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Employer-Supported Training in Canada and Its Impact on Mobility and Wages

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Employer-Supported Training in Canada and Its Impact on Mobility and Wages

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Résumé / *Abstract*

Cet article cherche à exploiter l'information sur la formation et les profils d'emploi contenue dans «Le suivi de l'enquête sur les sortants» de Statistique Canada afin de répondre aux trois questions suivantes: 1) quelles sont les caractéristiques des employés formés?; 2) les salaires de ces employés s'en trouvent-ils augmentés?; et 3) est-ce que la formation accentue la persistance de la relation d'emploi?. Je trouve que les travailleurs plus scolarisés sont plus susceptibles d'être formés que les décrocheurs à l'école secondaire, bien qu'il semble clair que les employeurs sélectionnent davantage ceux qui ont des attributs plus favorables. De plus, la formation contribue de façon significative à la croissance salariale des hommes, mais relativement peu à celle des femmes. Finalement, en utilisant un modèle de durée à effets fixes proposé par Chamberlain (1985), je montre que la probabilité conditionnelle que la relation d'emploi se termine est sensiblement réduite pour ceux qui ont été formés, ce qui est cohérent avec l'idée qu'une part des habiletés acquise par le biais de la formation est de nature spécifique à la firme.

Using information on job histories and on two training questions contained in Statistics Canada's Follow-Up to the School Leavers Survey, this paper seeks to answer three basic questions: 1) What are the characteristics of the trainees?; 2) Does the receipt of employer-supported training cause an increase in the wage paid to those young workers?; and 3) Does it improve the degree of job attachment?; I find that more educated young people are somewhat more likely to be trained than high school dropouts although there is strong evidence of selectivity as employers clearly seem to support training for those that have the most "favorable" characteristics. Also, controlling for unobserved individual characteristics, I find that training has a sizeable wage impact for men while the effect is much more modest for women. Finally, results using Chamberlain(1985)'s fixed-effects hazard model show that the conditional probability of the employment relationship being terminated decreases substantially for trainees, which is consistent with the notion that the skills learned by trainees may have a sizeable firm-specific component.

Mots-clés : formation, données longitudinales

Keywords: *training, panel data*

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1 Introduction

To what extent do young people moving from school to the labor market improve upon the skills they learned during formal schooling through on-the-job or off-the-job training? Do these additional skills seem to *cause* higher wages as well as longer employment relationships? Are there any significant differences in outcomes for women compared to men? This paper's purpose is to provide answers to these questions by making use of Statistics Canada's 1991 School Leavers Survey as well as its follow-up done in 1995. Although there are important limitations to the data used, the one major advantage of the School Leavers Survey and of its follow-up is the fact one can construct part of the employment history of each individual. This longitudinal dimension is then repeatedly exploited to control for possible confounding effects. Many studies (e.g. Lynch (1992), Brown (1989), Blanchflower and Lynch (1994)) have looked at the issue of the incidence and the effect of training using either U.S. or British data. However, to the best of this author's knowledge, this is the first opportunity one has of looking at training and its effects with Canadian longitudinal data.

Following Becker (1975)'s analysis, one would expect that any skills that are portable across jobs should be paid at their market-wide value and thus workers should reap the full benefits of any increase in their skill level. Although this particular prediction has generally found strong empirical support, the corollary implication that workers should finance all general training has received little support so far in the literature.[1] Another prediction of standard human capital theory is that firms and workers should share both the costs of and the returns from investments in firm-specific skills (Hashimoto (1981)). The reason is that by doing so, both parties minimize the risk of a costly separation. Hence, as is the case with general training, the wage of trainees should be greater than what they would have earned had they not been trained. While testing that prediction without knowing whether skills are firm-specific or not is problematic, the fact that skills are

[1] However, see Acemoglu and Pischke (1999) and Acemoglu and Pischke (1998) for models rationalizing why firms may finance general skills.

specific to the current employment relationship should nevertheless lead to a decrease in the probability that the employment relationship terminates. Therefore the specificity of the skills is something that can be inferred from results showing that trainees are less mobile than they otherwise would be. On the other hand, acquiring transferable skills should have little effect on the employment hazard: while having more general skills may translate into better alternative employment opportunities, the very fact these skills are general make those workers more productive in their current job as well.[2]

Given the data at my disposal, I will be able to assess two of those predictions: the one about the wage impact of employer-supported training and the effect on job mobility. In addition, I will estimate a model of the determinants of training, again taking into account unobserved individual characteristics. Many U.S. studies, such as Lynch (1992)'s, have generally found that more educated individuals are more likely to be selected by firms for training.

The main results generally show that more educated young people are somewhat more likely to be trained than high school dropouts although there is strong evidence of selectivity as employers clearly seem to support training for those that have the most "favorable" characteristics. Also, controlling for unobserved individual characteristics, training is found to have a sizeable wage impact for men while the effect is more modest for women. Finally, results using Chamberlain (1985)'s fixed-effects hazard model show strong evidence that the conditional probability of the employment relationship being terminated decreases substantially for trainees, which is consistent with the notion that the skills learned by trainees may have a sizeable firm-specific component.

[2]The reason that the prediction concerning the wage attached to firm-specific skills is problematic stems from the fact that firms have strong incentives to renege on any promise they make about post-training wages. In other words, once the investment is made, nothing, absent reputation effects, can prevent the firm from setting the worker's wage at the competitive level. Hashimoto's result on the optimal sharing rule rests on the assumption that both parties can commit not to terminate the relationship. On this point, see MacLeod and Malcomson (1993).

2 The School Leavers' Follow-Up Survey (SLS)

2.1 General Description of Sample Used

In 1991, Statistics Canada collected information on the school and post-school labor market experiences of 9,460 young people aged 18 to 20. One of the main purposes of that survey was to estimate the high school completion rate. The original sample was drawn from the Family Allowances Files, as they were the most complete listings of individuals under the age of 15 in Canada. Five years of Family Allowances Files were used to generate a sampling frame of 18-20 year-olds, and of the 18,000 individuals that were selected to be in the sample 10,782 were successfully traced and 9,460 responded. The interviews took place between April and June of 1991.

In 1994, Human Resources Development Canada commissioned Statistics Canada to re-interview the same individuals in 1995. For that interview, the response rate was 66.8% as 6,284 individuals provided information on their school and labor market experiences. These individuals were thus aged 22 to 24 at the time of the re-interview.

Given the retrospective nature of the Follow-up, the identification of the most important job experiences of respondents relied on the notion of a "reference job". Such a job had to last at least six months and individuals had to work at least 20 hours per week in it. Two such jobs (at most) are documented in the data set, the first one that the individuals had since they were last in school (in high school, junior high, or elementary), and the most recent one. In addition, respondents were probed about the job they held the week before the interview. That job may be the first reference job, or the most recent one, or another job if, for example, the individual has worked full time in it for less than six months. Data on all those jobs are collected on usual hours worked, occupation, industry, tenure, training incidence, and weekly wages. Thus, it is possible for a given individual to have had three separate jobs since leaving high school.[3] However, given that the respondents were aged between 22 and 24 at the time of the re-interview,

[3] Or, to be more precise, three jobs for which we have detailed information. Individuals were also asked how many jobs (any jobs) they had ever held.

one has to be cautious in using the information on all the jobs. The reason is that for many of those individuals, a reference job was held at the same time they were enrolled in a post-secondary institution. Consequently, it may not be entirely appropriate to treat those employment experiences as representative of those of the others who clearly moved full-time into the labor force through those reference jobs after, say, high school graduation.

To at least partly circumvent this data problem, I first eliminate all individuals who report themselves as full-time students at the time of the 1995 interview. Then I make use of a question about each individual's perception of her/his status in each reference job and also in the job held in the week before the 1995 interview. Individuals were asked whether they considered themselves to be mainly students or mainly workers while being employed in one of those jobs. I therefore further restrict the sample to individuals who considered themselves workers even though they may have been enrolled in a post-secondary institution. Also, I eliminate all individuals who, while employed, took any education or training from a school, a college, or a university. While this may eliminate people who genuinely took training programs directly related to their job but that simply happened to be provided by a school, college or university, it does eliminate all those who were simply getting further education as opposed to job related training.[4]

For the job held in the week prior to the interview and for the two reference jobs, I make use of the following questions on training:

1. "Did you take any career or job related education or training such as programs, courses, workshops, seminars and tutorials while you had this [...] job?"

2. "Did your employer pay for, provide transportation, give time off, or give any other support towards the education or training you took while you had this [...] job?"

In the analysis below, I make use of the first question and of the second one

[4]Note that this made only minor differences in the quantitative results and had no impact on the qualitative ones.

interacted with the first one to measure the extent of employer-supported training.[5]

Schooling attainment at the start of each job is determined by using the questions on the starting date of each employment relationship and the date at which individuals report obtaining, say, a B.A. degree. Thus, we have some variation in schooling attainment within each individual's job history which, together with the training dummy, can be exploited in studying the effect of schooling on the probability of getting employer-supported training.

In addition to the above-mentioned sample selection criteria, I also eliminated self-employed workers, multiple job holders, and I deleted all records with missing data on any of the relevant variables. Thus I end up with an unbalanced sample of 5,081 observations for 3,393 individuals.[6]

2.2 Data Limitations

At this point it seems appropriate to point out some of the weaknesses in the data set. Although the longitudinal structure of the SLS and its wide array of personal and family background questions makes it appealing, it does have some significant limitations. One of them is that all information is retrospective in nature given that the follow-up survey asks about jobs that may have begun as long ago as ten years earlier. Thus, measurement errors, especially in the wage and hours data may pose a problem. Secondly, although the questions are limited to the most "important" job experiences to try and limit the extent of the measurement problem just mentioned, there is no time-varying information on jobs. For example, individuals are not asked about the usual wage in a given job at different moments over the course of that employment relationship. Only one wage is recorded per job. One

[5]Note that over one-hundred individuals answered "yes" to the second question after answering "no" to the first one when probed about their training experience during their first reference job. The interaction of the answers to those two questions allows me to get rid of such apparent inconsistencies or measurement errors.

[6]Some records were kept by making imputations. For example, respondents who reported not knowing whether their parents had attended a college or a university were assigned a value of zero for the dummy indicator associated with that variable. Similarly, those not knowing whether they had collected unemployment insurance or welfare benefits were coded as not having received any.

can only presume that the reported wage is the end-of-job wage for past employment relationships, but there is really no way to be sure. The fact that I do not have time-varying information within jobs implies that I am only able to handle unobserved *individual,* as opposed to job-match, characteristics. Third, the training questions are obviously not as precise as one would like them to be: the fact that employers support or not training activities is useful information but it is not a perfect substitute for a direct question on whether employers *paid* for that training, the starting and ending dates of the programs, and the type of program (formal, informal, etc..).[7] In addition, we have no information about the timing of training, contrary to the U.S. National Longitudinal Survey of Youth (NLSY), for example. Consequently, it is impossible to test other hypotheses relevant to both human capital theory and the theory of job matching. For example, Loewenstein and Spletzer (1997) find, using the NLSY, that much employer-provided training is in fact delayed, a finding that goes against the standard theory of human capital in which all training should be front-loaded. However, this empirical regularity is consistent with employers waiting to see whether the job match turns out to be good or not. Unfortunately, I cannot address that question here. Also, an important dimension of training lacking in these data is that of *intensity of training,* as measured by hours per week on a training program. In addition, again contrary to the NLSY or the British National Child Development Survey (NCDS), the data set does not contain any information on the type of training program (on-site tutorials, seminars, etc.). Another issue that has received little attention in the training literature and that cannot be analyzed here is that of functional form assumptions and their impact of the measured wage effect of training (see Frazis and Loewenstein (1999) for an analysis with the NLSY data).

Finally, It is important to remember that the sample used for the analysis includes only individuals who either have had a reference job, that is a job lasting at least six months in which individuals worked at least 20 hours a week, or were working at the time of the follow-up survey (and had been

[7] For a careful analysis of the extent of formal vs. informal training, see Loewenstein and Spletzer (1999)

working in their job for possibly less than 6 months). Consequently, this is a selected group of individuals who may be more representative of people who have a higher degree of labor force market attachment. A possible consequence of such selection may be that it will be harder to detect the effect of observables on the probability of getting training, because of the underlying relative homogeneity of the sample in terms of unobservables.

2.3 Summary Statistics

Table 1 and Figures 1-4 show some simple descriptive statistics documenting the differences in the labor market experiences by schooling attainment.[8] In terms of family background variables, it seems clear that high school graduates come from family with better educated parents than is the case for dropouts (with no post-secondary education) and, also, they performed substantially better when they attended school, as reflected by the much higher proportion of individuals with a B grade point average or better. They also were significantly less likely to have failed a grade in elementary school. This last piece of information suggest that, at least to a degree, poor performances in school precede the process by which students start to contemplate dropping out of high school, instead of the idea of dropping out subsequently affecting school performance. In terms of the distribution of time between the end of schooling and the start of the first reference job, we can see from Figure 1 that high school dropouts, to a greater extent than graduates, started their first full-time job around the time they left school.[9] The graph for high school graduates, on the other hand, seems to suggest that some of them did enroll in some form of post-secondary education after

[8] See Appendix A for a description of the variables used in this paper. The kernel density estimates shown in Figures 1-4 can be seen as representing smoothed histograms. It takes the raw data points, whose plots are somewhat harder to visualize, and allows the main patterns present in the data to more clearly emerge. The cost associated with using kernel densities is that the choice of both the kernel and the bandwith are rather arbitrary which can lead to misspecification. In this application, the choice of a particular kernel doesn't make much of a difference. I use the Epanechnikov kernel but the normal would give pretty much the same picture. As for the bandwith, I experimented with different choices, and over fairly wide range the figures were very similar.

[9] Note that the schooling level I am referring to is the one reported at the time of the 1995 interview.

graduation, but for whatever reason did not complete the requirements and then had to enter the labor market. In fact, the peak in the smallest hump occurs at about 4 years following high school graduation which suggests that they enrolled in a university program but did not complete it. Regarding the two other educational categories, we can clearly see that, by and large, university graduates entered the labor market about 4 years after high school graduation which more or less coincides with the date of graduation from university. In fact, an equivalent way of looking at the transition time of university graduates is illustrated in Figure 3 where I show the time between the end of university and the start of the first reference job. As we can see, most transitions do occur around the time of graduation.

Table 1 indicates that although high school graduates seem to have fared better in terms of the incidence of reference jobs, the same cannot be said for labor earnings (or wages): both groups earn approximately the same on average and the distributions of log weekly wages shown in Figure 4 provide no evidence that high school graduates are doing much better compared to dropouts.[10]

It is interesting to note that the characteristics of dropouts who did pursue post-secondary education are different from the characteristics of the "real" dropouts on one important dimension: they are more likely to come from more educated families although they performed just as poorly in class. In fact, their parents are more educated than those of high school graduates. In terms of having at least one reference job, dropouts with some additional post-secondary schooling are doing slightly better than high school graduates. Therefore, it seems appropriate to separate them out from the other dropouts if one wants to evaluate the differences in labor market performance in general, and training experience in particular, between graduates and dropouts.

Not surprisingly, as both Table 1 and Figure 4 indicate, university graduates are earning a better wage on average, conditional on employment, than

[10] All dollar figures are deflated using the CPI. Since I don't have a "true" longitudinal history of the wage paid in each year on a given job (only one wage figure is reported), I use the end-of-job year as the reference point for deflating all nominal figures.

either one of the other groups. What might be surprising is the fact that a lower percentage of university graduates declares ever having had a reference job. However, this just serves to illustrate one of the limits of the SLS data in terms of analyzing the school-to-work transition of more educated people. First of all, they have been out of school for a shorter time, thus some of them may still be searching. Second, they may be in full-time jobs that began less than six months before the date of the interview, which disqualifies those jobs as being reference jobs.

Looking at training incidence, it appears to be strongly correlated with schooling attainment and one of the tasks in this paper is to try to establish whether there is any causal relationship between education and post-schooling skill acquisition. Finally, Table 2 shows cross-tabulations of the incidence of training by industry-occupation cells. As we can see from the last column of Table 2, the distribution of employment across industries is different for women as compared to men, but it seems that the incidence of training is rather similar across genders. At least it is on average.

2.3.1 Comparison with Data from the 1996 Canadian Survey of Consumer Finance.

To evaluate the representativeness of the School Leavers Survey data, I used the 1996 Survey of Consumer Finance (SCF), a data set that is roughly the equivalent of the March Supplement of the Current Population Survey to compute comparable sample means. I also used the 1996 Canadian Census to compute the fraction of individuals who report having a child at home. Those statistics are reported in Appendix B. Not surprisingly, not all variables present in the more specialized SLS are also available in either one of the cross-sectional surveys.

Although there are discrepancies across data sources, most notably for tenure, the sample averages for hours and weekly wages are roughly comparable, although the higher wages provide some evidence that the workers in the SLS are more positively selected than the average individuals in the population. The fact that the SLS is essentially a longitudinal data set, with

questions which are retrospective in nature, makes it relatively difficult to compare with the "snapshot" aspect of either the SCF or the Census. For example, even though I eliminated full-time students at the time of the interview, the questions on reference jobs in the SLS do include some past employment experiences while enrolled in school, as evidenced more particularly for the more educated individuals in the SLS (see Figure 3). By contrast, the employment data in the SCF shown in Appendix B are computed using individuals who were not enrolled during the previous calendar year (1995).

3 Training Incidence and its Effects

3.1 Individual Characteristics and the Receipt of Training.

Although the descriptive statistics do suggest that more educated individuals seem to benefit from greater training opportunity, the more relevant question to ask is whether the fact they have, say, a high school diploma does indeed *cause* an increase in the training receipt probability. In other words, would the same individuals have received training even in the absence of graduating. If not, this would then suggest that from the individual's point of view, graduating, say, from high school does not give any special advantage in terms of the receipt of training over not completing high school.

To explore that question, I exploit the panel structure of the data set in three different ways. I first estimate a random-effects probit model to study the effect of schooling attainment (and other personal characteristics) at the start of the employment relationship on the incidence of training. Note that I will make use of two 0-1 training variables, one for which the respondents simply answered whether they took any training during the time they were in a reference job, the other will be the previous one interacted with an indicator for the support of that training by the employer.

Let

$$y_{it}^{*} = \beta x_{it} + \alpha_i + u_{it} \tag{1}$$

$$y = 1 \text{ iff } y_{it}^* \geq 0$$

where $y_{it}^*$is a latent index describing the propensity to receive training, α_i is a random variable distributed according to a univariate function H indexed by a finite number of parameters δ, for example a normal distribution function. Assuming independence between the incidental parameters α_i and the x_{it}'s, β can be consistently estimated by maximizing the following log-likelihood function:

$$LogL = \sum_{i=1}^{N} log \int \prod_{t=1}^{T} (F(\beta x_{it} + \alpha)^{y_{it}} [1 - F(\beta x_{it} + \alpha)]^{1-y_{it}} dH(\alpha|\delta) \qquad (2)$$

However, it is well known that a random-effects specification gives biased estimates when there exists a correlation between the regressors and the error term. In the case of the receipt of training and of its support by the employer, we are likely to have such a correlation: more "able" individuals, or simply less intrinsically mobile individuals, two characteristics that we cannot observe in the data, should be more likely to receive training (e.g. see Lynch (1992) for U.S. evidence on the characteristics of individuals who receive company-provided training). Thus, although the random-effects model takes unobserved heterogeneity into account, it does so in a limited way and the resulting $\widehat{\beta}$ will likely suffer from omitted-variable bias.

One way to handle that problem is to postulate a linear relationship between the incidental parameter and all leads and lags of the regressors and estimate a correlated random-effects probit model (e.g. Chamberlain (1980), Chamberlain (1984)). In other words, let α_i be expressed as

$$\alpha_i = \gamma x_i + \eta_i$$

where η_i is independent of x_i, is normally distributed, and x_i is the vector

of regressors for individual i containing all leads and lags.[11] The idea, of course, is to capture the dependence between the incidental parameter α_i and the x_i's.[12] Then, the log-likelihood function becomes

$$LogL = \sum_{i=1}^{N} log \int \prod_{t=1}^{T} (F(\beta x_{it} + \gamma x_i + \eta_i)^{y_{it}} [1 - F(\beta x_{it} + \gamma x_i + \eta_i)]^{1-y_{it}} dH(\eta|\widetilde{\delta}) \tag{3}$$

Finally, again to assess whether unobserved individual characteristics that are correlated with training status play a role, I exploit the within-individual variation in schooling attainment at the start of jobs and estimate a fixed-effects linear probability model.

Judging from the results obtained with the random-effects estimator and reported in Table 3, there is some evidence that high school completion increases the probability of training for men, whether it be supported by the employer or not.[13] However, the marginal effects are relatively modest: using the usual formula to convert probit coefficients into marginal effects on the probability of a positive outcome,[14] the increase in the probability of getting employer-supported training is about 6.5%. For individuals with more education, the effect is substantially larger (about 23% for university graduates). Turning to the correlated random-effects estimator, however, we can see that the estimated effects of education actually increase except for the last category where there is a substantial decline (and the coefficient is rather imprecisely estimated). For high school graduates, though, the marginal effect on the probability of having employer-supported training is now about 15% whereas it increases to about 26% for those that have some post-secondary

[11] Recall that the time-varying or, perhaps more accurately, job-varying variables in the model are tenure, education at the start of each job, industry, and occupation. Thus, I project α_i on all leads and lags of the above-mentioned variables.

[12] Note by doing this one explicitly assumes that the regression function $E(\alpha_i|x_i)$ is indeed linear which in this context, contrary to when we have a linear model, is not an innocuous assumption.

[13] High school dropouts are the left-out category.

[14] $\frac{\partial Prob(y_i=1|x_i)}{\partial x_i} = \widehat{\beta}\overline{\varphi}(x_i\widehat{\beta})$ where $\overline{\varphi}$ is the sample average of the individual-specific value of the standard normal density $\varphi()$ evaluated at $x_i\widehat{\beta}$.

education. Finally, looking at the results obtained with the fixed-effects linear probability model, we find that in the case of employer-supported training, none of the education categories seem to have a statistically positive effect. This is particularly true for the more educated for whom the effect on training of having any post-secondary education is basically wiped out.[15]

The random-effects probit results for women suggest that education does not appear to be a major determinant of employer-supported training (although the coefficients are jointly significant): whether one has a high school degree or any post-secondary education does not seem to make much difference. However, when we look at the results from both the correlated random-effects probit model and the linear probability model, there is some evidence that having more schooling (at least a high school degree or some post-secondary education) does increase the probability of getting employer-supported training, although the coefficients are not very precisely estimated.[16]

Overall, I would tend to view the results as suggestive that any positive correlation between having at least a B.A. degree and the incidence of either form training (see Table 1) is probably due in large part to selectivity effects. Also, some of the regressors' impact varies substantially according to the dependent variable and according to gender. For example, while difficulties

[15] We should be somewhat cautious in interpreting the results from the fixed-effects model simply because there is not such great variation in the data, especially for men with more than a high school diploma. The same caveat applies to the correlated random-effects model which imposes more functional form assumption than the linear probability model.

[16] The large negative coefficient of -1.6153 for dropouts who get post secondary education is probably due to the fact that it is barely identified. Obviously, more variation in the data would have been useful. Note that I include the receipt of unemployment insurance and of welfare benefits simply as controls which serve to proxy unobservable characteristics. I give no interpretation to the estimated coefficients attached to these controls. Also, from Table 3 we can see that training incidence and employer tenure are positively related and competing structural interpretations can be given for that relationship. It could result from the fact that high tenure workers are intrinsically less mobile or have proven to be good matches and thus represent, all else equal, better candidates for employers to train (on this question, see Loewenstein and Spletzer (1997)). Or, it could result from the fact that the receipt of training develops firm-specific skills which make these workers more valuable to employers. I return to this question below.

in mathematics seem to decrease the likelihood of having employer-supported training for men, it does not have any effect on the more loosely defined training variable. For women, difficulties in languages seem to play the same role. This could simply reflect the fact that women tend to be in occupations which may require different skills. Also, perhaps surprisingly, the effect of having a child is larger for men than is the case for women.

In terms of the selectivity effects, one interesting pattern that emerges is the difference in the coefficients attached to the education variables as one goes from the random-effects probit (with its lack of control for the possible correlation between the regressors and the error term) to the fixed-effects linear probability model. Whereas for men the impact of being more educated decreases when we go from columns 1 and 5 to columns 4 and 8 respectively, which is something we would expect if these trainees are positively selected among the pool of individuals, it doesn't appear to be nearly the case for females. In fact, we can see from looking at columns 1 and 3, on the one hand , and columns 5 and 7, on the other hand, that controlling for observed characteristics doesn't make a large difference in terms of the magnitude of the coefficients associated to educational attainment: the main change for men is when I control for unmeasured individual characteristics. For women, though, controlling for observed characteristics (going from columns 3 and 7 to columns 1 and 5, respectively) makes a significant difference while controlling for unmeasured individual attributes doesn't. These results provide evidence that, in these data at least, selection into training based on unobservables is much more of an issue for men than it is for women. There is some evidence in the literature that women are less likely to benefit from investments in firm-specific on-the-job training than men.[17] Although this is perhaps speculative, one plausible reason for this is that due to life-cycle fertility considerations, firms may be reluctant to commit resources which could help women's careers within the firm. The degree of expected labour force

[17]In her paper on the wage effect of private sector training in the United States using data from the NLSY, Lynch (1992) shows that males are more likely to get on-the job training than females. In her other paper with the same data set (Lynch (1991)), she shows that on-the-job training reduces inter-firm mobility, which is suggestive that part of the skills acquired contain a firm-specific component.

(and firm) attachment across workers is something that is fairly difficult to proxy with the available measured characteristics, except gender. This may be the reason why the selection process into training for men depends to a greater extent on unmeasured characteristics that it does for women.

3.2 Training and Wages

To establish whether individuals with training are paid more, I exploit again the panel structure of the data set to estimate OLS and fixed-effects regressions of the log of weekly and hourly wages on an indicator variable for the presence of employer-supported training.

As we can see from Table 4, whether the analysis is carried out in levels or in first-differences, it seems clear that training is an important source of wage growth for men. Although it does not make any qualitative difference whether wages are measured per hour or per week, the latter measure is more sensitive to training which suggests that training also has a positive effect on hours worked. Concerning the results for women, three things seem to emerge from table 4. First, the wage impact of training is uniformly smaller than for men. Second, while the effect is marginally significant when I use weekly earnings, there is no evidence of a positive effect once I use hourly earnings. This suggests that any positive earnings effect of training occurs through hours worked more so than through hourly pay. Third, the ratio of the fixed effect coefficients to the levels coefficients is smaller for women than it is for men. Assuming that reporting error patterns do not differ across genders, this suggests that the pool of female trainees is more "positively selected" than is the case for men. Linking these results for women with those in the previous sub-section, the smaller wage impact could follow from more limited investment opportunities in firm-specific human capital. In fact, in related work (Parent (2001)) using the National Longitudinal Survey of Youth, I find rather strong evidence that the restrictions put on the covariance structure of earnings by the theory of firm-specific human capital are better supported by the data in the case of men, particularly for more educated individuals. In short, the theory of firm-specific human capital predicts that there should

be a trade-off between job-specific slopes and intercepts, generalizing to firm-specific wage growth previous work by Hause (1980). It turns out that this prediction finds stronger support for men than for women.

However, to reiterate, the data pertaining to training and to wages paid in each reference jobs are lacking in at least two important aspects: 1) there is no information on when training took place during the course of the employment relationship; and 2) the so-called "usual" weekly wages earned in each reference job are not linked to any particular period during the relationship except for those still working in a reference job at the time of the interview, for whom the wages paid likely reflect what they earn at interview time. For past employment relationships, wages could mean average wages over the course of the employment relationship, or end-of-tenure wages. Consequently, it is possible that the wages of some of the respondents could be lower if they are undergoing training at the time of the interview and their employers make them pay for their training through reduced wages, as predicted by standard human capital theory Becker (1975). This would tend to partially mask the overall positive relationship between completed training and wages. However, given the overall lack of empirical evidence in the literature on whether workers finance general training, this may not be a major concern in practice.

On the other hand, the positive effect estimated with fixed-effects is identified from cross-job variation for the same individual. Although this controls for individual fixed-effects, it does not control for the unobserved quality of job matches. Given that I do not have within-job variation in training outcomes with the SLS data (contrary, for example, to the National Longitudinal Survey of Youth), there likely subsists upward biases in the estimated effects.[18]

[18]However, the results in Parent (1999) suggest that this bias may be rather small.

3.3 The Receipt of Employer-Supported Training and Job Mobility

To investigate the possibility that employer-supported training leads to more job stability, I estimate a Cox proportional hazard model. To fully exploit the panel structure of the SLS data in order to eliminate individual-specific incidental parameters, I borrow from Chamberlain (1985)'s insight and stratifies the analysis within individuals. This causes all constant individual-specific terms to cancel out from the likelihood function, including each individual's baseline hazard. To be more specific, suppose that that for worker i we have n_i spells (ordered by their increasing length) and that the duration for each spell is denoted t_{ij}, where j stands for the spell number. Assuming all spells for the same person are independently distributed given the heterogeneity parameter, I can write the hazard function as

$$\lambda_{ij}(t) = exp(\beta' X_{ij}(t) + \alpha_i)\lambda_{i0}(t), \tag{4}$$

$$j = 1, ..., n_i;$$

$$i = 1, ..., N;$$

Then, it can be shown that the partial log-likelihood function is equal to (Lancaster (1990))

$$L = \sum_{i=1}^{N} \sum_{j=1}^{n_i} ln \left[\frac{exp(\beta' X_{i(k)}(t_{i(j)}))}{\sum_{k=j}^{n_i} exp(\beta' X_{i(k)}(t_{i(j)}))} \right] \tag{5}$$

where the denominator corresponds to the risk set of worker i. Note that both α_i and λ_{i0} do not appear in equation (5). One potential disadvantage of using this methodology is that individuals who have worked in at least two reference jobs and from whom the parameters are identified may not be a random sample of the population. To check whether that might be a real concern, Table 5 shows summary statistics for workers with at least two reference jobs and for those with just one reference job. The evidence

presented in Table 5 shows that the two sub-samples are somewhat different in terms of the underlying characteristics of their members. In fact, it seems pretty evident that a lot of single spell individuals come from the more educated subsample: the GPA of single-spell individuals is better and their parents are more educated as well. In any case, given the sampling scheme, that is pretty much what we should expect as university graduates have had less time to change jobs compared to, say, high school graduates.[19] Thus, it is possible that using a stratified Cox analysis eliminates incidental parameters at the cost of introducing systematic sample-selection biases.

Looking at Table 6, we can see that whether one analyzes job spells with or without conditioning out time invariant individual characteristics does not change the basic message that the receipt of training does seem to favor prolonged employment relationships.[20] In fact, wiping out unobserved individual effects (as well the individual baseline hazard) only reinforces the result. However, combining the evidence presented in Table 5 with that of Table 1, which shows that university graduates have by far the lowest average tenure levels, while at the same being much more likely to get employer-supported training, that is what one would expect as many of those single spell individuals are dropped from the fixed-effects analysis. On the other hand, note that failing to control for unobserved heterogeneity in a partial likelihood framework results in biasing the coefficients toward zero (Lancaster (1990), page 304). On the whole, the results suggest that a fair amount of the skills acquired through training have a certain firm-specific component thus (efficiently) prolonging the employment relationship.[21]

Looking at differences across genders, the most striking features of Table 6 is the absence of a sign reversal for the women's educational attainment

[19] Remember that these individuals are all aged between 22 and 24 in 1995.

[20] One must note, however, that the possible confounding effects of job-match quality are not controlled for using this methodology. In other words, it could still be the case that good matches which are destined to last longer irrespective of whether one receives training or not also happen to involve more training. In those cases, one cannot give a causal interpretation to the negative correlation between the hazard rate and the receipt of training.

[21] Although it could still be the case that these skills are general and that some of the mechanisms identified by, e.g., Acemoglu and Pischke (1999) are at play.

variables when going from the pooled estimation to the fixed-effects model. At a basic level, this means that while unmeasured time-invariant characteristics which influence mobility seem to be positively correlated with schooling for men, there 's hardly any sign of such a correlation for women: if anything, it is of opposite signs, although the coefficients are fairly imprecisely estimated. Again, it is possible that fertility issues may be behind this marked difference between men and women. Life-cycle fertility decisions, which of course affect the attachment to the firm, have to be made by women of all educational backgrounds. To explore this possibility, I re-estimate the hazard models with additional interaction terms between the dummy for the presence of at least one dependent child with the education dummies. Besides somewhat severe identification problems in the fixed-effects models, the results (available upon request) are not all that different, and they certainly do not eliminate the different patterns of correlations between unobservables and schooling for men and women. Of course, the control for the presence of children may be a fairly poor proxy for *future* fertility decisions, especially since the individuals in the sample are still very young (22 to 24 years of age).

3.4 Comparison of Results with the Existing Literature

By and large, the U.S. literature on the wage effect of training finds a positive effect, even after controlling for unobserved characteristics. For instance, using the NLSY Parent (1999) finds that one year of completed on-the-job training increases the wage by about 12%. In fact, the wage effect is basically the same whether the individual took that training with her/his current employer or with previous employers, suggesting a fairly large degree of portability. The difficulty in making comparisons is that the training variable I use in this paper is a simple dummy variable for the occurrence of employer-supported training. I do not have the accumulated number of weeks of completed training: in fact, I do not even know if the program was successfully completed or not. Although it would undeniably have been preferable to have top-quality data, at least it is plausible to conjecture that

including even "failed" training programs in the data would tend to attenuate the estimated wage effect towards zero, especially when using fixed-effects techniques.

Compared with the results in Blanchflower and Lynch (1994), where they have a much shorter panel of the NLSY but where they simply use, like in this paper, a dummy variable for training incidence, the results here point towards much larger wage effects, at least for men.[22] They are also much larger than the results Blanchflower and Lynch obtain with the British NCDS. A plausible explanation would be that the workers in the Canadian sample, being a more "positively selected" group overall because of the constraints imposed by the definition of a reference job than in either the NLSY or the NCDS, can better upgrade their skill level following a training program and, as such, benefit from a relatively large increase in their market value. However, this unobserved quality explanation would hold only for the unobserved quality of the job match since I am already controlling for unobserved individual characteristics in the fixed-effects models. Another possibility is that the definition of what qualifies as a training program is more vague here than in other previous studies. The NLSY and the NCDS contain information on essentially *formal* training programs lasting at least a month (at least in the waves used by Blanchflower and Lynch (1994)), whereas here, workers are not limited in what they can report as being a training program. Consequently, the wage effect measured with the SLS would be related to informal as well as formal programs. Indeed, it is interesting to note that according to a U.S. Bureau of Labor Statistics report based on the 1993 and 1995 Surveys of Employer-Provided Training, 70% of the hours spent training were spent in informal training programs and only 30% in formal programs (BLS (1996)). If I take the next step of applying the same ratio 70/30 to the *incidence* of training then the total proportion of workers receiving either form of training would be in the neighborhood of 40%, which is quite comparable to the

[22] It should be noted that Blanchflower and Lynch (1994) mix together uncompleted and completed on-the-job training spells with the current employer. As pointed out in Parent (1999), with the considerable benefit of having a longer panel dimension than Blanchflower and Lynch, uncompleted training was never found to have any significant wage effect in the NLSY.

percentage reported in Table 1. This calculation is based on the fact that Parent (1999) reports an incidence of completed (mainly formal) on-the-job training programs of over 13% for workers surveyed in the NLSY.

4 Conclusion

This paper exploits longitudinal data from Statistics Canada's School Leavers Survey and its Follow-up to study the determinants of training and its effects. Results generally show that more educated young people are more likely to be trained than high school dropouts despite strong evidence of selectivity biases. In addition, controlling for unobserved individual characteristics, training is found to have a sizeable wage impact for men while the evidence is mixed for women. Finally, there is strong evidence that employer-supported training does favor more lengthy employment relationships, as would be expected if either the skills acquired contain firm-specific components or the skills acquired are general but mobility costs are enough to prevent workers from quitting.[23]

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[23] Although if that were the case, one would have to explain why mobility costs are more important for trainees than for non-trainees. The results from the hazard model show that unobserved individual characteristics, such as innate stability favoring the receipt of on-the-job training are not what drives the results.

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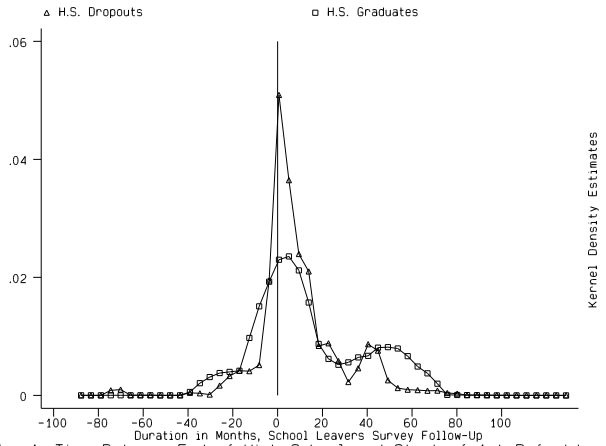

Fig. 1. Time Between End of High School and Start of 1st Ref. Job

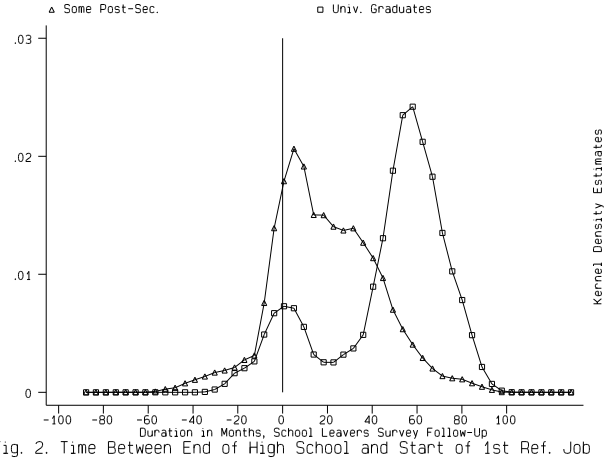

Fig. 2. Time Between End of High School and Start of 1st Ref. Job

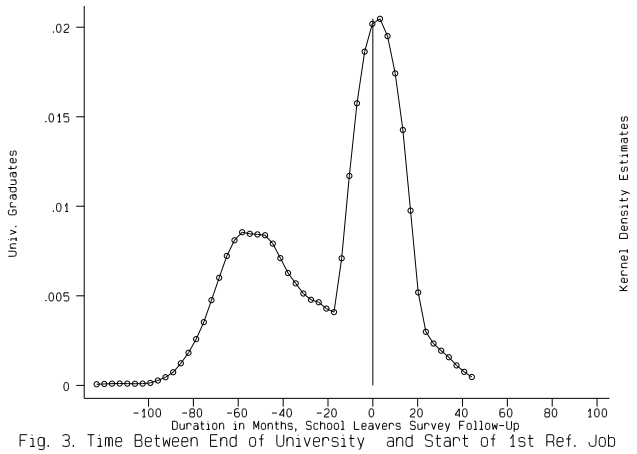

Fig. 3. Time Between End of University and Start of 1st Ref. Job

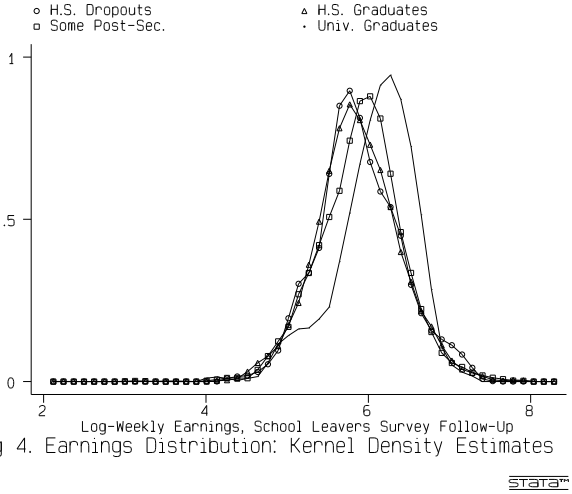

Fig 4. Earnings Distribution: Kernel Density Estimates

Appendix A: List of Variables (Dummy Indicators Except *)

Variable Name	Description
Father went to Coll./Univ.	Respondent's (R's) father/stepfather attended college or university
Mother went to Coll./Univ.	R's mother/stepmother attended college or university
Tenure*	Number of months with employer
Married	R is married
Difficulty in Maths.	R reports having had difficulty in maths in high school
Difficulty in Lang.	R reports having had difficulty in English/French
Failed in Primary School	R reports having failed a grade in elementary school
Went to Private School	R attented a private institution in elementary/secondary school
Had a Job in High School	R reports having held a job while attending high school
GPA of A	R's average during last term in elementary/secondary school was A
GPA of B	R's average during last term in elementary/secondary school was B
GPA of C	R's average during last term in elementary/secondary school was C
GPA of D	R's average during last term in elementary/secondary school was D
With a Child	R has at least one dependent child
Coll. UI in Last 12 Mos	R received income from UI over 12 months prior to '95 interview
Coll. Welf. in Last 12 Mos	R received welfare benefits over 12 months prior to '95 interview
Took Training	R took career or job related education or training while in reference job
Employer-Supported OJT	R's employer provided transportation, gave time off, or gave any other support towards the education or training R took while in reference job
Weekly Wages*	R's usual wage or salary before taxes in reference job
Hours Worked*	R's usual hours per week in reference job
Occupation	4-digit National Occupation Classification Code Aggregated to 1-digit
Industry	3-digit Standard Industrial Classification Code Aggregated to 1-digit

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