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# ESTIMATING THE EFFECT OF TRAINING PROGRAMS ON EARNINGS

Orley Ashenfelter\*

GOVERNMENTAL post-schooling training programs have become a permanent fixture of the U.S. economy in the last decade. These programs are typically advocated for diverse reasons: (1) to reduce inflation by the provision of more skilled workers to alleviate shortages, (2) to reduce unemployment of certain groups, and (3) to reduce poverty by increasing the skills of certain groups. All of these objectives require that training programs increase the earnings of trainees above what they otherwise would be. For example, alleviating shortages by training more highly skilled workers should increase the earnings of these workers. Likewise, the concern for unemployed workers is derived from a concern for the decreased earnings of these workers; and if trainees subsequently suffer less unemployment, their earnings should be higher. Finally, training programs are intended to reduce poverty by increasing the earnings of low income workers.

Evaluating the success of training programs is thus inherently a quantitative assessment of the effect of training on trainee earnings.<sup>1</sup> It is an important process both because it helps to inform discussions of public policy by shedding light on the past value of these programs as investments and because it can provide a means of testing our ability to augment the human capital of certain workers. Although there have been many studies of the effect of post-school classroom training on earnings it is by now rather widely agreed that very little is

reliably known about the actual effects of these programs.<sup>2</sup> Three main problems account for this state of affairs: (1) the large sample sizes required to detect relatively small anticipated program effects in a variable with such high variance as earnings, (2) the considerable expense required to keep track of trainees over a long enough period of time to measure the full inter-temporal impact of training, and (3) the extreme difficulty of implementing an adequate experimental design so as to obtain a group against which to reliably compare trainees.<sup>3</sup> The purpose of this paper is to report on efforts to cope with this third problem using a data collection system that comes some way towards resolving the first two.

The basic idea of this data system is to match the program record on each trainee with the trainee's Social Security earnings history. The Social Security Administration maintains a summary year-by-year earnings history for each Social Security account over the period since 1950 that may be used, under the appropriate confidentiality restrictions, for this purpose.<sup>4</sup> In this paper I have concentrated on an analysis of all classroom trainees who started training under the Manpower Development and Training Act (MDTA) in the first 3 months of 1964 so as to ensure their having completed training in that year. In choosing to analyze trainees from so early a cohort something is clearly lost. On the one hand, the nature of the participants in these early years was considerably different than in the later years. In particular, programs geared

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<sup>1</sup>See Reid (1976), for example, for a clear analysis of how knowledge of these effects is required in order to establish the impact of government training on the black/white wage differential.

<sup>2</sup>Surveys of many of these studies may be found in Stromsdorfer (1972) and O'Neill (1973).

<sup>3</sup>For further discussion of these points see Ashenfelter (1975).

<sup>4</sup>The idea for using these data to analyze the effectiveness of government training programs is apparently quite an old one, having been suggested by the National Manpower Advisory Committee (U.S. Department of Labor, 1972) to the Secretary of Labor at its first meeting in a letter dated October 10, 1962, the year of passage of the Manpower Development and Training Act. Actual efforts along these lines were ultimately reported by Borus (1967), Commins (1970), Farber (1970), and Prescott and Cooley (1972).

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to the most easily trained during the high unemployment years of the early 1960s gave way to programs geared to the so-called disadvantaged worker in the late 1960s. This change shows up in program administrative statistics as sharp increases in early terminations (dropouts) from the training programs and a decline in the average age and education level of trainees. In more recent years the MDTA has given way to the Comprehensive Employment and Training Act and a considerable decentralization of administrative control to state and local governments. For both of these reasons it is unclear how relevant any results for this early cohort of trainees are for current discussions of public policy. At the same time, the study of this 1964 cohort offers several advantages. First, it is technically feasible to follow this cohort's progress in the labor market for many years after training, something which cannot be done with recent cohorts. Second, if convincing estimates of trainee effects can be generated for the early years of the program these may serve as benchmarks against which to assess the desirability of the subsequent changes in the focus of government efforts in this area.

The plan of the paper is as follows: Section I contains a discussion of the conceptual framework for the analysis, including its connection with the emerging literature on investments in human capital. Section II contains a further discussion of the data to be used and the empirical results, while section III is a discussion of the limitations of the results and the considerable additional research required in this area.

## I. Earnings Generating Functions and Training

An adequate longitudinal data base on trainee earnings is not sufficient information for the analysis of the effect of training on earnings in a changing economy. It is also necessary to have an adequate comparison group of individuals against whom to benchmark the earnings of trainees so that general changes in earnings are not taken to be the effect of training. In the classical sample design some fraction of a training program's applicants would be randomly assigned to

training while the remainder would be reserved as a comparison group. For a variety of reasons, actual training programs have not been operated in this way and it therefore becomes necessary to look elsewhere for a comparison group. Although there are several possibilities, in this paper I have drawn on the 0.1% Continuous Work History Sample (CWHHS) to serve this purpose.<sup>5</sup> The CWHHS is a random sample of longitudinal earnings records on American workers that is maintained for general research purposes by the Social Security Administration. Since the trainee and comparison groups are obviously not being drawn from the same population it is thus necessary to control statistically for differences between the two groups. In order to do this it is necessary to have a specification of the earnings function that would prevail for both groups in the absence of the training program.

### A. The Earnings Function

A useful specification of earnings determination in longitudinal data is

$$y_{it} = \alpha + \sum_{j=1}^k \beta_j y_{i(t-j)} + \sum_{j=1}^{k'} \beta'_j (A_i + t)^j + \epsilon_t + \epsilon_i + \epsilon_{it}, \quad (1)$$

where  $y_{it}$  is the earnings of the  $i^{\text{th}}$  individual in period  $t$ ,  $A_i$  is the age of the  $i^{\text{th}}$  individual in period  $t=0$ , the  $\alpha$  and  $\beta$ 's are parameters, and the disturbance term  $\epsilon'_{it} = \epsilon_t + \epsilon_i + \epsilon_{it}$  is taken to have an effect  $\epsilon_t$  specific to an individual, an effect  $\epsilon_i$  specific to the time period, and a remainder  $\epsilon_{it}$  with zero expectation. In this framework current earnings are taken to be the sum of a polynomial in age and/or an

<sup>5</sup>I have also experimented with two other comparison groups. In one case dropouts from the program were used as a comparison group for completers of the program. In another case, trainees entering training in 1967 were used as a comparison group for the 1964 cohort of trainees. Both schemes led to large estimates of the effect of training on earnings, but in both cases the internal checks for similarity of the trainee and comparison group earnings structures that I report below for the CWHHS comparison group led me to conclude that while the CWHHS was far from ideal in this regard, it was more satisfactory for the 1964 cohort than were the alternatives. I am not so convinced that this would be the case with later cohorts where the selection criteria for program entrance had changed from enrollment of those most likely to be successful to enrollment of the disadvantaged worker.

autoregression in earnings plus the error components comprising fixed and random effects. The fixed effect  $\epsilon_i$  presumably captures such factors as ability, motivation, or other previous investments in human capital by a specific worker, while the effect  $\epsilon_t$  captures economy-wide movements in earnings. To the extent that these error components are removed in the estimation process it is worth observing that fitting equation (1) does not require explicit measurement of schooling level or any of the other unchanging variables usually taken to determine earnings. The effects of these variables on earnings are already captured in equation (1).

There are at least three alternative ways to rationalize the use of equation (1) as the basis for a predictor of earnings. At the most rudimentary level, surely any theory of the determination of earnings will imply that current earnings are the result of a variety of historical factors, such as education, experience, social class, and others, that influence earnings capacity. Moreover, good summary measures of this cumulative experience for a worker are surely his age and previous earnings. In effect, equation (1) exploits these rudimentary notions.

Alternatively, one may inquire as to whether equation (1) can characterize the known facts about the structure of earnings. One of these facts, for example, is the finding that over a wide range of the age distribution earnings increase with age, but at a decreasing rate.<sup>6</sup> It is obvious that the polynomial in (1) can accommodate these facts, but it is easy to see that the autoregression can do so also. Consider, for example, the first-order autoregression  $y_t = \alpha + \beta y_{t-1}$ . This difference equation has the solution  $y_t = [y_0 - (\alpha/(1-\beta))] \beta^t + (\alpha/(1-\beta))$ . For  $0 < \beta < 1$  and  $y_0 < \alpha/(1-\beta)$ , earnings approaches the asymptote  $\alpha/(1-\beta)$  gradually from below in just the manner of the empirical age-earnings profiles so often observed.<sup>7</sup>

Finally, equation (1) may also be rationalized as the end result of an optimal investment

program in human capital by individual workers. Rosen (1976) has called equations like (1) earnings generating functions, and one might reasonably characterize the emerging literature on the theory of optimum post-schooling investment as an attempt to define the restrictions on equations like (1) that arise if individuals are behaving so as to maximize  $\sum_t y_t(1+r)^{-t}$ , the discounted value of lifetime earnings at the discount rate  $r$ . To see that equation (1) is consistent with such theories consider the income accounting equation so widely used in the analysis of human capital investments,

$$y_t = y_0 + \sum_{i=0}^{t-1} r_i c_i - c_t, \quad (2)$$

where  $r_i$  is the average rate of return on the dollar investments  $c_i$  in the  $i^{\text{th}}$  period.<sup>8</sup> The sum  $y_0 + \sum r_i c_i$  is potential earnings in the  $t^{\text{th}}$  period and is greater than actual earnings by the dollar costs of current investments,  $c_t$ . An optimal path for the accumulation of human capital implies optimizing paths for the  $r_i$  and  $c_i$ . Suppose first that these may each be approximated by polynomial functions in  $i$ . It is then an easy matter to show that (2) will take the form of a polynomial in age.<sup>9</sup> Alternatively, suppose that the sequences  $r_i$  and  $c_i$  may each be approximated by a weighted sum of power functions. It is then an easy matter to show that (2) takes the form of the solution of a difference equation that is the equivalent of the autoregressive component of (1).<sup>10</sup> Of course,

rentheses), and a coefficient of determination ( $R^2$ ) of 0.716. This implies a static age-earnings profile of  $Y_t = \$4,118. + (Y_0 - 4,118.) (.83)^t$  which has the "typical" age-earnings shape.

<sup>8</sup>This accounting equation plays a large role in Becker's (1964) seminal work.

<sup>9</sup>Put  $r_i = \sum a_m(i)^m$  and  $c_i = \sum b_n(i)^n$  so that  $r_i c_i = \sum d_j(i)^j$  where  $d_j = \sum a_m b_n$ . Substituting into (2) then gives

$$\begin{aligned} y_t &= y_0 + \sum_{i=0}^{t-1} \sum d_j(i)^j - \sum b_n(t)^n \\ &= y_0 + \sum d_j \sum_{i=0}^{t-1} (i)^j - \sum b_n(t)^n \\ &= y_0 + \sum d_j \sum_{k=1}^{j+1} \delta_k(t)^k - \sum b_n(t)^n, \end{aligned}$$

where the  $\delta_k$  coefficients are given implicitly by the formulas for the sum of the powers of the first  $t-1$  integers, which is simply a polynomial in age.

<sup>10</sup>Put  $r_i = \sum d_m(\lambda_m)^i$  and  $c_i = \sum b_n(\mu_n)^i$  so that  $r_i c_i$

<sup>6</sup>See the extensive discussion by Mincer (1974).

<sup>7</sup>As an experiment, the fitted results of a regression of earnings in 1964 on earnings in 1963 in the CWS comparison group for white males gives  $\alpha = \$700$  (9.7),  $\beta = 0.83$  (.003) (with estimated standard errors in pa-

the fact that the data are consistent with equation (1) is not a test of models of optimum post-schooling investment unless there are further restrictions deduced from the theory that may be imposed onto equation (1). After all, polynomial and/or power function approximations to the sequences  $r_i$  and  $c_i$  exist even if the latter do not result from an optimizing model. Nevertheless, it is important to observe here that equation (1) is not a priori inconsistent with such models.

### B. The Effect of Training

To examine the effect of training on earnings it is convenient to re-write the  $K^{\text{th}}$ -order difference equation as a first order difference equation using the matrix notation  $z_t = [y_t, y_{t-1}, \dots, y_{t-k+1}]'$ ;  $z_{t-1} = [y_{t-1}, y_{t-2}, \dots, y_{t-k}]'$ ;  $d_{it} = [\alpha + \sum_j \beta_j'(A_i + t) + \epsilon_i]\gamma$ ; where  $\gamma$  is a  $k$ -component vector with a unity in the first row and zeros in the remaining  $k-1$  rows;  $b_i = \epsilon_i\gamma$ ; and  $u_{it} = \epsilon_{it}\gamma$ . Letting  $B$  represent a matrix of order  $(k \times k)$  with the elements of  $\beta_i$  as its first row and units below the diagonal, we may write equation (1) for the comparison group as

$$z_{it}^c = Bz_{i(t-1)}^c + d_{it} + b_i + u_{it}, \quad (3)$$

and for the trainee participant group as

$$z_{it}^p = Bz_{i(t-1)}^p + d_{it} + b_i + R_t + u_{it}, \quad (4)$$

where  $R_t$  is the incremental effect of training on trainee earnings in the  $t^{\text{th}}$  period. Of course,  $R_t = 0$  in the periods prior to training and it is likely that  $R_t < 0$  during the training period. Equation (1) may now be read off of the top row of (3) or (4).

In this framework the amount by which the earnings of a trainee in the  $t^{\text{th}}$  period are greater than they would have been in the absence of training cannot be obtained from equation (4) without further manipulation

$= d_j(\delta_j)'$  where  $d_j = a_m b_n$  and  $\delta_j = \lambda_m \mu_n$ . Substituting into (2) then gives

$$\begin{aligned} y_t &= y_0 + \sum_{i=0}^{t-1} \sum d_j(\delta_j)^i - \sum b_n(\mu_n)^i \\ &= y_0 + \sum d_j(1 - \delta_j)' / (1 - \delta_j) - \sum b_n(\mu_n)^i \\ &= B' + \sum d_j(\delta_j)^i - \sum b_n(\mu_n)^i, \end{aligned}$$

where  $B' = y_0 + \sum d_j / (1 - \delta_j)$  and  $d_j' = -d_j / (1 - \delta_j)$ , which is the solution of a difference equation like (1) and is thus an alternative expression for it.

because the effects  $R_t$  will cumulate through the earnings generation process. To determine the effect of training on earnings in the  $t^{\text{th}}$  period suppose that it is known that the period prior to the advent of training is the  $(t-s)^{\text{th}}$ . Writing equation (4) repeatedly in lagged form and continuously substituting then gives

$$\begin{aligned} z_{it}^p &= B^s z_{i(t-s)}^p + \sum_{\tau=0}^{s-1} B^\tau [d_{i(t-\tau)} + u_{i(t-\tau)} + R_{t-\tau}] \\ &\quad + \sum_{\tau=0}^{s-1} B^\tau b_i \\ &= B^s z_{i(t-s)}^p + d_{is}^* + R_s^* + b_{is}^* + u_{is}^* \end{aligned} \quad (5)$$

where the second line reflects some obvious notational simplification. The same process applied to equation (3) then gives

$$z_{it}^c = B^s z_{i(t-s)}^c + d_{is}^* + b_{is}^* + u_{is}^* \quad (6)$$

for the comparison group individual. Comparing (5) and (6) it is clear that the term  $R_s^* = \sum_{\tau=0}^{s-1} B^\tau R_{t-\tau}$  is the amount by which earnings are higher for trainees in the  $t^{\text{th}}$  period than would have been the case in the absence of training. In more conventional terms, the discounted present value of the net private benefits of training (to the trainee) is simply  $\sum_{s=1}^{\infty} R_s^* (1+r)^{-s}$  at period  $(t-s)$  when the discount rate is  $r$ .

For estimation purposes we may define the variable  $p_i = 1$  for those who become trainees in the  $(t-s+1)^{\text{st}}$  period and zero otherwise. Then observed earnings for the  $i^{\text{th}}$  individual are

$$\begin{aligned} z_{it} &= p_i z_{it}^p + (1-p_i) z_{it}^c \\ &= Bz_{i(t-1)} + d_{it} + b_i + R_t p_i + u_{it} \\ &= B^s z_{i(t-s)} + d_{is}^* + b_{is}^* + R_s^* p_i + u_{is}^*. \end{aligned} \quad (7)$$

Since  $u_{is}^*$  is uncorrelated with  $z_{i(t-s)}$  by construction and has expectation of zero as well, whether we fit the second line or the third line of equation (7) to the data is a matter of convenience. In the first case we merely regress earnings in each period on earnings in the  $k$  previous periods and include a dummy variable for trainee participation. In the latter case we regress earnings in each period on the earnings in the  $k$  periods prior to training and include a dummy variable for trainee participation. The latter scheme has the advantage that it provides direct estimates of the training effects because the  $R_s^*$  are treated directly as parameters for



estimation. The former scheme requires considerable additional manipulation to obtain the training effects since they must be derived from the parameters of explicit interest and will consequently suffer from additional imprecision in estimation. On the other hand, as we shall see, the former scheme provides a much more convenient framework for handling the fixed individual effects  $b_i$ .

Finally, it should be observed that throughout the preceding discussion the hypothesis is maintained that the earnings generating functions are of the same form for the trainees and the comparison group members. This is a very strong assumption, and it is subjected to some limited tests below. In effect, one advantage of longitudinal data is that we may test the veracity of this hypothesis on the data for periods prior to the advent of training. If we find the earnings generating functions are different for the two groups prior to training this may serve as a signal of serious problems with the maintained hypothesis.

## II. Data and Empirical Results

Table 1 contains sample statistics on the longitudinal earnings records of individuals aged 16 to 64 in four trainee and comparison groups broken down by race and sex. As can be seen from the table, all of the trainee groups suffer considerable declines in earnings in 1964, the year of training, and experience considerable increases in earnings after training. The table also reveals that the earnings of trainees tend to fall, both absolutely and relative to the

comparison group, in the year prior to training. In retrospect this is not very surprising since the Department of Labor was instructed to enroll unemployed workers in the MDTA programs in this period and it is just such workers who would be most likely to want to enter a training program. Nevertheless, this result introduces considerable ambiguity into the empirical analysis for it suggests that some part of the observed earnings increase following training may merely be a return to a permanent path of earnings that was temporarily interrupted by one form of transitory labor market phenomenon or another. To the extent that this is the case the earnings generating functions of the trainee and comparison groups may differ considerably in the period just prior to training and cause considerable ambiguity in untangling the effect of training from the effect of this transitory phenomenon. To make the discussion concrete it is useful to continue in the context of a special case of equation (7).

### A. Initial Estimates

In particular, suppose that  $B=0$  in equation (7) so that  $B^s=0$  also and that  $\beta_j'=0$  for  $j>1$  so that  $d_{it}=[\alpha+\beta_1'(A_i+t)+\epsilon_i]\gamma$ . In this case there is no autoregressive component in earnings and merely a linear effect of age plus the fixed effects for the individual and time period. Although this might be a satisfactory approximation for short periods of time it is unlikely to be satisfactory over longer periods. Still, it is a convenient point of departure,

TABLE 1.—MEAN EARNINGS PRIOR, DURING, AND SUBSEQUENT TO TRAINING FOR 1964 MDTA CLASSROOM TRAINEES AND A COMPARISON GROUP

	White Males		Black Males		White Females		Black Females	
	Trainees	Comparison Group	Trainees	Comparison Group	Trainees	Comparison Group	Trainees	Comparison Group
1959	\$1,443	\$2,588	\$ 904	\$1,438	\$ 635	\$ 987	\$ 384	\$ 616
1960	1,533	2,699	976	1,521	687	1,076	440	693
1961	1,572	2,782	1,017	1,573	719	1,163	471	737
1962	1,843	2,963	1,211	1,742	813	1,308	566	843
1963	1,810	3,108	1,182	1,896	748	1,433	531	937
1964	1,551	3,275	1,273	2,121	838	1,580	688	1,060
1965	2,923	3,458	2,327	2,338	1,747	1,698	1,441	1,198
1966	3,750	4,351	2,983	2,919	2,024	1,990	1,794	1,461
1967	3,964	4,430	3,048	3,097	2,244	2,144	1,977	1,678
1968	4,401	4,955	3,409	3,487	2,398	2,339	2,160	1,920
1969	\$4,717	\$5,033	\$3,714	\$3,681	\$2,646	\$2,444	\$2,457	\$2,133
Number of Observations	7,326	40,921	2,133	6,472	2,730	28,142	1,356	5,192

because it allows a comparison of more sophisticated schemes against one that has been widely used in previous studies.

Now if  $B=0$ ,  $R_s^*=R_t$  in equation (5) and this suggests a very simple estimator for  $R_s^*$ . In particular, equation (7) becomes

$$z_{it} = d_{it} + b_i + R_t p_i + u_{it}. \quad (8)$$

Writing this relationship for period  $t-s$  gives

$$z_{i(t-s)} = d_{i(t-s)} + b_i + u_{i(t-s)}, \quad (9)$$

so that the difference between (8) and (9) is

$$z_{it} - z_{i(t-s)} = (d_{it} - d_{i(t-s)}) + R_t p_i + (u_{it} - u_{i(t-s)}), \quad (10)$$

where  $d_{it} - d_{i(t-s)} = [\beta_1' s + \epsilon_t - \epsilon_{t-s}] \gamma$  and is constant across individuals. According to (10), estimates of the training effects may be obtained by regressing the change in earnings from the period immediately preceding training to the  $t^{\text{th}}$  period on a dummy variable indicating trainee participation. In using this procedure the individual  $b_i$  effects have been fully removed so that the effects on earnings of any variables that are unchanging have also been removed.<sup>11</sup>

Now the period  $t-s$  is supposed to be the period immediately preceding training. However, there is no reason why this period must be used since with  $R_t=0$  in the periods prior to training any base period will do equally well. Suppose, however, that there is a decline of  $T$  dollars in the earnings of trainees relative to the comparison group in the period prior to training. If this decline is permanent, using a base period prior to the period  $t-s$  will understate the training effect by  $T$  dollars. If the decline is transitory, and just offset by an increase of  $T$  dollars in earnings in the sequel, using a base period prior to the period  $t-s$  will give an unbiased estimator of the true training effects. Just the reverse will be the case in these two situations if the base period is taken to be the  $(t-s)^{\text{th}}$ . There does not seem to be any way to remove this ambiguity in the results within

<sup>11</sup> As a referee has pointed out, the  $b_i$  terms could be allowed coefficients different from unity and each other in equations (9) and (10) and equation (11) would be modified only to the extent that  $z_{i(t-s)}$  would appear with a non-zero coefficient on the right-hand side. The disadvantage of this procedure is that the composite disturbance of (11) would then be correlated with  $z_{i(t-s)}$ , so that least-squares would no longer be a consistent estimator for (11).

this framework, and so I have chosen to present results using both assumptions to see empirically how important this difficulty may be.

Estimates of the coefficients  $R_t$  obtained from fitting equation (10) to the data for white males are contained in table 2. As can be seen from the table, the estimates are sensitive to the base period used, varying by nearly \$200 per year from highest to lowest. As expected, using 1963 as the base period produces the largest estimated training effects, although the second and third columns of table 2 indicate some discrepancy between the results using 1962 and 1961 as the base periods as well. Broadly speaking, these results indicate that training may have increased the earnings of white male trainees permanently by between \$500 and \$800 per year, and that foregone earnings were between \$400 and \$600 during the year of training.

TABLE 2.—CRUDE ESTIMATES (AND ESTIMATED STANDARD ERRORS), ASSUMING  $B=0$  AND  $\beta_j'=0$  FOR  $j>1$ , OF THE EFFECT OF TRAINING ON EARNINGS DURING AND AFTER TRAINING, WHITE MALE MDTA 1964 CLASSROOM TRAINEES

Effect in (value of $t$ )	Value of Effects for		
	$t-s=1963$	$t-s=1962$	$t-s=1961$
1962	—	—	91 (13)
1963	—	-179 (14)	-88 (17)
1964	-426 (16)	-605 (18)	-514 (20)
1965	763 (20)	584 (22)	675 (23)
1966	697 (25)	518 (27)	609 (28)
1967	833 (28)	655 (30)	746 (31)
1968	745 (34)	566 (35)	657 (36)
1969	984 (37)	805 (39)	896 (40)

In this framework age enters equations (8) and (9) linearly only, and hence is eliminated from (10). It is a straightforward matter to relax this assumption. Taking the degree of the polynomial in age in (8) and (9) to be  $k'$  implies

that a polynomial in age of degree  $k' - 1$  should enter equation (10). By the usual sum of squared errors criterion  $k' = 3$  seemed satisfactory and this led to a regression of the change in earnings from the period immediately preceding training to the  $t^{\text{th}}$  on a dummy variable indicating trainee participation and a quadratic in age. This procedure is very similar to many of the conventional earnings functions that have been estimated in the literature, but is perhaps an improvement because the individual  $b_i$  effects have been fully removed also. This, of course, is not possible with cross-sectional data alone.

Estimates of the coefficients  $R_t$  obtained from fitting this modified version of equation (10) to the data for white males for the sequence of values of  $t$  between 1962 and 1969 are reported in table 3. Again, there is no reason why the base period,  $t - s$ , cannot be taken to be any of the periods prior to training. As can be seen by comparing tables 2 and 3, the modification in equation (10) changes the estimates of the training effects  $R_t$  in two ways. First, all of the estimates of the training effects are considerably reduced, although they remain significantly greater than zero by the usual statistical criterion. Broadly speaking, the results in table 3 indicate that training may have increased the earnings of white male trainees permanently by between \$200 and \$500 per year, and that foregone earnings were between \$500 and \$700 during the year of training. Second, the results in table 3 differ only within sampling error as between those using 1962 and 1961 as the base years. Clearly the results in table 3 are based on a better specification of earnings determination than those in table 2 and are consequently to be preferred.

### B. Additional Estimates

The specific assumption about the value of the matrix  $B$  used to generate simple estimators is convenient, but nevertheless unsatisfactory. Table 4 contains estimates of equation (7) for white males with  $t - s = 1961$  and values of  $t$  ranging from 1962 through 1969. By the usual statistical criterion a linear term in age seemed adequate with this specification. The estimates in the first row of the table are essentially

TABLE 3.—ESTIMATES, ASSUMING  $B=0$ , OF THE EFFECT OF TRAINING ON EARNINGS DURING AND AFTER TRAINING, WHITE MALE 1964 MDTA CLASSROOM TRAINEES

Effect in (value of $t$ )	Value of Effects for		
	$t - s = 1963$	$t - s = 1962$	$t - s = 1961$
1962	—	—	52 (14)
1963	—	-238 (14)	-186 (17)
1964	-489 (15)	-728 (18)	-676 (20)
1965	605 (19)	368 (21)	419 (22)
1966	496 (25)	258 (26)	310 (27)
1967	532 (28)	294 (28)	345 (29)
1968	341 (33)	103 (33)	155 (34)
1969	472 (35)	234 (36)	286 (36)

estimates of the first row of the second line of equation (7) and they clearly imply that the assumption  $B=0$  is a poor empirical description of the data. These results also imply that the trainee and comparison group earnings functions differ with respect to intercept prior to training and that trainee earnings declined by some \$300 to \$400 in this period.<sup>12</sup>

The broad outline of the results in table 4 is consistent with the structure anticipated for them, although there are some anomalies. For one thing, moving down the columns of the table the coefficients of the lagged earnings variables begin to decay as would be expected from the fact that each successive row of the table is the uppermost row of the matrix  $B^s$ . This process seems to taper off more rapidly than it should, however. The age variable is measured in months and its coefficient should be read accordingly. The fact that these age coefficients are negative is not inconsistent with

<sup>12</sup>I have also tested these equations for structural differences in the other coefficients as between trainees and the comparison group. When the dependent variables are earnings in 1962, or 1966 through 1969, these differences are small, apart from intercepts. However, these results are only slightly reassuring regarding the assumption of equivalent earnings structures for the two groups, and this issue clearly deserves further attention.



TABLE 4.—ESTIMATED REGRESSION COEFFICIENTS (AND ESTIMATED STANDARD ERRORS) OF EQUATION (7) FOR WHITE MALE MDTA TRAINEES

The Dependent Variable is Earnings in	Coefficient of								$R^2$
	Constant	Training Variable	Earnings in					Age	
			1961	1960	1959	1958	1957		
1962	536. (13.9)	- 42.2 (13.0)	.679 (.005)	.115 (.006)	.071 (.006)	.021 (.007)	.031 (.006)	- 1.98 (.434)	.78
1963	1072. (17.0)	- 347. (16.0)	.558 (.006)	.136 (.007)	.086 (.008)	.031 (.008)	.030 (.007)	- 6.70 (.534)	.65
1964	1665. (19.3)	- 907. (18.2)	.465 (.007)	.133 (.008)	.073 (.009)	.043 (.009)	.049 (.008)	- 12.2 (.604)	.55
1965	2405. (21.6)	139. (20.3)	.422 (.008)	.133 (.009)	.077 (.010)	.021 (.011)	.054 (.009)	- 23.4 (.676)	.40
1966	3505. (28.1)	150. (26.5)	.481 (.010)	.173 (.012)	.117 (.013)	.019 (.014)	.090 (.011)	- 41.6 (.881)	.35
1967	4190. (30.0)	138. (28.3)	.424 (.010)	.183 (.013)	.108 (.013)	.009 (.015)	.108 (.013)	- 54.5 (.942)	.28
1968	5268. (35.7)	- 7.3 (33.6)	.447 (.012)	.173 (.016)	.148 (.016)	.018 (.017)	.113 (.015)	- 74.6 (1.12)	.24
1969	6009. (38.0)	62.8 (35.8)	.401 (.013)	.161 (.017)	.143 (.017)	.000 (.019)	.126 (.016)	- 87.9 (1.19)	.20

the notion that earnings increase with age; the implied difference equations must be explicitly solved to examine the age-earnings profile.

One important deficiency of the results reported in table 4 is that no explicit attention is paid to the presence of the individual effects  $b_i$  in the estimation process. It should be made clear that ignoring these effects does not necessarily imply any bias for the estimated training effects, although it does imply inefficiency for the estimation method. There will be bias only if these specific effects are correlated with trainee participation after holding constant age and pre-training earnings levels.<sup>13</sup> Nevertheless, it is possible to examine this issue in somewhat more detail by writing equation (7) for the period  $t-s+1$  as

$$z_{it(t-s+1)} = Bz_{it(t-s)} + d_{it(t-s+1)} + b_i + R_{t-s+1}p_i + u_{it(t-s+1)} \quad (11)$$

<sup>13</sup>Thus, suppose that schooling level is a component of the individual effect  $b_i$ . The least squares estimate of the training effect ignoring schooling will be  $\hat{R}_s^* = \hat{R}_s^* + n(b_i, p_i; z_{it(t-s)}, A_i + t)$ , where  $\hat{R}_s^*$  is the least squares estimate of the training effect when accounting for variation in the  $b_i$  and  $n(b_i, p_i; z_{it(t-s)}, A_i + t)$  is the regression coefficient of the omitted specific effect on the trainee participation variable in a multiple regression that controls for  $z_{it-s}$  and  $A_i + t$ . The point is that this last coefficient is likely to be considerably reduced because  $z_{it-s}$  is controlled, which would not be the case when we assumed  $B=0$  above.

and subtracting from (7) to get

$$z_{it} - z_{it(t-s+1)} = (B^s - B)z_{it(t-s)} + (d_{is}^* - d_{it(t-s+1)}) + (b_i^* - b_i) + (R_s^* - R_{t-s+1})p_i + u_{is}^* - u_{it(t-s+1)} \quad (12)$$

In (12) the individual effects  $(b_i^* - b_i)$  are not zero, but they should be reduced. The results of fitting equation (12) to the data for white males for  $t-s=1960$  for various values of  $t$  are contained in table 5. As can be seen from the table the estimated training effects are increased slightly by this procedure, which is what one would expect if the individual effects  $b_i$  were negatively correlated with trainee status, as seems likely. Moreover, the coefficients of the lagged earnings variables in the successive rows of this table are estimates of the uppermost row of  $B^s - B$  and should approach  $-B$  if the underlying difference equation (1) is not explosive. If these conditions are satisfied, the implication of table 5 is that the coefficients of the lagged dependent variables are badly biased by the omission of the individual effects  $b_i$ , although the training effects do not seem to be severely affected.<sup>14</sup>

<sup>14</sup>Of course, simply fitting the second line of equation (7) in first-differences would eliminate the  $b_i$  effects completely. However, the resulting equation would then

TABLE 5.—ESTIMATED REGRESSION COEFFICIENTS (AND ESTIMATED STANDARD ERRORS) OF EQUATION (12) FOR WHITE MALE MDTA TRAINEES

The Dependent Variable is Earnings in	Constant	Training Variable	Coefficient of					Age	$R^2$
			1960	1959	1958	1957	1956		
1962	420. (14.6)	6.9 (13.6)	-.110 (.005)	.037 (.006)	-.010 (.007)	.015 (.007)	.011 (.005)	-0.199 (.464)	.023
1963	908. (18.1)	-280. (16.9)	-.174 (.007)	.040 (.008)	-.011 (.009)	.015 (.009)	.006 (.007)	-6.55 (.577)	.060
1964	1468. (20.7)	-826. (19.3)	-.242 (.007)	.018 (.009)	-.008 (.010)	.028 (.010)	.011 (.008)	-12.0 (.658)	.123
1965	2187. (23.1)	227. (21.5)	-.272 (.008)	.018 (.010)	-.034 (.011)	.040 (.011)	-.001 (.008)	-23.1 (.733)	.180
1966	3335. (29.2)	230. (27.2)	-.192 (.010)	.060 (.013)	-.032 (.014)	.041 (.014)	.052 (.011)	-42.2 (.928)	.102
1967	3998. (31.3)	227. (29.0)	-.222 (.011)	.045 (.014)	-.048 (.015)	.057 (.015)	.053 (.011)	-55.1 (.993)	.149
1968	5096. (36.7)	79.3 (34.2)	-.217 (.013)	.086 (.016)	-.037 (.018)	.048 (.018)	.074 (.013)	-75.7 (1.17)	.147
1969	5819. (39.2)	156.2 (36.5)	-.260 (.014)	.076 (.017)	-.059 (.019)	.062 (.019)	.072 (.014)	-88.9 (1.25)	.192

### C. Summary Empirical Results

Table 6 draws together the training effects estimated from fitting equation (7) to the data for each of the other three race-sex groups for  $t-s=1961, 1962$ , and  $1963$  and for values of  $t$  from 1962 to 1969. Each of these training effect estimates is from a separate regression, as in table 4, but I have deleted the details of these results to conserve space. Taken together, the results in these tables constitute a summary of the substantive results of the application of the methods described above to the basic data on the 1964 MDTA classroom trainees. The results in the columns headed 1961 take 1961 as the base period year and confirm for all four groups that trainee earnings differed little from comparison group earnings in 1962, given the previous five years of earnings. At the present juncture the training effect estimates in these columns might reasonably be taken as lower limit estimates on the assumption that the earnings declines of trainees in 1963 were transitory and that the trainee groups would

have recovered from them in any event. Not surprisingly, the results in the column headed 1962 and using 1962 as a base earnings year differ only slightly from those in the first column. Likewise, at the present juncture the training effect estimates in the columns headed 1963 that use 1963 as the base year might reasonably be taken as upper limit estimates on the assumption that the earnings declines of trainees in 1963 would not have disappeared in the absence of training.

The conjunction of these results suggests several conclusions. First, all of the trainee groups suffered unpredicted earnings declines in the year prior to training. The estimates of these declines range from \$150 to \$350, being in the lower range for black trainees and the upper range for white trainees. This suggests that simple before and after comparisons of trainee earnings may be seriously misleading evidence on the effect of training on earnings even when a non-random comparison group is available to account for economy-wide earnings changes.

Second, for all groups there do appear to be significant foregone earnings as a result of the training process itself and these must be reckoned with in the calculation of the full social costs of training programs. These foregone earnings estimates fall between \$900

provide estimates only of the terms  $R_t$ , while the estimates of  $R_s^*$  would depend in a complicated way on both the estimated terms  $R_t$  and the estimated matrix  $B$ , as the discussion surrounding equation (6) indicates. Since the terms  $R_s^*$  are the only coefficients of interest here I have not pursued this method.

TABLE 6.—ESTIMATED TRAINING EFFECTS (AND ESTIMATED STANDARD ERRORS) USING DIFFERENT BASE PERIODS FOR VARIOUS GROUPS OF MDTA TRAINEES

Training Effect in	Black Males Base Period is			White Females Base Period is			Black Females Base Period is		
	1961	1962	1963	1961	1962	1963	1961	1962	1963
1962	-18 (21)			-88 (15)			-22 (18)		
1963	-248 (26)	-231 (21)		-317 (20)	-238 (15)		-165 (24)	-146 (19)	
1964	-454 (32)	-439 (29)	-273 (25)	-412 (24)	-349 (22)	-142 (17)	-154 (29)	-139 (26)	-25 (22)
1965	318 (38)	331 (36)	470 (34)	354 (28)	408 (26)	572 (24)	441 (36)	456 (34)	552 (32)
1966	372 (48)	393 (46)	530 (45)	364 (34)	414 (33)	559 (31)	517 (44)	532 (43)	627 (41)
1967	198 (52)	218 (51)	337 (50)	409 (38)	452 (37)	576 (36)	460 (51)	475 (50)	563 (48)
1968	93 (62)	115 (61)	235 (60)	365 (43)	405 (42)	514 (42)	364 (58)	379 (57)	465 (56)
1969	126 (67)	146 (66)	259 (66)	496 (47)	535 (46)	636 (46)	419 (65)	433 (64)	527 (63)

and \$50, being in the upper end of this interval for males and the lower end of this interval for females.

Third, although there remains considerable ambiguity of interpretation, training does appear to have increased the earnings of all trainee groups. For males this effect is between \$150 and \$500 in the period immediately following training, but declining to perhaps half this figure after five years. For females this effect is between \$300 and \$600 in the period immediately following training and does not seem to decline in the succeeding years.

Finally, one may wonder how these crude estimates of the benefits from the MDTA programs in 1964 square up with the costs of these programs. In 1964 federal obligations for MDTA classroom training were around \$1,800 per trainee, but this figure includes a considerable sum for trainee stipends. Assuming that stipend transfer costs differed only slightly from foregone earnings costs suggests that a permanent increase in earnings of perhaps \$180 per year would be necessary for discounted benefits to equal costs at a discount rate of 10%. With the data available it is not possible to verify the satisfaction of this condition with any accuracy, but tables 4 and 6 suggest that it

may be roughly satisfied for the male cohorts and considerably exceeded for the female cohorts.

### III. Concluding Remarks

This paper contains only the barest fragments of the results that might ultimately be obtained from a more complete use of the Social Security earnings records linked with the administrative records from various training programs. What is required for more complete results is a better treatment of evaluation issues in the design of programs and the development of better data and statistical methods. There is a large agenda for further research.

One of the most serious limitations in the use of Social Security earnings records is the truncation of the earnings record at the Social Security taxable maximum. Although this problem is likely to be unimportant for groups of workers with low earnings it is no doubt a serious problem for many groups. One solution to this problem would be to obtain the more detailed quarterly earnings data on trainees that are contained in the original Social Security employer records. Alternatively, statistical methods already exist for handling this problem in the conventional regression context

and surely will be available soon for models with stochastic regressors as in equation (7).<sup>15</sup>

A second difficulty that must be coped with is the obvious problem of the selection bias in program participation that shows up clearly in these results. This problem may be extreme with respect to female trainees whose employment status may be the cause rather than the result of entrance to training. One solution to this problem would rely on more careful sample design with an explicit control on the selection procedure for program participation, but this approach has met enormous resistance by program managers.<sup>16</sup> An alternative approach may be to study the selection procedure more explicitly in the hope of identifying its structure.

Finally, the analysis of the attempt to augment the human capital of workers by post-schooling training programs contains only the smallest contact with the developing literature on human capital accumulation and earnings determination. Structural models where subsidies to training may be traced through for their effects on workers' choices and their implications for the life-cycle of earnings would be useful both for the development of a better framework for empirical work and for their normative implications for public policy.<sup>17</sup>

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<sup>15</sup>One discussion is Amemiya's (1973). Kiefer (1976) reports the results of using this method to examine the effect of training on earnings with a different data set and a model with fixed regressors. An excellent discussion of other problems encountered in using Social Security data in this context is contained in Assembly of Behavioral and Social Sciences of the National Research Council (1974).

<sup>16</sup>Some of the important issues involved are taken up by Cain (1975).

<sup>17</sup>Likewise, the role that these training subsidies play in the more general context of the equilibrium of labor markets deserves attention. See especially the discussion by Johnson (1978).