determine the solution. The case $h_0 > 0$ can be presented graphically (Figure 1).

The rate of return curve reflects Condition 2. It describes the demand for s_0 as a function of ℓ_1 and must have a negative slope. The slope of the demand for leisure curve (Condition 1) depends on the relative strength of the price and income elasticities and on the effect

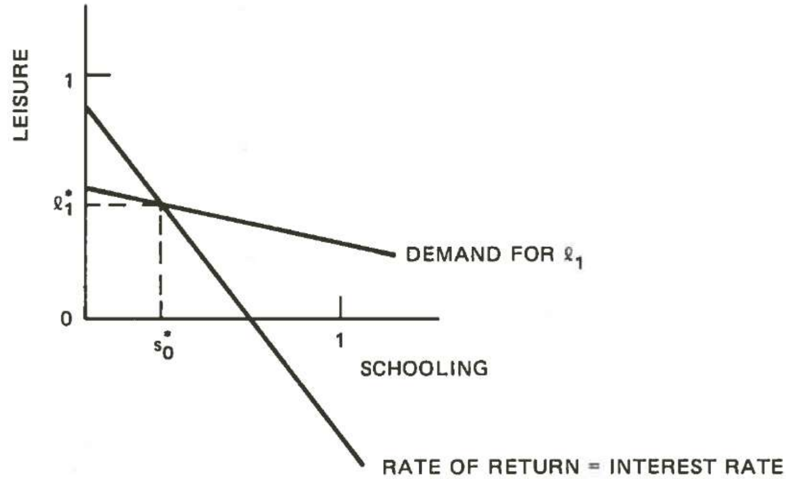

FIGURE 1 JOINT DETERMINATION OF SCHOOLING AND WORK

of schooling on potential earnings. At low levels of schooling, $\partial H/\partial s_0 > 0$, and any additional schooling leads to increases in wealth and in the price of leisure. The demand for leisure may increase if the income effect is dominant. At high levels of schooling, the additional schooling reduces wealth and increases the price of leisure. At this range we expect the demand for leisure to be negatively sloped. In any case, second-order conditions guarantee that the rate of return curve is relatively steeper. In this framework we may analyze the implications of the various treatments.

Implications of the Model

We begin with an analysis of permanent negative income tax and manpower programs. We shall then turn to examine the expected effects of an experiment of short duration.

The negative income tax program is characterized by three basic parameters: (1) a support level, (2) a tax rate, and (3) a breakeven income above which the net support is zero. Whether a person is above or below the breakeven level may depend on the investment in human capital. We shall therefore distinguish two basic cases:

(a) The recipient is permanently within the program.

(b) The recipient receives support during the schooling period, but on finishing school his earnings exceed the breakeven level, so that he is ineligible for support.

A recipient who is permanently within the program faces a lower wage rate (relative to the controls) but has an increase in income as a result of receiving support. As in the usual single-period analysis, these two effects combine to increase his demand for leisure, in both the present and the future period; the demand for leisure curve in Figure 1 shifts to the right. In the absence of additional shifts, this reduction in future work would induce a reduction in the investment in schooling. However, the rate of return (or the demand for schooling) curve may also shift.

Examining Condition 2, we see that a proportionate tax on earnings throughout life is not expected to affect the rate of return curve since the effects on current opportunity costs and on future earnings cancel. In the case where the tax extends over only a part of the postschooling period, the effect is usually to shift the rate of return curve to the right. An exception can arise, however, if there is a significant component of direct costs for schooling (as distinct from the opportunity costs due to the loss of earnings). In this case it is quite possible that a tax extending over a long period will reduce the investment. An additional consideration is that the program taxes all sources of income, including property income. The effect of a general increase in income tax is to reduce the after-tax rate of interest, and thereby to increase the demand for schooling. The net effect on the rate of return curve is ambiguous; therefore, the total effect of the financial support program on schooling cannot be determined a priori.

Somewhat sharper results can be derived for a recipient who is supported only during his schooling period. In this case, the demand for schooling is most likely to increase and the demand for future leisure is less likely to increase relative to a recipient who is also supported during his postschooling period. If the higher tax rate is applicable only during the schooling period, then opportunity costs of schooling diminish and the rate of return curve in Figure 1 shifts to the right. Furthermore, with respect to the demand for future leisure, there are now opposing income and substitution effects. The recipient is likely to shift work from the present to the future, where, with a lower tax rate, wages are higher. The net effect on the demand for leisure is still ambiguous; therefore, we cannot yet determine the net outcome on the investment in schooling. Nevertheless, we have the following important conclusion: The increase in the investment in schooling for a "transitory" recipient will exceed that of a "permanent" recipient.

Whereas the financial treatment reduces the opportunity cost, the manpower treatment reduces the direct costs of schooling. In most cases, this is a small part of the total costs, but it may be of importance for a low-wage population with limited access to the capital market. In contrast to the financial treatment, income effects are less likely to be important since no guarantee level is involved in this treatment. Furthermore, since the subsidy is limited to the schooling period, there is no sharp distinction in this case between transitory and permanent participation, and likewise between program and experiment effects.*

To analyze the effects of manpower treatment, we can refer again to Figure 1. A schooling subsidy unambiguously shifts the rate of return curve to the right and, ceteris paribus, induces more schooling. For individuals who would go to school in the absence of the subsidy, there may be an income effect that increases the demand for leisure and thus mitigates the normal increased demand for schooling.

We may differentiate the expected impact on various subgroups in the sample. In particular, we want to examine the manner in which the financial grant and training subsidy effects are expected to vary with age, preexperimental wage rates, sex, and preexperimental school or work status. Other subgroups are important to this analysis, but we select these groups for illustrative purposes.

Age and Preexperimental School Status--In a permanent program we would expect to observe a larger reaction to the treatment among the young, because the investment in human capital is concentrated at the

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*There still may be, however, a tendency to "flock" into a transitory experiment to take advantage of the fleeting opportunity.

early phase of the life cycle; older individuals, whether subsidized or not, are less likely to invest in human capital. If, on the other hand, we discuss an unexpected program that captures recipients at various points of their schooling investments, then comparison of the effectiveness of the treatment across age groups involves a comparison across groups with different levels of investment. The results depend critically on the relative speed at which the marginal productivity of schooling decreases with the investment in schooling. The outcome cannot be predicted a priori.

Preexperimental Wage--In the context of a permanent program, differences in initial wages may be viewed as related to differences in ability. The higher the initial wage, the more likely it is that the recipient will be only a transitory participant in the negative income tax program; therefore, we expect him to increase the investment in schooling more than a person with a lower wage. On the other hand, if we consider an experiment which is transitory for the whole population, the effect of the initial wage on the sensitivity to treatment is ambiguous. A person with higher preexperimental wage is likely to have higher opportunity costs, and he is likely to receive greater benefits from time spent at school because of the productivity of human capital. Depending on which factor--higher opportunity costs or greater benefits--is stronger, he may invest more or less in human capital. Again, because the sensitivity to changes in treatment will depend on the effects that differences in schooling have on the rate of decrease in the marginal productivity of schooling, a priori predictions are hard to make.

Sex and Preexperimental Work Status--Since our model assumes that schooling is taken to provide more attractive labor services in the job marketplace, we conclude that females are less likely to invest in human capital than males. Whether they are more or less sensitive to the various manpower treatments, however, depends on the elasticity of the marginal productivity of investment in schooling. When an experiment is enacted unexpectedly, the effect on males is predicted to be quite different from that on females. For male heads, it is likely that $h_0 > 0$, that is, that male heads will participate in the labor market. For many females, especially in two-head households, it may be true that $h_0 = 0$, i.e., potential wage in the market is below the value of time at home, $\lambda u_{\ell 0}$. In this case, the only effect of an income maintenance program on the investment in schooling is through the income effect, which will be to reduce the investment. The same consideration applies more generally to anyone who is out of the labor force before the experiment. It should be pointed out that this distinction is relevant only with respect to the financial treatment.

Preexperimental School Status--If there are costs of search or other types of start-up costs associated with the investment in schooling, then for an unexpected experiment, the schooling status before the experiment should be important. Generally speaking, those already in school are more likely to continue as a result of the experiment.

We have enumerated several forces that operate to increase or decrease the probability of making schooling investments. As we have seen, the relative strengths of the opposing forces depend on the duration of the income maintenance program.

IV SPECIFICATION OF THE EMPIRICAL MODEL

The theoretical model presented earlier provides a framework within which we can derive testable hypotheses and explain the results of the hypothesis tests. However, the empirical analysis requires the relaxation of some assumptions of the model. In this chapter, we discuss the specification of the empirical model and the estimation technique that is used.

Description of the Variables

Dependent Variable

To measure the demand for schooling, we used the number of quarters that the individual was in a diploma- or degree-oriented program during the first eight quarters of the experiment. This definition included part-time as well as full-time students. Typically, for those who wanted degrees, schooling took place at the Seattle or Denver Community College, at the University of Washington or Colorado, or at a similar college-level institution. Those who were enrolled in high school equivalency courses typically attended one of the community colleges or a local adult high school.

Independent Variables

The independent variables may be divided into three groups: (1) the basic control set, (2) the main treatment effects, and (3) the treatment effects interacted with a subset of the control set. The variables were defined for the preexperimental period to remove the possibility of simultaneous equations bias. Such a bias could arise if variables used as explanatory variables were simultaneously determined with the dependent variable. All variables were measured during the three quarters before the start of the experiment. Choosing the variables this way means that the same calendar quarter is the benchmark against which we measure the relative impact of the treatments. This approach is useful since school attendance is seasonal and this procedure minimizes seasonal factors.

The Control Set--The variables that constitute the control set increase the efficiency of the estimates and control for the stratification in the selection of the sample and for nonexperimental influences on schooling demand. Because the assignment to treatments was depenent on E-levels (or "normal income" levels), the E-level dummy variables are used to control for the assignment probabilities. In addition to the E-level variables, we used such demographic variables as age, race, years of schooling, preexperimental schooling status, and the gross wage rate. These and other variables were included to control for any nonrandom variations in the distribution of the assignment to the various treatments.

The Main Treatment Effects--To capture the basic components of the financial treatments (support level, tax function, and breakeven level), we used four separate variables: (1) DNWG, the change in net wage rates of the individual evaluated at the preexperimental wage rate*; (2) PASZ, the payment entitlement of the individual if he had worked at his preexperimental hours of work at the preexperimental wage rate (if preexperimental hours of work were zero, a dummy variable would be the basic support level for that family); (3) BRKEN, a dummy variable that took the value 1 if the individual was above the breakeven level; and (4) EVER, a nonlinear variable that designated the extent to which the individual was above the breakeven level.

A series of dummy variables was used to define the various manpower options available to the individual: M0, no manpower treatment; M1, counseling only; M2, counseling plus 50% training subsidy; and M3, counseling plus 100% training subsidy. In addition, a dummy variable

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*If the preexperimental wage rate is w_0, the preexperimental marginal tax rate is t_p, and the tax rate imposed by the experiment is t_e, then the net wage change variable is defined as $-w_0(t_p - t_e)$.

was used that took the value 1 if the individual had the M1, M2, or M3 treatment and was on the experiment for 3 years.

Control/Treatment Interaction Variables--A set of control/treatment interaction variables was used to test for differences in the impact among certain groups in the sample. In particular, we felt that age and preexperimental schooling status would cause individuals to respond differently to the treatments. Accordingly, a variable that took the value 1 if the individual was between the ages of 16 and 25 years and a variable that took the value 1 if the individual was in school in the preexperimental period were interacted with a set of treatment variables. The complete list of independent variables used, their definitions, means, and standard deviations are presented in Table 2.

Merger of the Sample

A major concern with regard to specification of the models was the way in which the groups were to be aggregated. The aggregation decision was made on the basis of OLS estimates of the probability model. The assignment of the treatments was based partially on site, family status, and race. These selection criteria resulted in 12 groupings in which the probability estimates could have been made. Since we desired to estimate two quarters for three separate equations, the complete interactions would have produced 72 equations. Such a large number of equations was theoretically undesirable; there was little reason to believe that complete interactions would produce significantly different results for the various groups. Since the estimation procedure was very expensive, the costs involved also dictated a reduction in the number of separate equations.

We grouped the data by family status: husbands who were the male heads of two-head households; wives who were the female heads of two-head households, and female heads of single-head households. However, we felt that the reactions of these groups were likely to vary sufficiently to make merger across the groups impractical. We also investigated the desirability of merging across the sites. When preliminary

Table 2

INDEPENDENT VARIABLES, MEANS, AND STANDARD DEVIATIONS USED IN ANALYSIS

		Mean Values (standard deviations)		
		Husbands	Wives	Female Heads
Control Variables				
CONS	Constant	--	--	--
INS	1 if in school during the preexperimental period	.122 (.327)	.068 (.251)	.102 (.303)
AGEL	1 if 16 ≤ age ≤ 25	.244 (.430)	.349 (.477)	.218 (.413)
AGEH	1 if 26 ≤ age ≤ 45	.573 (.495)	.528 (.499)	.611 (.488)
PREW	Wage rate in preexperimental period	3.112 (.824)	2.032 (.475)	2.199 (.638)
EMP2	1 if employed in preexperimental period	.759 (.428)	.302 (.459)	.507 (.500)
PRED	Preexperimental educational level	11.451 (2.688)	11.459 (2.696)	11.425 (2.460)
DENV	1 if family is in Denver	.453 (.498)	.452 (.498)	.468 (.499)
BLK	1 if black	.407 (.491)	.406 (.491)	.541 (.499)
E1	1 if normal income \$0-\$1,000	.022 (.148)	.022 (.148)	.160 (.367)
E2	1 if normal income \$1,001-\$3,000	.060 (.237)	.059 (.237)	.229 (.421)
E3	1 if normal income \$3,001-\$5,000	.159 (.366)	.159 (.366)	.247 (.431)
E4	1 if normal income \$5,001-\$7,000	.275 (.446)	.273 (.446)	.181 (.385)
E5	1 if normal income \$7,001-\$9,000	.283 (.451)	.284 (.451)	.122 (.327)
OHINS	1 if spouse was in school in preexperimental period	.066 (.249)	.121 (.326)	-- --
OHPRW	Wage rate of spouse in preexperimental period	2.031 (.471)	3.116 (.825)	-- --
OHEMP	1 if spouse employed in preexperimental period	.299 (.458)	.761 (.427)	-- --
OHED2	Preexperimental educational level of spouse	11.451 (2.697)	11.460 (2.692)	-- --

Table 2 (Concluded)

		Mean Values (standard deviations)		
		Husbands	Wives	Female Heads
Treatment variables				
MYRS	1 if manpower treatment and 3-year program	.404 (.491)	.404 (.491)	.441 (.497)
M1	1 if counseling only	.181 (.385)	.182 (.386)	.194 (.395)
M2	1 if counseling + 50% subsidy	.241 (.428)	.241 (.428)	.243 (.429)
M3	1 if counseling + 100% subsidy	.147 (.354)	.147 (.355)	.160 (.366)
PASZ	Annual NIT payment size/100	.570 (.963)	.574 (.962)	.724 (1.060)
DNWG	(PREW) $\cdot$ ($tax_{preexp.} - tax_{exp.}$)	.401 (.624)	.274 (.419)	.454 (.569)
EVER	$\begin{cases} 0 \text{ if BRKEN} = 0 \\ e^{-VERT} \text{ if BRKEN} = 1\text{, where} \\ \text{VERT is the amount of income} \\ \text{above the breakeven level} \end{cases}$	.037 (.140)	.037 (.140)	.045 (.163)
Interactions				
MLXAGE	1 if M1 = 1 and AGEL = 1	.046 (.210)	.061 (.239)	.044 (.206)
MHXAGE	1 if M2 or M3 = 1 and AGEL = 1	.092 (.289)	.139 (.346)	.089 (.285)
MLXINS	1 if M1 = 1 and INS = 1	.024 (.152)	.010 (.101)	.017 (.130)
MHXINS	1 if M2 or M3 = 1 and INS = 1	.044 (.206)	.025 (.156)	.042 (.201)
DNWGXAGE	DNWG if AGEL = 1	.091 (.322)	.101 (.284)	.114 (.337)
PASZXAGE	PASZ if AGEL = 1	.143 (.560)	.219 (.679)	.223 (.740)
DNWGXINS	DNWG if INS = 1	.040 (.238)	.020 (.145)	.051 (.249)
PASZXINS	PASZ if INS = 1	.058 (.373)	.041 (.330)	.079 (.452)
Sample size		1,825	1,833	1,465

testing of the site dummy variable interacted with the manpower and financial treatment variables showed no significant pattern, the decision was made to merge across sites (see Table 3).

We also considered the validity of merging the sample across races. The hypothesis that there was no effect of the treatment/race dummy interaction was tested. The F-statistics were generated under the assumption that the control portions of the equations varied only through shifts in the dummy for race. The results, presented in Table 3, indicate that we cannot reject the joint hypothesis of no significance of the treatment/race interactions. The sample was accordingly merged across races.

Estimation Technique

The use of quarters of schooling as a variable defined over the first eight quarters of the experiment poses data censoring problems of three types: (1) shortened observation spell over the life of the experiment, (2) truncation of the duration of time in school for some individuals in the sample, and (3) censoring at zero the amount of schooling investment that the individual can make. Thus, the observed choice must be between zero and eight quarters of school attendance, but we can imagine an underlying relationship determining desired schooling, which takes on negative values and values greater than eight.

Let $M = M(s,Z)$ denote the marginal gain (i.e., difference between marginal benefits and marginal cost) from an additional unit of schooling(s), where Z, a set of exogenous variables, is held constant. Assume that for a given Z, M can be inverted and denote the inverse by $s^* = F(M,Z)$ (s^* represents desired school attendance). An interior solution to the problem of choosing the optimal amount of schooling is given by $s^* = F(0,Z)$. Because of the constraint on s^*, however, the solution may be at a corner. More generally, we have the following decision rule:

$$s^* \begin{cases} = 0 & \text{if } F(0,Z) \leq 0 \quad , \\ = F(0,Z) & \text{if } 0 < F(0,Z) < 8 \quad , \\ = 8 & \text{if } F(0,Z) \geq 8 \quad . \end{cases}$$

Table 3

F-TEST* OF NO-EFFECT HYPOTHESIS FOR TREATMENT/SITE AND TREATMENT/RACE INTERACTIONS

	Treatment/Site	Treatment/Race
Husbands	1.623	1.151
Wives	1.737	2.209†
Female heads	.925	1.635

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*To calculate the F-statistic, let $y = \alpha C + \beta T + \gamma DT + \varepsilon$, where y is an N x 1 vector of observations on the dependent variable; C is an $N \times k_1$ matrix of control variables; T is an $N \times k_2$ matrix of treatment variables; DT is an $N \times k_3$ matrix of treatment/site or treatment/race interactions (D is the relevant dummy variable; α, β, and γ are vectors of coefficients of appropriate sizes; and ε is an $N \times 1$ vector of error terms. We test the joint hypothesis that $\gamma = 0$. This is a test of the hypothesis that the treatment effects act differently with regard to site or race. The F-statistic is

$$F(4, N-k_1-k_2-k_3) \quad \frac{(SSR_u - SSR_r)/(DF_u - DF_r)}{SSR_r/(N - k_1 - k_2 - k_3)} \quad ,$$

where SSR = residual sum of squares, u = unrestricted model (all the variables included), r = restricted model, and DF = degrees of freedom.

†Significant at 5% level.

In the empirical application, it will be convenient to assume that $F(0,Z)$ is a linear function of Z. The set of exogenous variables may include individual characteristics, market conditions, treatment effect variables, and random elements that represent unobserved individual or market elements.

We assume that the decision process is continuous and that s, the length of the schooling period, can vary continuously. We do not observe, however, all possible realizations, and in fact only a discrete approximation of s is available to us. A method of estimating the underlying linear function F when s is both censored and categorical is not available. The best available method is the tobit model, which allows us to account for the censoring of s but not the rounding off. Tobin's original statistical model permitted the estimation of the underlying linear relationship when the observed dependent variable was censored below a certain known limit. The model is easily generalized to the situation where the dependent variable is censored outside a known range. Thus, a generalized version of the tobit statistical model can include both upper and lower limits. Because it is a linear model, however, tobit treats the dependent variable as continuous rather than categorical. This is a defect of the statistical model for this analysis. But because the intervals that categorize the data are relatively short, we expect that the violation of this maintained hypothesis of the tobit model will not seriously affect estimation of the number of quarters of schooling taken in the observation period. The tobit model that is used is more fully developed in the appendix.

V EMPIRICAL RESULTS

In Table 4, we present the average number of quarters of schooling taken by husbands, wives, and female heads during the first eight quarters of the experiment. The averages are computed by manpower treatment and by financial treatment. The table shows that 23.9% of all single female heads attended school during at least one quarter during the first eight quarters of the experiment on an average of 1.062 quarters. Among husbands, 20.1% attended school for an average of .905 quarters. Wives were the least likely to attend school and had the lowest average attendance of the three groups. Only 16.0% attended school for an average of .593 quarters. The amount of school attendance is very small for each group, but there appears to be a relationship between school attendance and the experimental treatments. The average amount of schooling taken increases with increases in the potential value of the manpower treatment. The relationship of the average quarters taken and the financial treatments is more subtle and must await further analysis. The manpower and financial effects may be determined through estimation of the parameters of the demand equations.

Parameter Estimates

The normalized estimated coefficients of the tobit model with their asymptotic standard errors are presented in Table 5. Since the tobit model assumes a linear relationship, the coefficients can be interpreted directly, but with caution. The coefficients measure the estimated effect of changes in the independent variables on the underlying relationship. However, they can be used to estimate the effect of a change in the value of independent variables on the observed dependent variable only for individuals who are not at a limit and who do not reach one with the hypothesized change in the independent variables. Here, the underlying relationship represents the desired change in human capital, which

Table 4

AVERAGE QUARTERS OF SCHOOLING TAKEN AND PERCENTAGE OF INDIVIDUALS WHO TOOK ANY SCHOOLING DURING EIGHT QUARTERS

	Husbands		Wives		Female Heads	
	Average Quarters of Schooling	Percent with Schooling	Average Quarters of Schooling	Percent with Schooling	Average Quarters of Schooling	Percent with Schooling
Manpower treatment						
M0	.826	17.2	.456	13.1	.767	18.3
M1	.831	19.6	.396	10.8	.901	20.8
M2	.875	21.6	.646	17.7	1.298	27.8
M3	1.280	26.9	1.148	28.2	1.645	35.9
Total	.905	20.1	.593	16.0	1.062	23.9
Financial treatment						
F0	.989	21.1	.639	16.8	.995	22.4
F1	1.000	22.1	.717	16.8	1.241	26.2
F2	.796	20.4	.426	13.0	1.100	22.5
F3	.880	22.7	.613	16.0	.859	19.2
F4	.964	18.2	.164	9.1	1.046	27.7
F5	.766	18.9	.600	14.6	.627	14.7
F6	.605	14.9	.316	14.0	1.229	26.3
F7	1.227	26.1	1.120	23.9	1.299	27.6
F8	.589	16.7	.489	13.3	1.103	28.2
F9	1.093	22.2	.407	14.8	1.118	29.4
F10	.861	18.6	.174	9.3	1.298	28.1
F11	.553	13.6	.725	18.4	1.115	27.9
Total	.905	20.1	.593	16.0	1.062	23.9
Sample size	1,825		1,833		1,465	

Table 5

NORMALIZED COEFFICIENTS
(Standard Error in Parentheses)

		Husbands	Wives	Female Heads
Control Variables				
CONS	Constant	-11.480***	-14.824***	-14.245***
		(2.131)	(2.169)	(2.008)
INS	1 if in school in preexperimental period	10.679***	10.557***	12.366***
		(.994)	(1.102)	(1.157)
AGEL	1 if 16 ≤ age ≤ 25	5.656***	.073	4.636***
		(1.095)	(1.049)	(1.203)
AGEH	1 if 26 ≤ age ≤ 45	2.944***	.892	3.325***
		(.801)	(.781)	(.796)
PREW	Wage rate in preexperimental period	-.370	1.175**	.755*
		(.307)	(.481)	(.404)
EMP2	1 if employed in preexperimental period	.135	.280	-1.029*
		(.537)	(.494)	(.553)
PRED	Preexperimental educational level	.209**	.120*	.152*
		(.102)	(.071)	(.086)
DENV	1 if family is in Denver	-1.647***	-.826*	-1.145**
		(.472)	(.459)	(.478)
BLK	1 if black	.735	.506	.364
		(.473)	(.460)	(.471)
E1	1 if normal income \$0-\$1,000	-2.687	-.879	-.025
		(1.983)	(1.798)	(1.153)
E2	1 if normal income \$1,001-\$3,000	-.941	-.942	.489
		(1.091)	(1.138)	(1.065)
E3	1 if normal income \$3,001-\$5,000	-.198	-.341	1.167
		(.788)	(.800)	(1.068)
E4	1 if normal income \$5,001-\$7,000	.001	-.093	.875
		(.678)	(.668)	(1.130)
E5	1 if normal income \$7,001-\$9,000	-.657	.129	.180
		(.668)	(.623)	(1.210)
OHINS	1 if spouse was in school in preexperimental period	1.615**	.301	–
		(.774)	(.665)	
OHPRW	Wage rate of spouse in preexperimental period	-.419	-.368	–
		(.520)	(.296)	
OHEMP	1 if spouse employed in preexperimental period	-.920*	.163	–
		(.521)	(.553)	
OHED2	Preexperimental education level of spouse	.077	.333***	–
		(.084)	(.097)	

Table 5 (Concluded)

		Husbands	Wives	Female Heads
Treatment Variables				
MYRS	1 if manpower treatment and 3-year program	0.034 (.666)	-1.144* (.675)	-.611 (.712)
M1	1 if counseling only	1.718* (.917)	.189 (.939)	1.650* (.961)
M2	1 if counseling + 50% subsidy	2.397*** (.810)	2.298*** (.802)	3.650*** (.859)
M3	1 if counseling + 100% subsidy	3.392*** (1.016)	5.096*** (1.010)	4.963*** (1.059)
PASZ	Annual NIT payment size/100	.364 (.399)	.317 (.433)	-1.85 (.373)
DNWG	(Prew) $\cdot$ ($tax_{preexp.}$ - $tax_{exp.}$)	1.338** (.657)	1.952* (1.003)	-.884 (.648)
EVER	0 if BRKEN = 0; e^{-VERT} if BRKEN = 1, where VERT is the amount of income above the breakeven level	-3.220* (1.807)	-2.659 (1.870)	-.432 (1.499)
Interactions				
MHXINS	1 if M2 or M3 = 1 and INS = 1	.513 (1.156)	-2.583* (1.368)	-2.707* (1.406)
MLXINS	1 if M1 = 1 and INS = 1	.020 (1.426)	.330 (1.833)	.903 (1.799)
DNWGXINS	DNWG if INS = 1	-1.216 (1.096)	-2.595 (1.906)	2.442* (1.280)
PASZXINS	PASZ if INS = 1	-1.441** (.656)	-1.273 (.794)	.045 (.667)
MLXAGE	1 if M1 = 1 and AGEL = 1	-2.905** (1.349)	.178 (1.435)	-.854 (1.558)
MHXAGE	1 if M2 or M3 = 1 and AGEL = 1	-2.610** (1.045)	1.239 (.979)	.215 (1.202)
DNWGXAGE	DNWG if AGEL = 1	-.034 (1.088)	-.293 (1.549)	1.387 (1.199)
PASZXAGE	PASZ if AGEL = 1	.225 (.597)	.194 (.625)	.610 (.576)
σ: Standard error of the linear form		6.009*** (.299)	5.720*** (.304)	6.196*** (.312)
Number of observations		1,825	1,833	1,465

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

coincides with observed school attendance only when the desired change is between zero and eight quarters.

Manpower Effects

The coefficients of the manpower dummy variables clearly show that counseling and training subsidies induce some subgroups in the sample to increase the quantity of schooling they take. They show that the quarters of additional schooling increase with the increasing generosity of the manpower treatment. Among husbands, the option of full reimbursement of costs (M3) leads to an increase of 3.4 quarters in school attendance relative to controls. The results are even more dramatic for wives and female heads. The M3 treatment causes wives to increase the time that they attend school by a full 5.1 quarters, while female heads increase attendance by 5.0 quarters relative to females who do not have these treatments.* The net effects of the treatments are additive across the main treatment variables and the interaction variables. The interaction terms show that the subsidy treatments (M2 and M3) have smaller effects on younger husbands, wives, and female heads who were already in school in the preexperimental period.

Since the counseling-only option, M1, was intended to provide nondirective counseling but no training subsidies, it is somewhat surprising that the coefficients of the M1 treatment are positive and significantly different from zero at the 10% level for husbands and single female heads. The interaction terms indicate that the effect is more pronounced for younger individuals and for those who had already been in school, with the exception of husbands. One explanation for their findings is that counseling acted as an inducement toward increased school attendance. An alternative explanation is that, prior to counseling, individuals were unaware of opportunities for further schooling, so that the nondirective provision of information during the counseling process led to

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*We actually measure whether the individual is in school at any time during the quarter. It is assumed that the individual is in school for all three months of the quarter.

increased school attendance. There may be other equally plausible explanations for the positive M1 coefficients, but the results invite further study of the SIME/DIME counseling program.

Financial Effects

The effects of the financial treatment are, in general, not as clear as those of the manpower treatments. The coefficient of PASZ represents the income effect of the financial treatment on school attendance and should be negative. However, the coefficients of that variable and its interactions are almost all insignificant. The only exception is an interaction indicating a different income effect for husbands who were already in school in the preexperimental period. For most individuals, the increase in lifetime income caused by the financial treatment of this short-term support program seems not to have affected school attendance.

The substitution effect of the financial treatments is measured by the coefficient of DNWG and is statistically significant more frequently than the income effect. Both the income and substitution effects are expected to lead to a reduction in the total amount of schooling taken. The main effects for husbands and wives are significantly different from zero, implying that older heads of two-head families who were not already in school were induced to take additional schooling by the experiment. Substitution effects for younger heads and those in school were generally not different from zero. There were no significant substitution effects for single female heads.

Predicted Treatment Effects on Actual School Attendance

To test whether the manpower and financial treatments had any effects on school attendance during the experimental period, hypothesis tests were constructed on the difference in expected values of the dependent variable for a subset of "average" treatment and control observations.* We predicted

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*Since the dependent variable has a lower limit of zero and an upper limit of eight, its expected value for any set of independent variables lies between these extremes; its exact expectation is a function of the limits and the values of the independent variables. This expected value can be the quarters of schooling taken by the controls and then predicted how that amount of schooling would change when the individual received either the manpower or financial treatment. The impacts were predicted separately for husbands, wives, and female heads. To determine the impact of the financial treatments, we held the marginal tax rate of the treatment constant and varied the support level. Thus, we predicted the impact of the F1 treatment, $3,800 base support (for a family of four) and a .50 marginal tax rate; the F5 treatment, $4,800 base support and a .50 marginal tax rate; and the F9 treatment, $5,600 base support and a .50 marginal tax rate.

Response of Husbands

In Table 6, we show the mean differences between the predicted values of the number of quarters of school for husbands in the experimental group and for husbands in the control group. The differences are shown for the treatments averaged across each group and for the treatments by subgroup. The quarters of schooling taken by the controls (an average of .381) provides insight into the schooling decision. We note that the younger age groups spend more quarters in school than do the older groups; also, those who are in school at the start of the experiment spend considerably more time in school than do those who are not in school at the start of the experiment. These results are consistent with the assertions that schooling

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interpreted as the average number of quarters of schooling attendance for persons with average characteristics observed in the first 2 years of the experiment. The difference between the expected values of the dependent variables for two sets of independent variables is a more complicated function of the limits and the values of the two sets of independent variables. That function evaluated at the maximum likelihood values of the parameters of the model is the maximum likelihood estimate of the difference in expectations. The maximum likelihood estimates of the difference in expectations, like all maximum likelihood estimates, is asymptotically normal, and its asymptotic standard error is a simple transformation of the estimated variance-covariance matrix of the estimated model parameters. With a maximum likelihood estimate of the difference and an estimate of its standard error, an asymptotically normal test statistic can be calculated.

Table 6

MEAN DIFFERENCES IN THE PREDICTED VALUE
OF THE NUMBER OF QUARTERS OF SCHOOLING--HUSBANDS
(Change in Quarters Relative to Controls)

Category and Age	Controls (predicted quarters)	Manpower Treatment			Financial Treatment		
		M1	M2	M3	F1	F5	F9
In school							
16-25	4.592	-.590	.129	.599	-.417	-.534	-.797*
26-45	3.262	.836	1.410**	1.882***	-.508*	-.644**	-.939**
46-58	1.961	.720	1.268**	1.753***	-.397*	-.498	-.710**
Not in school							
16-25	.566	-.179	-.040	.137	-.066	-.031	-.007
26-45	.233	.179*	.276***	.450***	-.048*	-.037	-.037
46-58	.074	.072	.115**	.196***	-.018	-.014	-.014
Predicted average for group	.381	.140	.281***	.491***	-.073**	-.060	-.065

*Significant at 10% level.

**Significant at 5% level.

***Significant at 1% level.

is more attractive to the young (since they are likely to reap greater lifetime returns), and that those in school are more likely to remain in school.

Manpower Effects

The M1 counseling-only option had a significant impact only for those in the age group 26 to 45 who were not in school at the start of the experiment. For this group, the predicted quarters of schooling went from .233 for the controls to .412. Although the counseling provided through the M1 option is a more intensive program than that normally available, it does not differ in substance from alternative forms of counseling available to the controls. Hence, it is not surprising that the option that provides only counseling has virtually no impact on the schooling decision.

A surprising finding is that the M2 and M3 treatments have no net impact on the schooling decisions of those 16 to 25 years old. Among the controls, this age group is predicted to take more schooling than the older groups, but the subsidies do not significantly change those quantities. This finding suggests that this age group was investing at an optimal level at the start of the experiment, so that the introduction of the subsidies induced no further schooling investments.

It is among the older groups that the greatest impact is observed. The 26 to 45 age group who were in school previously are predicted to increase their schooling by 1.41 quarters if they receive the 50% subsidy and by 1.88 quarters if they receive the 100% subsidy. Among the older groups, the lowering of the cost of schooling induces a return to school, implying that there were unmet schooling needs.

The manpower options operated in the direction and with the intensity expected. When husbands are given subsidies for training, they are more likely to increase their schooling than if they are given only counseling. The amount of the increase is greater for those who were in school at enrollment compared to those not in school at enrollment, is greater for those with the 100% subsidy than for those with the 50% subsidy, and--in the only surprising finding--is greater for the older age groups than for the youngest age group.

Financial Effects

The predicted effects of the financial treatments show the sum of the income effects--the payment size variable--and the substitution effect--the change in the marginal tax rate variable relative to the marginal tax rate during the preexperimental period. If the income effect is dominant, our earlier analysis (see Chapter II) predicts that the demand for leisure will increase and the demand for schooling will decrease. The results for husbands suggest that, indeed, the income effect is dominant and that among some husbands (notably those older than 25) there are apparent increases in the demand for leisure, which lead to reductions in the demand for schooling.

Among those in school at the beginning of the experiment, the impact on schooling is significant and negative for the more lucrative financial programs, F5 and F9. Among the older husbands not in school, the lowest grant level, F2, is significant while the higher levels are not. In all instances in which there are significant effects, they are negative and very small. The largest reduction in the quantity of schooling is the reduction from 3.262 quarters to 2.323 quarters (a reduction of .939 quarters) for the age group 26 to 45. The financial treatment effects are more frequently significant for those in school than for those not in school. These results suggest that the financial treatments are likely to induce those husbands who were in school to reduce their schooling attendance and to provide disincentives to entering school to those husbands who were not in school.

Response of Wives

The mean differences between the predicted values of the number of quarters of schooling for wives in the experimental group and for wives in the control group are shown in Table 7. The number of quarters spent in school by the control group wives is larger for those wives who were in school at the start of the experiment than for those who were not in

Table 7

MEAN DIFFERENCES IN THE PREDICTED VALUE
OF THE NUMBER OF QUARTERS OF SCHOOLING--WIVES
(Change in Quarters Relative to Controls)

Category and Age	Controls (predicted quarters)	Manpower Treatment			Financial Treatment		
		M1	M2	M3	F1	F5	F9
In school							
16-25	3.065	-.218	-.094	1.330*	-.122	-.234	-.399
26-45	3.476	-.315	-.708	.704	-.287	-.427	-.671
46-58	3.029	-.302	-.673	.691	-.276	-.409	-.638
Not in school							
16-25	.177	-.047	.243**	.797***	-.022	-.011	-.004
26-45	.241	-.074	.123	.627***	-.053*	-.044	-.048
46-58	.172	-.055	.094	.499***	-.039	-.027	-.036
Predicted average for group	.272	-.018	.279***	.941***	-.049*	-.039	-.042

*Significant at 10% level.

**Significant at 5% level.

***Significant at 1% level.

school. This result is similar to that observed for husbands. However, wives in the 26 to 45 age group spend more quarters in school than either the older or younger groups. The predicted level of schooling for all age groups is similar once preexperimental school status is accounted for. This result suggests that school attendance is less age differentiated for wives than it is for husbands.

Manpower Effects

Few subgroups among wives have significant coefficients, and those that are significant tend to be small. The counseling-only option has no impact on the quantity of schooling taken, and the counseling plus 50% subsidy is significant only for wives in the age group 16 to 26 who were not in school in the preexperimental period. The counseling plus 100% subsidy (M3) option is significant and positive for most categories of wives and is most effective among wives who were not in school in the preexperimental period.

For those who were not in school, the results of the M3 treatment are very dramatic. For the age group 16 to 25, the amount of schooling increases by .804, from .179 predicted quarters to .983 predicted quarters. These results may mean either that a few wives are taking advantage of the option and are taking many additional quarters of school or that many wives are taking a few additional quarters. Table 4 shows that the percentage of wives who took any schooling was much larger for the M3 group than for the other manpower treatments. We conclude that many wives took advantage of the subsidies of the M3 program. There are similarly large and significant increases among the 26 to 45 and 46 to 58 groups. The results here suggest that wives will return to school only if there is complete subsidization of the schooling costs. For such wives, child care costs may be the most important of those schooling expenses that are subsidized.

Financial Effects

The lowest support level treatment, F1, is significant on average among those wives who receive that treatment. Among the subgroups, except for the significant negative impact of the F2 treatment for wives aged 26 to 45 who were not in school, there are no other significant impacts of the financial treatments on the schooling investments of wives. There are several explanations for this result. Wives are more likely than husbands or female heads to work in the home, and for such wives the income effect of the financial treatment will lead to the purchase of more goods, but not necessarily leisure. Those wives who are employed in the labor market may use the additional income to purchase more leisure and reduce schooling. The results suggest that, if there is an impact at all, the financial treatments act to increase leisure consumption and decrease schooling investment.

Response of Single Females

Table 8 shows the mean differences in the predicted values of the number of quarters of schooling for female heads. Single females who are household heads are more likely to be in the labor force than are the wives of two-head households. For this reason, single female heads are more likely than wives to undertake schooling investments that will enhance their attractiveness in the labor market. Among the controls, single female heads in each age group are predicted to take more schooling than either husbands or wives in similar age groups. The youngest group of single female heads is predicted to take almost 5 quarters of schooling--considerably more than the amount of schooling predicted for wives in the same group. As was observed for husbands and wives, those who were in school when the experiment started are predicted to take considerably more school than those who were not in school. Much of this difference may be attributable to the high start-up costs of school and the short lead time for a program of limited duration.

Table 8

MEAN DIFFERENCES IN THE PREDICTED VALUE
OF THE NUMBER OF QUARTERS OF SCHOOLING--SINGLE FEMALES
(Change in Quarters Relative to Controls)

Category and Age	Controls (predicted quarters)	Manpower Treatment			Financial Treatment		
		M1	M2	M3	F1	F5	F9
In school							
16-25	4.883	.490	.250	.818	-.599*	-.566	-.351
26-45	4.263	.908	.159	.773	-.479*	-.656	-.632
46-58	2.701	.898	.148	.756	-.424*	-.573	-.553
Not in school							
16-25	.449	.026	.645**	1.031***	+.010	.064	.102
26-45	.295	.117	.444***	.743***	.035	.028	.012
46-58	.087	.044	.181**	.324***	.013	.010	.004
Predicted average for group	.401	.188*	.576***	.936***	.019	.019	.013

*Significant at 10% level.

**Significant at 5% level.

***Significant at 1% level.

Manpower Effects

An immediate observation is that the counseling-only treatment has no effect on the schooling decision and that no treatment has any significant impact on those single female heads who were in school in the preexperimental period. The lack of significance of the M1 treatment is a result that was also observed for husbands and wives. The lack of impact of any treatment on those who were in school, coupled with the high amount of schooling predicted for the controls, means that for these females schooling investments would be made even in the absence of subsidies.

The response of single female heads largely follows the expected response, that is:

- The counseling-only option has no impact on the demand for school.
- The 100% subsidy has a larger impact on schooling demand than does the 50% subsidy.
- The younger age groups react to the training subsidies more than do the older age groups.
- The reaction to the subsidies is greater among those who were not in school when the program started.
- The subsidy effects are significant. In contrast to the findings for husbands and wives, the manpower options have the expected significant effects.

Financial Effects

The lowest financial treatment considered, F1, has the only significant impacts, and they are negative. Single female heads who were in school and received the F1 treatment tend to leave school at a faster rate than do those without any financial treatment. The significant impact of this plan was previously observed for wives who had not been in school. It is somewhat surprising that among females the smallest financial treatment significantly reduces schooling demand but the larger treatments do not. Among husbands, by contrast, there are significant reductions in all the financial treatments.

Summary

The results of the estimations and the predictions indicate the following:

- The older age groups among husbands, wives, and single female heads react more to the manpower treatments than does the 16 to 25 age group.
- Counseling without any subsidies, on average, has no impact on the investment in schooling.
- Among husbands, the impact of the training subsidies is distributed across those who were and those who were not in school in the preexperimental period; among females, the greater impact occurs with those who were not in school.
- The financial treatments are not significant as frequently as the training subsidy options; when they are significant, they are negative.

These results are preliminary and reflect only the first 2 years of a 3-year program. Research into the effects of the financial and manpower options over the full 3 years of the program will continue. Other research will evaluate the pattern of schooling investments over the life of the experiment.

Appendix

ESTIMATION AND TESTING OF THE DOUBLY CENSORED TOBIT MODEL*

1. The Statistical Model

Tobin's original model was appropriate for data censored below a common limit. The model can be written:

$$\left.\begin{array}{l} Y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \ldots + \beta_m X_m \\ S = Y - \varepsilon \text{ if } Y - \varepsilon \geq L \\ S = L \text{ if } Y - \varepsilon < L \quad , \end{array}\right\} \tag{1}$$

where $\varepsilon \sim N(0,\sigma^2)$.

Then the density function for S above the limit is:

$$f(S|X) = \frac{1}{\sigma} z\ [(Y-S)/\sigma] \quad , \tag{2}$$

and the probability of obtaining an observation at the limit is:

$$P(S = L|X) = P(\varepsilon \geq Y-L) = 1 - Z\ [(Y-L)/\sigma] \quad , \tag{3}$$

where z is the standard normal density and Z is the standard normal distribution function.

Then the likelihood function is the product of appropriate terms for each observation.

$$L = \prod_{i\ \varepsilon\{i|S_i > L\}} \left[\frac{1}{\sigma} z \left(\frac{Y-S_i}{\sigma}\right)\right] \prod_{i\varepsilon\ \{i|S_i=L\}} \left[1-Z\left(\frac{Y-L}{\sigma}\right)\right] \quad . \tag{4}$$

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* We wish to thank Christy Austermann and Paul McElherne for their assistance in developing the computer programs for the empirical use of this model.

Two things about this likelihood function make it easy to estimate a model for doubly censored data. First, a model with an upper limit can be transformed into an equivalent model with a lower limit by multiplying the dependent variable, the independent variables, and the limit by -1. Second, the likelihood function does not require that all limit observations be at the same limit. All that is required is that the appropriate limit be used for each observation. For the model

$$\left.\begin{array}{l} S = LL \text{ if } Y - \varepsilon < LL \\ S = Y - \varepsilon \text{ if } LL \leq Y - \varepsilon \leq UL \\ S = UL \text{ if } Y - \varepsilon > UL \ , \end{array}\right\} \qquad (5)$$

the likelihood function can be written:

$$L = \prod_{i\ \varepsilon\{i|S_i=LL\}} \left[1 - Z\left(\frac{Y-LL}{\sigma}\right)\right] \prod_{i\ \varepsilon\{i|S_i=UL\}} \left[1 - Z\left(\frac{UL-Y}{\sigma}\right)\right]$$

$$\prod_{i\ \varepsilon\{i|LL \leq S_i \leq UL\}} \left[\frac{1}{\sigma}\, z\left(\frac{Y-S_i}{\sigma}\right)\right] \ . \qquad (6)$$

Because the likelihood function for the doubly censored model is of similar form to the likelihood function shown above, the first and second derivatives of the log of the likelihood function can be maximized by the same method, and the resulting estimates will have the same properties. Asymptotic t-tests and likelihood ratio tests on the βs can be done in the same way as for the model with a single limit. One aspect of the model that does change is the expectation of the observed variable. The expectation for the doubly censored model is derived in the next section.

2. Expectation of the Observed Dependent Variable

The presence of both upper and lower limits complicates the formula for the expected value of S. We have [following Tobin (1958)]:

$$P(S = LL) = 1 - Z[(Y-LL)/\sigma]$$
$$P(S = UL) = 1 - Z[(UL-Y)/\sigma] \tag{7}$$

and the density function

$$f(S|X) = \frac{1}{\sigma} z\ [(Y-S)/\sigma] \quad .$$

Thus the expectation of S, the observed dependent variable, is

$$E(S) = LL\{1-Z[(Y-LL)/\sigma]\} + UL\{1-Z[(UL-Y)/\sigma]\} + \int_{LL}^{UL} \frac{S}{\sigma} z\ [(Y-S)/\sigma]dS \ . \tag{8}$$

The last term can be expanded into

$$\int_{-\infty}^{UL} \frac{S}{\sigma} z\ [(Y-S)/\sigma]\ dS \ - \int_{-\infty}^{LL} \frac{S}{\sigma} z\ [(Y-S)/\sigma]\ dS \quad .$$

Tobin shows that

$$\int_{-\infty}^{L} \frac{S}{\sigma} z[(Y-S)/\sigma]\ dS = Y\{1-Z[(Y-L)/\sigma]\} - \sigma z[(Y-L)/\sigma] \quad . \tag{9}$$

So the last term of equation (8) can be written as

$$\begin{aligned} &Y\{1-Z[(Y-UL)/\sigma\} - \sigma z[(Y-UL)/\sigma] - Y\{1-Z[(Y-LL)/\sigma]\} \\ &\qquad + \sigma z[(Y-LL)/\sigma] \end{aligned} \tag{10}$$

so that

$$\begin{aligned} E(S) = LL\{1-Z[(Y-LL)/\sigma]\} + UL\{1-Z[(UL-Y)/\sigma]\} - Y\{1-Z[(Y-LL)/\sigma]\} \\ + \sigma z[(Y-LL)/\sigma] + Y\{1-Z[(Y-UL)/\sigma]\} \\ - \sigma z[(Y-UL)/\sigma] \quad . \end{aligned} \tag{11}$$

Noting that

$$Z[(Y\text{-}UL)/\sigma] = 1 - Z[(UL\text{-}Y)/\sigma] \tag{12}$$

we can simplify the formula to

$$\begin{aligned} E(S) = LL &+ (Y\text{-}LL)Z[(Y\text{-}LL)/\sigma] - (Y\text{-}UL)Z[(Y\text{-}UL)/\sigma] \\ &+ \sigma\{z[(Y\text{-}LL)/\sigma] - z[(Y\text{-}UL)/\sigma]\} \quad . \end{aligned} \tag{13}$$

3. <u>Hypothesis Tests on the Observed Dependent Variable</u>

Because the observed dependent variable is not a linear function of the coefficients, hypothesis tests on the coefficients do not have direct implications for fitted values of the observed dependent variable. That is, the fact that the coefficient of a treatment variable is significantly different from zero would not necessarily imply that the expectation of the fitted value of the observed dependent variable for a treatment observation would be significantly different from the fitted value for a control, with the values of all other independent variables the same. To test whether a financial or manpower treatment has a statistically significant effect on individuals with a certain set of characteristics, we must do the test directly. Thus we must test whether the expectation of the fitted value of the observed dependent variable for one set of independent variables is different from that of another set. Such expectations, the general formula for which is derived above, are functions of the true parameters. Then the maximum likelihood estimates of these expectations are simply the same functions as the maximum likelihood estimates of the parameters of the model. The same statement can be made about the difference between the expectations, which gives us a way to calculate the maximum likelihood estimate of the quantity whose equality to zero we wish to test. And since the maximum likelihood estimates are asymptotically normal, it remains only to derive an estimate of the standard error of the difference function in order to do the test.

Suppose the two sets of independent variables are

$$(X_{11}, X_{21}\ldots,X_{m1}) \text{ and } (X_{12}, X_{22}\ldots,X_{m2}) \quad ,$$

and define

$$\begin{aligned} Y_1 &= \hat{\beta}_0 + \hat{\beta}_1 X_{11} + \hat{\beta}_2 X_{21} + \ldots + \hat{\beta}_m X_{m1} \quad , \\ Y_2 &= \hat{\beta}_0 + \hat{\beta}_1 X_{12} + \hat{\beta}_2 X_{22} + \ldots + \hat{\beta}_m X_{m2} \quad . \end{aligned} \tag{14}$$

The difference in expected value is

$$\begin{aligned} \Delta = {} & E(S_1) - E(S_2) = \\ & [LL + (Y_1-LL)Z[(Y_1-LL)/\sigma] - (Y_1-UL)Z[(Y_1-UL)/\sigma] + \sigma\{z[(Y_1-LL)/\sigma] \\ & \quad - z[(Y_1-UL)/\sigma]\}] - [LL + (Y_2-LL)Z[(Y_2-LL)/\sigma] - (Y_2-UL)Z[(Y_2-UL)/\sigma] \\ & \quad + \sigma\{z[(Y_2-LL)/\sigma] - z[(Y_2-UL)/\sigma]\}] \end{aligned} \tag{15}$$

$$\begin{aligned} & = (Y_1-LL)Z[(Y_1-LL)/\sigma] - (Y_2-LL)Z[(Y_2-LL)/\sigma] - (Y_1-UL)Z[(Y_1-UL)/\sigma] \\ & \quad + (Y_2-UL)Z[(Y_2-UL)/\sigma] + \sigma\{z[(Y_1-LL)/\sigma] - z[(Y_1-UL)/\sigma] \\ & \quad - z[(Y_2-LL)/\sigma] + z[(Y_2-UL)/\sigma]\} \quad . \end{aligned} \tag{16}$$

The variance-covariance matrix for a set of maximum likelihood estimates is minus the inverse of the matrix of second derivatives of the likelihood function with respect to the estimated parameters. We can derive a formula for the variance-covariance matrix for a function of maximum likelihood estimates from that of the basic parameters by a few manipulations.

Define $a_i = \beta_i/\sigma$ and $a_{m+1} = 1/\sigma$. These are the parameters that are actually calculated by the computer program that does tobit estimation. The β's and σ are then calculated in the obvious way. We can calculate the second derivative of the likelihood function from that of the a's by using the formula:

$$\frac{\partial^2 L}{\partial \Delta^2} = \frac{\partial a'}{\partial \Delta} \frac{\partial^2 L}{\partial a^2} \frac{\partial a}{\partial \Delta} \quad . \tag{17}$$

This is done indirectly. Consider the quadratic form

$$\frac{\partial \Delta'}{\partial a} \left(- \frac{\partial^2 L}{\partial a^2} \right)^{-1} \frac{\partial \Delta}{\partial a} \quad . \tag{18}$$

It can be shown* that

$$\left(\frac{\partial^2 L}{\partial a^2} \right)^{-1} = \frac{\partial^2 a}{\partial L^2} \quad . \tag{19}$$

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* Let $u = f(x,y)$ $\quad x = a(r,s)$

$v = g(x,y)$ $\quad y = b(r,s)$

We have

$$\begin{bmatrix} \frac{\partial f}{\partial r} & \frac{\partial f}{\partial s} \\ \frac{\partial g}{\partial r} & \frac{\partial g}{\partial s} \end{bmatrix} = \begin{bmatrix} \frac{\partial f}{\partial x} & \frac{\partial f}{\partial y} \\ \frac{\partial g}{\partial x} & \frac{\partial g}{\partial y} \end{bmatrix} \begin{bmatrix} \frac{\partial a}{\partial r} & \frac{\partial a}{\partial s} \\ \frac{\partial b}{\partial r} & \frac{\partial b}{\partial s} \end{bmatrix}$$

by the chain rule.

Now suppose $\quad u = f(x,y) = r$

and $\quad v = g(x,y) = s$

Then $\quad \frac{\partial f}{\partial r} = 1 \quad \frac{\partial f}{\partial s} = 0$

$\frac{\partial g}{\partial r} = 0 \quad \frac{\partial g}{\partial s} = 1$

So
$$\begin{bmatrix} 1 & 0 \\ 0 & 1 \end{bmatrix} = \begin{bmatrix} \frac{\partial f}{\partial x} & \frac{\partial f}{\partial y} \\ \frac{\partial g}{\partial x} & \frac{\partial g}{\partial y} \end{bmatrix} \begin{bmatrix} \frac{\partial a}{\partial r} & \frac{\partial a}{\partial s} \\ \frac{\partial b}{\partial r} & \frac{\partial b}{\partial s} \end{bmatrix}$$

or
$$\begin{bmatrix} \frac{\partial f}{\partial x} & \frac{\partial f}{\partial y} \\ \frac{\partial g}{\partial x} & \frac{\partial g}{\partial y} \end{bmatrix}^{-1} = \begin{bmatrix} \frac{\partial a}{\partial r} & \frac{\partial a}{\partial s} \\ \frac{\partial b}{\partial r} & \frac{\partial b}{\partial s} \end{bmatrix}$$

Since $u = r$ and $v = s$,

$x = a(u,v)$ and $y = b(u,v)$. (continued)

Therefore,

$$\frac{\partial\Delta'}{\partial a}\left(-\frac{\partial^2 L}{\partial a^2}\right)^{-1}\frac{\partial\Delta}{\partial a} = -\frac{\partial\Delta'}{\partial a}\frac{\partial^2 a}{\partial L^2}\frac{\partial\Delta}{\partial a} = -\frac{\partial^2\Delta}{\partial L^2}\ , \tag{20}$$

and by the same argument

$$-\frac{\partial^2\Delta}{\partial L^2} = -\left(\frac{\partial^2 L}{\partial\Delta^2}\right)^{-1}\ . \tag{21}$$

This is the variance-covariance matrix of the estimated difference in expectations. Thus, the required variance (since Δ is a single parameter) can be calculated from the quadratic form in (18). It remains only to calculate the derivatives with respect to Δ:

$$\frac{\partial\Delta}{\partial a_i} = \sigma x_{i1}\{Z[(Y_1-LL)/\sigma] - Z[(Y_1-UL)/\sigma]\}$$
$$-\sigma x_{i2}\{Z[(Y_2-LL)/\sigma] - Z[(Y_2-UL)/\sigma]\}\ ; \tag{22}$$

$$\frac{\partial\Delta}{\partial a_{m+1}} = -\sigma^2\{z[(Y_1-LL)/\sigma] - z[(Y_2-LL)/\sigma]$$
$$- z[(Y_1-UL)/\sigma] + z[(Y_2-UL)/\sigma]\}\ . \tag{23}$$

Using the variance-covariance matrix calculated by the computer program that produced maximum likelihood estimates of the parameters, and the derivatives calculated from equations (22) and (23), we calculated the estimated variances used to test the hypotheses that various Δ's were different from zero.

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* (continued)

So a and b are the inverse functions of f and g and

$$\begin{bmatrix} \frac{\partial f}{\partial x} & \frac{\partial f}{\partial y} \\ \frac{\partial g}{\partial x} & \frac{\partial g}{\partial y} \end{bmatrix}^{-1} = \begin{bmatrix} \frac{\partial\left(f^{-1}\right)}{\partial r} & \frac{\partial\left(f^{-1}\right)}{\partial s} \\ \frac{\partial\left(g^{-1}\right)}{\partial r} & \frac{\partial\left(g^{-1}\right)}{\partial s} \end{bmatrix}$$

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