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How Long Do the Benefits of Training Last? Evidence of Long Term Effects Across Current and Previous Employers, Education Levels, Test Scores, and Occupations

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Abstract

This paper uses NLSY data from 1979-1993 to estimate training's effect on one year wage growth. Year-by-year training histories are constructed which allow the returns to training received at both current and previous employers to vary over time. The time patterns of the returns to training are constructed for both long and short spells of training over nine and three year periods respectively. These returns are then estimated for different demographic groups in order to see how education level, test scores, and occupation influence the payoff to training. Both company training and formal schooling were associated with significant wage growth even nine years after they occurred. Company training was associated with significant wage growth effects irrespective of whether workers changed jobs, although wage growth was higher when the training occurred at a previous employer. Contrary to the conventional human capital model, employers appear to be sharing the costs and returns of general training. While training incidence was lowest for high school dropouts, their return to getting training was the highest. College graduates, in contrast, received the most training but benefited the least. These results suggest an under-supply of training opportunities for low skilled workers.

Keywords

training, effect, growth, employer, education, test score, occupation, wage, job, cost, return, graduate, skill

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WORKING PAPER SERIES

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Working Paper 96-18

How Long Do the Benefits of Training Last?

Evidence of Long Term Effects Across Current and Previous Employers, Education Levels, Test Scores, and Occupations

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ABSTRACT

This paper uses NLSY data from 1979-1993 to estimate training's effect on one year wage growth. Year-by-year training histories are constructed which allow the returns to training received at both current and previous employers to vary over time. The time patterns of the returns to training are constructed for both long and short spells of training over nine and three year periods respectively. These returns are then estimated for different demographic groups in order to see how education level, test scores, and occupation influence the payoff to training. Both company training and formal schooling were associated with significant wage growth even nine years after they occurred. Company training was associated with significant wage growth effects irrespective of whether workers changed jobs, although wage growth was higher when the training occurred at a previous employer. Contrary to the conventional human capital model, employers appear to be sharing the costs and returns of general training. While training incidence was lowest for high school dropouts, their return to getting training was the highest. College graduates, in contrast, received the most training but benefited the least. These results suggest an under-supply of training opportunities for low skilled workers.

I. Introduction

From both a public policy and a theoretical perspective, interest in training issues has remained strong in recent years. Proponents of training reform cite a need to upgrade the nation's skills in order to compete more effectively in the global economy. Critics, on the other hand, are skeptical of the need for a dramatic overhaul of the nation's training systems and warn against becoming overly reliant on formal training systems. From a theoretical vantage point, however, much of the interest in training stems from the long-standing debate over what explains wage variation between workers and the tendency for wage profiles to slope upward over time. While human capital theory remains the dominant theoretical model, its treatment of the division of the costs and returns to training by workers and firms has come under increasing scrutiny,

In undertaking this research, I hope my, analysis will contribute to both of these debates. Three primary questions are addressed. Specifically, how long do the benefits of training last and how portable is training from one employer to another? Furthermore, how substantially do the returns to training van- when respondents are categorized according to their education level, test scores, and occupation? By using a pooled sample from the National Longitudinal Survey of Youth (NLSY) and estimating training's effect on one year wage growth, I construct year-by-year training histories for each respondent using data from 1979-1993. While previous training studies assume training has a single, time variant effect, my method allows the returns to training received at both current and previous employers to vary over time. The time patterns of the returns to training are constructed for both long and short spells of training over nine and three year periods respectively.

Before continuing, a number of interesting findings deserve mentioning. First, long spells of company training as well as formal schooling were associated with significant wage growth effects even nine years after they occurred. For such training, no depreciation was observed; rather, the benefits increased steadily over time. Second, while long spells of company training were associated with significant wage growth whether or not workers changed jobs, the wage effects were considerably larger when the training was received at a previous employer. Contrary to the conventional human capital model, employers appear to be sharing the costs and returns of general training. Third, the wage impacts of training varied considerably for different demographic groups, particularly for respondents with different education levels. While training incidence was lowest for high school dropouts, their return to getting training was the highest. College graduates, in contrast, received the most training but benefited the least.

II. Background & Motivation

Estimating Training Depreciation Rates: The prediction that past training should have a positive effect on wage growth has received widespread empirical support (Lynch, 1992; Veum, 1995; Blanchflower and Lynch, 1994: Lengermann, 1996a). While wage growth studies have demonstrated that the magnitude of this effect varies considerably depending on factors like the type of training and who paid for the training, most assume training has a single, time invariant effect. According to this logic, the occurrence of an event of training simply shifts the wage function up by some proportion. However, does the occurrence of training change wages permanently or instead only temporarily" When one considers a longer time interval it seems quite likely that training enhances wages only temporarily before depreciating over time. This seems especially likely if the work environment is changing rapidly or if the worker obtains a new job or position where past training is less useful.

Lillard and Tan (1986) conducted one of only two studies that estimated training depreciation. Using the young men I cohort of the NLS, they construct time paths for the earnings effects of company training, business and technical training, and regular schooling. Company training had the largest effect on earnings, an effect estimated to last for 13 years. One year gestation effects were observed for business and technical training and regular schooling. Both effects were small in the initial period after training, grew to a maximum after one year, and then subsequently depreciated over nine and eight year periods respectively. However, the Lillard and Tan (1986) estimates are imperfect for a number of reasons. First, their training coefficients are probably biased upwards because they are calculated in cross sectional models. Additionally, Lillard and Tan (1986) estimate a constant annual depreciation rate for each type of training. Thus, training received 8 years ago is assumed to depreciate at the same rate as training received just one or two years ago. Whether or not training does in fact depreciate at a constant rate is certainly open to debate.

Lengermann (1996a) avoids these problems by comparing the effects of training received between 1989-1990,with the effects of training received between 1991-1992 on 1988-1992 wage growth. Using the NLSY, he also finds evidence of gestation effects for school based training and apprenticeships. Both the significance levels and magnitude of the effects associated with school based training and apprenticeships were much larger when the training was received in 1989-1990. However, company training appeared to depreciate more rapidly than in the Lillard and Tan (1986) study. The drawback to the Lengermann (1996a) study is the relatively short time period it considers. Estimates were not made of how long the wage growth effects of school based training and apprenticeships continue to grow before depreciating. Do these benefits persist for five years" What about ten years" Recent training studies have simply not considered these issues.

While the NLSY contains data on the training experiences of respondents from 1979 to the present, no study has yet attempted to integrate all of this information. It is quite possible. However, to analyze the wage effects of training using time intervals longer than three or four years. Doing so would allow for a better understanding of the longer term effects of training.

How Portable is Training Across Jobs? A second issue that has not been adequately addressed by the literature is the degree to which training is portable across jobs. Put simply, does training received at a previous employer continue to benefit a worker at his or her current employer? Human capital theory predicts that the benefits to general training should be the same irrespective of whether a worker changes jobs. As training becomes more firm specific, however, the returns should diminish or disappear when job change occurs. Because research suggests employers do in fact contribute to the costs of general training counter to the predictions of human capital theory (Baron et a], 1989, Bishop. 1991; Lynch, 1992) an interesting question to ask is how this cost sharing influences the degree of training portability between firms?

Cross sectional studies considering this issue obtained conflicting results. Using CPS data, Lillard and Tan (1986) found that company training and informal on-the-Job-training received at previous jobs had statistically significant effects on earnings at the current job. Lynch (1992), on the other hand, finds company training received at previous jobs to have a non-significant effect on current wage levels. In direct contrast to Lillard and Tan (1986), she concludes that company training is probably more firm specific than general.

Loewenstein and Spletzer (1996a) are the only researchers to consider training portability in models of wage growth. They find the returns to employer paid school based training and outside seminars were much larger when the training was received at a previous employer. The returns to formal company training and vendor training received at previous versus current employers, however, did not differ by nearly as much. These results suggest a possible solution to the question posed above. When employers pay for general training, Loewenstein and Spletzer (1996a) conclude they may subsequently extract some of the returns in exchange. Acknowledging that such an interpretation runs counter to standard human capital theory, the), develop a model in which employers offer new hires a binding future wage guarantee and show that under such a scenario the returns to general training will be shared.

However, the Loewenstein and Spletzer (1996a) results are in no way conclusive. As the explicit costs of the majority of school based training are not employer paid, an interesting test of their conclusion would be to determine whether the same results are obtained for self-financed training or for all training regardless of who paid for it. If this is the case, employer rent extraction may not be the source of the larger worker return to previous Job school based training.

Varying Returns for Different Demographic Groups: College graduates, managers, and professionals generally receive much more training than workers with lower education and fewer skills (Lillard and Tan, 1986; Baron et al, 1993; Royalty, 1996). A related issue, therefore, is how do these differential patterns of skill investment affect the returns to training? Do those who get the most training also benefit the most or are the returns *instead higher for those for whom training is less frequent? The deteriorating labor market for lower skilled workers has led advocates of training reform to call for policies geared exclusively at expanding training opportunities for those with low education and minimal jobs skills. Despite such calls, there is currently little empirical evidence of the returns to skill investments by low skilled workers. Other than Lynch (1992), no other NLSY study has compared the wage growth effects of training for different demographic groups. Evidence of a relatively high return from training received by low skilled workers would certainly suggest an under-supply of training opportunities.

III. The Econometric Model

Derivation of Wage Growth Model: Previous research has found a positive, consistent correlation between past receipt of training and higher wages (Lillard and Tan, 1986; Bowers and Swaim, 1992; Lynch, 1992). Such studies typically utilize a standard cross-section wage equation like the following in which the effects of both current (uncompleted) training and past training are distinguished

(1) $$W_{it} = \beta_k T_{ikt} + \pi_k Z_{ikt} + \theta_t X_{it} + \phi_t X_j + v_i + e_{it}$$

where:

- W_{it} is the log wage for person "i" in time “t”
- T_{ikt} is the total stock of the "k"th type of training for person "i" in year “t”
- β_k is the time invariant coefficient for the total stock of the "k"th type of training
- Z_{ikt} measures the proportion of the week spent in the "k"th type of training for individual "i" over time “t”

π_k is the time invariant coefficient for Z_{ikt}, i.e. the "effect" of the "k"th type of training while it is underway

X_{it} is a vector of background characteristics for individual "i" that may change over time

θ_t is the time variant coefficient of background characteristics which vary over time

X_j is a vector of time invariant characteristics such as race, gender, ethnicity, and test scores

ϕ_t is the time varying coefficient for time invariant background characteristics

v_i is a time invariant error specific to each individual

e_{it} is a mean zero error that is uncorrelated with the explanatory variables and v_i

Unlike many models, the ϕ coefficient measuring the effect of time invariant characteristics such as race, gender, and test scores varies over time. This seems reasonable in light of findings by Veum (1995) and Altonji and Spletzer (1991) showing that the effects of test scores on workers' labor market outcomes, while small at first, appear to grow, in significance over time. While human capital theory says we should expect a negative sign for the π_k coefficient measuring the effect of current training, little support has been found that workers bear substantial portions of the costs of training by accepting a lower wage while being trained (Baron et a]., 1989; Bishop, 1994; Loewenstein and Spletzer 1996a; Lengermann, 1996b).

However, cross section estimates of the wage effects of both past and current training are probably subject to selection bias. Individuals who are either more able or more motivated may be more likely to receive training. If the time invariant person specific error, v_i, is positively correlated with past and current training upward bias in both the β_k and π_k coefficients will result if equation (1) is estimated. Because of the potential biases of examining the impacts of training on wage levels, the causal effects of training are better estimated in models of wage growth. First differencing equation (1) is one method of eliminating the bias caused by v_i and obtaining more accurate estimates of β_k and $\pi_{k.}$ By subtracting equation (1) for year t-s from the same equation for year t, w obtain following wage growth equation is derived:

$$(2) \quad W_{it} - W_{i,t-s} = \beta_k [T_{ikt} - T_{ik,t-s}] + \pi_k [Z_{ikt} - Z_{ik,t-s}] + \theta_t X_{it} - \theta_{t-s} X_{i,t-s} + [\phi_t - \phi_{t-s}] X_j + [e_{it} - e_{i,t-s}]$$

Most wage growth studies utilize a wage growth model similar to equation (2) above. Note that this model assigns a single, time invariant effect, β_k, to each type of training received between years t-s and t. However, it is quite likely that training enhances wages only temporarily before depreciating over time. Equation (2) also does not distinguish training received at previous jobs from training received at the current job.

To allow for a consideration of the time pattern of the wage effects of training and to contrast the effects of training received at previous and current jobs, equation (2) is modified as follows:

$$(3) \quad W_{it} - W_{i,t-1} = \beta_{1k}\mathbf{T}_{ikt} + \ldots + [\beta_{n+1,k} - \beta_{nk}]\,\mathbf{T}_{ikt-n} + \Upsilon_{1k}\,T_{ikt} + \ldots + [\Upsilon_{n+1,k} - \Upsilon_{nk}]\,T_{ikt-n} + \pi_k\,[Z_{ikt-n} - Z_{ik,t-1}] + \theta_t X_{it} - \theta_{t-1} X_{i,t-1} + \phi_t - \phi_{t-1}]\,X_j + [e_{it} - e_{i,t-1}]$$

where:

where:	$\mathbf{T}_{ikt} \ldots \mathbf{T}_{ikt-n}$	measure the receipt of "k"th type of training at the current job for person "i" in each year from year t back to year t-n
	$\beta_{1k} \ldots \beta_{n+1,k}$	are the time invariant coefficients for the "k"th type of current job training received at year t back to year t-n
	$T_{ikt} \ldots T_{ikt-n}$	measure the receipt of "k"th type of training at all previous jobs for person "i" in each year from year t back to year t-n
	$\Upsilon_{1k} \ldots \Upsilon_{n+1,k}$	are the time invariant coefficients for the "k"th type of previous job training received at year t back to year t-n

The training variable, T_{ikt}, used previously to measure an individual's total stock of training at time "t" has been replaced by two series of variables, $\mathbf{T}_{ikt} \ldots \mathbf{T}_{ikt-n}$ and $T_{ikt} \ldots T_{ikt-n}$, which measure year-by-year training incidence from the most recent year, t, back to year t-n. Note equation (3) allows training received in different years to have different wage effects. It also allows for the possibility that training in the current and previous jobs may have different wage effects. Equation (3) provides the basis for all subsequent regression results. While the model is not unduly complicated, this is the first training study, to distinguish the effects of training received in different years and at different jobs.

While equation (3) eliminates the effects of time-invariant individual heterogeneity, unobserved individual characteristics that vary across individuals and over time will not be eliminated. In other words, any coefficient, α_t, associated with the individual fixed effect is assumed equal to unity. In addition, the year-by-year training coefficients are also assumed to be time invariant. Such assumptions are conventional to most studies utilizing a first difference methodology, although to the extent that they do not hold, the training coefficients in equation (4) will not be free from bias.

Training studies by Veum (1995) and Lynch (1992) utilized a two-step correction procedure built upon the advice of Heckman (1979) to control for the possibility that unobservable individual characteristics may vary over time. As neither found any of the Heckman (1979) selectivity terms to be individually significant, I have elected not to employ such a procedure in this analysis. It should be noted, though, that Veum (1995) rejects the null hypothesis that the selectivity terms are jointly equal to zero. This suggests that time-variant individual characteristics do in fact play at least a small role in the selection into training that influences wage growth.

Loewenstein and Spletzer (I 996b) note that unobserved job match characteristics may also bias coefficients measuring the impact of training. If training incidence is positively correlated with individual-job match values--i.e. if workers in high quality Job matches are more likely to receive training--upward bias in the training coefficients will result just as was shown to be the case above for unobserved individual fixed effects. Loewenstein and Spletzer (1996b) also point out that the individual-job match effect may vary over time due to the arrival of belated information. They decompose the job match effect into two parts, a net wage effect of unobserved job match variables, and a net wage effect of belated information.

Even after utilizing a first differencing equation, belated information about job match quality J could still lead to upward biased training coefficients. Loewenstein and Spletzer (1996b) note that this bias can be eliminated if the training variables are replaced by a suitable instrumented value that is correlated with $\mathbf{T}_{ikt} \ldots \mathbf{T}_{ikt-n}$ and $T_{ikt} \ldots T_{ikt-n}$ but not with the belated information. Because they limit their sample to job stayers and do not estimate the year-by-year effects of training, previous job training is excluded from their model. Since previous training has been shown to be correlated with current training, they use it as an instrument for current training. Because my analysis contrasts the year-by-year effects of current and previous job training, I was unable to devise a suitable training instrument to control for belated information bias. This fact should certainly be kept in mind when interpreting my regression results.

IV. Data & Methodology

NLSY Training Questions: Although the NLSY training questions were expanded considerably in 1988, both the older and more recent questions gather information about the starting and ending dates of all training spells across a employers. One can therefore determine whether a spell of training Is completed or uncompleted at the time of the interview and whether past training occurred at a previous or current employer. In the older training questions, potential types of training include business college, nursing programs, vocational technical institutes, apprenticeships, beauty and barber school, correspondence courses, and company training. Up to three spells of these older training questions were recorded for each respondent in every interview year from 1979-1986.

One drawback to the older training questions is that they only recorded training which lasted at least four weeks. The newer training questions do not impose any such length restriction and record information on up to four spells of training per year. The types of training are also somewhat different. The NLSY now distinguishes the following formal training programs: business college, vocational-technical, apprenticeship, correspondence, and company training. "Business" training typically provides instruction for entry level positions in fields such as office work or nursing and should not be confused w`1th business classes in college Or graduate school. Three types of "company" training are distinguished in the newer training questions: formal company training, vendor training, and outside seminars. The NLSY questionnaire defines "vendor" training as "seminars or training programs at work run by someone other than the employer." "Outside seminars" are training in which employees leave their worksite to receive instruction.

The date I choose to assign each spell of training is the average of the start and end dates. However, as the training variables I use in my regression models are based on the incidence of training in each year regardless of the number of spells, a suitable method for dating training on a yearly basis had to be devised. If training occurred in any given year, the date I assign to it is simply the average of 91 the dates of all spells of training that occurred within that year. To distinguish training received at a current or previous employer, the year-by-year training dates are then compared to the date each respondent started his or her current job. Since the NLSY does not explicitly record this information, the job start dates were derived by subtracting respondents' reported tenure from the date at which they were interviewed.

<u>Combining Old and Nev., NLSY Training Data:</u> In order to consider the longer term effects of training using NLSY data, it is necessary to the analyze data from both the older and more recent surveys. The task of combining the old and new NLSY training data was one of the primary obstacles faced in this research. Specifically, three problems had to be addressed: first the abridged format of the 1987 interview prevented any training questions from being asked that year; second, while largely the same, the types of training recorded by the old and new surveys are not identical; third, the old NLSY training questions only recorded information on training lasting four or more weeks.

Although no training data were collected during the 1987 interview, the 1988 survey records information on all training received since 1986. It is thus possible to create the 1987 training variables by using the start and end dates for each spell of training. Spells of training that began and ended in 1987 were labeled as "1987 training," while spells that began and ended in 1988 were labeled "1988 training." Unfortunately, the task of labeling training that began in 1987 but ended in 1988 was not as simple. As a rule of thumb, I chose to divide these spells equally between the two years. The reader should note that the creation of the 1987 training variables in this manner undoubtedly results in imperfect training measures for both 1987 and 1988. One reason for this is that the 1988 NLSY only recorded information on up to four spells of training even though it asks about training that occurred over a two year period If respondents received more than four spells of training between 1986-1988 these additional spells will be missed.

The second problem faced in combining the old and new training data was how to integrate the slightly different types of training measured in the two periods. The 1979-1986 surveys offer additional forms of school based training like nursing school and barber/beauty school, while the 1988-1993 surveys desegregate company training into formal company training, vendor training. and outside seminars. In order to make training in the two periods more compatible, I combine all of the various types into four training variables: apprenticeships, correspondence training, school-based training, and company training. While these four training categories are more general than before, I believe they are sufficiently diverse to permit useful comparisons. School based training consists of vocational and business school training from 1987-1993 and vocational, business school, nursing school, and barber/beauty school training from 1979-1986. The company, training variables from 1987-1993 consist of formal company training, vendor training, and outside seminars. It seems logical to expect that when respondents received such training between 1979-1986 they simply reported it as company training.

The final problem in combining the old and new training data was how to deal with the fact that the 1979-1986 surveys only recorded training that lasted four or more weeks. Clearly, if one were to simply combine the old and new training variables, older training spells would provide an underestimate of the amount of each type of training that actually occurred relative to the recent spells. To avoid this problem I categorize each spell of school based training and company training as either long (four weeks or more) or short (less than for weeks). While it is certainly possible to categorize apprenticeships and correspondence training in a similar fashion, I have elected not to do so. To begin with, most apprenticeships last longer than four weeks. Also, as the following section will demonstrate, the incidence of apprenticeship training was higher in earlier surveys. This suggests the undercounting of apprenticeships to be less of a potential problem than with other types of training. Correspondence training was not a type of training I was particularly interested in studying and it is not subsequently included it any of the regression models.

By categorizing school based and company training as either long or short, there are now five training variables: school based long, school based short, company long, company short, and apprenticeship training. The long training spells are fully compatible across the old and new NLSY surveys, while the short spells are unique to the post-1986 surveys. In 1989, for instance, data is available on 11 years of long training spells (including apprenticeships and correspondence training) and three years of short training spells. Prior to 1987 no information is available for spells of training lasting less than four weeks.

Constructing, Year-By-Year Training Histories: Having found a means to combine the old and new training data, albeit somewhat imperfectly, my next task involved constructing year-by-year training histories for NLSY respondents. In order for these histories to include both long and short spells of training, training histories were constructed for each of the 1987-1993 survey years. In each year, training is categorized as being one year old, two years old, three years old, et cetera. In 1991, for instance, one year old training was all training reported in the 1991 survey, whereas two year old training occurred in 1990. The 1991 survey allows for a training history to be built containing 13 years of long training and five years of short training. Thirteen year old training in 1991 is all training reported in 1979. In contrast, thirteen year old training in the 1993 survey, is all training received in 1981. The training histories have been norm-referenced in terms of the number of years since training occurred in order to permit the pooling together of training data from different survey years. Nontraining variables were also norm-referenced in a similar fashion.

Pooling NLSY data considerably increases the sample size of the regression models. Had we not pooled the data, it was feared that the cell sizes of some of the year-by-year training histories would be too small to yield reliable estimates, particularly when the training variables were further subdivided into previous job and current job training. However, the choice of which survey years to pool together was not immediately, obvious. There is a tradeoff between the number of available years of short training and size of the pooled sample. If we were to pool all of the 1987-1993 surveys (all surveys containing both long and short spells of training), the pooled sample size would be the sum of the samples in all seven surveys, but the number of years of short training common to each survey would be only, one, as only one year of short training is available in the 1987 survey. Pooling Just the 1992 and 1993 survey years, on the other hand, increases the number of years of short training common to both surveys to six but reduces the sample size considerably.

Consequently, I have chosen to pool the 1989-1993 surveys. Doing so a1lows the time patterns of long spells to be considered for up to an eleven year period. The time patterns for short spells of training can be observed over a three year period. While it would have been nice to study the effects of short training over a longer period. it is also likely that the wage effects of short spells of training depreciate more rapidly than long spells. Lengermann (1996b), for instance, found preliminary evidence suggesting that company training depreciated much more rapidly than longer lasting forms of training like apprenticeships and vocational training.

Regression models also include variables measuring respondents' year-by-year enrollment in formal schooling. The construction of these variables follows essentially the same logic as the construction of the training variables described above. Unlike the training questions, however, NLSY formal schooling questions have not changed very much over time, and as a result, the task of creating the schooling variables was considerably easier. At every interview respondents were asked whether or not they attended formal schooling (high school, college, or graduate school) since the last interview. By considering respondents responses to this same question between 1979-1993, year-by-year schooling histories are constructed in an identical fashion to the training history variables.

Current enrollment in each type of training is measured by subtracting the proportion of the week each respondent spends in training in year t-1 from the proportion of the week spent in training in year t.[1] If respondents were enrolled in training which temporarily reduced wages in either year, wage growth calculations will not reflect the change in their regular wages. By differencing the current enrollment training variables we can handle this problem.

All regression models also include variables measuring individual characteristics such as race, ethnicity, years or education,[2] current school enrollment, SMSA residence, and geographic region of residence. Individual ability is further controlled for with the inclusion of respondents' scores on the Armed Services Vocational Aptitude Battery (ASVAB), which Veum (1995) considers a measure a measure of "trainability." Employment characteristics include collective

[1] In the pooled sample of 1989-1993 NLSY surveys, year t-1 ranges from 1988-1992, while year t ranges from 1989-1993.

[2] About five percent of respondents in any given year were found to report fewer years of schooling than they did the year before. To reduce this problem, the variable I choose to include in the regression models is an average of respondents' reported years of schooling in years t-2, t-3, and t-4 (two, three, and four years after their most recent report).

bargaining status, firm size, plant size, employment sector (private, public, or self-employed), and tenure. A variable controlling for job change is also included in all models which should help limit any potential bias caused by unobserved job match effects. For occupations such as waiters, waitresses, child care workers, housekeepers., and cleaners, wage rates may not accurately reflect hourly earnings because large portions of earnings come from tips or non-pecuniary benefits like room and board. Consequently, controls for such occupations are included in all wage models. As the regression models are estimated using pooled data, dummy variables are also included to control for unmeasurable differences in each of the 1989-1993 NLSY survey years.

V. Results

Pooled Sample Training Characteristics: Table 1 contrasts training incidence and duration in the first year of the pooled sample (training received between 1989-1993) with the sixth year (training received between 1984-1988). Since most respondents were interviewed in all five years, they typically contribute five person-year observations. The mean hours and incidence of training in the first year of the pooled sample can simply be interpreted as the mean hours and incidence in any given year between 1989 and 1993. The mean hours and incidence in the sixth year of the pooled sample, on the other hand, can be interpreted as the mean hours and incidence in any given year between 1984 and 1988. The pooled sample of 32,722 person-year observations has been restricted to contain respondents with wage observations in both year one (1989-1993) and year two (1988-1992).[3]

The data indicate a number of interesting patterns. First, while the incidence of school based training lasting four or more weeks decreased from 3.3% per year between 1984-1988 to 1.2% per year between 1989-1993, average annual training incidence increased for company training. As respondents grow older they appear more likely to receive company training and less likely to receive school based training. Also, note that most company training occurs in shorter spells lasting less than four weeks. Nearly eleven percent of respondents received short company training in any given year between 1989 and 1993, much more than any other type of training. Short spells of company training averaged only 34 hours in length compared with 217 hours for long spells. Such a difference suggests the content of long and short company training

[3] Respondents reporting hourly wage rates above $100 an hour or below $1 an hour have also been deleted as well as approximately one hundred respondents who reported wage rates grossly out of line with their other employment information. Farm workers (less than one percent of the original pooled sample) have also been deleted. Of all these restrictions, the requirement that respondents report a wage rate in both periods was by far the most significant, eliminating approximately 25,000 observations from the overall pooled sample.

vanes considerably. This difference is even more pronounced when we compare the average hours per recipient for long school based training with short school based training. Given these differences, the fact that all previous NLSY training studies have failed to distinguish long versus short training would certainly appear to be a commonly shared shortcoming.

Table 1 also provides estimates of the frequency of different types of training over a nine year period for apprenticeships and long spells of school based and company training and a three year period for short spells of school based and company training. Over a mine year period approximately thirteen percent of respondents received long school based training once, five percent received it twice, and only two percent received it three or more times. Over a three year period, approximately 24% of the restricted pooled sample received at least one spell of short company training. This is more than any other type of training even when we move to consider training frequency over a nine year period. In general, less than half of those getting each type of training received it more than once. Repeat occurrences of short school based training and apprenticeship training were especially unlikely.

Table 2 presents estimates of annual training incidence and duration when respondents are categorized according to their education and occupation. As with Table 1, the estimates for the first year of the pooled sample (1989-1993) are contrasted with those from the sixth year (1984-1988). Four educational categories were constructed including those with less than a high school education, high school graduates, those with some post-secondary education. and those with a college degree or more. I chose to create three broad occupational categories: managerial, professional, and technical, service, retail sales, and administrative, and craft, operator, and production.[4] The seven demographic groups and their respective sample sizes are as follows: less than high school (4,112). high school (14,494), some post-secondary education (7,461), college or beyond (6,655), managerial, professional, technical (5,355), service, retail, administrative (8,289), and craft, operator, production (5,148).

Sizable differences emerge when we compare training incidence for different educational groups. In particular, the incidence of company training is dramatically higher for college graduates. Average annual training incidence for short company training was 19% for college graduates compared with 13% for those with some post-secondary schooling, 8% for

[4] The three groups where constructed using the 1980 Census Occupational Codes assigned to every employed NLSY respondent. Their respective Census Codes are as follows; Managerial, Professional, and Technical workers (Occupation Code 003-259); Service, Retail Sales, and Administrative workers (Occupational Code 263-469); and Craft, Operator, and Production workers (Occupational Code 503-859). Respondents are categorized according to the occupation they report in year t-1 (1988-1992).

high school graduates, and only 3% for high school dropouts. Company training and education thus appear to be highly complementary.

Training incidence also varies considerably by occupation. Company training occurred much more frequently for managerial, professional, and technical workers, although this difference is more pronounced between 1989-1993. As one would expect, training incidence among managerial, professional, and technical workers closely parallels that for college graduates. In both time periods service, retail, and administrative workers received more school based training and company training than craft and production workers.

The Time Patterns of the Wage Impacts of Training and the Degree of Inter-Firm Training Probability: Table 3 presents regression estimates of the wage impacts of both formal schooling and school based training. Complete regression results appear *in the appendices following the conclusion of the text. Both cross section and fixed effects estimates are included in order illustrate how the training coefficients change after controlling for unmeasurable individual heterogeneity. For the wage growth models, the dependent variable is the change in the log wage rate between year t (1989-1993) and year t-1 (1988-1992). while in the levels models the dependent variable is simply the log wage rate in year t. The time patterns of the wage effects of school based training lasting four or more weeks as well as formal schooling are estimated over a nine year period, while the effects of shorter spells of school based training are estimated over a three year period. In the fixed effects models. the effects of school based training received at respondents' current and previous jobs are also distinguished. The wage impacts of previous job training are estimated both for when respondents change Jobs immediately after receiving training as well as for when the training occurred three years prior to changing jobs.

Fixed effects training estimates represent the cumulative wage growth effect of training in the current year as well as all preceding years. For *instance, the overall wage growth effect of three year old training presented in Table 3 is simply the sum of the first year, second year, and third year training coefficients. F-tests were then run to determine whether the sum of these year-by-year effects were statistically different from zero. Of course, no such summation was necessary for the levels models. It should be noted that the coefficients for training received two and three years ago have been constrained to yield the same effect. The coefficients for training received four through six years ago and seven through mine years ago have been similarly constrained. Wage growth models were originally run in which the training coefficients were freely estimated but they often bounced around from year-to-year to a much greater extent than seemed plausible. Undoubtedly, some of this variation can be attributed to inter-correlations among the training history variables. By "smoothing out" the training coefficients, the goal was to present more reliable estimates of the year-by-year effects of training. The restricted fixed effects models do not appear to substantially alter the cumulative effect of training over time, only the year-by-year variation. Thus, the nine year cumulative effect of long spells of school based training was almost identical in the restricted and freely estimated models.

Table 3 clearly indicates how unobserved individual fixed effects can bias training coefficients. For school based training, the direction of this bias appears to be unambiguously downward. One year after receiving a long spell of school based training (402 hours on average), the levels model estimates a statistically significant 6.2% (F = 8.29) decline in wages. Over time, this negative effect gradually changes sign so that after rune years the coefficient is significant and positive. By simply relying on cross-sectional models to make inferences about the payoff to school based training, one might conclude that recipients will likely have to wait many years before seeing any positive benefits. Moving to a fixed effects model, however, alters this conclusion, The first year wage decline in the levels model is transformed more a small but positive wage growth effect. While never significant, this effect remains fairly constant over the entire time year period.[5]

Short spells of school based training (lasting only 22 hours on average) do not appear to be associated with any change in wages. Wage effects in the levels and first difference models were both small and statistically insignificant over one and three year periods. From this evidence, brief episodes of skill investment that are de-linked from the work-site do not appear to benefit workers.

In contrast, formal schooling was associated with statistically significant, long term wage growth. After a small decline in the first year, the cumulative wage growth effect of formal schooling grew to a small but significant two percent two years later. No evidence was found that the effect of formal schooling depreciated even nine years after it occurred. The F-statistics on the return to formal schooling both six and nine years after it occurred were larger than for any of the five types of training in any time period. It would certainly appear that individuals who return to school in their middle twenties and thirties benefit from the additional schooling, and that this benefit, while small at first, grows steadily over time.

[5] When the year-by-year formal schooling variables were constructed using the change in reported years of schooling instead of school enrollment, the wage growth effect of a long spell of school based training was statistically significant in every time period. This difference could be explained if respondents confused school enrollment with school based training when answering NLSY questions. However, this does not appear to be the case. In any given between 1989-1993, 9% of respondents enrolled in school, 1.2% received school based training, and only 0.12% reported both. Thus, noisy years of schooling reports may actually be the source of the difference between the regression results.

Differences in the returns to school based training become apparent when we distinguish the wage growth effects of school based training received at previous and current employers, While not significant, the return to school based training appears to be somewhat larger when the training was received at a previous job. This pattern is somewhat similar to the one obtained by Loewenstein and Spletzer (1996a), who found the effects of employer paid school based training were much larger when the training was received at a previous employer. Loewenstein and Spletzer (1996a) conclude that when employers pay for general training, they may subsequently extract some of the returns in exchange, and that consequently, the observed wage growth effects should be higher for such training when workers change jobs.

Earlier, I stated that a good test of Loewenstein and Spletzer (1996a) model would be to see whether the returns to previous job school based training continue to exceed those of current job training when we do not distinguish who paid for it. From Table 3, it would appear that while doing this still produces a similar pattern of results, the statistical power of the coefficients is considerably reduced. Unfortunately, by integrating older and newer NLSY training questions to capture the long term effects of training, I am unable to distinguish who pays for training in its analysis. Otherwise, it would have been interesting to see whether my pattern results changed to more closely resemble those of Loewenstein and Spletzer (1996a).

A number of other factors could also explain the slightly different results obtained by Loewenstein and Spletzer (1996a). The Loewenstein and Spletzer (1996a) measure of previous job training is much more narrow than the one employed in this research. Their pooled sample from the 1988-1991 NLSY only includes respondents that started a new job within one year of 1988 or later. Not only does this restriction likely result in a sample with an above average proportion of high turnover individuals, but respondents are only allowed to contribute information on previous job training that was at most three years old. Furthermore, Loewenstein and Spletzer (1996a) include apprenticeship training in their measure of school based training, while I distinguish apprenticeship's effects. Also, they do not distinguish long and short spells of training.

Table 4 follows the same logic as the preceding table but presents the time patterns of the wage impacts for apprenticeships and long and short spells of company training. In all three cases. the fixed effects model reduced the magnitude of the year-by-year training coefficients. In the levels model, the wage effect associated with apprenticeship training was negative in the first year after training was received before increasing rapidly to a statistically significant, effect of approximately eight percent after six years. However, much of this effect appears to be due to the unmeasurable characteristics associated with the kinds of people that receive apprenticeships rather than the training itself. In the fixed effects model, none of the apprenticeship coefficients are statistically different from zero.

Fixed effects estimates of the time pattern of company, training lasting four or more weeks were much closer to the corresponding cross-section coefficients. For such training, no depreciation was observed. Rather, the benefits increased steadily, over time. Company training was associated with first year wage growth of 4.4% (F = 15.23). After nine years, this effect rose approximately four percentage points to 8.2% (F = 5.25). This effect is considerably larger than the nine year effect of either long spells of school based training or apprenticeship training.

Short spells of company training were also significantly associated with wage growth in the first year after occurred. While the levels model yields approximately an 8% wage increase in both time periods, the size of these effects is reduced. Workers who receive short spells of company training experience immediate benefits but these benefits do not persist.

Again, different patterns emerge when the wage effects of training received at current and previous jobs are distinguished. Although never significant, the wage growth effect of apprenticeship training received on the current job grows considerably over time to approximately twelve percent after nine years. In direct contrast, the wage effects of previous job apprenticeship training become increasingly negative over time. To the extent that these estimates are reliable, staying on the job after receiving apprenticeship training would appear to be a very good idea. The small number of respondents who received apprenticeship training in any given year (approximately 5/10ths of one percent annually) suggests that perhaps these particular estimates should be interpreted with caution.

The problem of inadequate year-by-year cell sizes should be much less of a concern with company training. Long spells of company training were associated with statistically significant wage growth regardless of whether the training was received at a previous or current job. As the estimates in Table 4 suggest company training is highly portable, the frequent assumption that company training is primarily firm specific would appear to be somewhat misguided, contrary to the conclusions of Lynch (1992). Interestingly, wage growth was considerably higher for training received at previous Jobs both immediately, and over time. When respondents changed jobs immediately, company training was associated with wage growth of 8.3% (F = 6.32) in first year in which it occurred. Eight years later, the cumulative effect of a long spell of company training rose to 15.7% (F = 7.71).

In contrast, the associated wage growth effects of current job company training in the first and ninth years were estimated to be 4% (F = 11.22) and 3.8% (F = 0.38) respectively. Thus, not only is the immediate wage growth effect of a long spell of current job company training smaller than if the training were received at a previous employer, but over time the cumulative wage growth neither increases nor remains significant.[6] Even when respondents change jobs three years after receiving company training, the associated wage growth effects are larger than if they had remained at the firm that originally provided the training. The nine year cumulative wage growth effect of company training for respondents who changed jobs three years after getting trained was estimated at 8.2% (F = 3.27).

Loewenstein and Spletzer's (1996a) hypothesis fits this phenomenon very nicely. As employers pay for the overwhelming majority of company training, it could very well be that they limit the wage increases that result from higher worker productivity in order to recoup their investment. According to Loewenstein and Spletzer (1996a), employers offer workers a binding future wage guarantee in order to provide some assurance to workers that they will not extract excessive rents if they choose to remain at the firm instead of quitting. As a result, a marginal increase in worker productivity resulting from an increase in human capital will not cause the employer to pay a higher wage. Ultimately, this sharing of the returns to training makes workers less willing to pay for general training while simultaneously increasing employers' willingness to pay.

Still it should be noted that the same pattern is not observed when we contrast the returns to short spells of company training received at current and previous employers. When respondents changed jobs immediately, after the same training occurred, no significant effect was observed. Because of their brevity, short spells of company training may be poorly signaled to new employers when workers change jobs. According to the asymmetric information models of Bishop and Kang (1988) and Katz and Ziderman (1990), when training is poorly signaled to other employers the resulting effect may be to transform general skills into skills that are essentially firm specific. This argument aside, it may simply be that the skills obtained through short spells of training are in fact more firm specific, irrespective of any asymmetries.

Company training appears to be highly general in nature. In contrast, when we use tenure as a proxy for informal human capital accumulation, it appears that the greater a worker's tenure, the more substantial her loss when she changes jobs. A worker with one year of tenure who changes jobs experiences approximately four percent lower wage growth relative to if she

[6] Because Loewenstein and Spletzer (1996a) use only post-1987 NLSY data, they were able to distinguish the three sub-types of company training: formal company training, vendor training. and outside seminars. While coefficients for formal company training and outside seminars were both larger when received at previous jobs, the difference was much more pronounced for outside seminars.

had not changed jobs.[7] When a worker with ten years of tenure changes jobs, however, this differential increases sharply to approximately seventeen percent. Unlike more formal training programs, the informal training measured by tenure certainly appears to be highly firm specific, just as human capital theory would predict.

Even though the wage growth effect associated with company training is greater if workers change jobs, when the negative effect of forfeited tenure is also considered, total wage growth may often be higher for job stayers. For instance, the same worker with ten years of tenure, who just completed a spell of long company training, suffers a net decline of approximately fourteen percent relative to if she had remained at the firm providing the training.[8] Thus, while the return to formal training may be larger when workers change jobs, this fact should not be interpreted as meaning that it is always best to change jobs after getting trained.

<u>Wage Impacts by Educational Level:</u> Table 5 contrasts the returns to training for respondents with less than a high school education, high school graduates, those with some post-secondary education, and those with a college degree or more. These education levels are measured a year before the time of the current interview (year t-1).[9] The most striking results in this table are the large wage growth effects associated with respondents who have less than twelve years of schooling. While high school dropouts are the least likely to receive any of the five types of training, the results strongly suggest that skill investment by such workers produces sizable benefits. High school dropouts that received a long spell of school based training experienced statistically significant and immediate wage growth estimated at 13.9% (F = 4.31).

Even larger effects were observed for high school dropouts that received apprenticeship training, though again they were not significant. Similarly, the first year effect of long company training was estimated at nine percent for high school dropouts, but only five, four, and two percent respectively for high school graduates, recipients of some post-secondary education, and college graduates. From a social welfare perspective, these results point to an

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[7] The restricted fixed effects model in the appendix is used for these calculations. The wage growth effect of a worker with one year of tenure that gains a second year of tenure is (2 * -0.015) + -0.017 = 0.013. A worker with one year of tenure that changes jobs experiences a -0.009 effect from job change and a -0.017 effect from lost tenure. Thus, the differential wage growth effect for a worker with one year of tenure that changes jobs relative to remaining at the original job is 0.013 - (-0.009 + -0.017) = 0.039. The same logic applies to a worker with ten years of tenure.

[8] The wage growth effect for a worker with ten years of tenure that gams an eleventh year of tenure and receives long company training is ((11 * 0.015) + (10 * -0.017) + 0.044) = 0.039. The wage growth effect for a worker with ten years of tenure receives long company training and then changes jobs is ((10 * -0.017) + -0.009 + 0.083) = -0.096. Thus, the differential return is 0.039 + 0.096 = 0.135.

[9] Regressions were also run using respondents' education level in 1989 as well as an average of respondents reported years of schooling in years t-2, t-3, and t-4, but the pattern of the results was not noticeably different.

underinvestment in training for workers with low amounts of schooling. Approximately thirteen percent of the restricted pooled sample did not graduate from high school.

High school graduates and respondents with some post-secondary education also appear to benefit from most kinds of training, although to a lesser extent than high school dropouts. Company training lasting four or more weeks was associated with statistically significant effects in the first three years for those with some post-secondary), education and in every time period for high school graduates. High school graduates appear to benefit the most from short spells of company training, while the returns to apprenticeship training were greater for those with some post-secondary education. For these workers, three year old apprenticeship training produced wage growth of 12.9% (F = 3.19).

Interestingly, the wage impacts of training were smallest for college graduates. While a larger percentage of college graduates received both types of company training, only the first year of short company training had a significant effect on one-year wage growth. Relative skill shortages and surpluses among different categories of workers appear to play a substantial role in influencing the returns to training.

Table 5 also presents the time pattern of the wage impacts of formal schooling for respondents with different education levels. On the whole, returning to school in one's middle twenties and thirties certainly appears to raise wages for most categories of workers. However, while high school dropouts received sizable benefits from most kinds of training, beyond year one, the cumulative wage growth effects of schooling were negative in years three and six, and essentially zero after nine years. This finding certainly underscores the importance of obtaining a high school diploma in order for additional schooling to raise wages.

Why is it that the returns to training for low skilled workers were so large? Furthermore, as college graduates receive the most training, why do they realize such relatively low rates of return? According to human capital theory the wage distribution is determined by individual abilities or marginal efficiencies of investment in human capital (individual demand curves) and opportunities that depend on the marginal costs of investment (individual supply curves). The intersection of these curves determines both the interpersonal distribution of earnings and the distribution of the rates of return on investments in human capital (Becker, 1975). Mincer (1994) points out an important implication of the human capital framework: "In principle it is possible to detect whether inequality of opportunity or [inequality of] ability dominates the observed inequality in wages. The former would produce a downward sloping scatter of intersections, that is, marginal rates of return would tend to be lower for larger investors in human capital" (pg. 118).

Wage Impacts by Test Scores: Table 6 contrasts the wage impacts of training when respondents are categorized according to their scores on the Armed Services Vocational Aptitude Battery (ASVAB). Over 90% of respondents took the ASVAB as a part of the 1980 NLSY. The ASVAB score used here is an average of four sub-tests (distinguished in the regression models) that measure verbal, mathematical, and mechanical ability, as well as computational speed. Respondents have been categorized into lower, middle, and upper ranges based upon their average score. The lower range includes those who scored more than half a standard deviation below the mean, the middle range includes those who scored within half a standard deviation of the mean, and the upper range includes those who scored more than half a standard deviation above the mean. As would be expected, average educational attainment increased with respondents' test scores. Mean years of schooling for the low, middle, and high ranges are 11.7, 12.9, and 14.5 respectively.

To the extent that the ASVAB is a reliable indicator of ability, Table 6 does not provide any clear indication that high ability workers benefit any more substantially from training than low ability workers. Indeed, the immediate wage growth effect associated with a long spell of company training 9 was almost identical for all three groups: 4.1% (F = 2.38) for the lower range, 4.5% (F = 5.77) for the middle range, and 4.5% (F = 7.40) for the upper range. Over longer time periods, somewhat greater variation is observed, but not to nearly the same extent as was shown to be the case for training recipients with different education levels. Clearly, educational attainment exerts a more direct influence than test scores on the time patterns of wage impacts of different types of training.

Wage Impacts by Occupation: Finally, Table 7 presents the time patterns of the wage impacts of training for respondents when they are divided into three occupational groups: managerial, professional, and technical;, service, retail sales, and administrative; and craft, operator, and production. These groups are defined using the occupations reported by respondents in year t-1 (1988-1992). While this method allows us to estimate the returns to training for workers in their most recent occupation, if the past receipt of certain kinds of training helped workers move into higher paying occupations, measuring occupation at year t-1 instead of some earlier period will probably reduce the size of the training coefficients.

As seems logical, craft, operator, and production workers benefit the most from long spells of school based training. For such workers, the immediate wage effect of long school based training was estimated at 6.5% (F = 3.41). Over time, this effect depreciates somewhat and is no longer significant in any other time period. In the short run, professional workers appear to benefit somewhat more from long spells of company training. In the long run. however, craft workers benefit most substantially from long spells of company training. The nine year wage growth effect for craft workers was a statistically significant eighteen percent, compared to four and five percent for service and professional workers. While the majority of apprenticeships were received by craft workers, the wage growth effects were smaller for craft workers than for the other occupational groups. This result parallels the finding in Table 5 in which college graduates benefited the least from company training despite receiving it most frequently.

VI. Discussion

While this study is the first to distinguish the wage impacts of training received in different years and in different jobs, many important issues fell beyond the scope of my analysis. By integrating the old and new NLSY training questions to study the time pattern of the wage impacts of training, many of the advantages of the newer training data could not be exploited. For instance, while post-1987 NLSY surveys ask about who pays for training, the same question was not asked in the 1979, 1980, 1981, 1985, and 1986 surveys. As a consequence, one disadvantage of this study is that in considering both old and new training questions I could no longer distinguish who paid for training. Studies by Loewenstein and Spletzer (1996a) and Lengermann (1996b) both found that the wage impacts of training vary considerably depending upon whether the training was employer paid or individually financed.

By controlling for individual fixed effects, the first difference equation employed in my regression analysis clearly represents an improvement over past studies that relied only on cross-sectional models. While a first difference model eliminates unobservable fixed effects if they remain constant over time, time variant individual characteristics could still cause some bias in the regression results. Jakubson (1986) describes a number of techniques to test the adequacy of the standard first difference model, but at present these methods lie beyond the scope of my analysis. As noted earlier, belated information about job match quality could possibly exert an upward bias on training coefficients, while measurement error in the training variables could lead to a downward bias. To the extent that these two effects do not cancel out, my analysis will not be free from their influence.

Above all, additional considerations of the differential returns to training received at current and previous jobs are needed in order to determine how robust my findings really are. Regardless, the real world division of the costs and returns to training does not appear to mirror the predictions of standard human capital theory. A host of studies have found evidence that employers do in fact pay for some of the costs of general training. From this study, we now have evidence that workers subsequently realize different returns to training depending on whether or not they change Jobs. Why? While the Loewenstein and Spletzer (1996a) model of a binding wage guarantee certainly fits the pattern of my results, it is just one of a host of competing explanations. Worker liquidity constraints, asymmetric information, federal regulations, network externalities, and rapid technological change all likely contribute as well.

However, few of these explanations have been tested more than once, let alone against one another. Moving to do so would clearly appear to be a logical next step for training research. In my opinion, in addition to Loewenstein and Spletzer (1996a), Acemoglu and Pischke (1996) may be a step in the right direction. By treating quit rates as endogenous, the asymmetric information model they develop is unique in that it generates both high and low training equilibria. The former they liken to the high training outcomes in Germany, while the latter is likened to the low training outcomes in the United States. Such an approach could be adapted to explain different training outcomes at industry and regional levels as well.

Both company training and formal schooling appear to have long lasting effects on wage growth. While the original training programs certainly contribute to this observed effect, it is also likely that formal education and training may serve as a catalyst for the accumulation of more informal on-the-job learning. Thus, while the original training program may in fact become less and less useful over time, one of its side effects may be to stimulate development of additional but as yet unmeasurable skills that compensate for this depreciation. Such a complementarity between formal training and ensuing informal training has already been noted by Loewenstein and Spletzer (1994) in their analysis of the informal training questions added to the 1993 NLSY. As future NLSY surveys become available it will be possible to study the time patterns of the wage effects of formal training in conjunction with informal training. While the majority of human capital investment outside of formal schooling occurs informally, we know surprisingly little about it.

More immediately, however, I plan to estimate the year-by-year effects of training on both earnings growth and the change in hours worked per year. By doing so, I should then be able to construct estimates of the social and private rates of return to training, in which the benefits of training are weighed along with the costs. Previous attempts by Krueger and Rouse (1994), Mincer (1989), and Lengermann (1996b) to construct benefit/cost ratios for different types of training were all forced to make debatable assumptions about the duration of training benefits.

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Table 1: Pooled Sample Training Characteristics
(N=32,722)

	1989-1993			1984-1988			Frequency of Training (9 & 3 Year Periods)		
	Mean Hours Per Year	Percent Receive Per Year	Hours if Received	Mean Hours Per Year	Percent Receive Per Year	Hours if Received	Trained Once	Trained Twice	Trained Twice
School Based Training > 4 Weeks	4.83	1.2%	402	17.38	3.3%	527	12.7%	4.7%	1.8%
School Based Training < 4 Weeks	0.275	1.3%	22	--	--	--	4.7%	0.3%	0.0%
Apprenticeship	1.72	0.5%	344	2.28	0.4%	570	2.1%	0.7%	0.2%
Company Training > 4 Weeks	6.52	3.0%	217	7.35	1.7%	432	11.5%	2.5%	0.6%
Company Training < 4 Weeks	3.03	10.6%	34	--	--	--	15.7%	5.9%	2.1%

Table 2: Pooled Sample Annual Training Incidence by Education Level and Occupation
(mean hours per recipient in parentheses)

	1989-1993					1984-1988		
	School Based >4 Weeks	School Based < 4 Weeks	Apprenticeship	Company > 4 Weeks	Company < 4 Weeks	School Based > 4 Weeks	Apprenticeship	Company > 4 Weeks
Education:								
Less than High School	0.7%	0.6%	0.3%	1.4%	3.0%	1.8%	0.2%	0.6%
	(444)	(22)	(308)	(206)	(24)	(540)	(475)	(301)
High School	1.6%	1.3%	0.6%	2.5%	7.7%	4.0%	0.4%	1.5%
	(401)	(18)	(276)	(214)	(27)	(526)	(544)	(402)
Some Post-Secondary	1.3%	1.4%	0.6%	3.6%	12.8%	4.5%	0.6%	2.2%
	(417)	(19)	(461)	(234)	(30)	(562)	(721)	(434)
College+	0.6%	1.8%	0.2%	4.4%	19.1%	1.5%	0.2%	2.4%
	(292)	(25)	(583)	(208)	(29)	(349)	(525)	(466)
Occupation:								
Managerial, Professional,	0.4%	1.5%	0.2%	4.4%	18.7%	3.0%	0.2%	(442)
Service, Retail, Administrative	1.4%	1.4%	0.2%	2.7%	9.1%	3.9%	0.2%	1.5%
	(419)	(17)	(431)	(191)	(24)	(551)	(659)	(432)
Craft, Operator, Production	1.2%	1.1%	1.0%	2.0%	5.6%	3.0%	0.9%	1.2%
	(365)	(24)	(232)	(260)	(28)	(485)	(579)	(387)

Table 3: Time Patterns of the Wage Impacts of Formal Schooling and School Based Training
(F-statistics in parentheses)

	Year 1	Year 3	Year 6	Year 9
Formal Schooling				
Levels Model	-0.069***	(0.00)	0.027***	0.017**
	(18.60)	(0.00)	(11.16)	(2.59)
Fixed Effects Model	-0.016	0.020**	0.048***	0.053***
	1.48	(4.88)	(25.42)	(30.20)
School Based Training > 4 Weeks				
Levels Model	-0.062***	-0.018	0.009	0.029'
	(8.29)	(1.06)	(0.42)	(7.45)
Fixed Effects Models:				
all training	0.021	0.030	0.021	0.030
	(1.35)	(1.34)	(0.47)	(0.89)
current job training	0.025	0.017	-0.018	-0.005
	(1.23)	(0.20)	(0.10)	(0.01)
previous job training:	0.013	0.029	0.025	0.034
(change jobs immediately)	(0.21)	(0.60)	(0.36)	(0.64)
previous job training:	0.025	0.017	0.013	0.023
(change jobs after 3 years)	(1.23)	(0.20)	(0.09)	(0.26)
School Based Training < 4 Weeks				
Levels Model	0.002	0.019	--	--
	(0.01)	(0.01)		
Fixed Effects Models:				
all training	0.012	-0.005	--	--
	(0.30)			
current job training	0.024	-0.030	--	--
	(0.97)	(0.66)		
previous job training:	-0.043	-0.012	--	--
(change jobs immediately)	(0.68)	(0.04)		
previous job training:	0.024	0.055	--	--
(change jobs after I year)	(0.97)	(1.84)		

*significant at 10% level **significant at 5% level ***significant at 1% level

Table 4: Time Patterns of the Wage Impacts of Apprenticeship and Company Training (F-statistics in parentheses)

	Year 1	Year 3	Year 6	Year 9
Apprenticeship Training				
Levels Model	-0.032 (0.91)	0.034 (1.02)	0.083** (4.47)	0.0730* (4.59)
Fixed Effects Models:				
all training	0.009 (0.10)	0.039 (0.98)	0.026 (0.22)	0.023 (0.13)
current job training	0.012 (0.14)	0.056 (1.02)	0.064 (0.61)	0.117 (1.53)
previous job training: (change jobs immediately)	-0.003 (0.00)	0.018 (0.0.7)	-0.015 (0.03)	-0.049 (0.28)
Company Training > 4 Weeks				
Levels Model	0.041*** (9.28)	0.063*** (17.67)	0.036* (4.03)	0. 101 (26.75)
Fixed Effects Models:				
all training	0.044*** (15.23)	0.053*** (7.61)	0.060** (4.36)	0.0820* (5.25)
current job training	0.040*** (11.22)	0.038* (2.77)	0.044 (1.63)	0.038 (0.41)
previous job training: (change jobs immediately)	0.083** (6.32)	0. 114* (7.22)	0.125** (6.06)	0.157*** (7.71)
previous job training: (change jobs after 3 years)	0.040** (11.22)	0.038* (2.77)	0.049 (1.78)	0.082* (3.27)
Company Training < 4 Weeks				
Levels Model	0.079*** (100.34)	0.078*** (74.46)	--	--
Fixed Effects Models:				
all training	0.021 (10.28)	0.011 (1.08)	--	--
current job training	0.023*** (11.68)	0.010 (0.78)	--	--
previous job training: (change jobs immediately)	0.003 (0.03)	0.004 (0.02)	--	--
previous job training: (change jobs after 1 year)	0.023*** (11.68)	0.024 (1.77)	--	--

*significant at 10% level **significant at 5% level ***significant at 1% level

Table 5: Time Patterns of the Wage Impacts of Training by Education Level (F-statistics in parentheses)

	Year 1	Year 3	Year 6	Year 9
Formal Schooling				
Less than High School	0.075	-0.002	-0.019	0.007
	(1.36)	(0.00)	(0.11)	(0.02)
High School	0.013	0.027	0.083***	0.0700**
	(0.21)	(1.63)	(9.85)	(6.89)
Some Post-	-0.027	0.0250	0.046	0.064*
Secondary Schooling	(1.44)	(2.93)	(8.15)	(14.21)
College+	-0.031	0.014	0.039**	0.050***
	(1.96)	(0.85)	(5.71)	(7.82)
School Based Training > 4 Weeks				
Less than High School	0.139**	0.101	0.165	0.142
	(4.31)	(1.08)	(1.70)	(1.10)
High School	0.013	0.056*	0.029	0.028
	(0.32)	(2.95)	(0.54)	(0.44)
Some Post-Secondary Schooling	0.011	-0.017	-0.005	0.035
	(0.08)	(0.09)	(0.01)	(0.25)
College+	-0.036	-0.093	-0.113	-0.090
	(0.37)	(1.27)	(1.23)	(0.70)
School Based Training < 4 Weeks				
Less than High School	0.035	0.099	---	---
	(0.17)	(0.64)		
High School	0.013	-0.049	---	---
	(0.20)	(1.24)		
Some Post-Secondary Schooling	-0.040	-0.064	---	---
	(0.74)	(1.01)		
College+	0.042	0.070	---	---
	(0.60)	(1.12)		
Apprenticeship Training				
Less than High School	-0.013	0.087	0.215	0.247
	(0.02)	(0.28)	(0.78)	(0.56)
High School	0.024	-0.011	0.001	0.036
	(0.42)	(0.04)	(0.00)	(0.19)
Some Post-Secondary Schooling	0.017	0.129*	0.063	-0.010
	(0.10)	(3.19)	(0.39)	(0.01)
College+	-0.158*	-0.119	-0.193	-0.196
	(2.70)	(0.68)	(1.08)	(0.93)
Company Training > 4 Weeks				
Less than High School	0.092*	0.104	0.066	0.089
	(3.27)	(1. 11)	(0.17)	(0.23)
High School	0.051**	0.059*	0.072*	0.120**
	(7.65)	(3.60)	(2.57)	(4.54)
Some Post-Secondary Schooling	0.044**	0.062*	0.085	0.111
	(4.03)	(2.76)	(2.34)	(2.52)
College+	0.017	0.028	0.022	0.017
	(0.71)	(0.66)	(0.17)	(0.07)
Company Training < 4 Weeks				
Less than High School	0.024	0.014	---	---
	(0.43)	(0.06)		
High School	0.036***	0.037**	---	---
	(10.78)	(4.44)		
Some Post-Secondary Schooling	0.001	-0.017	---	---
	(0.00)	(0.69)		
College+	0.019*	0.006	---	---
	(5.40)	(0 12)		

*significant at 10% level **significant at 5% level ***significant of 1% level

Table 6: Time Patterns of the Wage Impacts of Training by ASVAB Test Scores (F-statistics; in parentheses)

	Year 1	Year 3	Year 6	Year 9
Formal Schooling				
Lower Range	-0.025	0.039*	0.062**	0.064***
	(0.58)	(2.85)	(6.10)	(7.00)
Middle Range	0.018	0.018	0.061***	0.050***
	(0.66)	(1.37)	(13.03)	(9.26)
High Range	-0.033*	0.012	0.034***	0.053***
	(3.06)	(1.01)	(6.83)	(15.17)
School Based Training > 4 Weeks				
Lower Range	0.024	0.030	0.049	0.043
	(0.51)	(0.41)	(0.73)	(0.51)
Middle Range	0.031	0.057	0.022	0.025
	(1.22)	(1.99)	(0.21)	(0.23)
High Range	0.009	-0.001	-0.004	0.022
	(0.08)	(0.00)	(0.01)	(0.13)
School Based Training < 4 Weeks				
Lower Range	0.072*	0.050	--	--
	(2.64)	(0.55)		
Middle Range	0.012	-0.029	--	--
	(0.12)	(0.05)		
High Range	-0.050	-0.038	--	-
	(1.75)	(0.63)		
Apprenticeship Training				
Lower Range	-0.012	0.025	-0.011	0.045
	(0.04)	(0.07)	(0.01)	(0.09)
Middle Range	-0.002	0.026	0.059	0.059
	(0.00)	(0.20)	(0.59)	(0.34)
High Range	0.044	0.073	0.016	-0.005
	(0.88)	(1.17)	(0.03)	(0.00)
Company Training > 4 Weeks				
Lower Range	0.041	0.071	0.115	0.136
	(2.38)	(2.28)	(2.38)	(1.84)
Middle Range	0.045**	0.074**	0.068	0.087
	(5.77)	(5.08)	(1.96)	(2.03)
High Range	0.045***	0.034	0.042	0.068
	(7.40)	(1.58)	(1.07)	(1.92)
Company Training < 4 Weeks				
Lower Range	0.038**	0.012	--	--
	(4.34)	(0.17)		
Middle Range	0.022**	0.026	--	--
	(3.85)	(2.12)		
High Range	0.016*	-0.001	--	--
	(3.17)	(0.01)		

*significant at 10% level **significant at 5% level ***significant at 1% level

Table 7: Time Patterns of the Wage Impacts of Training by Occupation (F-statistics in parentheses)

	Year 1	Year 3	Year 6	Year 9
Formal Schooling				
Managerial, Professional Technical	-0.01	0.012	0.033**	0.037***
	(0.29)	(0.95)	(5.58)	(6.65)
Service, Administrative, Retail Sales	-0.012	0.028*	0.068***	0.080***
	(0.29)	(3.79)	(18.01)	(25.23)
Craft, Operator, Production	-0.048	-0.009	0.030	0.021
	(1.71)	(0.13)	(1.13)	(0.60)
School Based Training > 4 Weeks				
Managerial, Professional, Technical	-0.011	-0.032	-0.035	-0.022
	(0.07)	(0.32)	(0.30)	(0.11)
Service, Administrative, Retail Sales	0.004	0.024	0.014	0.026
	(0.02)	(0.42)	(0.10)	(0.29)
Craft, Operator, Production	0.065*	0.075	0.057	0.055
	(3.41)	(2.16)	(0.84)	(0.72)
School Based Training < 4 Weeks				
Managerial, Professional, Technical	0.028	0.013	--	--
	(0.38)	(0.05)		
Service, Administrative, Retail Sales	-0.008	-0.002	--	--
	(0.06)	(0.00)		
Craft, Operator, Production	0.019	-0.034	--	--
	(0.21)	(0.32)		
Apprenticeship Training				
Managerial, Professional, Technical	0.088	0.137	0.174	0.136
	(1.38)	(1.70)	(1.73)	(0.88)
Service, Administrative, Retail Sales	-0.039	0.030	0.000	0.056
	(0.46)	(0.14)	(0.00)	(0.14)
Craft, Operator, Production	0.030	0.022	-0.053	-0.070
	(0.64)	(0.17)	(0.55)	(0.72)
Company Training > 4 Weeks				
Managerial, Professional, Technical	0.067***	0.071	0.083**	0.050
	(16.08)	(6.31)	(3.87)	(0.93)
Service, Administrative, Retail Sales	0.036*	0.012	0.004	0.039
	(3.35)	(0.13)	(0.01)	(0.38)
Craft, Operator, Production	0.010	0.074*	0.082	0.179**
	(0.14)	(2.73)	(1.61)	(4.93)
Company Training < 4 Weeks				
Managerial, Professional, Technical	0.022**	0.002	--	--
	(5.71)	(0.02)		
Service, Administrative, Retail Sales	0.025**	0.018	--	--
	(4.80)	(0.99)		
Craft, Operator, Production	0.013	0.021	--	--
	(0.62)	(0.67)		

*significant at 10% level **significant at 5% level ***significant at 1% level

Appendix: Fall Regression Results, Entire Sample

This appendix presents the complete regression output--ANOVAs, all training and non-training coefficients, F-tests for significance of cumulative wage growth effects--for the entire pooled sample. For the sake of brevity, complete regression results for when respondents are categorized by education level, test scores, and occupation are not included. As the variables used in the regressions and F-tests may be hard to follow, they are defined below. It should be noted that while the text discussion refers to variables occurring in years 1-9, in the regression runs the same years were labeled as years 0-8 (for instance APO-APS) . Thus "year ill in the text is analogous to "year 01, in the data below, llyear2ll is analogous to "year 1,11 etc. In the restricted fixed effects models, schooling and training received between years 1-2, 3-5, and 6-8 are constrained to have the same effect. Thus, the variables for apprenticeship training, for example, are denoted as APO, AP12, AP35, AP68.

Non-training variables besides those measuring race, gender, test scores, and the year-by-year dummies are constructed differently in the levels and fixed effects models. In the levels models, such variables often end with the number 11 0, 11 indicating that that they are measured at year 0 (for instance, NSMSAO) . In the fixed effects models, the same variables are constructed as differences between year 0 and year 1 and consequently do not end with a number. For example, the variable NSMSA in all fixed effects models denotes the change in the variable between year 1 and year 0.

<u>Variable Names in Regression Output:</u>

INTERCEP = intercept
BLCK = black
MTH = ASVAB--math
MCH = ASVAB--mechanical
NSMSA = not in an SMSA
NC = north-central region
ENRSCH = currently in school
SCHOOL = attended school
SELFEE = self-employed
JCHG = job change
BIGEST = plant size > 100
Y90-Y93 = year-by-year dummies
WAITRE = waiter/waitress
APSH = currently in apprenticeship
AP = apprenticeship training
CMSHT = company training < 4 weeks
SCSHT = school based tr. < 4 weeks
FEMALE = female
HIS = Hispanic
CMPT = ASVAB--computational speed
VERB = ASVAB--verbal
NE = northeast region
WS = west region
SCAVG432 = avg. schooling, yrs 2-4
GOVTEE = government employee
TENTJR = tenure
GT = firm size > 1000
WGCB = wage set under union contract
CHILD - private child-care worker
COMSH = currently in company training
SCSH = currently in school based tr.
CMLO = company training > 4 weeks
SCLO = school based training > 4 weeks
APPRE = previous job apprenticeship
APCU = current job apprenticeship
CMLPRE = previous job company training > 4 weeks
CMLCU = current job company training > 4 weeks
CMSPRE = previous job company training < 4 weeks
CMSCU = current job company training < 4 weeks
SCLPRE = previous job school based training > 4 weeks
SCLCU = current job school based training > 4 weeks
SCSPRE = previous job school based training < 4 weeks
SCSCU = current job school based training < 4 weeks
WGRATEO = log wage rate, year 0

WGGRWTH = log wage rate, year 0 - log wage rate, year 1

<u>F-Test Names:</u>

SCH01 = sum of the wage growth effects of formal school attendance for years 0 and 1 (the same logic applies to years 0-2, . . . , 0-8).

AP01 = sum of the wage growth effects of apprenticeship training for years 0 and 1 (the same logic applies to years 0-2, . . . , 0-8).

CML001 = sum of the wage growth effects of company training > 4 weeks for years 0 and 1 (the same logic applies to years 0-2, . . . , 0-8).

CMSHT01 = sum of the wage growth effects of company training < 4 weeks for years 0 and 1 (the same logic applies to years 0-2).

SCL001 = sum of the wage growth effects of school based training > 4 weeks for years 0 and 1 (the same logic applies to years 0-2, 0-8).

SCSHT01 = sum of the wage growth effects of school based training < 4 weeks for years 0 and 1 (the same logic applies to years 0-2).

APPRE02 = sum of the wage growth effects of previous job apprenticeship training for years 0-2 (the same logic for years 0-5, 0-8).

APCU02 = sum of the wage growth effects of current job apprenticeship training for years 0-2 (the same logic for years 0-5, 0-5).

CMLPRE02 = sum of the wage growth effects of previous job company training > 4 weeks for years 0-2 (the same logic for years 0-5, 0-8).

CMLCU02 = sum of the wage growth effects of current job company training > 4 weeks for years 0-2 (the same logic for years 0-5, 0-8).

SCLPRE02 = sum of the wage growth effects of previous job school based training > 4 weeks for years 0-2 (the same logic for years 0-5, 0-8).

SCLCU02 = sum of the wage growth effects of current job school based training > 4 weeks for years 0-2 (the same logic for years 0-5, 0-8).

CMSPRE02 = sum of the wage growth effects of previous job company training < 4 weeks for years 0-2.

CMSCU02 = sum of the wage growth effects of current job company training < 4 weeks for years 0-2.

SCSPRE02 = sum of the wage growth effects of previous job school based training < 4 weeks for years 0-2.

SCSCU02 = sum of the wage growth effects of current job school based training < 4 weeks for years 0-2.

SCLCHG05 = sum of the wage growth effects of school based training > 4 weeks if change jobs after year 2 for years 0-5 (the same logic for years 0-8).

CMLCHG05 = sum of the wage growth effects of company training > 4 weeks if change jobs after year 2 for years 0-5 (the same logic for years 0-8).

SCSCHG02 = sum of the wage growth effects of school based training < 4 weeks if change jobs after year 0 for years 0-2.

CMSCHG02 = sum of the wage growth effects of company training < 4 weeks if change jobs after year zero for years 0-2.

Sample: All
Model: Levels
Dependent Variable: WGRATEO
Used in Table(s): 3,4

Analysis of Variance

Source	DF	Sum of Squares	Mean Square	F Value	Prob>F
Model	69	3156.45884	45.74578	286.457	0.0001
Error	30585	4884.28011	0.15970		
C Total	30654	8040.73895			

Root MSE	0.39962	R-square	0.3926
Dep Mean	2.19532	Adj R-sq	0.3912
C.V.	18.20322		

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for HO: Parameter=0	Prob > |T|
INTERCEP	1	0.974023	0.01819017	53.547	0.0001
FEMALE	1	-0.204600	0.00485650	-42.129	0.0001
BLCK	1	-0.053783	0.00635067	-8.469	0.0001
HIS	1	0.011751	0.00704905	1.667	0.0955
MTH	1	0.083765	0.00422338	19.834	0.0001
CMPT	1	0.049043	0.00337457	14.533	0.0001
VERB	1	0.022161	0.00406891	5.447	0.0001
NSMSA0	1	-0.137382	0.00605374	-22.694	0.0001
NEO	1	0.141193	0.00683695	20.651	0.0001
NCO	1	-0.024707	0.00624129	-3.959	0.0001
WSO	1	0.096359	0.00683002	14.108	0.0001
ENRSCH	1	0.019200	0.01532607	1.253	0.2103
SCAVG432	1	0.023618	0.00125230	18.860	0.0001
SCHOOL0	1	-0.069035	0.01600587	-4.313	0.0001
SCHOOL12	1	-0.000210	0.00913399	-0.023	0.9817
SCHOOL3	1	0.007277	0.00873411	0.833	0.4047
SCHOOL4	1	0.011589	0.00848080	1.366	0.1718
SCHOOL5	1	0.027278	0.00816750	3.340	0.0008
SCHOOL6	1	0.017164	0.00795134	2.159	0,0309
SCHOOL7	1	0.009954	0.00775246	1.284	0.1992
SCHOOLB	1	0.017289	0.00666894	2.592	0.0095
GOVTEE0	1	-0.010155	0.00711158	-1.428	0.1533
SELFEE0	1	-0.000110	0.01139524	-0.010	0.9923
TENUR0	1	0.026717	0.00075895	35.203	0.0001
JCHG0	1	-0.019685	0.00659889	-2.983	0.0029
GT0	1	0.014687	0.00504031	2.914	0.0036
BIGEST0	1	0.106224	0.00516007	20.586	0.0001
WGCB0	1	0.139578	0.00646665	21.584	0.0001
Y90	1	0.061507	0.00700258	8.783	0.0001

Y91	1	0.102665	0.00735306	13.962	0.0001
Y92	1	0.138205	0.00752551	18.365	0.0001
Y93	1	0.165401	0.00767005	21.565	0.0001
CHILD0	1	-0.863989	0.04378395	-19.733	0.0001
WAITRE0	1	-0.148848	0.02246523	-6.626	0.0001
COMSH	1	-0.018157	0.02293958	-0.792	0.4286
APSH	1	-0.003460	0.05089103	-0.068	0.9458
SCSH	1	-0.030676	0.04298073	-0.714	0.4754
APO	1	-0.032145	0.03365310	-0.955	0.3395
A.P1	1	0.017366	0.03490772	0.497	0.6188
AP2	1	0.034687	0.03435727	1.010	0.3127
AP3	1	0.053564	0.03390387	1.580	0.1141
AP4	1	0.064787	0.03503709	1.849	0.0645
AP5	1	0.083133	0.03932347	2.114	0.0345
AP6	1	0.079527	0.04118578	1.931	0.0535
AP7	1	0.118053	0.03696517	3.194	0.0014
APB	1	0.072530	0.03384540	2.143	0.0321
CML00	1	0.041380	0.01358423	3.046	0.0023
CML01	1	0.043031	0.01393199	3.089	0.0020
CML02	1	0.063060	0.01500447	4.203	0.0001
CML03	1	0.030756	0.01623748	1.894	0.0582
CML04	1	0.038776	0.01730203	2.241	0.0250
CML05	1	0.035777	0.01782812	2.007	0.0448
CML06	1	0.066526	0.01892594	3.515	0.0004
CML07	1	0.082803	0.01918715	4.316	0.0001
CMLOB	1	0.101516	0.01962770	5.172	0.0001
CMSHT0	1	0.078829	0.00786968	10.017	0.0001
CMSHT1	1	0.069558	0.00853654	8.148	0.0001
CMSHT2	1	0.078452	0.00909148	8.629	0.0001
SCL00	1	-0.062405	0.02161157	-2.888	0.0039
SCLO1	1	-0.045265	0.01768317	-2.560	0.0105
SCL02	1	-0.017895	0.01736976	-1.030	0.3029
SCL03	1	-0.009894	0.01576748	-0.627	0.5309
SCL04	1	-0.008348	0.01457922	-0.573	0.5669
SCLOS	1	0.008774	0.01356284	0.647	0.5177
SCL06	1	0.008016	0.01271434	0.630	0.5284
SCL07	1	0.018689	0.01173322	1.593	0.1112
SCLOB	1	0.028959	0.01061948	2.727	0.0064
SCSHT0	1	0.001982	0.02661795	0.074	0.9406
SCSHT1	1	-0.006923	0.02113155	-0.328	0.7432
SCSHT2	1	0.001985	0.01877898	0.106	0.9158

Sample: All
Dependent Variable: WGGRWTH

Model: Restricted Fixed Effects
Used in Table(s): 3, 4

Analysis of Variance

Source	DF	Sum of Squares	Mean Square	F Value	Prob>F
Model	so	80.24150	1.60483	14.383	0.0001
Error	30604	3414.63452	0.11157		
C Total	30654	3494.87602			

Root MSE	0.33403	R-square	0.0230
Dep Mean	0.05930	Adj R-sq	0.0214
C.V.	563.31304		

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for HO: Parameter=0	Prob > ITI
INTERCEP	1	0.052342	0.01739183	3.010	0.0026
FEMALE	1	-0.014350	0.00492481	-2.914	0.0036
BLCK	1	-0.002372	0.00530345	-0.447	0.6546
HIS	1	0.001821	0.00564388	0.323	0.7469
MTH	1	0.003571	0.00369943	0.965	0.3344
CMPT	1	-0.000125	0.00281506	-0.044	0.9646
MCH	1	-0.004018	0.00413003	-0.973	0.3306
VERB	1	-0.000383	0.00380983	-0.101	0.9199
NSMSA	1	-0.031387	0.01017635	-3.084	0.0020
NE	1	0.057574	0.02250286	2.559	0.0105
NC	1	0.021851	0.02102714	1.039	0.2987
WS	1	0.100246	0.02189421	4.579	0.0001
ENRSCH	1	0.012957	0.01270510	1.020	0.3078
SCAVG432	1	0.001019	0.00103889	0.981	0.3266
SCHOOL0	1	-0.016178	0.01327740	-1.218	0.2231
SCHOOL12	1	0.017833	0.00740455	2.408	0.0160
SCHOOL35	1	0.009588	0.00274480	3.493	0.0005
SCHOOL68	1	0.001474	0.00211876	0.696	0.4867
GOVTEE	1	0.052081	0.00891774	5.840	0.0001
SELFEE	1	-0.039184	0.01056911	-3.707	0.0002
TENUR0	1	0.014477	0.00183622	7.884	0.0001
TENUR1	1	-0.016977	0.00179590	-9.453	0.0001
JCHG0	1	-0.008249	0.00738544	-1.117	0.2640
GT	1	0.001805	0.00307908	0.586	0.5577
BIGEST	1	0.017829	0.00441791	4.036	0.0001
WGCB	1	0.050995	0.00591307	8.624	0.0001
Y90	1	0.015036	0.00580408	2.591	0.0096
Y91	1	-0.015585	0.00613544	-2.540	0.0111
Y92	1	-0.017258	0.00625761	-2.758	0.0058
Y93	1	-0.010399	0.00638533	-1.629	0.1034
CHILD	1	-0.350352	0.03185552	-10.998	0.0001
WAITRE	1	-0.007444	0.01988099	-0.374	0.7081
COMSH	1	-0.014470	0.01894195	-0.764	0.4449

APSH	1	0.049096	0.04101763	1.197	0.2313
SCSH	1	-0.031982	0.03423839	-0.934	0.3503
AP0	1	0.008853	0.02773217	0.319	0.7496
AP12	1	0.015180	0.01763791	0.861	0.3894
AP35	1	-0.004562	0.01396659	-0.327	0.7440
AP68	1	-0.001007	0.01275639	-0.079	0.9371
CML00	1	0.044223	0.01133039	3.903	0.0001
CML12	1	0.004452	0.00817342	0.545	0.5860
CML35	1	0.002348	0.00765205	0.307	0.7589
CML68	1	0.007275	0.00791402	0.919	0.3579
CMSHT0	1	0.020972	0.00653948	3.207	0.0013
CMS12	1	-0.005187	0.00471555	-1.100	0.2713
SCL00	1	0.020908	0.01801581	1.161	0.2458
SCL12	1	0.004407	0.00966849	0.456	0.6485
SCL35	1	-0.002864	0.00612976	-0.467	0.6403
SCL68	1	0.003121	0.00418785	0.745	0.4561
SCSHT0	1	0.012074	0.02212493	0.546	0.5853
SCS12	1	-0.008668	0.01151353	-0.753	0.4515

<u>F-Tests for Significance of Cumulative wage Growth Effects</u>

Dependent Variable: WGGRWTH					
Test: SCHOB	Numerator:	3.3690	DF: 1	F value:	30.1954
	Denominator:	0.111575	DF:30604		Prob>F:0.0001
Dependent Variable: WGGRWTH					
Test: SCH02	Numerator:	0.5448	DF: I	F value:	4.8827
	Denominator:	0.111575	DF:30604		Prob>F:0.0271
Dependent variable: WGGRWTH					
Test: SCH05	Numerator:	2.8362	DF: 1	F value:	25.4195
	Denominator:	0.111575	DF:30604		Prob>F:0.0001
Dependent variable: WGGRWTH					
Test: AP08	Numerator:	0.0145	DF: 1	F value:	0.1301
	Denominator:	0.111575	DF:30604		Prob>F:0.7183
Dependent Variable: WGGRWTH					
Test: AP02	Numerator:	0.1089	DF: I	F value:	0.9763
	Denominator:	0.111575	DF:30604		Prob>F:0.3231
Dependent Variable: WGGRWTH					
Test: AP05	Numerator:	0.0250	DF: 1	F value:	0.2238
	Denominator:	0.111575	DF:30604		Prob>F:0.6361
Dependent Variable: WGGRWTH					
Test: CML08	Numerator:	0.5859	DF: 1	F value:	5.2512
	Denominator:	0.111575	DF:30604		Prob>F:0.0219
Dependent Variable: WGGRWTH					
Test: CML02	Numerator:	0.8491	DF: 1	F value:	7.6099
	Denominator:	0.111575	DF:30604		Prob>F:0.0058
Dependent Variable: WGGRWTH					
Test: CML05	Numerator:	0.4861	DF: 1	F value:	4.3565
	Denominator:	0.111575	DF:30604		Prob>F:0.0369
Dependent Variable: WGGRWTH					
Test: CMS02	Numerator:	0.1208	DF: 1	F value:	1.0829
	Denominator:	0.111575	DF:30604		Prob>F:0.2981

Sample: All (Previous /Current Job Training Distinguished)
Model: Restricted Fixed Effects
Dependent variable: WGGRWTH
Used in Tables: 3, 4

Analysis of Variance

Source	DF	Sum of Squares	Mean Square	F Value	Prob>F
Model	66	81.72074	1.23819	11.096	0.0001
Error	30588	3413.15528	0.11158		
C Total	30654	3494.87602			

Root MSE	0.33404	R-square	0.0234
Dep Mean	0.05930	Adj R-sq	0.0213
C.V.	563.33829		

Parameter Estimates

Variable	DF	Parameter Estimate	Standard Error	T for HO: Parameter=0	Prob > ITI
INTERCEP	1	0.051698	0.01741270	2.969	0.0030
FEMALE	1	-0.014165	0.00492865	-2.874	0.0041
BLCK	1	-0.002287	0.00530652	-0.431	0.6665
HIS	1	0.001906	0.00564580	0.338	0.7357
MTH	1	0.003649	0.00370139	0.986	0.3242
CMPT	1	-0.000225	0.00281655	-0.080	0.9364
MCH	1	-0.003991	0.00413198	-0.966	0.3341
VERB	1	-0.000382	0.00381233	-0.100	0.9202
NSMSA	1	-0.031130	0.01017923	-3.058	0.0022
NE	1	0.057963	0.02251577	2.574	0.0100
NC	1	0.022197	0.02103629	1.055	0.2914
WS	1	0.100196	0.02190002	4.575	0.0001
ENRSCH	1	0.013114	0.01271002	1.032	0.3022
SCAVG432	1	0.000991	0.00103966	0.954	0.3403
SCHOOL0	1	-0.016164	0.01328225	-1.217	0.2236
SCHOOL12	1	0.017976	0.00740875	2.426	0.0153
SCHOOL35	1	0.009612	0.00274732	3.499	0.0005
SCHOOL68	1	0.001546	0.00211953	0.729	0.4658
GOVTEE	1	0.052270	0.00892326	5.858	0.0001
SELFEE	1	-0-039220	0.01057033	-3.710	0.0002
TENUR0	1	0.014872	0.00185497	8.018	0.0001
TENUR1	1	-0.017104	0.00179831	-9.511	0.0001
JCHG0	1	-0.009016	0.00744950	-1.210	0.2262
GT	1	0.001813	0.00307992	0.589	0.5560
BIGEST	1	0.017764	0.00441863	4.020	0.0001
WGCB	1	0.050796	0.00591434	8.589	0.0001
Y90	1	0.014626	0.00580901	2.518	0.0118
Y91	1	-0.016050	0.00614246	-2.613	0.0090
Y92	1	-0.017661	0.00626665	-2.818	0.0048

Y93	1	-0.010898	0.00639403	-1.704	0.0883
CHILD	1	-0.350678	0.03185919	-11.007	0.0001
WAITRE	1	-0.008233	0.01988755	-0.414	0.6789
COMSH	1	-0.014219	0.01901342	-0.748	0.4546
APSH	1	0.049712	0.04153994	1.197	0.2314
SCSH	1	-0.029657	0.03433065	-0.864	0.3877
APPRE0	1	-0.002880	-0.05614574	-0.051	0.9591
APPRE12	1	0.010168	0.02320202	0.438	0.6612
APPRE35	1	-0.010778	0.01735110	-0.621	0.5345
APPRE68	1	-0.011539	0.01553229	-0.743	0.4575
APCU0	1	0.011866	0.03193658	0.372	0.7102
APCU12	1	0.021985	0.02606021	0.844	0.3989
APCU35	1	0.002774	0.02411323	0.115	0.9084
APCU68	1	0.017609	0.02261580	0.779	0.4362
CMLPREO	1	0.082774	0.03294208	2.513	0.0120
CNLPRE12	1	0.015579	0.01392896	1.118	0.2634
CMLPRE35	1	0.003696	0.00978453	0.378	0.7056
CMLPRE68	1	0.010597	0.00921296	1.150	0.2500
CMLCUO	1	0.039935	0.01197382	3.335	0.0009
CMLCU12	1	-0.001072	0.01006995	-0.106	0.9153
CNLCU35	1	0.001940	0.01237829	0.157	0.8755
CMLCU68	1	-0.001815	0.01568672	-0.116	0.9079
CMSPREO	1	0.003321	0.01995024	0.166	0.8678
CMSPRE12	1	0.000208	0.00848541	0.024	0.9805
CMSCUO	1	0.023368	0.00683688	3.418	0.0006
CMSCU12	1	-0.006535	0.00552403	-1.183	0.2368
SCLPREO	1	0.013250	0.02901628	0.457	0.6479
SCLPRE12	1	0.007695	0.01201565	0.640	0.5219
SCLPRE35	1	-0.001209	0.00678889	-0.178	0.8587
SCLPRE68	1	0.003132	0.00439514	0.713	0.4761
SCLCUO	1	0.024687	0.02224581	1.110	0.2671
SCLCU12	1	-0.003880	0.01624448	-0.239	0.8112
SCLCU35	1	-0.011627	0.01458301	-0.797	0.4253
SCLCU68	1	0.004342	0.01443626	0.301	0.7636
SCSPREO	1	-0.043081	0.05218019	-0.826	0.4090
SCSPRE12	1	0.015649	0.01719718	0.910	0.3628
SCSCUO	1	0.023801	0.02411212	0.987	0.3236
SCSCU12	1	-0.026807	0.01487996	-1.802	0.0716

F-Tests for Significance of Cumulative Wage Growth Effects

Dependent Variable: WGGRWTH						
Test: SCH08	Numerator:	3.4410	DF:	1	F value:	30.8375
	Denominator:	0.111585	DF:	30588	Prob>F:	0.0001
Dependent Variable: WGGRWTH						
Test: SCH02	Numerator:	0.5612	DF:	1	F value:	5.0297
	Denominator:	0.111585	DF:	30588	Prob>F:	0.0249
Dependent Variable: WGGRWTH						
Test: SCH05	Numerator:	2.8777	DF:	1	F value:	25.7896
	Denominator:	0.111585	DF:	30588	Prob>F:	0.0001

Dependent Variable:	WGGRWTH				
Test: APPRE08	Numerator:	0.0316	DF: 1	F value:	0.2833
	Denominator:	0.111585	DF:30588	Prob>F:	0.5945
Dependent Variable:	WGGRWTH				
Test: APPRE02	Numerator:	0.0077	DF: 1	F value:	0.0694
	Denominator:	0.111585	DF:30588	Prob>F:	0.7923
Dependent Variable:	WGGRWTH				
Test: APPRE05	Numerator:	0.0037	DF: 1	F value:	0.0329
	Denominator:	0.111585	DF:30588	Prob>F:	0.8560
Dependent Variable:	WGGRWTH				
Test: CMLPRE08	Numerator:	0.8606	DF: 1	F value:	7.7121
	Denominator:	0.111585	DF:30588	Prob>F:	0.0055
Dependent Variable:	WGGRWTH				
Test: CMLPRE02	Numerator:	0.8057	DF: 1	F value:	7.2208
	Denominator:	0.111585	DF:30588	Prob>F:	0.0072
Dependent Variable:	WGGRWTH				
Test: CMLPRE05	Numerator:	0.6763	DF: 1	F value:	6.0611
	Denominator:	0.111585	DF:30588	Prob>F:	0.0138
Dependent Variable:	WGGRWTH				
Test: CMSPRE02	Numerator:	0.0025	DF: 1	F value:	0.0224
	Denominator:	0.111585	DF:30588	Prob>F:	0.8811
Dependent Variable:	WGGRWTH				
Test: SCLPRE08	Numerator:	0.0711	DF: 1	F value:	0.6372
	Denominator:	0.111585	DF:30588	Prob>F:	0.4247
Dependent Variable:	WGGRWTH				
Test: SCLPRE02	Numerator:	0.0664	DF: 1	F value:	0.5954
	Denominator:	0.111585	DF:30588	Prob>F:	0.4403
Dependent Variable:	WGGRWTH				
Test: SCLPRE05	Numerator:	0.0401	DF: 1	F value:	0.3593
	Denominator:	0.111585	DF:30588	Prob>F:	0.5489
Dependent Variable:	WGGRWTH				
Test: SCSPRE02	Numerator:	0.0041	DF: 1	F value:	0.0371
	Denominator:	0.111585	DF:30588	Prob>F:	0.8472
Dependent Variable:	WGGRWTH				
Test: APCUOS	Numerator:	0.1702	DF: 1	F value:	1.5257
	Denominator:	0.111585	DF:30588	Prob>F:	0.2168
Dependent Variable:	WGGRWTH				
Test: APCU02	Numerator:	0.1140	DF: 1	F value:	1.0214
	Denominator:	0.111585	DF:30588	Prob>F:	0.3122
Dependent Variable:	WGGRWTH				
Test: APCU05	Numerator:	0.0680	DF: 1	F value:	0.6090
	Denominator:	0.111585	DF:30588	Prob>F:	0.4352
Dependent Variable:	WGGRWTH				
Test: CMLCU08	Numerator:	0.0453	DF: 1	F value:	0.4058
	Denominator:	0.111585	DF:30588	Prob>F:	0.5241
Dependent Variable:	WGGRWTH				
Test: CMLCU02	Numerator:	0.3062	DF: 1	F value:	2.7442
	Denominator:	0.111585	DF:30588	Prob>F:	0.0976

Dependent Variable:	WGGRWTH				
Test: CMLCU05	Numerator:	0.1821	DF: 1	F value:	1.6318
	Denominator:	0.111585	DF:30588	Prob>F:	0.2015
Dependent Variable:	WGGRWTH				
Test: CMSCU02	Numerator:	0.0873	DF: 1	F value:	0.7828
	Denominator:	0.111585	DF:30588	Prob>F:	0.3763
Dependent Variable:	WGGRWTH				
Test: SCLCU08	Numerator:	0.0006	DF: 1	F value:	0.0053
	Denominator:	0.111585	DF:30588	Prob>F:	0.9421
Dependent Variable:	WGGRWTH				
Test: SCLCU02	Numerator:	0.0219	DF: 1	F value:	0.1962
	Denominator:	0.111585	DF:30588	Prob>F:	0.6578
Dependent Variable:	WGGRWTH				
Test: SCLCU05	Numerator:	0.0113	DF: 1	F value:	0.1013
	Denominator:	0.111585	DF:30588	Prob>F:	0.7503
Dependent Variable:	WGGRWTH				
Test: SCSCU02	Numerator:	0.0734	DF: 1	F value:	0.6578
	Denominator:	0.111585	DF:30588	Prob>F:	0.4173
Dependent Variable:	WGGRWTH				
Test: SCLCHG05	Numerator:	0.0106	DF: 1	F value:	0.0948
	Denominator:	0.111585	DF:30588	Prob>F:	0.7581
Dependent Variable:	WGGRWTH				
Test: SCLCHG08	Numerator:	0.0292	DF: 1	F value:	0.2617
	Denominator:	0.111585	DF:30588	Prob>F:	0.6089
Dependent Variable:	WGGRWTH				
Test: CMLCHG05	Numerator:	0.1989	DF: 1	F value:	1.7822
	Denominator:	0.111585	DF:30588	Prob>F:	0.1819
Dependent Variable:	WGGRWTH				
Test: CMLCHG08	Numerator:	0.3655	DF: 1	F value:	3.2759
	Denominator:	0.111585	DF:30588	Prob>F:	0.0703
Dependent Variable:	WGGRWTH				
Test: SCSCHG02	Numerator:	0.2052	DF: 1	F value:	1.8394
	Denominator:	0.111585	DF:30588	Prob>F:	0.1750
Dependent Variable:	WGGRWTH				
Test: CMSCHG02	Numerator:	0.1972	DF: 1	F value:	1.7673
	Denominator:	0.111585	DF:30588	Prob>F:	0.1837