CRDE

CAHIER 0699

SECTOR-SPECIFIC ON-THE-JOB-TRAINING : EVIDENCE FROM U.S. DATA

Lars VILHUBER

Université de Montréal

Centre de recherche et développement en économique

C.P. 6128, Succursale Centre-ville
Montréal (Québec) H3C 3J7

Téléphone : (514) 343-6557
Télécopieur : (514) 343-5831
Adresse électronique : crde@crde.umontreal.ca
Site Web : http://www.crde.umontreal.ca/

CAHIER 0699

SECTOR-SPECIFIC ON-THE-JOB TRAINING : EVIDENCE FROM U.S. DATA

Lars VILHUBER[1]

[1] Centre de recherche et développement en économique (C.R.D.E.) and Département de sciences économiques, Université de Montréal, and Department of Economics, York University

March 1999

The author wishes to thank Thomas Lemieux and David Margolis for their enduring support and rich input throughout this project. David Blanchflower, Jean Farès, Bentley MacLeod, and Daniel Parent provided helpful comments. Financial and logistic support by CIRANO and the C.R.D.E. is gratefully acknowledged.

RÉSUMÉ

Nous utilisons des données américaines provenant du National Longitudinal Survey of Youth (NLSY) pour examiner l'effet de la formation formelle en lieu de travail par rapport à la mobilité observée des jeunes travailleurs américains. Des modèles de durée paramétriques nous permettent d'évaluer l'impact économique de la formation sur le temps productif passé avec un employeur. Nos résultats sont cohérents avec la plupart des études précédentes, qui trouvaient un impact positif et significatif. Cependant, la durée de la relation de travail nette du temps passé en formation n'augmente pas de manière significative. Nous procédons par la suite à l'analyse de la mobilité intrasectorielle et intersectorielle àfin de permettre l'inférence par rapport à la spécificité du capital humain acquis par la formation, soit du capital humain spécifique à la firme, soit spécifique à l'industrie, soit général. L'analyse économétrique permet de rejeter un modèle séquentiel de choix de secteur en faveur d'un modèle à risques concurrents. Nos résultats présentent une forte évidence en faveur de la spécificité de la formation à l'industrie. La probabilité d'un changement de secteur d'activité suite à une séparation d'emploi décroît avec la formation reçue dans l'industrie présente, peu importe si celle-ci a été reçue du dernier employeur ou d'un employeur précédent. La probabilité de détenir un emploi suite à une séparation augmente avec la formation sur le tas. Ces résultats sont robustes à des variations du modèle de base.

Mots clés : formation sur le tas, durée de l'emploi, mobilité sectorielle, capital humain spécifique au secteur, modèles de durée paramétriques, modèle à risques concurrents

ABSTRACT

Using data from the National Longitudinal Survey of Youth (NLSY), we re-examine the effect of formal on-the-job training on mobility patterns of young American workers. By employing parametric duration models, we evaluate the economic impact of training on productive time with an employer. Confirming previous studies, we find a positive and statistically significant impact of formal on-the-job training on tenure with the employer providing the training. However, the expected net duration of the time spent in the training program is generally not significantly increased. We proceed to document and analyze intra-sectoral and cross-sectoral mobility patterns in order to infer whether training provides firm-specific, industry-specific, or general human capital. The econometric analysis rejects a sequential model of job separation in favor of a competing risks specification. We find significant evidence for the industry-specificity of training. The probability of sectoral mobility upon job separation decreases with training received in the current industry, whether with the last employer or previous employers, and employment attachment increases with on-the-job training. These results are robust to a number of variations on the base model.

Key words : on-the-job training, employment duration, sectoral mobility, industry-specific human capital, parametric duration models, competing risks model

Sector-specific On-the-job Training: Evidence from U.S. Data

Lars Vilhuber *
Department of Economics, York University,
Toronto, Ontario, Canada.

and

Département de sciences économiques,
Université de Montréal
Montréal, Québec, Canada

December 7, 1998

*Correspondence to: York University, Dept. of Economics, 4700 Keele St., Toronto M3J 1P3, Canada. I thank Thomas Lemieux and David Margolis for their enduring support and rich input all through this project. David Blanchflower, Jean Farès, Bentley MacLeod, and Daniel Parent provided helpful comments. Financial and logistic support by CIRANO and CRDE at the Université de Montréal is gratefully acknowledged. vilhuber@yorku.ca

Abstract / *Résumé*

Using data from the National Longitudinal Survey of Youth (NLSY), we re-examine the effect of formal on-the-job training on mobility patterns of young American workers. By employing parametric duration models, we evaluate the economic impact of training on productive time with an employer. Confirming previous studies, we find a positive and statistically significant impact of formal on-the-job training on tenure with the employer providing the training. However, expected duration net of the time spent in the training program is generally not significantly increased. We proceed to document and analyze intra-sectoral and cross-sectoral mobility patterns in order to infer whether training provides firm-specific, industry-specific, or general human capital. The econometric analysis rejects a sequential model of job separation in favor of a competing risks specification. We find significant evidence for the industry-specificity of training. The probability of sectoral mobility upon job separation decreases with training received in the current industry, whether with the last employer or previous employers, and employment attachment increases with on-the-job training. These results are robust to a number of variations on the base model.

Nous utilisons des données américaines provenant du National Longitudinal Survey of Youth (NLSY) pour examiner l'effet de la formation formelle en lieu de travail par rapport à la mobilité observée des jeunes travailleurs américains. Des modèles de durée paramétriques nous permettent d'évaluer l'impact économique de la formation sur le temps productif passé avec un employeur. Nos résultats sont cohérents avec la plupart des études précédents, qui trouvaient un impact positif et significatif. Cependant, la durée de la rélation de travail nette du temps passé en formation n'augmente pas de manière significative. Nous procédons par la suite à l'analyse de la mobilité intrasectoriel et intersectoriel à fin de permettre l'inférence par rapport à la spécificité du capital humain acquis par la formation, soit du capital humain spécifique à la firme, soit spécifique à l'industrie, soit général. L'analyse économétrique permet de rejeter un modèle séquentiel de choix de secteur en faveur d'un modèle à risques concurrents. Nos résultats présentent de l'évidence forte en faveur de la spécificité de la formation à l'industrie. La probabilité d'un changement de secteur d'activité suite à une séparation d'emploi décroît avec la formation reçue dans l'industrie présente, peu importe si celle-ci a été reçue du dernier employeur ou d'un employeur précédent. La probabilité de detenir un emploi suite à une séparation augmente avec la formation sur le tas. Ces résultats sont robustes à des variations du modèle de base.

Keywords: On-the-job Training, Employment duration, Sectoral mobility, Industry-specific human capital, Parametric duration models, Competing risks model.

JEL: J24 Human Capital Formation, J41 Specific Human Capital, J62 Sectoral Mobility.

Mots-clés: Formation sur le tas, durée de l'emploi, mobilité sectoriel, capital humain spécifique au secteur, modèle de durée paramétrique, modèle à risques concurrents.

JEL: J24 Formation de capital humain, J41 Capital humain spécifique, J62 Mobilité sectoriel.

"Almost half [of British business people surveyed] preferred to poach trained workers rather than to educate them; and more than a third worried that trained people were more likely to leave the company."

(The Economist 1997)

1 Introduction

Recent focus on the issue of whether wages rise with tenure or with experience have revealed the importance of controlling for the industry in which experience was acquired[1]. A parallel literature has focused on the effect of formal employer-provided training on wages and mobility (Barron, Berger & Black 1997, Lynch 1991, Lynch 1992a, Parent forthcominga). Missing is the link between training and industry mobility. This paper attempts to redress this lack using data from the National Longitudinal Survey of Youth (NLSY). We model the transition pattern of young workers using duration and competing risks models, which allows us to integrate industry mobility. The objective is to document differential effects of training on industry stayers as opposed to industry changers and those workers who leave employment all together.

This paper differs from the previous literature in the way we treat mobility. The analysis in this literature usually concerns only the expected duration with a given firm conditional on training, without regard to where the job changer changes to, once she quits her job. Most authors have used Cox partial likelihood, ignoring the baseline hazard, and can thus only provide information on the sign, but not the magnitude of the impact of training (Lynch 1992a, Parent forthcominga). Gritz (1993) used competing risks models, but considered only the effect of training on the duration and frequency of employment spells without regard to either specific jobs nor industry tenure.

Our goal is twofold. The initial quotation points to the worries involved: Are workers more likely to leave their current firm after receipt of training? Though previous studies have found a positive effect of training on tenure, we argue that this is not enough, since measured tenure includes the time

[1] Neal (1995), Parent (1995). See Altonji & Shakotko (1987), Abraham & Farber (1987), and Topel (1991) for the framework in which this debate occurs.

spent in training. In this paper, we measure the effect of training on duration by using parametric duration models. We find that the quoted worries, though exaggerated, may be justified. The statistically significant impact of training found in previous studies is not *economically* significant.

The second concern is a follow-up question to the previous one. When workers leave, where do they go to, and what information is provided by mobility patterns? We provide new evidence on the connection between training and the mobility of the workers concerned, by distinguishing between intra-sectoral and cross-sectoral mobility as well as exits to non-employment. We develop a simple "inspection-good" model of jobs as a function of (the stock of) human capital, allowing us to distinguish the degree of specificity of training. This model suggests using a competing risks model to capture the effect of training on transitions to different states, a model which we favor against a sequential model of separation.

The paper is organized as follows. The following section reviews the theoretical framework of the impact of training. Section 3 describes the data and provides some descriptive statistics. Section 4 outlines the empirical model used and Section 5 presents the results. Section 6 concludes.

2 Theoretical framework

Human capital theory, though primarily interested in the wage and its remuneration of human capital, has implications as to the mobility of workers. This obviously depends on the degree of specificity of the human capital acquired, either through formal or informal training. Its theoretical predictions, however, are based on a dichotomy between firm-specific and universally-general capital formation. Recent empirical work (Neal 1995, Parent 1995) has shown that this stark dichotomy may be too imprecise, and the amalgamation of the empirical results into some theoretical framework is still lacking.

If human capital is general, then the knowledge accumulated is of productive use elsewhere irrespective of the company or the sector in which the training was received. Competitive pressures come to play, ensuring that workers get the full return on the investment, and consequently pay for all costs. In equilibrium, the mobility of a trained worker is no different than that of an untrained worker. The mobility aspects follow from the characteristics of the human capital itself.[2] If human capital is firm-specific, then theory finds that the returns and costs should be paid for by the firm, though turnover and transaction cost arguments lead to some splitting of both (Becker 1964,1993, Hashimoto 1981). Both worker and firm have an increased interest in sticking with the relationship in the presence of specific capital, and turnover should decrease. Similar results are obtained from contract theory (MacLeod & Malcomson 1993).

The similar set of predictions may arise from matching theory (Jovanovic 1979b). A worker will switch firms if her expected match-specific utility is higher elsewhere. If training is firm-specific, it increases the value of the firm-worker match, and ceteris paribus decreases the value of other arriving job offers to the worker, thus probability of the worker switching firms (Jovanovic 1979a). If, on the other hand, training is general, then the value of all match draws are increased by the same factor, and we again obtain that there should be no impact on mobility. Finally, if training is industry-specific, a combination of the two above arguments lead to a reduction of mobility across industries, though intra-industry mobility would not be affected. As a result, conditional on leaving, the worker is more likely to take up a new job in the same industry.

However, Spence (1974)-like selection models may generate similar predictions with respect to tenure and completed training[3]. If training serves as a test to discern good from bad workers, then workers who have completed training, and thus successfully passed the test, will be recognized as good workers. If good workers tend to have longer tenures, then the correlation between (completed) training and tenure is not due to increased human capital, but due to a separation of the good from the bad, invisible to an econometrician's eye.

Against this stands a different type of selection story. Suppose that the firm has to choose training recipients among workers whose productivity is unknown. However, the firm can observe other characteristics related to a worker's mobility. Then for any type of training for which the firm pays, the firm will prefer less mobile workers in order to get the highest possible return on its

[2]Transaction costs or other market imperfections may lead to some quasi-rents in the relation between firm and worker, and to reduced mobility as a consequence (Acemoglu & Pischke 1996). However, the reduction will be in the baseline mobility, and investment in general human capital will not affect mobility except under certain conditions.

[3]E.g. Salop & Salop (1976) and Weiss & Wang (1990). Margolis (1995) provides evidence for a model of self-selecting workers with heterogeneous hazards into firms offering different seniority rewards.

investment. We would observe a correlation between training and tenure. From a human capital point of view, this correlation is spurious.

Previous findings

Most previous empirical studies have concentrated on the effects of training on wages and the propensity to change jobs without distinguishing occupational and sectoral changes. On-the-job training (OJT) increases wages with the current employer. As we have seen, this could be consistent with both general and firm-specific human capital. The literature is not clear on whether employers remunerate OJT received from previous employers. Lynch (1992b) finds that these returns are nil, whereas Parent (forthcominga) and Loewenstein & Spletzer (1998), using more representative samples and more elaborate techniques, find that returns to previously obtained OJT are as high as for training received with the current firm, indicating that training is of a general nature. However, OJT does not seem to be paid for by the employee through reduced starting wages (Barron et al. 1997, Loewenstein & Spletzer 1998, Veum 1995a), which is consistent with the idea that human capital thus formed is of a (firm-)specific nature. Disagreement occurs on whether these results are also true for off-the-job training (OFT). Whereas Lynch (1991) finds that OFT is not remunerated by the current employer, Parent (forthcominga) shows that returns to training are the same independent of the type of training, and Veum (1995a) reports that OFT leads to higher starting wages as long as it is financed by employers.

Some results reported in the literature lend support to the mobility-based selection story. For instance, results reported in Lynch (1992b) indicate that married workers and more experienced workers are significantly more likely to receive training, where both characteristics are habitually correlated with longer tenure.[4]

Only a few studies have used duration analysis to look at the mobility patterns associated with training. Estimates of duration models have shown that the probability of separation from the current employer is reduced, conditional on having received some OJT (Lynch 1992a, Parent forthcominga). Combined with the reported results on the wage effects of training, this is interpreted

[4] See also Altonji & Spletzer (1991).

as evidence for the presence of some firm-specific component to formal training, or at least in contradiction with the interpretation of training as portable across employers. In contrast, a recent paper (Veum 1997) finds no effect of on-the-job training on tenure.[5]

Few previous studies, and none in the training literature, have considered the distinction between intra-sectoral mobility and cross-sectoral mobility, focusing only on duration on the job. Neal (1995) and Parent (1995) estimate the effects of industry mobility on wages, but do not consider the determinants of such mobility. Their results showing that industry tenure explains away the entire firm-specific tenure effect on wages is a finding which is deeply related to the present paper, since it points to the presence of sector-specific *informal* human capital. Neal (1996) and McCall (1990) go one step further. Neal (1996) addresses the question of complexity of job changes. He finds evidence that the propensity for cross-sectoral changes decreases with industry experience, but does not relate these changes to training variables or job tenure. McCall (1990) considers occupational matching, finding some evidence that previous experience in the same occupation increases tenure in the current job. Thomas (1996) estimates a parametric model of sectoral mobility for persons experiencing unemployment, distinguishing exits from jobs only as to voluntary quits or involuntary job losses and neglecting direct job-to-job transitions. He finds that the probability of changing sectors increases with the duration of unemployment. Furthermore, tenure on the previous job increases the duration of unemployment.

A model of sector-specific human capital

Most previous studies have thus been framed by the dichotomy between firm-specific and completely general capital. Nevertheless, already Becker had in mind that human capital could be of use elsewhere, but not necessarily by everybody:

> " *General training is useful in many firms besides those providing it; for example, a machinist trained in the army finds his skills of value in steel and aircraft firms, and a doctor trained at one hospital finds his skills useful at other hospitals.*

[5] Veum (1997) uses a slightly different classification of training. Furthermore, he uses a subsample of our dataset.

(Becker 1964,1993, pg. 33)

Hence, some training will be of use only to a restricted subset of all firms in the economy, and will therefore be less then completely general. On the other hand, there may well exist training which is truly of use only to the training firm, and other training, one has only to think of word processing skills, that will be of use to such a large set of firms that we can truly say it is completely general.

To fix ideas, consider the following model. It is a model of jobs as inspection goods (Jovanovic 1979b), coupled with the usual assumption of an increase in marginal product due to human capital formation (Becker 1964,1993). There is no active job search, but job offers arrive at constant rates, which may differ across sectors.[6] There are two sectors. By convention, the worker is initially employed in sector 1, receiving a (log) wage $w_0 = \gamma(k)$, a positive function of the stock of human capital (k). For simplicity, we assume a linear function, $\gamma(k) = \gamma k$. The degree of transferability of human capital to other firms and sectors is denoted by α_i, $i = 1, 2$, and without loss of generality, α_i are either unity or zero ($\alpha_i \in \{0, 1\}$. The firm pays for the training irrespective of its specificity, and the worker's wage is increasing in k: $\gamma > 0$. Offers $w_i(k)$ arrive at a constant rate r. A fraction q of offers comes from sector 2. Both sectors are competitive, and in each sector, (log) wage offers (the value of worker-firm matches) are normally distributed with mean $\gamma k \alpha_i$ and variance $\sigma = 1$.[7] The worker will switch firms and/or sectors if he receives a wage offer $w_i(k) > w_0(k)$, which occurs with probability $1 - \Phi_i(w_0(k) - w_i(k)) = F_i(w_0)$. Abstracting from ties, the probability of a sectoral move per period, the inter-sectoral transition intensity, is $\theta_2(k) = r \cdot q \cdot F_2(w_0)$. The intra-sectoral transition intensity is defined equivalently as $\theta_1(k) = r \cdot (1 - q) \cdot F_1(w_0)$. The hazard function $\lambda(k)$ is simply the sum of the transition intensities. The probability of a sectoral move conditional on leaving the current job is $M_2(k) = \theta_2/(\theta_1 + \theta_2) = qF_2/[(1 - q)F_1 + qF_2]$. Suppose that initially $k = 0$, hence all distributions have the same mean.

If training, the process of human capital acquisition, is firm-specific, then $\alpha_1 = \alpha_2 = 0$. Industry-specific capital is the case where $\alpha_1 = 1$ and $\alpha_2 = 0$: training is perfectly portable within the same sector, but not across sectors. Finally, general training is portable across sectors, hence $\alpha_1 = \alpha_2 = 1$.

[6] Similar in spirit, but without the emphasis on mobility, is (Stevens 1994).

[7] We assume that the variance is equal across sectors. This is a sufficient condition, but not necessary for our results to hold.

Now consider the acquisition of dk units of human capital through training. Initially, all distributions have mean zero, $\theta_2(0) = r \cdot q/2$, $\theta_1(0) = r \cdot (1-q)/2$, $\lambda(0) = r/2$, and $M_2 = rq$. If training is firm-specific, then $\partial F_i(w_0)/\partial k < 0$ for $i = 1, 2$. Both transition intensities decline, and so does the hazard. This is so because the firm will share part of the return on human capital with the worker[8] and match most outside wage offers. The conditional probability of a sectoral move $M_2(k)$, however, is unchanged, since the desirability of wage offers from both sectors relative to the current wage decline in the same manner.

If training is general, then both transitions intensities remain unchanged, and so does the overall hazard.[9] Furthermore, as in the firm-specific case, $\partial M_2(k)/k = 0$, since the desirability of wage offers from both sectors increase in the same manner.

However, if training is industry specific, the transition intensity to Sector 2 decreases, i.e. $\partial\theta_2(k)/\partial k < 0$, but the transition intensity to the same sector remains unchanged, $\partial\theta_1(k)/\partial k = 0$, since the mean productivity for other firms in the same sector increases by the same amount as for the present firm. This implies that the conditional probability of a sectoral move $M_2(k)$ decreases, since $sign(\partial M_2(k)/\partial k) = sign(\theta_1 \partial\theta_2/k - \theta_2 \partial\theta_1/\partial k) < 0$. Note that the hazard λ also declines, although by less than in the firm-specific case.

Thus, it is possible to distinguish the three cases by estimating the conditional probability of a sectoral move. A reduction in this probability following the acquisition of human capital is inconsistent with both firm-specific and general human capital.

The model can easily be extended to include non-employment as a third sector. "Wage offers" from the non-employment "sector" can be interpreted as shocks to the reservation wage. Assume that $w_3(k) = 0$, i.e. human capital has no effect on leisure. The hazard is now defined as the sum over all three transition intensities. Define $M_{job} = (\theta_1 + \theta_2)/\lambda$, the conditional probability of finding a job. Under the above assumptions, θ_3 always declines in k. Hence, for $\alpha_1 = \alpha_2 = 0$, $\partial M_{job}/\partial k = 0$, but for the other two cases, $\partial M_{job}/\partial k > 0$. This is another way of saying that (conditional) labor force attachment increases with training if training is not firm-specific, but remains unchanged in

[8]This was suggested by Becker (1964,1993) and formalized by Hashimoto (1981).

[9]Note that in this model, everything is observable. Any informational rent obtained by the employer may lead to different predictions (Acemoglu & Pischke 1998).

the case of more general training. M_2 is now reinterpreted as the probability of a sectoral change, conditional on being employed in the next period. Table 1 on page 41 summarizes the testable hypotheses.

Table 1 here.

Though essentially a model of job quits, the model also has implications as to training received in previous jobs, where separation may have occurred as a layoff. If training received on previous jobs was firm-specific, then in subsequent jobs, it is as if the worker had never received this training, and previously received training should have no impact on any of the above measures. In particular, the effect of such training on the hazard should be nil. If training is industry-specific, it obviously depends on whether training was acquired in the same industry or not. If it was, then we obtain the same predictions as for industry-specific capital above, as if the current company itself had provided the industry-specific training. On the other hand, if it was not, then the effect is the same as for previously acquired firm-specific capital, i.e. zero. Finally, if training is general in nature, then the only effects are a reduction in the transition intensity to non-employment and as a consequence, an increase in conditional labor force attachment.

Procedural outline

In this paper, we take a closer look at mobility patterns of workers in the National Longitudinal Survey of Youth. First, we estimate parametric duration models. In order to discern a tenure-lengthening effect of on-the-job training, we argue that tenure should increase by more than the time spent in the training program itself. The increase needs to be greater than the (full-time) equivalent of the duration of the training program itself for there to be an *economic* impact of training. Hence, if a 10-week training program increases expected tenure by 10 weeks, we argue that the economic impact, the *net* increase is nil. Our results show that, in general, training has no such economic impact on tenure, casting doubt on the interpretation of training as firm-specific human capital.

Is the measure of "net" increase appropriate? In the sense that formal training is usually dispensed in a classroom setting, separate from productive activities, this seems to us uncontroversial. In the case of apprenticeships, this may be less so, since apprenticeships are a mix of learning-by-doing and classroom settings. However, even in the case of apprenticeships, the "net" increase will give us an indication of how strong the tenure effect of training truly is.

We then proceed to estimate the conditional probabilities as suggested by the above model in a competing risks framework. If training does in fact contain a firm-specific component, then we would expect training to significantly reduce exits to all destinations, which is already reflected in a reduction of the overall job separation hazard. If training is industry-specific, we would expect no effect on intra-sectoral mobility, and a negative effect on inter-sectoral mobility. Finally, a finding that training has no effect on mobility is consistent with general human capital.

3 Data

The National Longitudinal Survey of Youth (NLSY) has followed 12,686 individuals since 1979, originally selected for being between 14 and 21 years of age. The survey tracks (among other things) their employment, schooling and training. We use data from all waves of the NLSY up until 1993. Jobs are excluded if their starting dates are before 1979. We use all reported training spells to compute total training (excluding education) received. However, it should be pointed out that prior to 1987, only training spells longer than 4 weeks were reported, and this might bias the controls for previous training received. The only alternative, i.e. taking into account persons who entered the labor force after 1986,[10]is even less attractive as an alternative. Individuals who have their first job contact after 1986 are at least 21 years old, and this cannot be considered a representative sample even of the youth population.

A further constraint could be that the NLSY contains information on a maximum of five job spells and four training spells having occurred since the last interview. In practice, only about one percent of persons holding at least one job since the last interview also provide information on a fifth job, and on average only 1.6 percent of those receiving at least one training spell also provide

[10]No training questions were asked in 1987. However, the questions in 1988 refer to training received since 1986/ last interview.

information on a third or fourth training spell.[11] Thus, this restriction does not seem to impose a major constraint.

Table 2 here.

In our analysis, we exclude persons in the military subsample and not working for private companies.[12] We also exclude workers who have not entered the labor force on a permanent basis. To be included, a worker had to work for at least 25 weeks and on average at least 30 hours per week for at least 3 of the next 5 years. For these individuals, we keep all valid job-spell observations, including those before the permanent transition, arguing that training may be received before the worker permanently transits into the work force.[13] The final sample includes 41 126 observations for 8 088 individuals. For the econometric analysis, we also eliminate all job spells less than 4 weeks in length. Table 2 on page 44 provides more details.[14] Sample means for the full sample and the subsample with strictly positive training are given in Table 6 on page 46.

Table 2 and Table 6 here.

Time frame for search activities

To construct transition data, we need to define appropriate exit states. In the simplest transition model, the state following an employment spell can be easily defined. A person is either employed

[11]To be more precise: In the years in which up to three training spells of at least 4 weeks could be reported, only 0.52 percent of those receiving at least one training spell also reported a third training spell. In later years, respondents were asked about a maximum of 4 spells of at least a week in length, and the corresponding number then is 2.2 percent.

[12]We also experimented with excluding the oversampled population, which reduces the sample size to 24 618 observations for 4 610 individuals. Since the results did not change, we used all observations for the results reported here.

[13]None of the results seem to change if we include workers not satisfying this criterion.

[14]Parent (forthcominga) uses essentially the same sample minus two years. His final sample only includes 8,097 observations. However, his exclusion restrictions are more severe. Using a four years instead of his six to exclude transitory workers leaves us with a larger sample (see previous footnote). Furthermore, he excludes workers with less than two completed spells. In our sample, about 10 percent of all spells are censored. Finally, and perhaps the major difference, he excludes all workers occupying more than one job at the same time. We have not implemented this distinction. We base our identification of transitions on the primary job code in the work history data file of the NLSY. Ignoring the presence of dual jobs, we may capture some job-to-job transitions which are in fact only a reallocation of time towards the second job. In a certain sense, this is also a job and possibly an industry transition. Furthermore, we believe, though we have not checked, that on-the-job (though not off-the-job) training will occur with the employer with whom the worker works the highest number of hours, which is the criterion used to designate the primary job in the work history file, and that it is unlikely that a worker will receive training with two employers simultaneously. However, we acknowledge that the impact of these restrictions remains to be evaluated.

in another job, which might be in the same or in a different industry. She may have enlisted in the military, she may be unemployed or have withdrawn from the labor market. In this paper, we will describe three different types of transitions only: transitions to jobs within the same industry, transitions to jobs in a different two-digit[15] industry, and non-employment, which groups not only the obvious economic definition, but also those transitions which end in military enlistment.

However, in doing this, we have not completely solved our problem. Since our model is a partial equilibrium model using single-cycle data, we need to do some aggregation in the temporal dimension. In other words, when a person leaves a job, when does she arrive in the new state? If this person suffers 2 days of unemployment between two jobs, do we classify this transition as a transition to a new job, or to unemployment? What if the unemployment spell between two jobs is three months? In a more complete, multiple-cycle analysis, we would include the effect of previous on-the-job training on the probability of exiting from unemployment, and this question would not be a problem. However, to keep the econometrics a bit simpler, we introduce some simplifications, and heuristically test for their impact later.

We thus argue that the first transition should be coded as a job-to-job transition, since it is more likely that the new job was lined up before the old job ended than otherwise,[16] and that hence unemployment (or non-employment) is likely to be voluntary. The second transition, however, should be coded as a job-to-unemployment transition, again on the grounds that it is more likely that unemployment was involuntary, rather than voluntary. More precisely, we assume that any job started within a *window* of s weeks of having left another job qualifies as a job-to-job transition. In other words, after having left one job at time t, any unemployment spell from t to $t+s$ is not taken into account. We then test the robustness of our results to several different values of s.

[15] See Table 3 on page 44 for the industry grouping we use. "One-and-a-half" digit industry would be more appropriate, since our classification is wider than one-digit SIC, but narrower than two-digit SIC.

[16] Some control for lined-up jobs is possible, since in some years, respondents were asked specifically if they had a new job lined up before leaving their last job. In the present analysis, we have not yet integrated this question.

Information on training spells

A total of 11 categories have been allowed over the years for classification of the training institution. The job to be trained for is registered, as well as duration, intensity and if training was successfully completed. As interest in training increased over the years, supplementary questions were added. Thus, since 1988, the respondent was asked whether her employer had sponsored training, if the training was used on the job, if it helped or was necessary to get a promotion, whether it helped in getting a different job, etc. Questions were asked about the respondent's evaluation of transferability of the training received to other tasks and employers.

We group three categories of training as "on-the-job training".[17] Apprenticeships are obviously on-the-job, as are training programs run by the employer. We further classify training provided at work by outside suppliers as on-the-job training, arguing that this is also likely to be organized by the employers. All other training codes are considered off-the-job training.[18]

Training is a variable which by definition varies within a job, and an appropriate econometric model should allow for time-varying covariates. In this paper, we approximate the impact of time-varying training by using completed hours of training at the time the worker leaves the firm.[19] We also experiment with training intensity, defined as hours of training per week of tenure, and computed using as above total hours of training received and total weeks of tenure. Note that the first measure covaries in a mechanical fashion with tenure, since an employee cannot receive 10 weeks of training on a job that lasts 5 weeks. The second measure is a correct measure if training intensities were to be defined at the start of the job, and if they did not vary over time. A large percentage of training occurs at the start of a job spell, but the proportion of training in later years is not nil (Parent forthcominga, Loewenstein & Spletzer 1996). The results reported here should hence be interpreted with caution.

[17]See Table 5 on page 45 for the complete listing of job categories.

[18]A different approach, taken by Veum (1995b, 1997), is to use the information provided since 1988 on who paid for the direct costs of training.

[19]Parent (forthcominga) and Veum (1997) use the same method. Lynch (1992a) uses a time-varying specification.

Preliminary data analysis

Some preliminary analysis is appropriate. In order to choose an appropriate baseline hazard, a plot of the raw hazard rate is of use to obtain some idea of the form of the baseline hazard. Panel 1 of Figure 1 on page 42 shows the usual form of the exit hazard (Kaplan-Meier estimates), with a large peak at around 12 weeks, as first noted by Farber (1994). Note that the hazard is non-monotonous, hence Weibull or exponential hazard models are would seem inappropriate.

Figure 1 on page 42 here.

The other panels of Figure 1 show plots obtained by graphing empirical transition intensities to the appropriate states using different values for the size of the "transition window", again using Kaplan-Meier estimators. The functional form of transition intensities seems to remain the same, and does not seem to differ across exit states, although transitions to same industry jobs decline less rapidly after the peak in the 12th week.[20] Note that the hazard for industry movers always lies above the hazard for industry stayers. The implication, as also reported by Table 4 on page 44, is that young workers frequently change industry. It possibly reflects search and matching activities (Neal 1996). Furthermore, it would appear that the (relative) probability of observing a change of industries rather than a job in the same industry is not time-constant, a point we will approach formally in the next two sections.

Table 4 here.

Figure 2 on page 43 shows how each transition intensities evolves when we change the size of the transition window. Enlargening the length of the transition window increases both job transition intensities by reducing the number of individuals who are classified as non-employed, though the transition intensity to jobs in other industries seems to grow more strongly. [21] . Note that the largest increase occurs when enlargening the window size from one to five weeks, whereas enlargening

[20]No formal tests have been performed, and our methodology in the competing risk framework used here does not depend on the form of the hazard.

[21]Thomas (1996) shows that the probability of changing industries increases relative to the probability of finding a job in the same industry when explicitly modeling unemployment durations.

it further to thirteen weeks has a proportionately smaller effect. In most of our analysis, we thus report results using a window size of five weeks.

Figure 2 on page 43 here.

Table 6 on page 46 here.

Table 6 on page 46 shows means of most relevant variables for the full sample and for the restricted sample with strictly positive on-the-job training. The subsample differs from the full sample in several aspects. Trained workers have more experience, work longer hours, are more likely to be unionized. Related to our parameters of interest, they have higher (initial) wages and longer jobs, as seen both in completed tenure and in the number of right-censored jobs, i.e. the balance of the destination frequencies. Turning to the sample frequencies of the three exit states, trained workers appear to be more likely to avoid non-employment when leaving a job. Furthermore, as a first indication of a possible industry-specificity of training, conditional on finding a job within five weeks, trained workers are more likely to find a job in the same industry.[22] Thus, the difference in sample frequencies of destinations between trained workers and the full sample would lead us to conclude that training confers industry-specific skills. The difference in transitions to non-employment indicates an increase in labor force attachment, which would seem inconsistent with pure firm-specificity of training. These observations provide a suggestive starting point for the duration analysis in the following chapters.

In the next section, we develop a multivariate framework giving us more insight into the relation between inter and intra-sectoral transitions.

4 Empirical framework

In this section, we give a brief review of the econometric models used[23]. We first go into some detail concerning the duration (single exit) model, which is similar to the models used in previous

[22] The balance of destinations are censored observations: individuals who either disappeared (temporarily) from the dataset, for whom it was impossible to reconstruct in what industry they detained their next job, or who are still at their job at the last interview during that job tenure (non-movers).

[23] For a more extensive expostion, see Lancaster (1990)

papers. The multiple destination model follows. We then derive two specializations, the competing risks model and a sequential, or separable, model. As we show, it is possible to distinguish between the two models in the data by a fairly intuitive test, allowing us to concentrate on the appropriate model in further analysis.

4.1 Single exit duration models

Duration models are based on a random variable T representing the time until exit from a job. The hazard rate $\lambda(t)$ is defined as the (instantaneous) probability of an event occuring in period t, conditional on the event not having occured until now:

$$\lambda(t) = \lim_{dt \to 0} Prob(t < T \leq t + dt | T > t)/dt \tag{1}$$

and is equal to $f(t)/S(t)$, where $S(t)$ is the survivor function $1 - F(t)$, and $f(t) = -dS(t)/dt$ the density. Thus, the hazard can also be written as $\lambda(t) = -\partial lnS(t)/\partial t$. From this, a useful identity is

$$S(t) = \exp(-\int_0^t \lambda(s)ds). \tag{2}$$

It can be shown that the integrated hazard has a unit exponential distribution. Specification of the hazard rate defines the distribution of durations, and vice versa.

Covariates can be modeled to affect the distribution in various ways. The parametric methods used in this paper assume an accelerated failure-time model:

$$T = k_1(t)k_2(x) \tag{3}$$

where $k_1(t)$ is a transformtion of time, and $k_2(x)$ a proportionality factor. Hence, any two persons differing in their x's have the same baseline duration distribution of $k_1(t)$, but differ in their observed event times by a constant proportional factor of $k_2(x_1)/k_2(x_2)$. In the simplest specification, $k_1(t) = t$ and $k_2(x) = \exp(-X\beta)$. Throughout this paper, the specification of the proportionality function in exponential form is maintained. This allows us to rewrite (3) as a linear regression model:

$$\log T - X\beta = u. \tag{4}$$

In the case of a constant hazard, $\log T - X\beta$ is just the integrated hazard, which implies that $\exp(u)$ follows an Exponential distribution, but generalizations lead to Weibull, Gamma, and Normal distributions[24]. If u had a normal distribution and censoring were not a problem, this could be estimated by OLS. However, most data contains censored spells, and this needs to be reflected in the likelihood.

Another possibility is the *proportional hazard* specification:

$$\lambda(t;x) = \tilde{k}_1(t)\tilde{k}_2(x) \tag{5}$$

In this case, $\tilde{k}_1(t)$ is the baseline hazard function common to all individuals. Now, two persons differing in their x's differ in their *hazard* by a constant proportionality factor of $\tilde{k}_2(x_1)/\tilde{k}_2(x_2)$.

The advantage inherent to proportional hazard models is the possibility of estimating $\tilde{k}_2(x)$ independently of the baseline hazard in a partial likelihood approach (Cox 1972). However, inference as to the expected duration is not possible. On the other hand, it's ease of use allows inference on a number of other dimensions, as we will see further on.

In both cases, the (log-)likelihood contribution of an observed exit from employment is just $f(u)$ if not censored, and $S(u)$ if censored, and using $f(t) = \lambda(t)S(t)$, we can write this as

$$l = \sum_i (1 - c_i) \log \lambda(u_i) + \log S(u_i) \tag{6}$$

where c_i is an indicator variable equal to unity if an observation is censored.

Choice of the wrong distribution in estimation may lead to misspecification and hence biased results, particularly in duration analysis[25]. However, for inference on the quantitative effect of the

[24]See Lancaster (1990) for more details.

[25](Meyer 1990, Sueyoshi 1992). (McCall 1990) compares Weibull estimates with semi-parametric estimates.

covariates, specifying the duration distribution is useful. Following the preliminary analysis in the previous section, we decided to use distribution functions which allow for single-peaked hazards. The present paper presents results using Gamma and log-normal specifications, with some results also available for the (monotonous hazard) Weibull specification. The results we obtain are of course conditional on having chosen the correct baseline hazard .

For some of our results, it is not necessary to know the distribution of duration. In the case of a proportional hazard model, a partial likelihood can be derived. Denote by $t(j)$ the jth observed exit time, $x(j)$ the characteristics of the individual exiting at time $t(j)$, and R_j the risk set at $t(j)$, i.e. the individuals who could still have exited at this time. Then the (Cox) partial likelihood is

$$l = \sum_{j=1}^{J} \left[\log \tilde{k}_2(x(j)) - \log \sum_{k \in R_j} \tilde{k}_2(x(j)) \right] \tag{7}$$

which does not depend on $\tilde{k}_1(t)$. Furthermore, by simply redefining the risk set R_j to include only multiple observations for the same individual, it is straightforward to control for (multiplicative) individual heterogeneity in the hazard function.[26] We will use the partial likelihood approach in the analysis of multiple destinations, as explained in the next section.

4.2 Multiple destinations

The analogous quantity to the hazard rate in a multiple destination framework is the transition intensity $\lambda_m(t;x)$. Let d_m be a dummy variable equal to unity if exit occurs to destination m. Then the transition intensity is defined as the (instantaneous) probability of departure to destination m given survival to t

$$\theta_m(t;x) = \lim_{dt \to 0} Prob(t < T \leq t + dt, d_m = 1 | T > t; x)/dt \tag{8}$$

[26] See Lancaster (1990) for more details.

The hazard function is equal to the sum of transition intensities over all possible destinations m:

$$\lambda(t;x) = \sum_{m=1}^{M} \theta_m(t;x) \tag{9}$$

and the survivor function is defined as by (2).

For any given individual, we observe a M-vector of indicators $\{d_m\}$ and exit time t, besides the covariates x[27]. The contribution to the likelihood is given by the probability that she left for destination m at time t:

$$P(\text{left for } m \text{ at time } t) = \theta_m(t;x)S(t;x) \tag{10}$$

which can be rewritten as

$$p(d_1, \ldots, d_m, t; x) = \exp\left\{-\int_0^\infty \sum_{m=1}^{M} \theta_m(s)ds\right\} \prod_{m=1}^{M} \theta_m(t;x)^{d_m} \tag{11}$$

For our purposes, it is useful to specify a number of different probabilities. First, define the *marginal probabilities* of the destinations, i.e. the probability that when exit occurs, it occurs to destination m. Integrating (10) over t yields

$$\pi_m = \int_0^\infty S(s)\theta_m(s)ds. \tag{12}$$

Another useful measure is the *probability of choosing destination m over destination k*, where $\{m, k\}$ is a subset of M. For instance, as pointed out in Section 2, we are interested in the probability of changing sectors, conditional on switching jobs and on t, and the probability of finding a job, conditional on leaving the current job and on t. With (10), the former can be seen to be

$$\begin{aligned} M_2(t) &= \frac{P(\text{left for sector 2 in period } t)}{P(\text{left for sector 2 in period } t) + P(\text{left for sector1 in period } t)} \quad \text{as } h \to 0 \\ &= \frac{\theta_2(t;x)}{\theta_1(t;x) + \theta_2(t;x)} \end{aligned} \tag{13}$$

[27]Note that censoring in this context can be modeled as another destination, and a censoring indicator can thus be subsumed into the M indicators.

where "period t" is understood to mean "between t and $t+h$". In general, $M_2(t)$ is time-dependent and will depend on the estimated baseline hazards for each risk. However, in the context of the proportional hazard model, with $k_2(x) = \exp(x\beta)$, the sign of the derivative of $M_2(t)$ with respect to a covariate x_j is time-invariant:

$$\begin{aligned} \frac{\partial M_2(t)}{\partial x_j} &= \frac{\theta_2(t)\beta_{2j} \cdot (\theta_1(t) + \theta_2(t)) - \theta_2(t) \cdot (\theta_1(t)\beta_{1j} + \theta_2(t)\beta_{2j})}{[\theta_1(t) + \theta_2(t)]^2} \\ &= \frac{\theta_1(t)\theta_2(t)}{[\theta_1(t) + \theta_2(t)]^2}(\beta_{2j} - \beta_{1j}) \end{aligned} \tag{14}$$

Thus, $sign(\partial M_2(t)/\partial x_j) = sign(\beta_{2j} - \beta_{1j})$, which does not depend on the destination-specific hazards, a very useful property of the proportional hazard models. The probability of finding a job once the current job has ended was defined in Section 2 as

$$M_{job}(t) = \frac{\theta_1(t) + \theta_2(t)}{\lambda(t)} \tag{15}$$

where $\lambda(t)$ is defined as in (9) as the sum of the destination-specific transition intensities. The derivative of (15) with respect to a covariate x_j is

$$\frac{\partial M_{job}(t)}{\partial X_j} = \frac{\theta_3(t)}{[\sum_{i=1}^{3} \theta_i(t)]^2} \left[\theta_1(t)(\beta_{1j} - \beta_{3j}) + \theta_2(t)(\beta_{2j} - \beta_{3j})\right]. \tag{16}$$

which may be of ambiguous sign. However, by aggregating all job exits irrespective of industry of the next job held, i.e. $\theta_{job} = \theta_1 + \theta_2$, we find an equivalent expression to (14), which can be unambiguously signed.

4.3 Competing risk model

Now consider a person drawing from M independent distributions of tenure $f_m(t_m)$, hazards λ_m, and survivor functions $S_m(t_m) = \exp\{\int_0^{t_m} \lambda_m(s)ds\}$. Each represents the risk of exiting from the present job to destination m. However, only the smallest realization $t = \min_m\{t_m\}$ is observed, hence the term "competing". All other draws are (right-)censored. Then the likelihood of observing an exit to destination m is the product of the observed density of distribution m, $\lambda_m(t)S_m(t)$ and the probability that all other draws are larger than t, $\prod_{j \neq m} S_j(t)$. Using (2) and the independence of t_m,

$$\begin{aligned} P(\text{exit to } m \text{ in period } t) &= \lambda_m(t) S_m(t) \prod_{j \neq m} S_j(t) &(17)\\ &= \lambda_m(t) \exp\left\{-\int_0^t \lambda_m(s) ds\right\} \exp\left\{-\sum_{j \neq m} \int_0^t \lambda_j(s) ds\right\} \\ &= \lambda_m(t) \exp\left\{-\sum_j \int_0^t \lambda_j(s) ds\right\} \\ &= \lambda_m(t) S(t). &(18)\end{aligned}$$

It can be seen that (18) is equivalent to (10) with $\lambda_m(t) = \theta_m(t)$. On the other hand, using (17) to write the likelihood of an individual observation,

$$\begin{aligned} p(d_1, \ldots, d_m, t; x) &= \prod_{m=1}^{M} \left\{ \lambda_m(u) S_m(u) \prod_{j \neq m}^{M} S_j(u) \right\}^{d_m} \\ &= \prod_{m=1}^{M} \lambda_m(u)^{d_m} \prod_{m=1}^{M} S_m(u) \\ &= \prod_{m=1}^{M} L_m &(19)\end{aligned}$$

where independence of the distributions of all T_m was assumed, and $L_m = \lambda_m(u)^{d_m} S(u)_m$. Since L_m is equivalent to the (log-)likelihood of a duration model given by (6), it can be estimated separately. The contribution of an observed exit to destination $n \neq m$ to likelihood L_m is thus the same as that of a censored observation in the duration model. Again, a partial likelihood can be derived in the case of the proportional hazards model, where the model partial likelihood is the product of the destination-specific partial likelihoods.[28] The assumption of independence is restrictive, though often seen in the literature.[29]

[28] See Lancaster (1990), Chapter 9, for more details.
[29] E.g. Belzil (1993), Booth & Satchell (1993)

4.4 Sequential model

It is of interest to distinguish the competing risks model from another specialisation of the multiple destination model. Call it a sequential model, for reasons which will become apparent. Consider the case where transition intensities are identical up to a time-independent proportionality factor k_{2m}, i.e.,

$$\theta_m(t) = k_1(t,x)k_{2m}(x;\beta_m) \tag{20}$$

Using (9) and cancelling out the common factor $k_1(t,x)$, we obtain a *proportional intensity model* with proportionality factor μ_m defined as

$$\frac{\theta_m(t;x)}{\lambda(t;x)} = \frac{k_{2m}(x;\beta_m)}{\sum_{j=1}^{M} k_{2j}(x;\beta_m)} \quad = \quad \mu_m \qquad \forall k \tag{21}$$

Then the marginal probability of destination m as defined by (12) can be written as

$$\begin{aligned}
\pi_m &= \int_0^\infty S(s)\lambda_m(s)ds \\
&= \mu_m \int_0^\infty S(s)\lambda(s)ds \\
&= \mu_m \int_0^\infty S(s)\frac{f(s)}{S(s)}ds \\
&= \mu_m
\end{aligned}$$

Thus, π_m, the probability that when exit occurs, it occurs to destination m is simply the proportionality factor associated with transition intensity m. If $k_2(x;\beta_m) = \exp(-x\beta_m)$, then μ_m and the marginal probability π_m take the form of a logit model:

$$\mu_m = \pi_m = \frac{\exp(-x\beta_m)}{\sum_{j=1}^{K}\exp(-x\beta_j)} \tag{22}$$

Note that the commonality of time-dependent components of the hazard across destinations is a necessary condition for this result to hold. Assume it does not. Then μ_m is a function of time and

$$\pi_m = \int_0^\infty S(s)\lambda_m(s)ds$$

$$
\begin{aligned}
&= \int_0^\infty S(s)\lambda(s)\mu_m(s)ds \\
&= \int_0^\infty \mu_m(s)f(s)ds
\end{aligned}
$$

which cannot be estimated as a standard logit model. In fact, since in this case the baseline transition intensities differ across destinations, it is more appropriate to use the competing risks model.

If the assumption holds, we can rewrite the model as

$$\lambda_m(t;x,\beta) = k_1(t,x_1;\beta_1)k_{2m}(x_2;\beta_{2m}) \tag{23}$$

where x_1 are the variables included in the estimation of the common baseline hazard, x_2 are those included in the estimation of the marginal probabilies of destinations (possibly overlapping), and β_j, $j = 1,2$ the parameters associated with each model. This is why we call this a *sequential model*: It implies that the process determining spell duration is completely separable from the process determining destination. In other words, there is one set of parameters determining when a worker leaves a firm, and another set of parameters determining her labor market activity afterwards. Each component can be estimated separately to obtain consistent estimates of the βs, k_1 as a standard duration model, k_2 as multinomial logit (or probit)[30]. The logit model thus defines the likelihood for all observations conditional on separation.

An obvious implication is that inference as to the effect of covariates on the length of jobs will not be affected by the extension to multiple exit states. By including training variables in x_2, the effect of training on the choice of sector after job separation can be analyzed. Note that we can compute the signs of $\partial M_2/\partial x_j$ and $\partial M_{job}/\partial x_j$ from the logit estimates in the same way as for the proportional hazard model.

A simple test can be performed between the appropriateness of the sequential or the competing-risks formulation by estimating a logit model of choice of destination on all person-jobs which have ended, irrespective how long the preceding job. Under the null hypothesis of the appropriateness

[30]Of course, we are assuming that errors for each component are independent.

of (20), the logit model does not depend on tenure on the last job held. We present results for this test in the empirical section.

5 Results

We start out with a discussion of the results obtained in the single-exit duration model, as these results are comparable with those obtained by other authors (Parent forthcomingb, Lynch 1992b).

5.1 Duration analysis

Panel (a) of Table 7 on page 47 reports estimates of the effect of training variables using gamma, log-normal and Weibull distributions of duration. The qualitative results are robust to the specification of the baseline distribution, and in the discussion below, we concentrate on results obtained for the gamma distribution.[31] The training variables are all significant, and of substantial impact. Consistent with previous results, training on the current job and off-the-job training increase expected tenure, whereas training received on previous jobs increases mobility.

Tables 7 and 8 here.

Contrary to previous studies, the use of a parametric duration distribution allows us to perform some inference on expected durations. Computing the expected tenure with and without training permits us to quantify the net impact of on-the-job training, i.e. the increase in expected tenure after time spent on the training program, measured as full-time equivalent weeks, has been deducted. The following example, results for which are reported in Table 8 on page 47, will serve to clarify this.

Consider an individual having 4 years of labor market experience acquired on three different jobs with no previous training, and working 35 hours on the current job. This is an "average" individual in our sample. His[32] expected tenure will then be approximately 107 weeks. Assuming he receives

[31] The Weibull model is a restricted versions of the Gamma distribution. The relevant parameter restrictions can be rejected at the 1 percent level. The Log-normal specification is rejected on the basis of a LR test with test statistic of 938.3. The statistic is $\chi^2(1)$, with a 1 percent critical value of 6.635.

[32] The coefficient on the included dummy for the sex of the individual is small, on the order of one percent, and not significant on a 5 percent level.

training, he can expect to spend about 320 hours on training over the duration of the current job, or about 9 weeks of full time equivalent.[33] Training increases his expected tenure by about 6.82 weeks, with an upper bound of the two-sided 95 percent confidence interval of 8.73 weeks. Expressed in expected average weekly hazards, the value is 0.94 percent before training. Training decreases the expected average value to 0.88 percent, but subtracting the duration of the training spell from total expected tenure and recomputing training intensity, the *net* hazard is 0.96 - slightly higher than without the training spell. The result also holds when using the log-normal distribution.

Another possibility is to compute the impact on the average median worker.[34] The median worker in our sample has an expected duration of 45.99 weeks. Setting hours of training to zero leads to an expected duration of 45.33 weeks. If all workers were then trained for 320 hours, the median worker's expected duration rises to 53 weeks.[35]

In the current specification, we do not control for heterogeneity in the parametric models. Results from the partial likelihood estimates reported later show that controlling for heterogeneity is likely to increase the effect of training. The resultant increase of the parameter on training in the Cox partial likelihood is on the order of 40 percent, with associated standard errors about twice as large. Results would still hold approximately.

This example illustrates a first conclusion of this paper: We cannot reject the hypothesis that the increase in tenure is actually less than the time spent on the training program, and that training thus has no net impact on tenure with the firm providing the training. Another way to put this result is that the estimated increase in expected tenure due to training can be fully attributed to the length of the training spell itself. In other words, expected training does not increase the net working time the worker spends with the training employer, confirming, it seems, popular fears as expressed in the initial quotation. The same result obtains if we include weeks of training rather than total hours of training over expected tenure. This result obviously depends on the specification

[33] Again, these numbers approximately reflect sample averages. The sample mean of hours worked per week is 36.26 hours.

[34] Formally, we compute the expected duration evaluated at the .5 percentile for the whole sample, and take the average.

[35] Performing the same exercise with the log-normal distribution of tenure leads to values of 55.63, 54.36, and 68.61 weeks, with a lower bound of the confidence interval on the latter value at 63.28 weeks. This reflects the form of the duration distribution, which is more tail-heavy for the log-normal. The conclusion, however, still holds.

of the duration distribution, but it seems robust to variations thereof. It holds for the "typical" and for the median worker, suggesting that, though positive, the impact of training on tenure may have been overstated.[36] Of course it can be argued that though it does not hold for the median worker, there are still workers for whom the net impact is positive. Our aim here is not to assert that there is never any effect, but to cast doubt on the assertion that there always is positive effect.

The question then arises whether training actually confers firm-specific abilities, as has been the general conclusion in the literature. The results here cast doubt on that conclusion. An analysis of the mobility effects of training may allow to answer this question, and will be the subject of the next subsections.

5.2 Sequential model

As a first step to the estimation of transition intensities, we add to the previous single-exit duration model a multinomial logit model of sectoral allocation[37] process. The underlying assumption here is that the single-exit model correctly captures the determinants of exit, of which training does not seem to be one, but that a "second-stage" model of sectoral allocation is required. In other words, the duration model captures any factors common to all three destinations.

Table 9 here.

Panel (a) of Table 9 on page 48 presents multinomial logit estimates of the reduced-form parameters of sectoral allocation, the three categories being the usual ones used in this paper.[38] The probability of entering non-employment conditional on leaving a job decreases with experience and tenure. Unionized workers are more likely to find a job than non-unionized workers, but the number of jobs held in the past decreases the probability of finding a job. However, these variables do not seem to affect the probability of a sectoral change. On the other hand, the probability of a sectoral change decreases with experience at the start of the job and with hours worked on the job.

[36] In results not reported here, we have performed a fair amount of sensitiviy analysis, and the results are quite robust to sample selection and specification issues

[37] To ease terminology, we treat non-employment as another sector.

[38] The transition window in Table 9 is set to five weeks. Results for windows of one and nine weeks do not differ significantly.

Turning to the training variables, the most striking result is the absence of any effect of training with the last employer. Neither the probability of employment nor the probability of sectoral change are affected by training with the last employer. More in line with a model of sector-specific training, training received with previous employers in the same industry (other industries) decreases (increases) the probability of sectoral move.[39]

Adding to these results those from the previous section, we could conclude that on-the-job training neither increases tenure with the training firm in an economically meaningful way, nor affects sectoral allocation. Both results are consistent with a model of general training. This would obviously conflict with the interpretation we can give to the coefficients on training received with previous employers.

However, the model does not pass the test expounded in Section 4.4. The coefficient on tenure in the last job before separation is significantly greater than zero. Furthermore, results for a flexible specification in tenure reported in Panel (b) show that the time dependency for all three destinations differ substantially.[40] Hence, our test rejects the appropriateness of the sequential model, and we would favor a competing risks model. And the result that training has no effect on sectoral allocation must seem premature at this stage .

5.3 Competing risks

As a next step, we estimate a competing risks model in a proportional hazards setting. This allows us to quantify the impact of training on each destination-specific risk as well as on the probability of a sectoral move and on labor force attachment. Contrary to the sequential model previously estimated, time to exit and choice of exit are modeled jointly. Table 10 on page 49 reports coefficients on training variables.[41] Column (a) is the hazard model as already reported earlier. Columns (b) through (d) report coefficients from a model with the three competing destinations “job in a different industry”, “jobs in the same industry”, and “no job found, non-employed”. Column (e) reports coefficients

[39]All differences in coefficients are significantly different from zero at the 5 percent level.

[40]The joint hypothesis that tenure has no effect in all destinations can be easily rejected.

[41]Estimates using the accelerated failure-time models of Section 5.1 yielded the same signs for the training variables, but in those models, the sign of the probability of sectoral change depends on all coefficients of the model, and can only be approximated by the comparison we provide here. Results for those models are available on demand.

when aggregating the two former categories into a category "job found" without distinguishing the industry in which the next job is located. The results were obtained assuming a transition window of five weeks.

Table 10 here.

The effects of other variables (reported in Table 11 on page 50) are as follows. Women are less likely to change sector, and more likely to transit into non-employment than men, though no differences seem to exist as to the transition intensity to same-industry jobs. In all transition intensities, education has no significant effect. Experience increases transition intensities to both industries, but reduces transitions out of employment. Though this might seem counter-intuitive at first glance, remember that the effect on the overall hazard is negative, thus implying that more experienced workers are less likely to separate from their current job, but upon separation are more likely to stay employed. The number of jobs ever held decreases both job transitions, but increases the transition intensity to non-employment, possibly serving as an indicator for people with a lower labor force attachment. Usual hours worked on the current job are correlated with lower transition intensities out of the current industry, but increases the intra-sectoral transition intensity.[42] Jobs with higher initial wages are correlated with lower transition intensities to job in other industries and out of employment, but wages have no effect on intra-sectoral transition intensities.

The coefficients of interest are those on on-the-job training. All coefficients on training with the current firm are negative, implying the increase in tenure observed earlier, though the present specification does not allow us quantify the relative impacts. Barring selection aspects, which we will explore later, this implies that training is correlated with higher firm-attachment. However, it is clear from the estimates that training has different effects on each risk. Thus, the coefficient of on-the-job training is smaller in absolute value for transitions to same-industry jobs than for transitions to jobs in other industries. Furthermore, whereas training in other industries has no significant effect on transitions to same-industry jobs, training received in the same industry has no

[42]This may be coherent with a multidimensional utility function and the idea that hours worked is an industry characteristic. Since the mean industry-specific effect of hours is captured by the industry dummy, the hours variable captures any variations beyond this. Higher hours in the current industry make other industries seem more attractive for a given wage and wage offer. I thank David Margolis for pointing this out to me.

effect on transitions to jobs in other industries, and training received in other industries increases these transitions, suggesting industry-specificity of training. Previously received training never has any effect on transition intensities out of employment, whether acquired in the current or another industry. Finally, off-the-job training does not seem to have any impact on job transitions, but reduces transition intensities to non-employment. This is what we analyze more formally furtheron, using the conditional probabilities discussed earlier.

Table 12 here.

Table 12 on page 51 reports results when heterogeneity is controlled for in the Cox partial likelihood framework. The tenure-increasing effect of training is increased by about 40 percent. There no longer seems to be any differential effect of training with the current employer according to destination. This pattern seems more in line with firm-specific training. Remember from Section 5.1, though, that the quantitative effect of of this effect is negligible. Furthermore, the effect of previously received training reduces the overall hazard, irrespective of the industry in which training was received, but this effect seems to come entirely from a reduction of the transition intensity into non-employment. The interpretation in our model is that wage offers from any sector have become relatively more attractive. This belies firm-specificity, and points towards general or industry-specific training.

Thus, results from an analysis of the effect on transition intensities do not provide a clear picture. Possibly, and not surprisingly, training has both general and specific components. A clearer picture appears when we compute the conditional probabilities laid out earlier. Tables 13 to 20 on pages 52 to 59 provide the empirical counterparts to Table 1 in Section 4. Column (a) in Table 13 computes the approriate probabilities for the results reported in Table 10, and Column (b) for those in Table 12. In columns (c) and (d), we control for the fact that the NLSY oversamples certain demographic groups, and columns (e) and (f) reports results for when we include controls for whether or not the trainee completed the program or not.

The first row of Table 13 shows the effect of training with the current employer on the probability of a sectoral move when changing jobs, $\partial M_2(t)/\partial ONCJT$. It is consistently negative, though those specifications which control for individual heterogeneity provide noisier estimates. The impact of training on previous jobs differs with its source. In most specifications, if training was acquired in the same industry (row 3), sectoral mobility is reduced. If it was acquired in a different industry (row 2), sectoral mobility is increased. These results suggest that training has a component which is sector-specific, since the signs of the effect of all three on-the-job training variables are inconsistent with the mobility patterns of either firm-specific or general human capital.

As reported in Table 6 on page 46, only about 10 percent of training is not completed. Controlling for incomplete training duration does not change coefficients on completed training, as reported here.[43]

Columns (b), (d), and (f) control for individual heterogeneity, and the results suggest that a large amount of the mobility patterns associated with training may be due to this kind of heterogeneity. Note however that this generally occurs because of increased standard errors, and not because the sign of the point estimate for the probability of a sectoral move changes. However, controls for heterogeneity also take out any effect constant per individual, but heterogeneous in the data, possibly hiding more general patterns. In Tables 14 to 16, we explore the impact of control for gender. A comparison of Panel (a) of Table 14 on page 53 with its theoretical counterpart, Table 1 on page 41, remains inconclusive. However, once individual heterogeneity is controlled for, the pattern is clearer. While the coefficients for training with the current firm would suggest that training is firm-specific, the effect of previous training in the same industry seems more consistent with industry-specific training, as are, to a lesser degree, those on training acquired in other industries. Training acquired in the same industry reduces the transition intensity to non-employment, which suggests industry-specific or general training. The effect of training on the overall hazard confirms this. Turning again to the probability of sectoral moves in Table 16 on page 55 reinforces support for the interpretation of training as industry-specific capital: Both training with the current firm and with prior employers in the same industry reduce the probability of a sectoral move. Inconsistent with the expounded theory, training received in other industries reduces the probability of quitting the current industry.

[43]Not reported here, coefficients on incompleted training generally are of same magnitude and opposite sign as those on completed training, cancelling out any effect of completed training.

For women, the pattern is less clear. Whereas the effect of different types of training on the transition intensity to non-employment again suggest firm-specific training even after controlling for heterogeneity, all types of training uniformly reduce both job transition intensities, which our theory cannot accommodate. The effect on the overall hazard again suggests firm-specificity. Again turning to the probability of sectoral moves, Table 16 shows that training with the current firm reduces the probability of a sectoral move, consistent with industry-specificity, but previous training in the industry actually increases the probability of a sectoral change. However, all these probabilities are not significantly different from zero, which may suggest either firm-specific training or general training.

This is possibly linked to different occupational patterns of men and women, which are not controlled for in this paper. As an example, if women are more likely to be in clerical occupations, and training occurs for these occupations, it may well be that employment options are increased in other industries as well. This subject remains to be explored.[44] The coefficient of training acquired in other industries by men on the probability of a sectoral move implies that although training was received in a different industry, it reduces the probability of leaving the current industry. For women, a different story emerges: training received in the current industry actually increases the possibility of leaving the current industry. If there exist "entrance" or "feeder" industries which are used as starting points for careers which end in other industries,[45] such a pattern could be observed if our data consists primarily of men who have already left the "feeder" industry and of women who are still overwhelmingly in their "feeder" industries.[46] This leaves substantial room for future research.

The fact that controls for heterogeneity substantially weaken the reported effect on the probability of sectoral mobility may be due to selection problems referred to in Section 2. If training is dispensed only to individuals who are less mobile, then measuring hours of training without controls for individual heterogeneity in the baseline hazard could lead to the observed correlation between

[44]See McCall (1990) for a test of occupational matching, though not mobility.

[45]See Jovanovic & Nyarko (1997) for a possible theoretical explanation.

[46]See McCall (1990) for some evidence on "feeder" occupations. In the context of intra-firm mobility, Baker, Gibbs & Holmstrom (1994) provide evidence of occupational career ladders within an organization.

training and mobility. Training proxies for intrinsic mobility observable by the employer. In that case, the same should be true for an indicator of training receipt. To explore this further, we replaced hours of training by an indicator for the incidence of training with the current company as regressor. Results reported in Table 17 on page 56, columns (a) and (b), do not seem to support this interpretation. Incidence is robust to the specification of heterogeneity except when used for training acquired in other industries. Incidence of training is correlated with a decline in sectoral mobility as long as training is acquired in the same industry, where it is not important whether the current employer or previous employers provided it. This would seem at odds with selection purely based on mobility.

A different selection story would say that training is not dispensed arbitrariy, and that whatever characteristic the employer uses as a selection criterion may be spuriously correlated with differences in mobility patterns. To explore this, we restricted our sample to those observations for workers who had already received training with some previous employer, and who have changed employers since. If there were a systematic difference between workers receiving training and others, then it could be expected that any residual mobility effect of training would be captured without controls for heterogeneity, i.e. the subsample of observations thus selected provides adequate control for selection-based heterogeneity. Table 17, columns (c), shows results without controls for heterogeneity. This selected subset of workers, homogeneous in the respect that they have already been selected at least once for training, still shows the by now typical pattern of sectoral mobility, corresponding to the case of industry-specific training, though the effect is weaker than for the full sample. Thus, the mobility patterns found so far cannot be solely attributed to a selection bias into training. However, column (d) highlights the fact that controls for heterogeneity still increase the standard errors, thus reducing the level of significance substantially, without changing the signs of the computed probabilities.

These results suggest that at least in part, the endogeneity of the separation decision with respect to training might still be biasing our results. A valid exogenous instrument for separation that has been frequently used in labor economics is that of plant closure. The resultant displacement of workers is assumed to be the result of factors outside the worker-firm match.[47] Restricting the sample to displaced workers yields the results reported in column (e) of Table 17. Here, the probability of sectoral change is decreased by training acquired in the same industry, and increased by training in other industries, though none are significant, possibly to the small sample size.[48] These results for this small sub-sample of workers would again seem to indicate the presence of industry-specific training.

We next turn to the conditional probability of finding a job, expressed by M_{job} as defined in Section 2. Tables 18 to 20 report results for the same specifications explored previously. The results are fairly robust across all specifications, revealing the beneficial effects of training with respect to the probability of being employed after a job separation: Training, whether on or off-the-job, increases the probability of re-employment conditional on separation. Again, this seems inconsistent with (pure) firm-specificity. Some differences from this general pattern, however, are worth pointing out. As Table 18 on page 57 shows, though positive, the employment effect of training with the current firm is not significantly different from zero when excluding the oversampled population. Though this may again suggest firm-specificity, it disappears once the effect of incomplete training is taken out (columns (e) and (f)). Employment attachment is then increased for all types of training, possibly giving an indication of training serving as a signal. The positive employment attachment effect of training seems to be equally strong for training acquired in the same industry as for training with the current firm, but weaker if training was acquired in another industry. Although our theory does not provide much guidance in evaluating the relative size of the impact, this may suggest industry-specificity: The probability of receiving a job offer from the own sector is stronger.

Turning to gender-specific results in Table 19 on page 58, we again note some differences in the effect of training on employment attachment probabilities between men and women, possibly related to occupational mobility patterns. Whereas training received with the current employer

[47]See Neal (1995) for an application to identify industry-specific informal training (experience). An extensive analysis of the long-term income effects of displacement is found in Jacobson, LaLonde & Sullivan (1993).

[48]Regressions for displaced workers controlling for heterogeneity did not yield results. Only 217 worker in the sample experienced displacement more than once. The sample means show that their jobs are in general in areas of higher unemployment, that completed tenure is lower, and that they are paid lower wages. The sample average of training is actually higher than for the full sample, but otherwise the sample means do not seem to differ substantially from the full sample.

increases employment attachment for both sexes, for men it turns out that the effect of training received with previous employers in the same industry is stronger than for training received with employers in other industries. For women, however, any previously acquired training increases labor force attachment by about the same factor.

Replacing hours of training with its incidence (Table 20 on page 59, columns (a) and (b)) leads to the insignificance of training received in the current industry, though the signs are still positive when heterogeneity is controlled for. The strongest effect seems to come from training in other industries. Columns (c) and (d) reports results for hours of training when incidence is added as supplementary explanatory variable instead of replacing hours as in columns (a) and (b).[49] When both incidence and hours of training are included as explanatory variables, the effect of training with the current employer is still very imprecisely estimated. However, hours of training received with previous employers have an effect above and beyond a pure incidence effect, particularly when heterogeneity is controlled for. Thus, even if though selection into training may play a role with the current company, the duration of training received with previous companies does show a positive impact on the probability of employment, inconsistent with a pure selection argument.

Finally, the evidence for displaced workers having training in other industries, column (e), is unclear, but the effect of previous training in the same industry, though too noisy an estimate, points in the direction consistent with non-firm-specific training (general or industry-specific). Note also that the employment effect of off-the-job training, which significantly increases the probability of employment after a job separation in most of the specifications considered, does displaced workers no good. If off-the-job training serves as a preparation for a career move, then displaced workers are possibly surprised by their displacement, and cannot focus such activities.[50]

[49]The coefficients on incidence do not change substantially when duration is included.

[50]Note however that Jacobson et al. (1993) point out that earnings for displaced workers decrease several quarters before displacement, indicating that workers should have ample notice of displacement.

6 Conclusion

In this paper, we have used the detailed data on formal on-the-job training available in the NLSY to re-evaluate the mobility effects of such training. We report estimates on the quantitative impact of training as well as on the intra- and inter-sectoral mobility patterns associated with training.

We find that although training does increase expected tenure with the training firm, the increase does not seem to exceed the length of the training spell itself, whether evaluated at the mean or the median duration of job spells: Net working time is unaffected by training. This would be consistent with human capital theory if the capital formed through training were applicable to a number of firms, either throughout the economy (general human capital) or within the same industry (industry-specific human capital). It confirms results obtained on the remuneration of training by the training firm and subsequent employers, which showed that training was remunerated by the latter at the same rate as by the training firm itself, suggesting transferability of human capital acquired through training.

To determine the degree of specificity, we analyze the mobility patterns of workers after job separation, concentrating on the sectoral mobility, with non-employment modeled as a third sector. Conditional on leaving the current firm, a multinomial logit finds no effect of training on the sectoral allocation of workers. However, we test and reject the sequential multinomial model in favor of a competing risks specification.

The results from a proportional hazard specification of the competing risks model provide substantial evidence for industry-specificity of training, though the mobility patterns also reveal some firm-attachment related with training. The effect of training with the current firm seems to uniformly reduce transition intensities to all destinations, though as the result on duration implies, the increase may not be substantially more than the time spent on training programs.

Consistent with a model of sector-specific human capital, training acquired in the current industry, whether with the current employer or with previous employers, is associated with a reduction in the probability of a sectoral move. Strongest evidence for industry-specificity comes from men, for whom the probability of a sectoral change is substantially reduced by training within the same industry. The industry-specificity is especially present when incidence of training is used instead of total hours of training, suggesting that the interplay of training and mobility may be more complex than what can be captured by hours of training. However, the pattern provided by training acquired in other industries does not conform well with a matching-augmented model of human capital.

The evidence for sector-specificity from the probability of employment attachment is less strong. Though training with previous employers generally increases employment attachment, the effect of training with the current firm seems less clear.

Overall, the evidence points to a strong sector-specific character of training when mobility patterns are taken into account. This helps to partially explain why previous studies have found that firms remunerate training received with prior employers, though subsequent analysis should take into account the industry in which prior training was acquired. However, it increases the mystery of why firms would pay for training which is of use to other employers, as the same wage regressions seems to show. More research in this area is thus called for.

References

Abraham, K. & Farber, H. (1987). Job duration, seniority and earnings, *American Economic Review* **77**.

Acemoglu, D. & Pischke, J.-S. (1996). The structure of wages and investment in general training, *mimeo*, MIT.

Acemoglu, D. & Pischke, J.-S. (1998). Why do firms train? Theory and evidence, *Quarterly Journal of Economics* **113**(1): 79–119.

Altonji, J. G. & Shakotko, R. A. (1987). Do wages rise with job seniority?, *Review of Economic Studies* **54**: 437–59.

Altonji, J. G. & Spletzer, J. R. (1991). Worker characteristics, job characteristics, and the receipt of on-the-job training, *Industrial and Labor Relations Review* **45**(1): 58–79.

Baker, G., Gibbs, M. & Holmstrom, B. (1994). The internal economics of the firm: Evidence from personnel data, *Quarterly Journal of Economics* **109**(4): 881–919.

Barron, J. M., Berger, M. C. & Black, D. A. (1997). How well do we measure training?, *Journal of Labor Economics* **15**(3): 507–28.

Becker, G. S. (1964,1993). *Human capital: a theoretical and empirical analysis, with special reference to education*, University of Chicago Press.

Belzil, C. (1993). An empirical model of job-to-job transition with self-selectivity, *Canadian Journal of Economics* **26**(3): 536–51.

Booth, A. L. & Satchell, S. E. (1993). Apprenticeships and job tenure: A competing risks model with time- varying covariates, *Technical Report 762*, CEPR.

Cox, D. (1972). Regression models and life tables (with discussion), *Journal of the Royal Statistical Society* **B**(34): 187–220.

Farber, H. S. (1994). The analysis of interfirm worker mobility, *Journal of Labor Economics* **12**(4): 554–592.

Gritz, R. M. (1993). The impact of training on the frequency and duration of employment, *Journal of Econometrics* **57**: 21–51.

Hashimoto, M. (1981). Firm-specific human capital as a shared investment, *American Economic Review* **71**(3): 475–82.

Jacobson, L. S., LaLonde, R. J. & Sullivan, D. G. (1993). Earnings losses of displaced workers, *American Economic Review* **83**(4): 685–709.

Jovanovic, B. (1979a). Firm-specific capital and turnover, *Journal of Political Economy* **87**(6): 1246–1260.

Jovanovic, B. (1979b). Job matching and the theory of turnover, *Journal of Political Economy* **87**(5): 972–990.

Jovanovic, B. & Nyarko, Y. (1997). Stepping stone mobility, *Carnegie Rochester Conference Series on Public Policy* **46**: 289–325.

Lancaster, T. (1990). *The econometric analysis of transition data*, Econometric Society Monographs, Cambridge University Press.

Loewenstein, M. A. & Spletzer, J. R. (1996). Belated training: The relationship between training, tenure, and wages, *mimeo*, U.S. Department of Labor, Bureau of Labor Statistics.

Loewenstein, M. A. & Spletzer, J. R. (1998). Dividing the costs and returns to general training, *Journal of Labor Economics* **16**(1): 142–71.

Lynch, L. (1992a). Differential effects of post-school training on early career mobility, *Working paper 4034*, NBER.

Lynch, L. M. (1991). The role of Off-the-Job vs. On-the-Job training for the mobility of women workers, *AEA Papers and Proceedings* **81**(2): 151–156.

Lynch, L. M. (1992b). Private-sector training and the earnings of young workers, *American Economic Review* **82**(1): 299–312.

MacLeod, W. B. & Malcomson, J. M. (1993). Investments, holdup, and the form of market contracts, *American Economic Review* **83**(4): 811–837.

Margolis, D. N. (1995). Firm heterogeneity and workers self-selection bias estimated returns to seniority. Université de Montréal Département de Sciences Economiques Working Paper.

McCall, B. P. (1990). Occupational matching: A test of sorts, *Journal of Political Economy* **98**: 45–69.

Meyer, B. D. (1990). Unemployment insurance and unemployment spells, *Econometrica* **58**(4): 757–782.

Neal, D. (1995). Industry-specific human capital: Evidence from displaced workers, *Journal of Labor Economics* **13**(4): 653–77.

Neal, D. (1996). The complexity of job mobility among young men, *mimeo*, University of Chicago.

Parent, D. (1995). Industry-specific capital and the wage profile: Evidence from the NLSY and the PSID, *Working paper 95s-26*, CIRANO.

Parent, D. (forthcominga). Wages and mobility: The impact of employer-provided training, *Journal of Labor Economics* .

Parent, D. (forthcomingb). Wages and mobility: The impact of employer-provided training, *Journal of Labor Economics* .

Salop, J. & Salop, S. (1976). Self selection and turnover in the labor market, *Quarterly Journal of Economics* **90**.

Stevens, M. (1994). A theoretical model of on-the-job training with imperfect competition, *Oxford Economic Papers* **46**(4): 537–62.

Sueyoshi, G. (1992). Semi-parametric proportional hazards estimation of competing risks models with time-varying regressors, *Journal of econometrics* **51**: 25–58.

The Economist (1997). Face value: the Margaret Thatcher of training (Laurie Bell), **May 17, 1997**.

Thomas, J. (1996). An empirical model of sectoral movements by unemployed workers, *Journal of Labor Economics* **14**(1): 126–153.

Topel, R. (1991). Specific capital, mobility and wages: Wages rise with job seniority, *Journal of Political Economy* **99**(1): 145–176.

Veum, J. (1995a). Training, wages, and the human capital model, *Working paper 262*, U.S. Department of Labor, Bureau of Labor Statistics.

Veum, J. R. (1995b). Sources of training and their impact on wages, *Industrial and Labor Relations Review* **48**(4): 812–26.

Veum, J. R. (1997). Training and job mobility among young workers in the United States, *Journal of Population Economics* **10**: 219–33.

Weiss, A. & Wang, R. (1990). A sorting model of labor contracts: Implications for layoffs and wage-tenure profiles, *Working Paper 3448*, NBER.

Appendix

Table 1: Theoretical implications

Derivative of	λ	θ_2	θ_1	θ_3	M_2	M_{job}
with respect to:						
On-the-job training with current employer						
Firm-specific	< 0	< 0	< 0	< 0	$= 0$	$= 0$
Industry-specific	< 0	< 0	$= 0$	< 0	< 0	> 0
General	< 0	$= 0$	$= 0$	< 0	$= 0$	> 0
On-the-job training with previous employer, different industry						
Firm-specific	$= 0$	$= 0$	$= 0$	$= 0$	$= 0$	$= 0$
Industry-specific	> 0	> 0	$= 0$	$= 0$	> 0	> 0
General	< 0	$= 0$	$= 0$	< 0	$= 0$	> 0
On-the-job training with previous employer, same industry						
Firm-specific	$= 0$	$= 0$	$= 0$	$= 0$	$= 0$	$= 0$
Industry-specific	< 0	< 0	$= 0$	< 0	< 0	> 0
General	< 0	$= 0$	$= 0$	< 0	$= 0$	> 0

θ_1 is transition intensity to same industry, θ_2 to other industry, θ_3 to non-employment. M_2 is the probability of changing sectors conditional on switching jobs and on t, and M_{job} is the conditional probability of being employed after leaving the current job in t.

Figure 1: Transition intensities

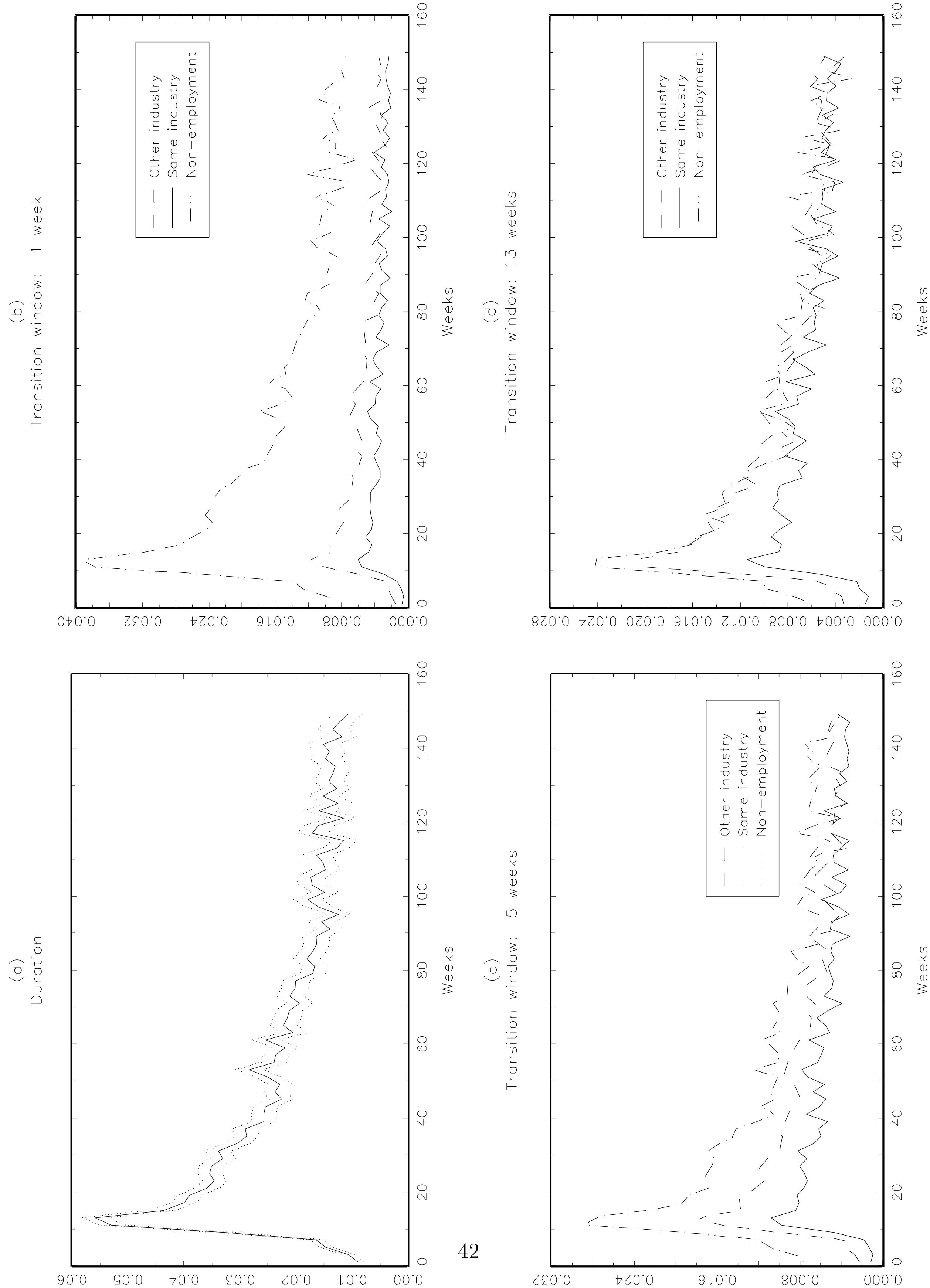

Figure 2: Transition intensities and transition windows

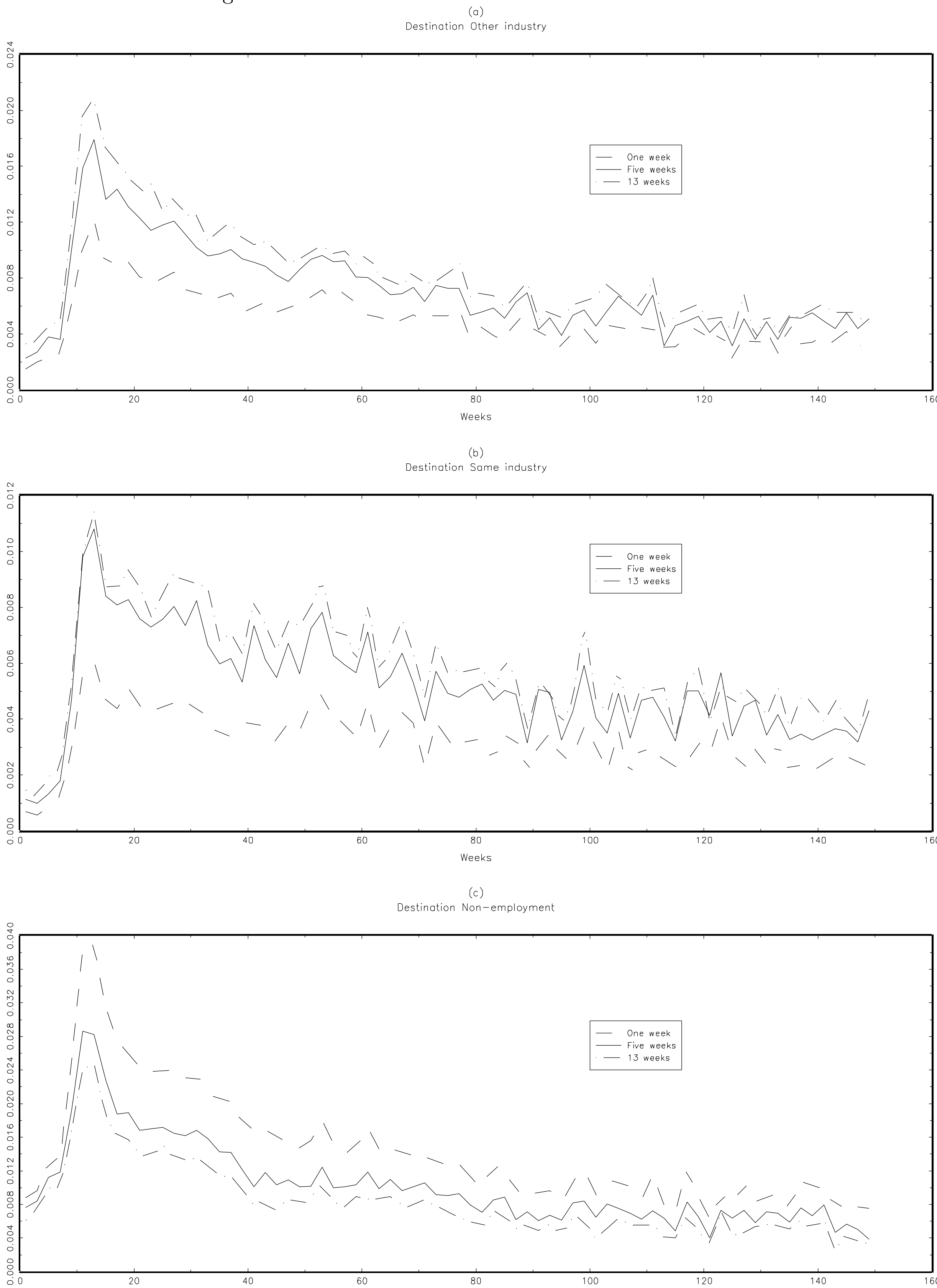

Table 2: **Sample selection**

	No. obs.	No. persons
Base sample		12,686
valid job observations	102,307	12,342
excl. military sample	97,795	11,254
excl. non-private co.	69,054	10,963
excl. missing variables	54,467	10,357
excl. jobs starting before Jan.1, 1979	47,645	9,791
Only permanent transitions	41,126	8,088
Spells > 4 weeks	40,059	8,058

Table 3:
Industry aggregation

Industry	*SIC codes*	*Name*
01	017-028	Agriculture,forestry and fisheries
02	047-057	Mining
03	067-077	Construction
04	107-398	Manufacturing
05	407-479	Transportation,communication, public utilities
06	507-698	Wholesale and retail trade
07	707-718	Finance,insurance and real estate
08	727-759	Business and repair services
09	769-798	Personal services
10	807-809	Entertainment and recreation services
11	828-897	Professional and related services
12	907-937	Public administration

Table 4: Exit frequencies
as a function of window size

Window size in weeks	*1*	*5*	*13*
Job in other industry	19.6	27.8	32.5
Job in same industry	12.3	19.6	22.5
Non-employment	57.9	42.0	33.6

Total number of observations: 40 059.

Table 5: **Training codes in NLSY**

Code	On-the-job	Description
1		Business school
3	yes	Apprenticeship program
4		Vocational or technical institute
7		Correspondence course
8	yes	Formal company training run by employer or military training
9	yes	Seminars or training programs at work not run by employer
10		Seminars or training programs outside of work
11		Vocational rehabilitation center
12		Other

Table 6: **Sample means**

	Full sample	Positive training
Tenure w/ Employer (weeks)	82.36	224.96
Actual exp since 1978	185.40	241.28
Hrs per Wk at Job	37.07	40.33
Hourly Wage (Cvtd) Job	6.94	22.88
Wage set by Union Job	0.13	0.18
Highest Grade completed (years)	12.12	12.27
Number Unique jobs held	5.70	6.85
Married	0.35	0.37
Female	0.44	0.45
Next job: Other industry	0.278	0.182
Next job: Same industry	0.196	0.175
Job ends in non-employment	0.420	0.250
On the job training current (hours)	16.98	315.02
ONJT current (incomplete, hours)	1.59	29.51
ONJT current (weeks)	0.81	15.10
Prior ONJT	50.35	95.38
of which in same industry	14.76	35.73
Prior ONJT (incomplete, hours)	7.69	16.15
Prior ONJT (weeks)	2.33	5.10
Off-the-job training (hours)	1261.75	2997.35
Off-the-job training (weeks)	73.48	168.26
Observations	40 059	2 553

Transition window size is five weeks for means on transition data.

Table 7: **Base specification**
Duration analysis

	Gamma	*Normal*	*Weibull*	*Cox*
On-the-job training	0.0194	0.0205	0.0161	0.0772
	(0.0026)	(0.0027)	(0.0027)	(0.0059)
Prior ONJT	-0.0132	-0.0141	-0.0120	-0.0082
	(0.0013)	(0.0014)	(0.0011)	(0.0021)
Off-the-job training	0.0013	0.0013	0.0011	0.0014
	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Log-likelihood	-58900.63	-59369.78	-60185.80	

Parameter estimates from parametric duration models and Cox partial likelihood model. 40 059 obs. Dependent variable for the parametric models is log tenure. Coefficients for the Cox model are the negative of the effect on the baseline hazard. Training variables in 100s of hours of training. All regressions include indicators for sex, union status, race and marital status, years of completed schooling, weeks of labor market experience, hourly wage rates, weekly hours, local unemployment rate, plus region, year and industry dummies. All variables are taken at the start of the job. All coefficients significant at 1 percent level.

Table 8:
Impact of training programs
Duration analysis

Training program of 320 hours = 9.14 weeks

Duration in weeks:	**Gamma**	**Log-normal**
Standard worker	6.82	6.16
	[4.95 , 8.73]	*[4.71 , 7.62]*
Median worker	7.85	14.25
	[4.50 , 11.45]	*[8.92 , 20.13]*

Increase in expected tenure due to on-the-job training received with the current firm. See Table 7 and text for raw coefficients and other details. 95 percent confidence intervals in square brackets.

Conditional sectoral allocation

	Other industry job		Same industry job	
	Estimate	*Standard Error*	*Estimate*	*Standard Error*
	(a) Linear time			
Intercept	-0.7618	*0.0783*	-1.4171	*0.0898*
On-the-job training				
w/ last employer	0.0056$^{(=)}$	*0.0091*	0.0098$^{(=)}$	*0.0093*
other industry	0.0174	*0.0057*	-0.0030$^{(=)}$	*0.0070*
same industry	-0.0100$^{(=)}$	*0.0107*	0.0303	*0.0091*
Off-the-job training	0.0014	*0.0005*	0.0022	*0.0005*
Tenure	0.0868	*0.0169*	0.2040	*0.0179*
Initial exp.	0.0027	*0.0001*	0.0034	*0.0001*
Hours/Week	0.0027	*0.0010*	0.0073	*0.0012*
Wage	-0.0002$^{(=)}$	*0.0010*	0.0018$^{(+)}$	*0.0008*
Jobs ever held	-0.0630	*0.0039*	-0.0705	*0.0044*
Union	0.1369	*0.0194*	0.1011	*0.0220*
Schooling	-0.0062$^{(=)}$	*0.0055*	-0.0116$^{(-)}$	*0.0063*
Female	0.1465	*0.0132*	0.0509	*0.0150*
Race	-0.0522	*0.0179*	-0.0446$^{(+)}$	*0.0205*
Married	0.0083$^{(=)}$	*0.0137*	-0.0020$^{(=)}$	*0.0156*
	(b) Polynomial time			
Tenure	0.6840	*0.0732*	1.1676	*0.0809*
Tenure^2	-0.3041	*0.0415*	-0.4741	*0.0447*
Tenure^3	0.0337	*0.0057*	0.0511	*0.0060*
Initial exp.	0.9774	*0.0568*	0.8957	*0.0645*
Experience^2	-0.2291	*0.0241*	-0.1828	*0.0267*
Experience^3	0.0180	*0.0029*	0.0145	*0.0031*

Parameter estimates from multinomial logit model. 33 586 observations. Omitted category is non-employment. Training variables in 100s of hours of training, tenure and experience in 100s of weeks. Transition window length is 5 weeks. Estimates from the regression in Panel (b) are available on demand. All coefficients significant at 1 percent level except (+) not significant at 1 percent level, (-) not significant at 5 percent level, (=) not significant at 10 percent level.

Table 9: Multinomial logit estimates

Table 10:

Proportionality factor
Cox partial likelihood
Base specification

		Transition intensities			
	Hazard	*Other industry*	*Same industry*	*Non-employment*	*Job*
On-the-job training:					
Current job	-0.077	-0.074	-0.054	-0.094	-0.066
	(0.006)	(0.010)	(0.010)	(0.010)	(0.007)
Prior, other industry	0.008	0.014	$0.001^{(=)}$	$0.003^{(=)}$	0.009
	(0.002)	(0.003)	(0.005)	(0.004)	(0.002)
Prior, same industry	$0.003^{(=)}$	$-0.013^{(-)}$	0.017	$-0.004^{(=)}$	$0.005^{(=)}$
	(0.003)	(0.007)	(0.005)	(0.007)	(0.004)
Off-the-job training	-0.014	$-0.008^{(+)}$	$-0.002^{(=)}$	-0.025	$-0.005^{(+)}$
	(0.002)	(0.004)	(0.041)	(0.003)	(0.004)

Parameter estimates from Cox partial likelihood models. Standard errors in parentheses. 40 059 obs. On-the-job training in 100s of hours, off-the-job training in 1000s of hours. For other details, see footnote to Table 7. For full results, see Table 11. All coefficients significant at 1 percent level except (+) not significant at 1 percent level, (-) not significant at 5 percent level, (=) not significant at 10 percent level as determined by a $\chi^2(1)$ test.

Table 11:

Competing hazard specification
Proportionality factor
Cox partial likelihood

Exit to	*Other industry*		*Same industry*		*Non-employment*	
Variable	*Parameter Estimate*	*Standard Error*	*Parameter Estimate*	*Standard Error*	*Parameter Estimate*	*Standard Error*
On-the-job training:						
current job	-0.0743	*0.01006*	-0.0536	*0.00979*	-0.0942	*0.01021*
previous, other industry	0.0140	*0.00285*	0.0005	*0.00465*	0.0028	*0.00406*
previous, same industry	-0.0130	*0.00737*	0.0165	*0.00459*	-0.0035	*0.00683*
Off-the-job training	-0.008060	*0.00356*	-0.001499	*0.00405*	-0.024623	*0.00315*
Years of education	-0.000923	*0.00431*	-0.009123	*0.00530*	0.005360	*0.00358*
Initial experience	0.000394	*0.00012*	0.000687	*0.00013*	-0.002407	*0.00010*
Jobs ever held	-0.022913	*0.00328*	-0.030657	*0.00397*	0.035449	*0.00260*
Hours per week	-0.000425	*0.00084*	0.006534	*0.00106*	-0.002714	*0.00068*
Hourly wage $	-0.0183	*0.00300*	-0.000031	*0.00011*	-0.0204	*0.00303*
Dummies:						
Union	-0.270993	*0.03128*	-0.238551	*0.03715*	-0.058934	*0.02344*
Married	0.002866	*0.02132*	-0.001924	*0.02593*	0.007088	*0.01752*
Female	-0.192796	*0.02172*	0.007624	*0.02690*	0.092506	*0.01742*
Race	0.083687	*0.02793*	0.056811	*0.03408*	0.008201	*0.02216*

Parameter estimates from Cox partial likelihood models. 40 059 obs. On-the-job training in 100s of hours, off-the-job training in 1000s of hours. All regressions include controls for local unemployment rate, region of residence, industry of origin, and calendar year of job start. All variables measured at start of job.

Table 12:

Proportionality factor
Cox partial likelihood
Heterogeneity

		Transition intensities			
	Hazard	*Other industry*	*Same industry*	*Non-employment*	*Job*
On-the-job training:					
Current job	-0.111	-0.116	-0.112	-0.125	-0.114
	(0.011)	(0.019)	(0.021)	(0.017)	(0.014)
Prior, other industry	-0.022	-0.007$^{(=)}$	-0.004$^{(=)}$	-0.071	-0.005$^{(=)}$
	(0.007)	(0.011)	(0.014)	(0.014)	(0.009)
Prior, same industry	-0.021	-0.012$^{(=)}$	0.004$^{(=)}$	-0.067	-0.004$^{(=)}$
	(0.008)	(0.013)	(0.015)	(0.017)	(0.010)
Off-the-job training	-0.050$^{(+)}$	-0.046	-0.036$^{(+)}$	-0.065	-0.043
	(0.007)	(0.013)	(0.017)	(0.010)	(0.010)

Parameter estimates from Cox partial likelihood models. Standard errors in parentheses. 40 059 obs. On-the-job training in 100s of hours, off-the-job training in 1000s of hours. For other details, see footnote to Table 7. For full results, see Table 11. All coefficients significant at 1 percent level except (+) not significant at 1 percent level, (-) not significant at 5 percent level, (=) not significant at 10 percent level, as determined by a $\chi^2(1)$ test.

Table 13:

Probability of sectoral move
Cox partial likelihood

Derivative of $M_2(t)$ with respect to:	(a)	(b)	(c)	(d)	(e)	(f)
On-the-job training:						
Current job	-0.0207	-4.60E-03	-0.054	-0.022	-0.033	-0.022
	(2.175)	(0.026)	(9.658)	(0.418)	(4.525)	(0.476)
Prior, other industry	0.0135	-2.44E-03	0.014	0.013	0.014	-0.003
	(6.155)	(0.018)	(4.423)	(0.417)	(6.152)	(0.029)
Prior, same industry	-0.0295	-0.016	-0.021	0.005	-0.029	-0.017
	(11.544)	(0.681)	(3.838)	(0.044)	(11.488)	(0.728)
Off-the-job training	-0.66E-05	0.99E-05	-0.81E-02	-0.034	-0.66E-02	-0.95E-02
	(1.478)	(0.218)	(1.404)	(1.618)	(1.488)	(0.202)
Oversample excluded:	No	No	Yes	Yes	No	No
Incomplete training:	No	No	No	No	Yes	Yes
Heterogeneity:	No	Yes	No	Yes	No	Yes

Parameter estimates from Cox partial likelihood models. 40 059 obs. $\chi^2(1)$ values in parentheses. $\chi^2_{0.90}(1) = 2.706$, $\chi^2_{0.99}(1) = 6.635$. For other details, see footnote to Table 7.

Table 14:

Training coefficients
Cox partial likelihood
Men

Derivative of	λ	θ_2	θ_1	θ_3
with respect to:				
	(a) No heterogeneity			
On-the-job training:				
Current job	-0.060	-0.057	-0.045	-0.073
	(0.006)	(0.010)	(0.011)	(0.011)
Prior, other industry	0.008	0.011	$0.001^{(=)}$	$0.006^{(=)}$
	(0.002)	(0.003)	(0.005)	(0.005)
Prior, same industry	$0.003^{(=)}$	$-0.010^{(=)}$	0.016	$-0.005^{(=)}$
	(0.004)	(0.008)	(0.005)	(0.008)
Off-the-job training	-0.013	$-0.001^{(=)}$	$-0.001^{(=)}$	-0.030
	(0.003)	(0.005)	(0.006)	(0.005)
	(b) Heterogeneity			
On-the-job training:				
Current job	-0.089	-0.204	-0.099	-0.203
	(0.011)	(0.025)	(0.021)	(0.045)
Prior, other industry	$-0.018^{(-)}$	-0.056	$-0.007^{(=)}$	$-0.024^{(=)}$
	(0.008)	(0.017)	(0.012)	(0.029)
Prior, same industry	$-0.017^{(-)}$	$-0.056^{(+)}$	$-0.010^{(=)}$	$-0.124^{(+)}$
	(0.009)	(0.024)	(0.014)	(0.057)
Off-the-job training	-0.038	-0.062	-0.036	-0.061
	(0.010)	(0.011)	(0.017)	(0.021)

Parameter estimates from Cox partial likelihood models. 22 420 obs. For other details, see footnote to Table 7. All coefficients significant at 1 percent level except (+) not significant at 1 percent level, (-) not significant at 5 percent level, (=) not significant at 10 percent level as determined by a $\chi^2(1)$ testl.

Table 15:

Training coefficients
Cox partial likelihood
Women

Derivative of	λ	θ_2	θ_1	θ_3
with respect to:				
	(a) No heterogeneity			
On-the-job training:				
Current job	-0.132	-0.180	-0.079	-0.141
	(0.013)	(0.031)	(0.021)	(0.020)
Prior, other industry	$0.005^{(=)}$	$0.019^{(-)}$	17.4e-$5^{(=)}$	$-0.002^{(=)}$
	(0.005)	(0.008)	(0.011)	(0.008)
Prior, same industry	$0.003^{(=)}$	$-0.063^{(-)}$	$0.016^{(=)}$	$0.009^{(=)}$
	(0.008)	(0.028)	(0.009)	(0.012)
Off-the-job training	-0.015	-0.015	$-0.006^{(=)}$	-0.020
	(0.003)	(0.005)	(0.005)	(0.004)
	(b) Heterogeneity			
On-the-job training:				
Current job	-0.145	-0.095	-0.255	-0.102
	(0.042)	(0.018)	(0.042)	(0.015)
Prior, other industry	$-0.049^{(=)}$	-0.058	-0.100	$-0.006^{(=)}$
	(0.037)	(0.016)	(0.027)	(0.009)
Prior, same industry	$0.016^{(=)}$	-0.055	$-0.088^{(-)}$	$-0.005^{(=)}$
	(0.040)	(0.018)	(0.038)	(0.010)
Off-the-job training	$-0.043^{(=)}$	-0.059	-0.069	$-0.032^{(-)}$
	(0.027)	(0.015)	(0.014)	(0.013)

Parameter estimates from Cox partial likelihood models. 22 420 obs. For other details, see footnote to Table 7. All coefficients significant at 1 percent level except (+) not significant at 1 percent level, (-) not significant at 5 percent level, (=) not significant at 10 percent level as determined by a $\chi^2(1)$ testl.

Table 16:

Probability of sectoral move
Cox partial likelihood
by gender

Derivative of $M_2(t)$ with respect to:	*Men* (a)	(b)	*Women* (c)	(d)
On-the-job training:				
Current job	-0.012	-0.105	-0.101	-0.050
	(0.607)	(10.631)	(6.910)	(1.145)
Prior, other industry	0.011	-0.050	0.019	0.010
	(3.052)	(5.574)	(1.720)	(0.055)
Prior, same industry	-0.026	-0.046	-0.079	0.072
	(7.641)	(2.711)	(6.827)	(2.575)
Off-the-job training	0.03E-02	-0.026	-0.84E-02	0.010
	(0.001)	(1.645)	(1.232)	(0.232)
Heterogeneity:	No	Yes	No	Yes

Parameter estimates from Cox partial likelihood models. 40 059 obs. $\chi^2(1)$ values in parentheses. $\chi^2_{0.90}(1) = 2.706$, $\chi^2_{0.99}(1) = 6.635$. For other details, see footnote to Table 7.

Table 17:

Probability of sectoral move
Cox partial likelihood

Derivative of $M_2(t)$ with respect to:	*Incidence* (a)	(b)	*Conditional* (c)	(d)	*Displ. workers* (e)
On-the-job training:					
Current job	-0.152	-0.265	-0.108	-0.003	0.048
	(3.418)	(3.912)	(10.162)	(0.002)	(0.878)
Prior, other industry	0.143	-0.105	0.017	0.027	0.070
	(5.812)	(0.709)	(3.781)	(0.346)	(1.004)
Prior, same industry	-0.594	-0.379	-0.030	-0.119	-0.102
	(37.758)	(5.063)	(7.480)	(0.071)	(2.305)
Off-the-job training	0.059	0.061	-0.013	0.001	-0.021
	(2.333)	(0.339)	(0.748)	(0.000)	(0.393)
Heterogeneity:	No	Yes	No	Yes	No
Observations:	40 059		4 179		1 438

Parameter estimates from Cox partial likelihood models. 40 059 obs. $\chi^2(1)$ values in parentheses. $\chi^2_{0.90}(1) = 2.706$, $\chi^2_{0.99}(1) = 6.635$. For other details, see footnote to Table 7.

Table 18:

Probability of employment attachment
Cox partial likelihood

Derivative of $M_{job}(t)$ with respect to:	(a)	(b)	(c)	(d)	(e)	(f)
On-the-job training:						
Current job	0.0281	0.011	0.011	0.012	0.107	0.079
	(5.101)	(0.258)	(0.573)	(0.227)	(57.480)	(9.966)
Prior, other industry	0.006	0.067	0.004	0.089	0.099	0.190
	(1.805)	(15.975)	(0.660)	(19.605)	(435.232)	(129.990)
Prior, same industry	0.008	0.063	0.022	0.094	0.003	0.071
	(1.097)	(10.184)	(3.375)	(13.360)	(0.185)	(12.480)
Off-the-job training	0.019	0.022	0.017	-0.005	0.019	0.023
	(21.685)	(2.328)	(10.377)	(0.078)	(21.673)	(2.391)
Oversample excluded:	No	No	Yes	Yes	No	No
Incomplete training	No	No	No	No	Yes	Yes
Heterogeneity:	No	Yes	No	Yes	No	Yes

Parameter estimates from Cox partial likelihood models. 40 059 obs. $\chi^2(1)$ values in parentheses. $\chi^2_{0.90}(1) = 2.706$, $\chi^2_{0.99}(1) = 6.635$. For other details, see footnote to Table 7.

Table 19:

Probability of employment attachment
Cox partial likelihood
by gender

Derivative of $M_{job}(t)$ with respect to:	*Men* (a)	(b)	*Women* (c)	(d)
On-the-job training:				
Current job	0.020	0.098	0.017	0.153
	(2.274)	(3.590)	(0.366)	(11.667)
Prior, other industry	0.002	0.021	0.013	0.095
	(0.097)	(0.411)	(1.615)	(10.657)
Prior, same industry	0.009	0.123	-0.011	0.083
	(0.944)	(4.247)	(0.448)	(4.330)
Off-the-job training	0.029	0.0298	0.010	-0.022
	(20.843)	(1.007)	(3.385)	(1.120)
Heterogeneity:	No	Yes	No	Yes

Parameter estimates from Cox partial likelihood models. 40 059 obs. $\chi^2(1)$ values in parentheses. $\chi^2_{0.90}(1) = 2.706$, $\chi^2_{0.99}(1) = 6.635$. For other details, see footnote to Table 7.

Table 20:

Probability of employment attachment
Cox partial likelihood

Derivative of $M_2(t)$ with respect to:	*Incidence* (a)	(b)	*Hours and Incidence* (c)	(d)	*Displ. workers* (e)
On-the-job training:					
Current job	-0.054	0.004	0.006	-0.005	-0.015
	(0.755)	(0.002)	(0.621)	(0.089)	(0.284)
Prior, other industry	0.305	0.281	-0.006	0.030	-0.046
	(42.488)	(8.877)	(1.579)	(3.628)	(1.415)
Prior, same industry	-0.002	0.158	0.012	0.036	0.069
	(4.76E-4)	(1.545)	(1.579)	(3.600)	(2.032)
Off-the-job training	0.122	0.214	0.013	0.010	0.022
	(19.800)	(8.269)	(7.067)	(0.410)	(0.872)
Heterogeneity:	No	Yes	No	Yes	No
Observations:	40 059		40 059		1 438

Parameter estimates from Cox partial likelihood models. Columns (a) and (b) report coefficients on incidence variables, all others on 100s of hours of on-the-job training and 1000s of hours of off-the-job training. $\chi^2(1)$ values in parentheses. $\chi^2_{0.90}(1) = 2.706$, $\chi^2_{0.99}(1) = 6.635$. For other details, see footnote to Table 7.