Trois essais sur la mobilité et la formation des travailleurs en Allemagne et aux États-Unis

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Thèse présentée à la Faculté des études supérieures
en vue de l'obtention du grade de
PhilosophiæDoctor (Ph.D.)
en sciences économiques

Septembre, 1998

Université de Montréal
Faculté des études supérieures

Cette thèse intitulée :

Trois essais sur la mobilité et la formation des travailleurs en Allemagne et aux États-Unis

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Accepté le 19 novembre 1998

Sommaire

Le chapitre 1 passe en revue les modèles et les résultats pertinents à la formation formelle en milieu de travail, tandis qu'un bref survol des modèles qui prévoient une corrélation entre l'utilité alternative et le salaire des travailleurs est incorporé au chapitre 2.

Au chapitre 2, nous analysons la corrélation entre les revenus de travail contemporains et les conditions sur le marché du travail, à savoir le taux de chômage, mesuré à différents moments pendant la durée du contrat de travail. Les données que nous utilisons proviennent du Panel Socio-économique Allemand, et comprennent la période 1984-1994. Contrairement aux résultats pour le marché américain, nous trouvons que l'état actuel du marché du travail est un important facteur même en contrôlant pour le taux de chômage en vigueur au début de la relation d'emploi. Les élasticités estimées varient entre 9 et 15 pourcent pour l'élasticité des revenus de travail par rapport au taux de chômage contemporain, et entre 6 et 10 pourcent par rapport au taux de chômage en début de contrat. Notamment, tandis que le taux de chômage régional affecte le niveau des revenus d'emploi, le taux national influence la variation dans les revenus. Ces résultats sont cohérents avec la présence simultanée de conventions collectives et de contrats individuels, tels les modèles de contrats implicites, qui expliquent une partie de la variance de revenus de travail et des mouvements de revenu à long terme. En plus de la variation régionale, nous étudions l'hétérogenéité des contrats selon certains caractéristiques des travailleurs et des emplois. En particulier, nous constatons que les contrats de travail diffèrent selon la taille de l'entreprise et le type de travailleur. Un travailleur dans une grande entreprise est remarquablement plus isolé de fluctuation du marché de travail qu'un travailleur dans toute autre taille d'entreprise, suggérant l'importance des marchés du travail internes pour ces firmes.

Les chapitres 3 et 4 portent sur la mobilité des travailleurs tel qu'affecté par la formation formelle en milieu de travail. Au chapitre 3, 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 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 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 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.

Finalement, le chapitre 4 étudie la mobilité des travailleurs allemands à la lumière d'un modèle de capital humain dont la spécificité est sectoriel. En outre, j'utilise et décris des données peu utilisées sur la formation formelle ayant lieu après la fin d'un apprentissage. Comparativement aux États-Unis, un plus grand nombre de travailleurs suit une formation annuellement, et ce, en dépit d'une incidence élevée d'apprentissage précédemment. Tandis que plusieurs autres études font uniquement une distinction entre capital humain spécifique à une seule firme et capital humain général, je montre que les travailleurs allemands ont une plus grande probabilité de trouver un emploi dans un secteur s'ils ont suivi une formation formelle dans ce même secteur. Ce résultat n'est cohérent ni avec la présence de capital humain spécifique à une seule firme, ni avec du capital humain complètement général. Conjointement avec des résultats semblables pour des travailleurs américains, ils suggèrent l'importance du capital humain spécifique à l'industrie. Par ailleurs, l'effet de la formation sur la mobilité semble sensible à l'état de la conjoncture, suggérant une interaction entre offre et demande plus complexe que celle décrit par la plupart des modèles théoriques.

MOTS-CLÉS :

- Chapitre 2 :

  Courbe de salaire, contrats implicites, structure des salaires, Allemagne.

  JEL : J41 (Contrats), J31 (Niveau et structure des salaires), J23 (Détermination de l'emploi)

- Chapitre 3 :

  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).

- Chapitre 4 :

  Formation en lieu de travail, durée de l'emploi, mobilité sectoriel, capital humain spécifique au secteur, modèles multinomial.

  JEL : J24 (Formation de capital humain), J41 (Capital humain spécifique), J62 (Mobilité sectoriel), P52 (Études comparatives d'économies particulières).

Summary

Chapter 1 reviews models of on-the-job training and the empirical evidence in light of those models. A brief overview of the theory correlating alternative utility to a worker's wages is given within the context of Chapter 2.

In Chapter 2 we look at how labor market conditions at different points during the tenure of individuals with firms are correlated with current earnings. Using data on individuals from the German Socioeconomic Panel for the period 1985 to 1994, we find that both the contemporaneous unemployment rate and prior values of the unemployment rate are significantly correlated with current earnings, contrary to results for the American labor market. Estimated elasticities vary between 9 and 15 percent for the elasticity of earnings with respect to current unemployment rates, and between 6 and 10 percent with respect to unemployment rates at start of current firm tenure. Moreover, whereas local unemployment rates determine levels of earnings, national rates influence contemporaneous variations in earnings. We interpret this result as evidence that German unions do in fact bargain over wages and employment, but that models of individualistic contracts, such as the implicit contract model, may explain some of the observed wage drift and longer-term wage movements reasonably well. Furthermore, we explore the heterogeneity of contracts over a variety of worker and job characteristics. In particular, we find evidence that contracts differ across firm size and worker type. Workers of large firms are remarkably more insulated from the job market than workers for any other type of firm, indicating the importance of internal job markets.

Formal on-the-job training and its impact on the sectoral mobility of workers is the subject of Chapters 3 and 4. In Chapter 3, 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.

Finally, Chapter 4 studies mobility patterns of German workers in light of a model of sector-specific human capital. Furthermore, I employ and describe little-used data on continuous on-the-job training occurring after apprenticeships. Results are presented describing the incidence and duration of continuous training. Continuous training is quite common, despite the high incidence of apprenticeships which precedes this part of a worker's career. Most previous studies have only distinguished between firm-specific and general human capital, generally concluding that training was general. Inconsistent with those conclusions, I show that German men are more likely to find a job within the same sector if they have received continuous training in that sector. These results are similar to results obtained for young U.S. workers, and suggest that sector-specific capital is an important feature of very different labor markets. Furthermore, the results suggest that the observed effect of training on mobility is sensitive to the state of the business cycle, indicating a more complex interaction between supply and demand that most theoretical models allow for.

KEYWORDS :

- Chapitre 2 :

  Wage curve, Implicit contracts, Wage structure, Germany.

  JEL : J41 (Contracts), J31 (Wage Level and Structure), J23 (Employment Determination)

- Chapitre 3 :

  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).

- Chapitre 4 :

  On-the-job Training, Employment duration, Sectoral mobility, Industry-specific human capital, Multinomial models.

  JEL : J24 (Human Capital Formation), J41 (Specific Human Capital), J62 (Sectoral Mobility) P52 (Comparative Studies of Particular Economies) .

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Introduction

La présente thèse s'inscrit dans un grand débat, celui sur le chômage et ses causes possibles. Atteignant aujourd'hui une partie importante de la population active, le chômage est un fléau que plusieurs jugent trop élevé. On attribue souvent au Canada une position intermédiaire entre une Europe au chômage élevé qui serait sclérosée dans ses structures sociales trop généreuses, et les États-Unis qui délaisseraient le réseau social au profit d'un marché peu réglementé, par surcroît plus flexible et accusant un niveau de chômage bien plus bas.

Les réponses, sont-elles si simples ?

Prenons l'Allemagne comme contre-exemple des États-Unis. Aujourd'hui, le taux de chômage élevé de ce géant économique de l'Europe semble être le paradigme de l'eurosclérose. La couverture syndicale, qui dépasse les 80 pourcent en Allemagne, et la grande sécurité d'emploi des Allemands sont vues comme des entraves à la flexibilité. Les travailleurs aux États-Unis, où le taux de syndicalisation est tombé en deçà des 15 pourcent, ont des emplois plus courts, reflétant leur grande flexibilité.

D'autre part, le système de formation de la main-d'œuvre allemande, le "système dual" de formation en entreprise et de scolarité publique, est souvent cité à titre d'exemple d'un système efficace et flexible, permettant aux Allemands de garder une productivité et, par extension, des salaires au-dessus de la moyenne mondiale.

Comment réconcilier ces deux phénomènes ?

Cette thèse comprend deux volets, repartis sur trois articles. Dans un premier temps, j'étudie la flexibilité des revenus de travail des travailleurs allemands face aux conditions régnant sur le marché du travail. Amorcée dans le but de pouvoir comparer cette relation avec son pendant aux États-Unis, l'étude est également importante pour comprendre de façon plus générale le fonctionnement des marchés du travail. Le chapitre 2, qui décrit les résultats, énonce plusieurs modèles que l'on retrouve dans la littérature. Il en découle plusieurs relations entre le revenu de travail et l'utilité qu'un travailleur pourrait avoir ailleurs sur le marché du travail à divers moments pendant la relation de travail. Utilisant des micro-données d'enquête, bien que l'étude empirique porte sur l'Allemagne, elle est structurée de façon à pouvoir comparer les résultats avec ceux obtenus pour les États-Unis. Afin de tenir compte de la structure particulière des relations industrielles en Allemagne, l'échantillon est décomposé en plusieurs dimensions de façon à faire ressortir l'éventuelle coexistence de dynamiques différentes pour des sous-groupes de travailleurs. La comparaison avec les résultats américains, en utilisant des statistiques comparables, ne permet pas de conclure que les revenus de travail allemands réagissent dans une moindre mesure aux conditions sur le marché du travail. Sans pouvoir dégager des conclusions définitives à ce sujet, l'étude indique également que la flexibilité des salaires face aux changements contemporaines sur le marché du travail pourrait justement provenir des syndicats.

Si dans la première partie la flexibilité des salaires durant une relation de travail était l'objet d'étude, l'attention se tourne par la suite à la flexibilité des travailleurs eux-mêmes. Un des faits saillants des marchés du travail américains est la grande mobilité de sa main d'œuvre. Puisqu'on y associe simultanément une faible formation moyenne, certains pourraient conclure à une relation inverse entre la formation de la main-d'œuvre et sa mobilité. Plusieurs auteurs se sont penché sur la question, en étudiant la spécificité

du capital humain, dont la formation. Si le capital humain investi en un travailleur était spécifique à une seule firme, ce travailleur serait enclin à rester avec cette firme par les incitations que celle-ci lui offre, avec comme conséquence une faible mobilité. Par contre, un capital humain général à toute l'économie, portable, n'empêcherait en rien la mobilité.

L'approche utilisée pour faire le lien entre la mobilité et la formation est celle du capital humain. Le chapitre 1 passe en revue les modèles et les résultats pertinents à la formation formelle en milieu de travail. S'intéressant d'avantage sur les effets sur le salaire, l'aspect de la mobilité a été quelque peu négligé dans la littérature. À l'instar des études sur les travailleurs déplacés, les recherches rapportées dans les chapitres 3 et 4 se concentrent sur les flux de mobilité des travailleurs formés et non-formés directement, au lieu de passer par les effets de la formation sur les salaires. Tandis que le chapitre 4 utilise les mêmes données en panel sur les travailleurs allemands que le chapitre 2, l'étude rapportée au chapitre 3 utilise des données, également en panel, sur des jeunes travailleurs américains, le *National Longitudinal Survey of Youth*. Ce dernier a souvent été objet d'étude sur la formation et offre ainsi une base adéquate pour comparer les résultats, obtenus ici moyennant une nouvelle approche, avec les résultats d'autres chercheurs. Quant aux données allemandes, l'information qu'elles contiennent sur la formation formelle de la main-d'œuvre n'a été que très peu utilisée.

Chapitre 1

La spécificité de la formation en milieu de travail : Un survol des contributions théoriques et empiriques récentes

1.1 Introduction

La formation en entreprise constitue un sujet qui suscite régulièrement l'intérêt des économistes depuis la réalisation des travaux de Becker (1962). Elle s'intègre dans le contexte plus large du capital humain, dont les composantes sont notamment l'éducation de base, les études universitaires et la formation des chômeurs. Elle fait l'objet d'une attention particulière parce qu'on lui attribue des effets plus généraux, non seulement pour son bénéficiaire, mais également en raison des externalités positives pour l'économie agrégée.

L'ampleur de l'externalité dépend de la spécificité du capital humain. Dans la mesure où le capital humain est spécifique à une seule firme, l'externalité se fait moindre, voire inexistante, comparativement à un capital humain plus général, comme nous allons voir. En conséquence, l'étude du capital humain plus généralement et de la formation plus particulièrement, porte sur sa spécificité. L'intervention originale de Becker permettait d'encadrer ces concepts dans un contexte néo-classique, d'acteurs rationnels, et de faire ressortir des implications pratiques. La question est surtout d'ordre empirique, puisque

rien ne permet d'exclure *a priori* le capital humain général ou spécifique. Le choix en la matière par des acteurs rationnels dépend justement des circonstances économiques, qui elles peuvent être manipulées par la politique économique.

Précisons tout d'abord que la littérature à ce sujet se fragmente dans maintes directions. Nous allons nous concentrer sur la formation formelle payée et habituellement fournie par des entreprises et ce, sans intervention politique directe. Nous excluons alors des programmes gouvernementaux de formation de la main d'œuvre comme le JTPA aux États-Unis, le YTS en Grande-Bretagne et les différents *Arbeitsbeschaffungsmaßnahmen* (mesures de placement) en Allemagne, dont l'étude présente certains problèmes économétriques particuliers[1]. Une distinction supplémentaire sera faite entre la formation formelle, qui a lieu en classe ou en séminaire, et la formation sur le tas. Néanmoins, nous allons nous référer à la littérature sur la formation sur le tas à quelques reprises, puisque les méthodes et les résultats qu'elle présente vont s'avérer utile pour l'étude de la formation formelle.

Compte tenu de ces restrictions, le lecteur intéressé peut toutefois se référer à d'autres recensions d'écrits, comme Bishop (1997), Brown (1991) et Parent (1996), qui traitent plus amplement des sujets que nous n'aborderons pas ici. Se voulant complémentaire à la littérature existante, la présente recension élabore le sujet de la spécificité de la formation formelle. Pour des discussions au sujet de l'échec du marché et des suggestions quant à la réponse de la politique économique, voir Bishop (1991) Snower & Booth (1996) et Stern & Ritzen (1991).

Nous commencerons dans la prochaine section par un survol sommaire de la théorie pertinente, débutant par le modèle classique. Le survol des modèles plus récents permettra de dégager les contraintes qui agissent sur les études empiriques essayant de déterminer la spécificité de la formation. Un regard critique sera porté sur des études

[1] Voir à ce sujet *inter alia* les publications de James Heckman et de ses coauteurs.

utilisant des données américaines, britanniques et allemandes dans la section 1.4, les données elles-mêmes étant discutées dans la section 1.3. Nous conclurons avec la section 1.5.

1.2 Théorie du capital humain et formation

La théorie classique du capital humain s'applique au choix de l'éducation aussi bien qu'à la formation en milieu de travail. En nous appuyant sur cette théorie, nous allons recenser quelques contributions à la littérature grandissante sur la formation formelle et informelle en lieu de travail. Soit dit en passant que l'économie n'est pas la seule discipline traitant de ce sujet, et la contribution de Becker à cette inter-disciplinarité à été reconnue à juste titre par le comité Nobel, qui lui a attribué son prix en 1992

> *«for having extended the domain of microeconomic analysis to a wide range of human behaviour and interaction, including nonmarket behaviour. [...] Becker has [...] had an indirect impact on scientific approaches in social sciences other than economics; more frequently than in the past, sociologists and political scientists work with models based on theories of "rational choice". »*
> *(Royal Swedish Academy of Sciences 1992)*

Théorie classique

La théorie du capital humain fait maintenant partie intégrante de la théorie économique. Formulée pour la première fois par Becker (1962)[2], cette théorie est maintenant incluse dans tous les manuels d'économie du travail[3].

Le principe de base est simple. Considérons l'éducation comme un capital physique, qu'il faut créer en effectuant des investissements et qui possède une productivité posi-

[2]Voir aussi Becker (1964,1993)

[3]Voir Benjamin, Gunderson & Riddell (1998), Borjas (1996), Ehrenberg & Smith (1997) et Cahuc & Zylberberg (1996), pour ne citer quelques exemples. Voir aussi Willis (1986) pour une exposition plus détaillée.

tive lors de son utilisation. Ce capital est alors appelé ***capital humain*** puisqu'il lui est particulier qu'il soit accumulé dans une personne.

Comme tout investissement, on suppose que les agents rationnels en choisissent la quantité optimale, compte tenu des coûts et bénéfices. Concrètement, prenons l'exemple d'une personne devant le choix de continuer son éducation jusqu'au temps T (voir Graphique 1 .1) ou de s'insérer sur le marché du travail immédiatement, au salaire W_1. Dans le premier cas, elle continue à investir dans son capital humain, moyennant des frais de scolarité et de matériel C. De plus, la contrainte de temps physique à laquelle toute personne est assujettie implique que le temps passé à étudier ne peut pas être utilisé pour gagner un revenu. Ce manque à gagner constitue un coût d'opportunité. La somme de ces coûts est notée AA dans le graphique. Par contre, le capital humain ainsi formé lui apportera un salaire plus élevé une fois l'éducation terminée, $W_2 > W_1$. La différence entre le salaire majoré et le salaire W_2' qu'elle aurait eu si elle n'avait pas investi dans son capital humain - avec un éventuel accroissement dû à l'expérience, une autre constituante du capital humain - constitue son bénéfice, BB. L'investissement est alors fait si les bénéfices BB escomptés sont plus grands que les coûts AA escomptés.

Application à la formation en lieu de travail

Tandis que la section précédente présente l'application de la théorie du capital humain à la décision d'éducation d' un individu preneur de prix sur le marché du travail - qui inclut le marché du capital humain dans ce cas-ci -, cette théorie s'est d'abord appliquée dans le cadre de la formation en lieu de travail (Becker 1962). Plutôt que de concerner seulement une personne preneur de prix face au marché, les relations de travail ajoutent un élément contractuel, à savoir les contrats bilatéraux et de partage de rente. La question principale qui découle de la théorie illustrée plus haut est celle de la distribution des coûts et des revenus. Dans le modèle beckerien, la répartition dépend

du degré de spécificité du capital humain créé par l'investissement. La spécificité peut être définie succinctement de la façon suivante :

> *«Completely general training increases the marginal productivity of trainees by exactly the same amount in the firms providing the training as in other firms. [...] Training that increases productivity more in firms providing it will be called specific training. Completely specific training can be defined as training that has no effect on the productivity of trainees that would be useful in other firms.»*
> *(Becker, 1993, pg. 40)*

Plus précisément, nous allons utiliser tout au long de ce texte le terme *capital humain général (CHG)* pour le cas où la productivité dans la firme formatrice i est égale à celle obtenue dans d'autres firmes j, $PM_i = PM_j$, et le terme *capital humain spécifique (CHS)* pour le cas où $PM_i > PM_j \geq 0$, sauf indication contraire. Cette distinction n'est pas la seule à apparaître dans la littérature, mais il demeure important de pouvoir distinguer entre le CHS découlant de cette définition et ce qui *ressemble* à du capital spécifique pour d'autres raisons que la différence de productivité, comme nous allons voir plus loin.

Prenons d'abord l'exemple du CHG. Dans un marché parfaitement compétitif et en information parfaite, il résulte de la définition de CHG que les frais sont payés par le travailleur, qui accapare également la totalité des bénéfices. Pour voir ceci, considérons que ce n'est pas le cas, que l'employeur paie, au contraire, la totalité des frais et reçoit toute la rente et, pour simplifier, cadrons la logique dans un modèle à deux périodes. En l'absence de formation, le travailleur a une productivité marginale de PM_1. Après une formation d'une période, pendant laquelle sa productivité est égale à zéro, sa productivité marginale sera de PM_2 dans toutes les firmes : $PM_i = PM_j = PM_2$.

Si l'employeur paie tous les frais de formation et reçoit toute la rente, le travailleur est embauché à un salaire $W_1 = W_2 = PM_1$, sa productivité marginale sans formation, pour les deux périodes (voir Graphique 1 .2). Puisque la productivité en première période est nulle, l'employeur fait une perte en première période de AA. Par contre,

son profit en deuxième période est de BB, puisque le travailleur ne reçoit aucune rente de l'investissement en CHG. Si la différence escomptée entre BB et AA est positive, l'investissement est fait.

Mais puisque le capital humain ainsi investi en la personne du travailleur est de nature générale, il a de l'utilité pour d'autres employeurs : en compétition parfaite, le salaire alternatif du travailleur sera PM_2. L'employeur doit augmenter le salaire de deuxième période à PM_2 également ou voir partir le travailleur, faisant zéro profit en deuxième période dans les deux cas. Sachant ceci en première période, l'investissement n'est pas fait.

La solution ici est simple. Puisque le travailleur peut toujours se garantir de recevoir la rente de l'investissement en CHG en allant sur le marché du travail en deuxième période, il devra aussi en payer les frais, soit directement, soit en acceptant un salaire plus bas en première période - ici par simplicité égal à zéro. Son salaire pourrait alors être décrit par la ligne $0T_1xy$ au lieu de MP_1tu. La correspondance empirique serait un salaire d'apprenti.

La situation est toute autre si le capital humain n'est d'utilité que pour la firme actuelle. Il s'agit alors de CHS ; et supposons pour l'instant que la productivité n'est augmentée que chez l'employeur actuel : $PM_{2i} = PM_2$ mais $PM_{2j} = PM_1$. En deuxième période, le travailleur ne peut plus que s'assurer un salaire $W_2 = PM_1$, sa productivité marginale sur le marché. C'est donc à son tour de ne pas assumer les coûts AA en première période. Dans ce cas, l'employeur peut s'assurer le profit BB, et payer pour la formation.

Dans le cas du CHS, un problème nouveau se produit. En présence de CHG, et l'entreprise et le travailleur sont indifférents quant à une éventuelle séparation. Ceci n'est plus vrai lorsque le capital est spécifique à la relation de travail présente. L'employeur

ne peut récupérer ses dépenses dans la mesure où le travailleur ne rompt pas la relation de travail en deuxième période. Le travailleur de son côté est indifférent puisqu'il reçoit toujours son salaire de marché PM_1.

Becker prévoit un partage des bénéfices et des coûts pour que le travailleur soit incité à rester. Cet aspect est étudié plus en détails par Parsons (1972), qui souligne le rôle du capital humain spécifique (CHS) et du partage de sa rémunération dans les départs volontaires et involontaires[4]. Il définit le CHS comme la différence entre la somme escomptée de la productivité marginale du travailleur dans sa firme actuelle i et son plus grand produit marginal ailleurs, *net des coûts de transfert* C_j :

$$CHS_j = \sum_{t=0}^{N} [PM_{it} - (PM_{jt} - C_{jt})]\delta^t \tag{1.1}$$

où N est le nombre de périodes travaillées. Dans cette spécification, il est à noter qu'en l'absence de coûts de transaction, du CHS ne peut exister que si les productivités marginales diffèrent. Par contre, en présence de coûts de transfert, il peut exister du CHS dans la spécification de Parsons même si les productivités marginales ne diffèrent pas, tandis qu'il s'agirait alors de CHG comme nous l'avons définit plus haut. Du point de vue d'une firme décidant quelle rémunération offrir à ses travailleurs et si elle devrait leur offrir une formation, cette distinction n'est guère importante. Pour le travailleur, la distinction devient importante dans le cas éventuel d'une mise-à-pied, un point que Parsons néglige dans son modèle[5]. Si $PM_i = PM_j$, la formation acquise chez l'employeur j s'avère utile chez d'autres employeurs. Par contre, si $PM_i > PM_j$, c'est-à-dire, le capital humain acquis est plus productif chez l'employeur présent, le travailleur subit une perte de productivité après une séparation. Il est donc important de distinguer si l'aspect de

[4]Parsons, comme Becker, ne dérive pas explicitement ce partage d'un argument contractuel, ce qui est fait plus tard par Hashimoto (1981).

[5]Elle n'est pas importante en cas de départ volontaire, puisque la décision de séparation est endogène au travailleur.

spécificité provient d'une qualité intrinsèque à la formation, ou plutôt de la présence de coûts de transaction.

Becker et Parsons soulignent tous deux les effets sur le taux de roulement que peut avoir la présence de capital humain spécifique. Supposons qu'un choc transitoire sur la demande de la firme se produise. En l'absence de CHS, la firme peut simplement licencier des travailleurs pour réduire, à la fois, ses coûts et la production. Lorsque la demande reprend, elle est indifférente entre embaucher les mêmes travailleurs ou des travailleurs ayant reçu le même capital humain général d'une autre firme. Par contre, en présence de capital humain spécifique, elle a intérêt à réembaucher les travailleurs qu'elle a elle-même formés, puisque embaucher des travailleurs non-qualifiés entraîne des coûts supplémentaires. En les mettant à pied lors du choc, elle court le risque qu'ils ne soient plus disponibles lorsque la demande reprend. Par conséquent, elle retarde la mise-à-pied, lissant ainsi la réponse de la demande de travail aux chocs. La même argumentation peut se poursuivre avec des chocs positifs. Notons simplement que puisqu'il existe un écart entre la productivité marginale et le salaire de ses travailleurs qualifiés, une augmentation des salaires de réserve pour ceux-ci n'entraîne pas nécessairement une séparation, la firme pouvant ajuster leur salaire et encore faire des profits positives[6].

Il devrait alors être simple de distinguer la présence de CHS de celle de CHG. Une augmentation du stock de capital humain, par exemple moyennant l'expérience acquise sur le tas ou par une formation formelle, *ceteris paribus* augmente le salaire chez l'employeur présent et chez d'autres employeurs futurs, mais n'affecte pas la probabilité de séparation si le capital ainsi obtenu est général. En présence de CHS, l'augmentation n'a pas lieu chez des employeurs futurs, mais la probabilité de séparation est réduite pour les raisons énoncées plus haut.

[6]Voir par exemple MacLeod & Malcomson (1993) à ce sujet.

Contributions récentes

Motivés entre autres par les résultats empiriques trop souvent contradictoires, des contributions récentes ont mis en évidence le fait que les modèles beckeriens dépendent de la présence de marchés parfaits[7]. Déjà dans le modèle de Parsons (1972), il est évident qu'en présence de coûts de transaction suffisamment élevé ($C_{jt} > 0$ pour au moins certains t), le capital humain général dans la définition de Becker, $PM_i = PM_j$, peut avoir les mêmes effets quant au salaire et à la probabilité de séparation que le capital humain spécifique dans la définition stricte, $PM_i > PM_j$.

Deuxièmement, certains modèles donnent un avantage informationel à la firme formatrice, soit parce que le niveau de formation n'est pas vérifiable par d'autres firmes (Chang & Wang 1996, Katz & Ziderman 1990), soit parce que l'employeur apprend sur la qualité de son employé de façon indépendante du fait de le former (Acemoglu & Pischke 1998a). Dans le premier cas, même si le capital humain est de nature générale, le travailleur ne trouve pas de rémunération (complète) sur le marché du travail. Il en résulte soit la sous-optimalité du niveau d'investissement (Chang & Wang 1996), soit le financement du CHG par la firme (Katz & Ziderman 1990). Par contre, dans le deuxième modèle, la firme a de l'information privilegiée concernant l'*abilité* du travailleur, mais la formation est parfaitement vérifiable. Il en résulte la séléction adverse sur le marché du travail, ce qui génère un pouvoir de monopsone pour la firme. La taille de la rente de monopsone peut être augmentée par l'investissement en capital humain, qu'il soit générale ou non. C'est l'*ampleur* de la rente qui est lié à la formation, et non son *existence*. Ainsi, considérons deux individus avec la même formation, mais dont celui qui demeure avec la firme formatrice a un salaire plus élevé que celui qui se sépare de la firme. Dans l'interprétation beckerienne, ceci est dû au fait que son capital humain *spécifique* se déprécie complètement lors de son départ. Par contre, dans le modèle d'Acemoglu

[7]Becker lui-même n'a pas exigé la présence de marchés parfaits.

& Pischke (1998a), le capital humain peut être général, mais le travailleur partant est moins habile[8].

Les modèles d'asymétrie d'information prévoient alors que la firme puisse investir dans du CHG, puisqu'elle peut capturer une partie de la rente. Notons que dans tous ces cas, la probabilité qu'un individu quitte la compagnie formatrice est réduite en équilibre[9]. Il en découle l'impossibilité de distinguer le CHG du CHS par ces deux statistiques, le salaire et le taux de roulement.

De plus, d'autres particularités des marchés du travail variant entre pays sont souvent menés à expliquer pourquoi des firmes fournissent du CHG. Ainsi, une période explicite de formation - un apprentissage - peut servir comme une période d'essai ou d'acquisition d'information si la rupture d'un contrat de travail normal est difficile ou chère, comme c'était longtemps le cas en Allemagne[10], mais ceci n'est pas suffisant pour expliquer pourquoi la provision de capital humain serait général. De plus, une externalité positive peut être engendrée par la présence d'autres firmes formatrices. Dans un marché où toutes les firmes forment, la création et le financement du CHG peuvent être profitable pour la firme parce qu'elle gagne autant de travailleurs formés qu'elle ne perd, l'échange de travailleurs ne reflétant que l'appariement aux firmes (Soskice 1994). À noter que dans les modèles du genre Acemoglu & Pischke (1998a), l'externalité engendré par l'asymétrie d'information est *négative*, ne se reflétant que dans le départ de travailleurs qualifiés suite aux offres des concurrents, tandis que l'argument de Soskice (1994) est basé sur une externalité *positive* puisque la firme formatrice peut aussi embaucher des travailleurs qualifiés venant d'autres firmes[11].

---

[8] L'argument dans Acemoglu & Pischke (1998a) est essentiellement le même que dans Gibbons & Katz (1991).

[9] Plus précisément, des équilibres multiples peuvent exister, mais à un équlibre avec faible taux de roulement est associé une forte incidence de formation.

[10] Les contraintes quant à des contrats à durée limitée ont été relâchées en 1985, voir par exemple Hunt (1994).

[11] Voir aussi Acemoglu (1997).

Finalement, la structure du marché du travail peut influencer les incitations auxquelles font face les acteurs. Même si d'autres firmes peuvent utiliser le capital humain créé, et que nous classifions cet investissement comme général, il faut que la demande pour ce capital existe. Si la firme formatrice est dans une position monopsonistique dans un marché local, l'investissement dans le capital humain se fait comme s'il était spécifique, un fait déjà reconnu par Becker (1964,1993). Plus généralement, il est probable que la distinction entre *parfaitement général* et *parfaitement spécifique à la firme* ne concerne que deux cas extrêmes. Pour reprendre la notation de Stevens (1994), notons par un vecteur

$$v = (v_o, v_1, v_2, v_3, \ldots, v_k, \ldots)$$

les productivités d'un travailleur dans sa firme formatrice v_0 et dans les autres firmes de l'économie, v_k, $k = 1, 2, 3, \ldots$. Un programme de formation parfaitement générale dans le sens strict du terme tel que défini plus haut équivaut à

$$v_g = (g, g, g, g, \ldots, g, \ldots),$$

pour un $g > 0$, et un programme parfaitement spécifique à une seule firme peut être représenté par

$$v_s = (s, 0, 0, 0, \ldots, 0, \ldots),$$

pour $s > 0$. Mais d'autres configurations peuvent exister, telles que

$$v_{t1} = (t, t, t, 0, \ldots, 0, \ldots),$$

pour $v_k = t > 0$ pour $k \in I$, $v_k = 0$ autrement, et

$$v_{t2} = (v_o, \lambda_1 v_0, \lambda_2 v_0, \lambda_3 v_0, \ldots, \lambda_k v_0, \ldots)$$

pour $v_0 > 0$ et $0 \leq \lambda_k \leq 1$. La situation v_{t1} correspond à une situation d'oligopsone, et celle décrite par v_{t2} à une situation où les firmes ont des exigences différenciés en

capital humain (Stevens 1994). Comme nous allons voir dans la section 1.4, il est possible de caractériser v_{t2} ou l'ensemble I plus précisément, par exemple en spécifiant que I englobe toutes les firmes d'un même secteur ou d'une même région économique. I peut dépendre des caractéristiques du travailleur, du capital humain acquis - auquel cas la dimension de I est éventuellement endogène -, de l'état de la conjoncture, ou d'autres facteurs. Il est clair que la quantité de capital humain acquis dans un tel contexte, sa spécificité, et le partage de coûts et bénéfices peuvent varier en fonction du vecteur v. Par exemple, Stevens (1994) montre que s'il existe un nombre fini de firmes pouvant utiliser la formation du travailleur (v_{t1}), avec présence de chocs hétérogènes à la productivité potentielle du travailleur dans chacune d'elles, et si la compétition pour le travailleur se fait à la Bertrand, le travailleur ne capte pas toute la rente due au capital humain. Si l'on classe les productivités réalisées par rang, notant par v^1 la plus haute, ensuite v^2, jusqu'à v^n la plus basse, alors le travailleur formé reçoit $v_2+\epsilon$, ϵ petit, tandis que sa productivité dans la firme *gagnante* est de v^1. La firme fait alors un profit de $v^1-v^2-\epsilon > 0$ et ce, même si le capital " est d'utilité dans un grand nombre de firmes". L'accroissement de salaire du travailleur due à la formation est inférieure à l'accroissement de productivité[12], comme les études empiriques l'attribuent souvent à la présence de capital humain spécifique. De plus, si l'on admet que les cas v_s et v_g sont irréalistes puisqu'extrêmes, les difficultés empiriques de distinguer entre capital humain *général* et *spécifique* à partir d'observations salariales deviennent prééminentes.

Mais le modèle de Stevens (1994) a également des implications pour la mobilité des travailleurs que le modèle beckerien avec la simple distinction entre capital spécifique et capital général ne permet pas de tirer[13]. Notons que toute augmentation de la durée de la relation de travail est due au fait que la firme formatrice, pour diverses raisons,

[12]Évidemment, dans le cas déterministe, c'est-à-dire, en présence d'au moins deux firmes avec $v_k = v^1$, on obtient que le travailleur reçoit toute la rente.

[13]Ces conclusions ne sont pas présentes dans Stevens (1994).

peut offrir un salaire plus élevé que d'autres, incitant ainsi le travailleur à rester. Ceci n'est plus nécessairement vrai dans un contexte comme v_{t1} ou v_{t2}, ou le travailleur peut avoir une utilité similaire, voir plus grande dans d'autres firmes. Mais des conclusions semblables peuvent être tirées pour l'*ensemble* des firmes I telles que $v_k > 0$ pour $k \in I$. Considérons le cas le plus simple, où v_{t1} est déterministe. Alors, même si sous certaines hypothèses le travailleur ne capte pas toute la rente, il est certainement vrai que l'ensemble des firmes I peut offrir au travailleur un salaire plus élevé que les firmes dans I^C. Au lieu d'augmenter la durée de la relation de travail, l'incidence de capital humain prolonge la durée du séjour du travailleur (en probabilité) à l'intérieur de l'ensemble I. Autrement dit, la probabilité de passage de l'ensemble I à l'ensemble complémentaire I^C est diminuée. Au lieu d'estimer si le capital est spécifique à la firme ou non, il est possible d'estimer si le capital est spécifique à un groupe I en définissant une caractéristique de l'ensemble I, par exemple le secteur ou la région.

Une telle spécification est robuste à l'asymétrie d'information sur le type du travailleur (Acemoglu & Pischke 1998a). La distinction principale dans ce modèle est faite entre des travailleurs qui restent avec la firme formatrice et ceux qui partent, tandis que le modèle ci-haut permet de distinguer, parmi les travailleurs qui partent, ceux qui ont été formé et ceux qui n'ont pas reçu de formation. Ce modèle peut être également dans un contexte non-vérifiabilité de la formation (Chang & Wang 1996) si on suppose que l'ensemble I est défini par la précision avec laquelle des firmes peuvent observer la formation.

1.3 Données disponibles

Il est d'abord utile de préciser, au-delà des concepts théoriques, quelles sont les informations disponibles aux études empiriques quant à la formation formelle.

Mesures du capital humain

Avant les années 1980, il n'était pas possible de déceler la formation précisément. L'approximation que l'on utilisait consistait à prendre le temps écoulé dans certaines circonstances. Ainsi, l'ancienneté accumulée dans une entreprise était presumée capturer l'effet moyen du capital humain spécifique acquis durant ce temps, et le temps passé sur le marché du travail, l'accumulation de capital humain général. La rémunération de l'expérience et de l'ancienneté représentait alors le rendement au capital humain (Mincer 1974). Puisque la deuxième mesure n'était souvent pas disponible, on l'approximait par l'expérience potentielle, calculée comme *expérience = âge - années d'éducation/scolarité - âge de scolarisation (6)*. Les données longitudinales disponibles depuis - PSID, GSOEP, NLSY - permettent de caractériser plus précisément ces deux mesures, rendant plus riche l'information utilisée dans des études. Ainsi, il est non seulement possible de calculer plus ou moins exactement le temps qu'un individu a passé à travailler, mais aussi de distinguer l'expérience acquise dans différentes industries ou occupations. Les articles utilisant cette information supplémentaire se font trop nombreux pour que l'on puisse les énumerer ici. En guise d'exemple, citons Kim & Polachek (1994), qui démontrent que le rendement du capital humain, contrairement à l'effet observé typiquement en utilisant l'expérience potentielle, ne diffère guère entre hommes et femmes si on le mesure par l'expérience réelle. Citons également Parent (1995), dont l'étude est plus pertinente pour la présente recension. Il distingue l'ancienneté dans l'entreprise de l'expérience acquise dans le secteur de cette même entreprise, cette dernière s'avérant être la mesure du capital humain pertinente pour la rémunération. Nous reviendrons sur cette étude plus loin.

Mesures de la formation formelle

La disponibilité d'enquêtes de plus en plus diverses a également permis de raffiner d'autres mesures plus directes (de l'acquisition) du capital humain. Nous soulignons ici l'acquisition par une formation à l'intérieur d'une entreprise ou plus précisément, durant une relation de travail. Sans se vouloir exhaustive, l'énumération suivante dresse un portrait sommaire sur le genre d'information disponible.

Il faudrait d'abord spécifier ce que les études et les répondants comprennent par «formation formelle». Les tableaux 1.I, 1.II et 1.III aux pages 39 à 43 énumèrent quelques unes des bases de données fréquemment utilisées aux États-Unis, en Grande Bretagne, au Canada[14] et en Allemagne. Des extraits des questions utilisées pour recueillir l'information pertinente sont repris. En général, on entend par une formation formelle un cours, une présentation, ou une leçon, donné en général par une personne spécialement formée dans le but d'enseigner le matériel. Il est toutefois possible d'inclure des cours autoguidés (voir la question de l'*EOPP*) dans cette division, en autant qu'ils aient lieu pendant les heures de travail.La plupart des enquêtes n'ont pas une définition aussi stricte. Il est évident qu'il existe une grande diversité quant à la précision des questions. Par exemple, le PSID ne demande que le temps de formation moyen d'un travailleur «typique », tandis qu'à l'autre extrême, le NLSY recueille de l'information détaillée sur jusqu'à quatre formations formelles par année, et le SEPT95 demande aux employeurs et aux employés d'enregistrer contemporellement sur un calendrier tous les évènements de formation pouvant avoir lieu pendant deux semaines. Toutefois, la divergence des résultats rapportés dans la section 1.4 ne semble pas être uniquement attribuable à ces différences.

[14]Les données canadiennes n'ont cependant pas été utilisées dans des travaux à caractère académique.

Des différences institutionnelles modifient également la teneur des questions. Par exemple, l'institution de l'apprenti n'est presque plus en existence aux États-Unis, est en déclin en Angleterre et est toujours la source majeure de la formation formelle des jeunes en Allemagne. Des comparaisons sont faites pour essayer de déceler la raison du succès ou de l'échec de chaque système par rapport à l'autre (Blanchflower & Lynch 1994, Harhoff & Kane 1997). Souvent, les comparaisons peuvent surprendre. Ainsi, tandis que l'incidence annuelle de la formation formelle en lieu de travail est d'environ 5.7 pourcent dans le NLSY (Barron, Berger & Black 1997, tab. 1), elle est d'environ 20 pourcent dans le GSOEP (calcul de l'auteur).

Qualité des données

L'incidence mesurée de formations formelles étant fonction des questions, elle varie d'une source de données à l'autre et ne peut pas toujours être corrigée pour les cadres d'échantillonage différents. Ceci complique la comparaison d'études utilisant des bases différentes. Ainsi, l'EOPP et le SBA, bien que passablement similaires, donnent des taux d'incidence dans les trois premiers mois de 15 pourcent et 21 pourcent (Barron et al. 1997, tab. 1). Le NLSH72 et le CPS mesurent tous les deux l'incidence obtenu dans l'emploi actuel, l'un rapportant une incidence de 27.8 pourcent, l'autre de 12.0 pourcent (Barron et al. 1997, tab. 2).

Mais la qualité de données dépend également de la personne à l'origine de l'information. Deux sources sont possibles : le travailleur qui suit une formation, et l'employeur qui l'offre. La comparaison lors des enquêtes conjointes, mais pas nécessairement appariées, permet de réconcilier en quelque sorte les différentes réponses, quoiqu'imparfaitement. Ainsi, l'analyse du SEPT95, qui pose des questions à l'employeur concernant *toute* incidence de formation dans l'entreprise et à l'employée concernant la formation que *lui* - ou *elle* - a reçu, dévoile que 92.5 pourcent des employeurs fournissent de la formation, mais

que 69.8 pourcent des employées en reçoivent (Frazis, Gittleman, Horrigan & Joyce 1998, tab. 1). Krueger & Rouse (1998), utilisant des données administratives appariées à des enquêtes auprès des travailleurs, trouvent que 10.5 pourcent des réponses des travailleurs sont mal classifiées, et Barron et al. (1997) trouvent 28.2 pourcent de désaccord entre la réponse de la firme et celle de l'employé. Quoique les résultats ne semblent pas changer de beaucoup (Barron et al. 1997), il bon de se rappeler que les répondants aux enquêtes peuvent commettre des erreurs.

1.4 Résultats empiriques récents

Dans cette section, nous tournons le regard vers des résultats récents pouvant être vus sous l'angle de la spécificité du capital humain. Puisque cette recension de la littérature concerne la formation formelle en milieu de travail, une attention particulière sera portée à la définition de la mesure de capital humain. Pour d'autres formes de la capital humain, voir par exemple le survol de Parent (1996).

Parmi les études du capital humain créé en milieu de travail, deux filières peuvent être distinguées. L'une s'intéresse à la formation formelle, et fera objet de cette section. L'autre se préoccupe plutôt de Toutefois, la deuxième apporte des informations importantes quant à la spécificité du capital humain et utilise des méthodes utiles. Aussi, la décrirons nous.

Beaucoup d'études se penchent sur les questions du rendement de l'expérience générale vis-à-vis de celle de l'ancienneté dans la firme. L'idée découle du partage des bénéfices d'une formation : la part de l'augmentation salariale attribuable à l'expérience générale dans toutes les entreprises représente la rémunération du CHG. Si en plus le travailleur reçoit un accroissement salariale qui est purement imputable à l'ancienneté dans la firme, et qu'il perd lorsqu'il change d'entreprise, on déduit la présence de CHS.

Son absence indiquerait soit qu'il n'y a pas de partage des bénéfices, soit que tout capital humain est général[15].

D'autre part, un grand nombre d'articles s'intéresse aux travailleurs ayant subi une séparation involontaire (*displaced workers surveys, DWS*) et regarde l'effet d'une telle séparation sur les revenus de travail des travailleurs[16]. Ce deuxième groupe met en évidence l'importance de l'appartenance sectorielle d'un travailleur, qu'on pourrait interpréter comme du capital humain qui n'est ni spécifique à une seule firme, ni d'utilité pour toutes les firmes (Fallick 1993, Neal 1995). Parent (1995) joint ces deux courants et confirme ce résultat dans un cadre plus général. Utilisant le PSID et le NLSY, il mesure l'accumulation de l'expérience d'industrie pour des travailleurs à travers le temps, concluant qu'il n'y a pas d'évidence pour la présence de capital humain spécifique à une seule firme. Toutefois, un rendement positif à l'expérience générale persiste, indicant que le capital humain ainsi mesuré est de nature spécifique à l'industrie.

Les résultats précédents utilisent sans exception une mesure approximative du capital humain : le temps écoulé, autrement dit, l'expérience sur le marché du travail ou l'ancienneté dans la firme ou dans l'industrie. Toutefois, des mesures plus «exactes»sur la création sont disponibles depuis les années 80, en provenance de questions sur la formation, qu'elle soit formelle ou informelle, reçue en lieu de travail ou financée par l'employeur. Le restant de cette recension se CONCENTRERA sur les résultats découlant de l'étude de ces données.

Notons par rapport aux modèles décrits dans Section 1.2 que la définition de la spécificité du capital humain se rapporte à la productivité, mais que la plupart des études ne peuvent se baser que sur des mesures du salaire. Or, une augmentation de

[15]Voir la discussion à ce sujet dans les articles de Altonji & Shakotko (1987), Abraham & Farber (1987), et de Topel (1991). Abowd, Kramarz & Margolis (forthcoming) caractérisent plus explicitement la variation dans les paramètres d'ancienneté estimés à travers firmes et travailleurs.

[16]Voir par exemple Fallick (1993), Jacobson, LaLonde & Sullivan (1993) et Ruhm (1991), et Fallick (1996) et Kletzer (1998) pour des survols récents à ce sujet.

salaire nécessite une augmentation de productivité peu importe le partage des rentes, mais l'absence d'augmentation de salaire ne permet pas de conclure à l'absence d'augmentation de productivité. Les quelques études mesurant directement la productivité des travailleurs formés ne le font que dans une seule entreprise[17]. Par conséquent, la question de spécificité doit être approchée indirectement.

Dans une première section, nous couvrons les études basées sur une régression de salaire incluant, entre autres variables, une mesure du capital humain dont le degré de spécificité resterait à déterminer. Certaines études permettent également d'évaluer les coûts directs occasionnés par la formation ; d'autres se penchent sur la durée de la relation de travail. Seulement un petit nombre d'études se concentre plus explicitement sur la mobilité liée à la formation de capital humain et se joint à la littérature étudiant les *DWS*. Nous concluons cette partie avec certains résultats qui ne peuvent être classé dans ces catégories, mais apportent néanmoins une information supplémentaire quant à la transférabilité de la formation formelle.

Évaluations subjectives de la transférabilité

On peut se faire une première idée de la spécificité de la formation formelle tout simplement en demandant aux répondants comment ils évaluent l'application chez d'autres employeurs de la formation reçue. La question reste généralement hypothétique au moment où elle est posée, puisque le travailleur n'a pas encore eu l'occasion de l'appliquer ou de recevoir une offre d'emploi. Loewenstein & Spletzer (1997b) rapportent des résultats en provenance de l'*EOPP* et du *NLSY*, qui sont repris au tableau 1.IV. Ce tableau rapporte également des fréquences provenant du GSOEP. Peu importe si la question est posée aux employeurs ou aux travailleurs, ou même dans un contexte institutionnel très différent, il est évident qu'au moins deux tiers des répondants jugent leur formation

[17] À une exception près : l'EOPP mesure la productivité à l'embauche d'un travailleur, et peut la corréler avec le fait d'avoir reçu une formation ailleurs avant l'embauche.

«très transférable». La désagrégation d'après le lieu et l'organisateur révèle quelques différences (Loewenstein & Spletzer 1997b, Tableau 1b), mais même parmi les travailleurs ayant joui d'une formation organisée par l'entreprise, plus que 72 pourcent jugent que plus de la moitié de leur connaissances sont applicables chez un autre employeur. Ce taux atteint 82 pourcent pour les séminaires organisés par quelqu'un d'autre, mais qui se tiennent sur les lieux de l'entreprise, et qui souvent sont payés par l'entreprise. Le fait qu'une bonne partie soit quand même non-transférable indique une éventuelle complémentarité entre capital spécifique et général. Mais l'évaluation semble avoir un impact peu différencié quant au salaire ou à la mobilité (Loewenstein & Spletzer 1997b, tab. 4, 5 et 6) et la teneur en information des auto-évaluations reste discutable.

Est-ce que le salaire augmente avec la formation ?

La plupart des enquêteurs se sont concentrés à tester (a) si la formation reçue chez un employeur augmente le salaire pendant la relation de travail en vigueur et (b) si la formation reçue avec des employeurs précédents augmente le salaire actuel de plus ou de moins que la formation reçue avec l'employeur actuel. Notons que dans le contexte du modèle classique, le capital spécifique augmenterait le salaire plus chez l'employeur formateur, le CHG l'augmenterait d'un montant égal ou même supérieur (Parsons 1972). Dans les modèles plus récentes, cette distinction n'est plus nécessairement valable.

Le cadre analytique utilisé dans ces études émule celui des études portant plus généralement sur l'estimation des rendements à l'éducation et au capital humain depuis Mincer (1962, 1974), en régressant le salaire d'un travailleur sur la durée de la formation reçue. Quelques unes remplacent la durée par l'incidence d'une formation. Les premières études portant sur la formation en milieu de travail (Brown 1989, Duncan & Hoffman 1979, Lillard & Tan 1992, Mincer 1962) ont le désavantage d'utiliser des enquêtes dont les questions généralement ne permettent pas de faire la distinction quant à

la forme et le lieu de la formation. Ainsi, il est difficile d'évaluer à partir des questions du PSID l'impact de la formation formelle en particulier. Néanmoins, l'impact trouvé par ces études et des études plus récentes est généralement positif (voir ci-haut et Barron, Black & Loewenstein (1993), Loewenstein & Spletzer (1997b) et Parent (forthcomingb), *inter alia*), l'exception la plus citée étant vraisemblablement Lynch (1992a)[18]. Krueger & Rouse (1998) ne trouvent aucun impact sur le salaire présent, mais observent une augmentation de la probabilité de postuler pour d'autres postes, mieux rémunérés, une observation aussi faite par Büchel & Pannenberg (1994) pour l'Allemagne.

En général, les études en provenance d'autres pays montrent également un effet positif de la formation payée ou fournie par l'employeur[19,20].

Mais rappelons que ces résultats se réfèrent au salaire avec l'employeur actuel, ce qui n'est pas une bonne mesure pour pouvoir établir la présence de CHG ou de CHS. Plus utiles dans ce contexte sont les régressions de salaire sur la formation reçue avec des employeurs précédents. La teneur des études effectuant cet exercice est semblable. Tandis que Blanchflower & Lynch (1994) trouvent un impact plutôt mitigé et Bishop (1994) ne trouve aucun impact significatif, Loewenstein & Spletzer (1997b), Parent (forthcomingb) et Veum (1995a) trouvent un impact positif sur le niveau de salaire et Veum (1995b) trouve un effet *négatif* sur la croissance de salaire. Notons que l'absence d'impact de

[18]Lynch (1992a) ainsi que Loewenstein & Spletzer (1997b) et Parent (forthcomingb) utilisent le NLSY. La différence entre leur résultat est éventuellement attribuable au fait que les travailleurs dans l'échantillon qu'utilise Lynch (1992a) sont plus jeunes et qu'ils sont au début de leur carrière (Parent 1996).

[19]Arulampalam, Booth & Elias (1997) et Blanchflower & Lynch (1994) pour la Grande Bretagne, Büchel & Pannenberg (1994), Pannenberg (1996) et Pfeiffer & Brade (1995) pour l'Allemagne. Le résultat peut dépendre du contenu de la formation, s'il s'agit de *training* ou d'*education* (Arulampalam et al. 1997), de la durée (Büchel & Pannenberg 1994) ou de la présence ou absence de certificat (Blanchflower & Lynch 1994). Mais il y a toujours un type de formation financée ou fournie par l'employeur qui accroît le salaire.

[20]Il est difficile de calculer le rendement d'un apprentissage allemand étant donné qu'il s'agit d'un port d'entrée pour une carrière et que le groupe de comparaison n'est pas bien défini. Le rendement relatif d'un apprenti par rapport à une personne avec formation scolaire mais sans aucune formation post-secondaire est semblable à celui d'un ressortissant des écoles secondaires américaines *(high school)* par rapport à celui de quelqu'un sans diplôme secondaire (Harhoff & Kane 1997), mais il n'est pas clair que cette comparaison soit appropriée.

la formation reçue préalablement dans l'étude de Bishop (1994) est accompagnée d'une réduction de la probabilité de formation chez l'employeur actuel et d'une productivité initialement plus grande.

En comparant les effets sur le salaire provenant, d'une part, d'une formation avec l'employeur actuel et, d'autre part, d'une formation avec un employeur précédent, Loewenstein & Spletzer (1997b) et Parent (forthcomingb) les trouvent de même ampleur, tandis que Loewenstein & Spletzer (1998a) pour les États-Unis et Acemoglu & Pischke (1998a) pour l'Allemagne trouvent que l'impact est plus grand chez la firme formatrice.

Qui paie pour la formation ?

L'autre implication majeure du modèle beckerien est que le travailleur paie pour la formation générale, soit directement, soit par l'intermédiaire d'un salaire plus bas pendant la période de formation s'il fait face à des contraintes financières. L'employeur paie pour la formation spécifique. Réitérons que les modèles de marchés imparfaits impliquent souvent également que l'employeur paie une partie des frais d'acquisition de capital général, toujours sous la contrainte qu'il peut (en moyenne) faire un profit non-négatif durant la carrière du travailleur. Plusieurs auteurs se sont donc penchés sur (a) une éventuelle réduction du salaire pendant la formation et (b) l'évidence que l'employeur paie directement les coûts de la formation.

La plupart des études concernant l'impact de la formation sur le salaire montrent que ce dernier ne serait pas affecté ni pendant la formation (Loewenstein & Spletzer 1998a, Parent forthcomingb), ni en début d'emploi pendant lequel le travailleur sera formé (Veum 1995a)[21]. Ceci est également vrai en ce qui concerne l'Allemagne pour la formation après apprentissage (Pfeiffer & Brade 1995). Les apprentis allemands en dernière année de formation gagnent environ un tiers du salaire d'un travailleur qualifié

[21] Voir aussi les sources cités par Bishop (1997).

débutant (Bundesministerium für Arbeit und Sozialordnung 1994), mais malgré cette participation du travailleur dans le financement des coûts de sa formation, les firmes formatrices ont des coûts nets positifs (Harhoff & Kane 1997).

Quant aux frais directs, il semble également que l'employeur en paie une bonne partie, d'après les rapports des travailleurs (Loewenstein & Spletzer (1998a), voir aussi Tableau 1.V), mais aussi d'après les employeurs (EOPP). Néanmoins, les résultats des enquêtes d'entreprise indiquent des accroissements d'index de productivité bien supérieurs aux gains de salaire (Barron et al. 1997, Bishop 1991, Bartel 1995). Contrairement à l'Allemagne, les estimations de profit de Bishop (1991) indiquent un profit positif pour les employeurs qui forment.

La durée de la relation de travail et la mobilité

Implicite dans tous les modèles, la différence de mobilité est une mesure que des enquêtes de travailleurs peuvent capter. Eu égard au capital humain non relié à la formation formelle, des auteurs ont utilisé la mobilité forcée des travailleurs qui font partie d'un licenciement collectif («travailleurs déplacés ») pour étudier l'impact de l'ancienneté chez leur dernier employeur sur leur mobilité sectorielle. L'ancienneté dans la firme est souvent une bonne approximation de l'ancienneté dans l'industrie, et une diminution de la probabilité de se trouver un emploi dans une autre industrie due à un accroissement de cette mesure est alors interprétée comme un indice pour la présence de capital humain *spécifique à l'industrie*. C'est en fait ce que trouvent la plupart des études de ce genre (Fallick 1993, Jacobson et al. 1993, Neal 1995). Parent (1995) a étudié la question dans un contexte plus général.

Les études sur la formation formelle n'ont généralement pas utilisé cette approche, se concentrant plutôt sur l'impact de la formation sur la durée moyenne des périodes d'emploi et de chômage ou, plus communément, sur la durée de la relation de travail.

Étant à ma connaissance la seule étude dans la première catégorie, Gritz (1993) trouve que la formation par l'employeur réduit les périodes de chômage et prolonge les périodes d'emploi pour les femmes, augmentant clairement leur performance sur le marché du travail. L'effet est ambigu pour les hommes, puisqu'il augmente à la fois la longueur des périodes de chômage et d'emploi.

La plupart des auteurs ayant considéré la question de la durée de la relation d'emploi ont conclu que celle-ci est prolongée par la formation formelle. Les études basées sur des enquêtes de travailleurs trouvent généralement un effet positif sur la probabilité de rétention peu importe la méthode d'estimation utilisée (Loewenstein & Spletzer 1997b, Loewenstein & Spletzer 1997a, Lynch 1991, Parent forthcomingb), l'exception étant Veum (1997), qui trouve un effet positif mais généralement non significatif. Par contre, les études basées sur des enquêtes auprès des entreprises ne trouvent des effets positifs sur la probabilité de rétention que parmi quelques sous-groupes d'entreprises (Bishop 1991, Krueger & Rouse 1998).

Les méthodes d'estimation utilisées par les auteurs cités ci-haut diffèrent quelque peu, et il est utile de les expliquer brièvement. Deux méthodes sont généralement distinguées. L'une utilisé la méthode de Cox (1972)[22], estimant la vraisemblance partielle d'un modèle de hasard proportionnel. Puisque la vraisemblance partielle fait abstraction de la distribution de base, il n'est pas possible de faire des inférences quant à l'impact quantitatif d'une variable sur la durée[23], mais on obtient des estimés qui permettent d'évaluer l'impact relatif entre autres variables, et de connaître le signe de l'impact.

La deuxième méthode consiste à considérer simplement la probabilité de rétention d'une période à l'autre, équivalent à la fonction de survie. Cette méthode peut être utilisée pour faire des inférences quantitatives, mais dans les études mentionnées ici,

[22]Voir aussi Kalbfleisch & Prentice (1980).

[23]Il est possible de faire quelques inférences quant à la distribution de base à partir des résidus.

elle est généralement utilisé sur deux périodes discrètes très larges, dû à la qualité des données. Sans des informations supplémentaires concernant la distribution de la variable de durée, il n'est également pas possible de faire des inférences quantitatives.

En outre, il faut noter que la durée de l'emploi mesurée dans toutes les études sauf celle de Gritz (1993) inclut la durée de formation. Puisque les travailleurs en formation formelle ne sont pas productifs (par définition même de la formation formelle, voir plus haut), il semble logique qu'une firme rationnelle ne s'intéresse qu'au temps passé à produire. Pour étudier l'impact de la formation sur le temps *productif* passé avec la firme, il faut alors séparer les deux périodes distinctes lors de l'estimation, ce qui est fait seulement par Gritz (1993). Faute de cette correction, il est nécessaire d'étudier l'impact *quantitatif* de la formation sur la longueur de la relation de travail, ce qu'aucune des études citées ci-haut ne fait. L'effet positif sur la durée de la relation d'emploi qu'elles trouvent sont alors difficile à évaluer.

Ailleurs, la plupart des études se sont concentrées sur les apprentis. Le problème économétrique noté précédemment ne s'applique pas dans ce cas, puisque l'on considère le premier emploi *après* la fin de l'apprentissage. Par contre, un nouveau problème surgit : l'identification d'un groupe de comparaison. Les études comparent en général les apprentis aux travailleurs non-qualifiés, mais il n'est pas clair que ce soit le bon groupe de comparaison. Il est à noter que la durée de formation moyenne d'un apprenti est de 46 mois en Angleterre (Booth & Satchell 1996, pg. 290) et de 36 mois en Allemagne (Harhoff & Kane 1997). Si on admet la comparaison avec les travailleurs non-qualifiés, alors la probabilité de rétention est généralement augmentée par un apprentissage (Booth & Satchell 1996, Winkelmann 1996).

À ma connaissance, les seules études traitant explicitement de la mobilité en ce qui a trait à la formation formelle sont celles de Gritz (1993) et Pannenberg (1996) pour la formation en milieu de travail et Winkelmann (1994,1996) pour l'apprentissage. Gritz

(1993), comme on l'a déjà mentionné, regarde la période d'emploi au lieu de la durée de la relation de travail. Si elle inclut des changements d'employeurs, une augmentation de la durée d'emploi implique nécessairement que le travailleur a acquis une qualité qui le rend plus productif pour d'autres employeurs, c'est-à-dire que le capital humain est transférable. Les résultats de Gritz sont ambigus pour les hommes, puisque la durée des périodes de chômage augmente en même temps que la durée des périodes d'emploi. Pour les femmes, la formation semble créer des externalités positives quant à leur employabilité. Pannenberg (1996) rapporte que le nombre de changements d'employeur n'augmente pas avec l'incidence de la formation. Ses résultats peuvent indiquer également la généralité de la formation, puisque l'absence d'effet sur le nombre d'emploi dans une période fixe implique que la probabilité de séparation *moyenne* ne diminue pas. Finalement, Winkelmann trouve que la probabilité de rétention dans le premier emploi est supérieur pour les apprentis changeant d'employeur immédiatement après la fin de leur apprentissage (Winkelmann 1996), et que les apprentis sont moins probable de changer de secteur (Winkelmann 1994) que le groupe de comparaison, les travailleurs non-qualifiés.

D'autres résultats

Les études en milieu d'entreprise permettent souvent de mesurer d'autres variables relatives à la transférabilité de la formation des travailleurs, moins indirectement que les mesures de salaires ou de productivité dans l'entreprise formatrice. Ainsi, utilisant le NFIBS, Bishop (1994) trouve que le temps habituel de formation est réduit par la présence de formation formelle acquise chez des employeurs précédents. De plus, l'évaluation de la productivité initiale par l'employeur est accrue, *ceteris paribus*, quoique cet effet disparaît plus tard. Il apparaît que la formation acquise chez des employeurs précédents peut remplacer la formation chez l'employeur actuel, une indicatrice du degré

de transférabilité de capital humain. Il est remarquable dans ce contexte que les mêmes données ne permettent de déceler aucun impact de la présence de formation lors de l'embauche sur le salaire.

Krueger & Rouse (1998) montrent que le taux d'absentéisme et la performance au travail diminue avec la formation. Un taux d'absentéisme réduit est certainement un atout que plus d'un employeur évalue, mais puisque l'étude de Krueger & Rouse (1998) utilise des données d'entreprise, on ignore le sort de ces travailleurs une fois qu'ils ont quittés l'entreprise.

1.5 Conclusion

Que faut-il tirer comme conclusion sur la spécificité du capital humain créé par une formation formelle fournie par une firme? Rappelons que l'aspect de la formation que nous essayons de mesurer est sa productivité, et seulement en second lieu sa rémunération. La distinction est importante, puisque la rémunération peut être liée à d'autres contraintes - coût de transaction, contraintes informationelles - tandis que la productivité est structurelle.

Malgré la panoplie d'articles se penchant sur la question de l'accroissement de salaire à l'intérieur de la firme formatrice, le survol théorique montre que l'existence ou l'absence d'un accroissement salariale peut être cohérente avec et le CHG et le CHS et ne révèle rien sur la spécificité en raison de la multitude de contraintes informationelles et contractuelles. Les études empiriques ne montrent indirectement (et parfois directement) qu'une seule chose : que la formation est corrélée avec une plus grande productivité du travailleur formé. Mais l'absence de l'effet observé n'aurait pas rejeté cette hypothèse.

Il en est de même avec la comparaison des rendements à l'intérieur et à l'extérieur de la firme formatrice. Encore une fois, la théorie ne nous permet pas de distinguer si la

différence devrait être positive (le cas du capital spécifique à la firme ou du capital général avec coûts de transaction), nulle (capital humain spécifique sans partage de bénéfices ou parfaitement général), ou même positive (Acemoglu & Pischke 1998a). Empiriquement, chacune de ces hypothèses trouve du support et le mécanisme sous-jacent n'est pas évident. Tout de même, la plupart des études récentes tirent la conclusion que la formation observée est de caractère général. Il est d'autant plus étonnant alors que la presque totalité des études montrent en même temps que les employeurs payent les frais directs et indirects des formations, contrairement à ce que suggère le modèle beckerien.

Puisque la plupart des contraintes informationelles décrivent la firme formatrice face au reste du monde, la modélisation explicite de la mobilité permettrait de rajouter une dimension supplémentaire. Sachant où le travailleur trouve son prochain emploi après une séparation, les auteurs étudiant les travailleurs déplacés ont pu conclure à la présence de capital humain spécifique à l'industrie. Peu d'études ont utilisé cette méthode en ce qui a trait à la formation formelle. Notons encore que cette méthode permettrait de distinguer entre le capital humain général avec présence de coûts de transaction et le capital humain spécifique provenant des différences de productivité.

Une réconciliation entre la généralité du capital humain - son utilité au-delà de la firme formatrice - et le fait que les firmes payent des frais substantiels peut être fait de deux façons. D'abord, il est aussi évident à partir des études basées sur des enquêtes d'entreprise que la productivité du travailleur augmente proportionnellement plus que le salaire des travailleurs. Ceci pourrait être dû à la présence simultanée de capital général et spécifique, mais il reste à expliquer pourquoi la firme semble payer la totalité de frais. Une explication pourrait être l'argument que l'éducation générale est un prérequis pour la formation spécifique (Soskice 1994).

Une autre explication pourrait se trouver dans un modèle du type presenté par Stevens (1994). La firme paye pour la totalité des frais de la formation générale puisqu'en

l'absence de compétition parfaite elle peut se garantir une rente strictement positive. Un des cas particulier dans ce modèle, intuitivement attractif, est que l'utilité du capital humain ne s'étend qu'à un groupe restreint de firmes. Soskice (1994) décrit un modèle où il existe un équilibre dans lequel les firmes payent pour la formation générale sans qu'il y ait des contraintes sur la mobilité des travailleurs et sans coordination explicite. Si on pouvait caractériser ce groupe plus spécifiquement, la plausibilité de ce modèle serait augmentée. Mais parmi les études recensés, seul Winkelmann (1994) essaye une telle caractérisation en étudiant la mobilité des apprentis à travers les différents secteurs.

L'intérêt que suscite la formation et son éventuel soutien fiscal est fondé seulement si la formation génère une externalité positive et que l'investissement qu'elle nécessite soit réduite en deça de l'optimum social à cause de l'externalité. Dans le présent texte, nous avons essayé de distinguer le capital humain spécifique à la firme d'un capital humain plus général. Seul ce dernier crée une externalité, et c'est lui seulement qui justifie un soutien quelconque. Mais malgré le grand nombre d'auteurs qui amènent cette conclusion et malgré quelques études donnant des indices circonstanciels, l'évidence n'est pas concluante quant à la présence de formation formelle plus générale, incluant la formation qui serait spécifique à un sous-groupe suffisamment grand d'entreprises. Une méthode plus claire consisterait à estimer des modèles de mobilité, dans la même veine que les études portant sur les travailleurs déplacés, mais peu d'études l'ont fait.

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Annexe

TAB. 1.I: Enquêtes d'employeurs sur la formation

Année	*Obs.*	*Commentaires*	*Utilisés par*
Employer Opportunity Pilot Project (EOPP) [a]			
1982	2 000	Formation formelle et informelle Contenu de formation : inconnu. Lieu de formation : inconnu. *«... how much time was "spent" in* the first three months *on [...] (1) "formal training such as* self-paced learning *programs or training done by* specialized training personnel," *(2) "training activities in which he or she is watching others do the job rather than doing it himself," (3)* "total *number of hours management and line supervisors spent away from other activities giving* informal *individualized training or extra supervision," and (4)* "total *number of hours co-owrkers [...] spent away from their normal work giving* informal *individualized training or extra supervision»* (Bishop 1997, p. 24).	Loewenstein & Spletzer (1997b), Barron, Black & Loewenstein (1993)
Bartel (1995)			
1986-1990	19 000	Formation formelle. Contenu de formation : connu. Lieu de formation : inconnu. Données administratives.	Bartel (1995)
National Federation of Independent Business Survey (NFIBS) [b]			
1987	1 000	Formation sans distinction chez l'employeur actuel, formelle et informelle chez des employeurs précédents. Enquête auprès des entreprises. Contenu de formation : inconnu. Lieu de formation : inconnu. *«How many hours did you or an employee spend training or closely supervising [the worker] ?»*	Bishop (1994)

(cont.)

TAB. 1.I: Enquêtes d'employeurs sur la formation (suite)

Année	*Obs.*	*Commentaires*	*Utilisés par*
Sondage National sur la Formation (SNF91) [c]			
1991	7 500	Formation formelle et informelle Contenu de formation : connu. Lieu de formation : connu. Données canadiennes	
Small Business Administration (SBA92)[d]			
1992	1 000	Formation formelle et informelle Contenu de formation : inconnu. Lieu de formation : connu. Questions similaires à l'*EOPP.*	Barron, Berger & Black (1997)
Survey of Employer Provided Training (SEPT93) [e]			
1993	8 500	Formation formelle Contenu de formation : connu. Lieu de formation : inconnu. Exemple de question : *«During 1993, did your establishment provide or finance formal training in basic reading, writing, arithmetiç or Enlish language skills for any of its employees ?»* (Frazis, Herz & Horrigan 1995, p. 4).	

Details : [a] Barron, Black & Loewenstein (1989). Guillemets et emphase présent dans l'original. [b] Bishop (1994) [c] Barron et al. (1997) [d] Centre canadien du marché du travail et de la productivité (1993) [e] Frazis et al. (1995)

TAB. 1.II: Enquêtes de travailleurs sur la formation

Année	*Obs.*	*Commentaires*	*Utilisés par*
Panel Study of Income Dynamics (PSID)[a]			
1968 -1995	5 000	Formation habituelle pour un travailleur dans cet emploi. Contenu de formation : inconnu. Lieu de formation : inconnu. *«On a job like yours, how long would it take the average new person to become fully qualified ?»*	Duncan & Hoffman (1979)
National Longitudinal Survey 1979 Youth Cohort (NLSY)[b]			
1979- 1994	12 000	Formation formelle reçue dans l'année précedente. En 1993, questions sur l'évaluation subjective de la formation réçue. Contenu de formation : inconnu. Lieu de formation : connu. *«[...] Did you attend any (other) training program or any on-the-job training designed to help people find a job, improve job skills, or learn a new job ? Which category on this card best describes where you received this training ? (1) business school (3) apprenticeship program (4) vocational or technical institute (7) correspondence course (8) formal company training run by employer or military training (9) seminars or training programs at work not run by employer (10) seminars or training programs outside of work (11) vocational rehabilitation center (12) other.*	Lynch (1992), Blanchflower & Lynch (1994), Loewenstein & Spletzer (1997a), Parent (1995), Vilhuber (1997)
British National Child Development Study (NCDS)[c]			
1981, 1991	6 000	Formation au moment de l'enquête. Contenu de formation : inconnu. Lieu de formation : inconnu. (1) *«Since March 81, have you been on any courses that were meant to lead to qualifications ?»* et (2) *«Since March 1981 have you been on any training courses designed to help you develop skills that you might use in a job [...] ?»*	Arulampalam, Booth & Elias (1997)
National Longitudinal Survey of High School class of 1972 (NLSHS72)[d]			
1986	800	Formation formelle reçue dans l'année précedente. Contenu de formation : inconnu. Lieu de formation : connu.	Altonji & Spletzer (1991)

(cont.)

TAB. 1.II: Enquêtes de travailleurs sur la formation (suite)

Année	*Obs.*	*Commentaires*	*Utilisés par*
Current Population Survey (CPS)[e]			
1983		Formation informelle reçue depuis le début de l'emploi en cours. Contenu de formation : inconnu. Lieu de formation : inconnu.	Lillard & Tan (1992)
German Socio-Economic Panel (GSOEP)[f]			
1989, 1993	2300	Formation formelle reçue dans les trois années précedentes Contenu de formation : connu. Lieu de formation : connu en partie. Séries de questions sur des les trois derniers cours pour *berufliche Fortbildung* (avancement profesionel).	Pischke (1996), Vilhuber (1998)
(Canadian) Adult Education and Training Survey (AETS)[g]			
1990, 1992, 1994		Formation formelle reliée au travail et aux intérêts personels Contenu de formation : connu. Lieu de formation : connu.	
British Household Panel Survey (BHPS)[h]			
1993	5 000	Formation reçue dans l'année précedente Contenu de formation : inconnu. Lieu de formation : inconnu. *«Since September 1st last year, have you taken part in any education or training schemes or courses, as part of your present employment ?»* (Freed, Brice, Buck & Prentice 1997)	

Details : [a] Duncan & Hoffman (1979). [b] Loewenstein & Spletzer (1997b). Questions en provenance du questionnaire de 1989, les choix peuvent varier par année. [c] Arulampalam et al. (1997). Trois derniers formations reçus depuis dix ans. [d] Barron et al. (1997) [e] Lillard & Tan (1992) [f] Burkhauser (1991) and Vilhuber (1998). Durée, intensité et l'information si le cours avait lieu pendant les heures de travail sont disponibles pour tous les cours. Financement, lieu de formation et évaluation subjective ne sont disponibles que pour le cours «le plus important». Seulement des cours/formations pendant les trois dernières années. [g] Statistics Canada (1997). [h] Freed et al. (1997).

TAB. 1.III: Enquêtes d'employeurs et de leurs employées sur la formation

Année	*Obs.*	*Commentaires*	*Utilisés par*
Krueger & Rouse (1998)[a]			
1992-1994	700	Données administratives. Contenu de formation : éducation de base (générale). Formation en lieu de travail, en classe/séminaire.	Krueger & Rouse (1998)
Upjohn Institute Survey [b]			
1993	300	Formation formelle et informelle. Contenu de formation : connu en partie. Lieu de formation : connu en partie.	Barron et al. (1997)
Survey of Employer Provided Training (SEPT95) [c]			
1995	1000	Formation formelle et informelle. Contenu de formation : connu. Lieu de formation : connu. Données sur la formation provenant d'un "training log" maintenu pendant deux semaines par l'entreprise, rapportant *"all the formal training events provided or financed by the establishment"* (Frazis, Gittleman, Horrigan & Joyce 1998, p. 4), et par l'employée sur 10 jours, rapportant *"any activity in which they were taught a skill or were provided with new information to help them do their job better, [...] who or what helped them learn the skill or information, how they learned the skill or information..."* (*ibidem*).	

Details : [a] Les données concernant l'employeur proviennent des données administratives, celles sur les employées d'un questionnaire qui permet de vérifier les informations administratives. [b] Barron et al. (1997). [c] Frazis et al. (1998). La classification des activités rapportés par les employées en *formel* et *informel* a été fait par algorithme basé sur les réponses élicités par les questions ci-dessus.

TAB. 1.IV: Mesures subjectives de la spécificité de la formation

EOPP [a]			
	How many of the skills learned by new employees in this job are useful outside of the company ?		
	almost all	58.35%	58.35 %
	most	13.56%	71.91 %
	some	19.91%	91.82 %
	almost none	8.19 %	100.01%
NLSY [b]			
	How many of the skills that you learned in this training program do you think (could be/could have been) useful in doing the same kind of work for an employer different than (current employer) ? Would you say...		
	all or almost all	62.88%	62.88 %
	more than half	13.90%	76.78%
	about half	11.86%	88.64%
	less than half	6.12 %	94.76 %
	none/almost none	5.23 %	99.99 %
GSOEP [c]			
	How well could you use this training in case you changed jobs ?		
	completely	29.76 %	29.76 %
	to a large extent	36.74 %	66.5 %
	partially/limited	24.10 %	90.6 %
	not at all	9.41 %	100.01 %

[a] Loewenstein & Spletzer (1997b), Tableau 2. Question à l'employeur, peut inclure la formation informelle. [b] Loewenstein & Spletzer (1997b), Tableau 1a. Formation formelle reçue depuis la dernière entrevue. [c] Calculé à partir du GSOEP, vague 10. Concerne une ou plusieurs formations formelles reçues dans les trois années précedent l'enquête.

TAB. 1.V: Financement des coûts directs d'une formation

NLSY [a]

Type de formation	Pourcentage payé par l'employeur
Formation formelle chez l'employeur	95.71%
Séminaires sur les lieux de travail	91.22%
Séminaires ailleurs	81.96%
École de commerce, institut professionel	42.22%
Autres	45.45%

GSOEP 1989, 1993 [b]

Assistance financière reçue	
Oui	66.4%
dont :	
de l'employeur	88.5%
du Bureau d'emploi	11.4%
autres	1.6%

NCDS 1991 [c]

Type de formation	Pourcentage fourni par l'employeur
Formation	91.75%
Cours éducationels	60.24%

EATS 1994 [d]

Type de formation	Pourcentage payé par l'employeur
Activités reliés au travail	72 %

[a] Loewenstein & Spletzer (1998), Tableau 1. [b] Calculé à partir du GSOEP, vagues 6 et 10. Question se réfère à la formation "la plus importante" dans les trois ans précédant l'entrevue. [c] Arulampalam et al. (1997, Tableau 3). Les réponses se réfèrent à qui a "fourni" (*provided*) la formation. [d] Hommes (Statistics Canada 1997, p. 69). 34% des formations payés par l'employeur (mais pas nécessairement reliés au travail) ont lieu au travail, 35 % dans ces centres de formation ou de conférence.

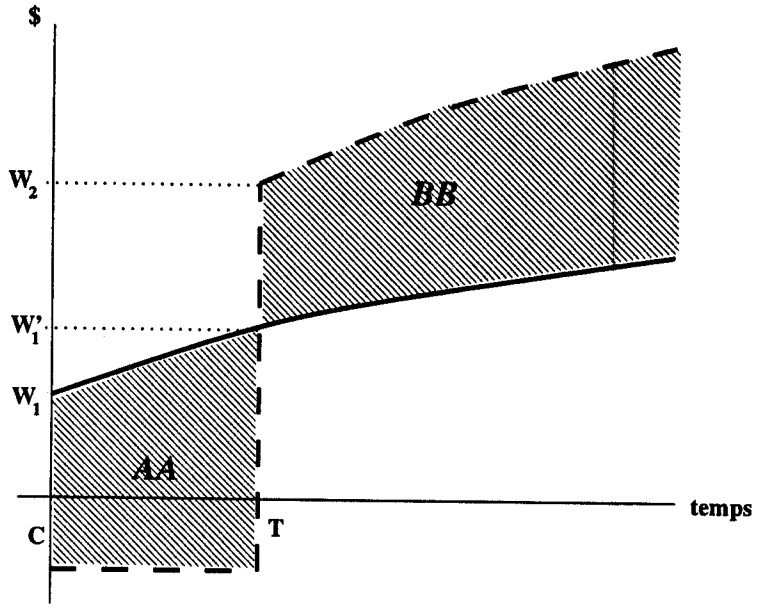

FIG. 1 .1: Choix d'éducation

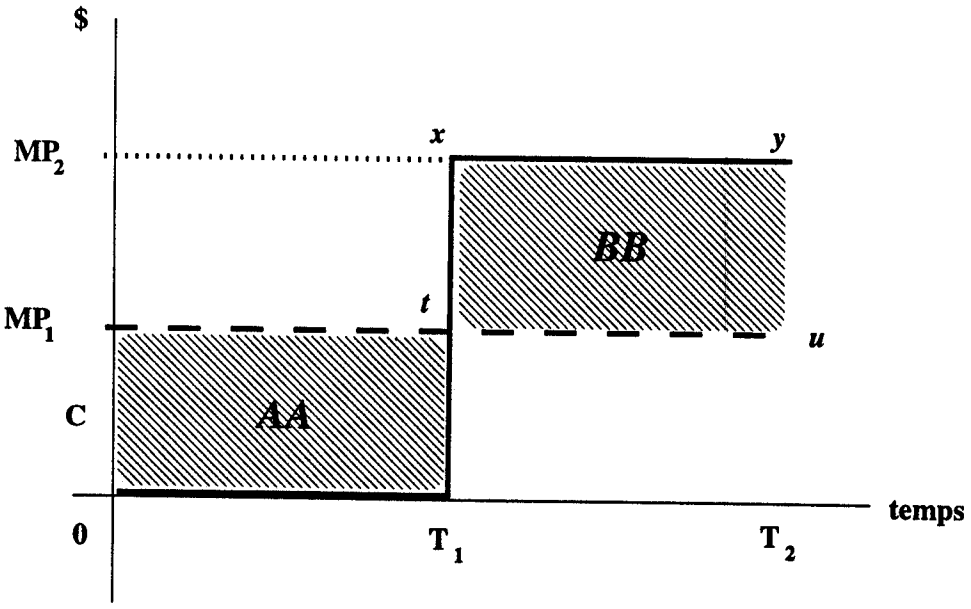

FIG. 1 .2: Formation en milieu de travail

Chapitre 2

Wage Flexibility and Contract Structure in Germany

2.1 Introduction

Earnings constitute a large fraction of household income, and factors affecting earnings thus have a major impact on the distribution of income. The secular rise in unemployment in recent years in Europe and Canada has renewed interest in the interaction between labor market conditions and earnings. In the present paper, we report results from an analysis of German panel data in the view of a set of wage models. The results shed new light on some aspects of the dynamics of German earnings with respect to labor market conditions, and underlines the fact that labor markets in Europe are different from North American markets.

Specifically, we look at how measures of labor market conditions at different points during the tenure of individuals with firms affect their current earnings. These measures are chosen to approximate different types of contractual models. In a simple model of implicit contracts, if workers are not mobile, their wage will depend on their alternative (employment) utility at the start of the current job if employers can commit to long-term contracts. On the other hand, if they are mobile, current wages will depend on their best alternative utility since the start of their job. Here as in other papers, the

alternative utility at a point in time is approximated by the rate of unemployment. In contrast hereto stand models in which the current wage will depend exclusively on current labor market conditions. This may be consistent with a number of models, including a standard labor demand model, an efficiency wage model, as well as a union bargaining model. Given the German institutional background, we argue that the most likely interpretation is the latter.

Using data on individuals from the German Socioeconomic Panel for the period 1985 to 1994, we find that both the contemporaneous unemployment rate and prior values of the unemployment rate are significantly correlated with current earnings. We interpret this result as evidence that German unions do in fact bargain over wages and employment, but that models of individualistic contracts, such as the implicit contract model, may explain some of the observed wage drift and longer-term wage movements reasonably well. The elasticity of earnings with respect to contemporaneous unemployment is between 9 and 15 percent, on par with previous studies of the German labor market. The effect of initial unemployment lies between 6 and 9 percent. Decomposing the unemployment measure into regional components reveals that regional labor market conditions determine the (initial) level of earnings, and national labor market conditions affect contemporaneous variations.

Furthermore, we explore the heterogeneity of contracts over a variety of worker and job characteristics. In particular, we find evidence that contracts differ across industries and across firm size. Workers of large firms are remarkably more insulated from the job market than workers for any other type of firm, indicating the importance of internal job markets. Blue collar workers are more strongly affected by contemporaneous labor market conditions than white collar workers.

The results obtained in this paper provide empirical evidence in line with previous articles on implicit contracts in the United States (Beaudry & DiNardo 1991). It aug-

ments and qualifies results reported in the literature on the wage curve (Blanchflower & Oswald 1994, Wagner 1994), where current earnings are correlated with current unemployment. Beaudry & DiNardo (1991) showed that this empirical result is not robust to the inclusion of unemployment rates appropriately chosen during the current employment spell. Our results bridge this gap, implying that the latter result may be an artifact specific to the U.S. economy, and only partially true for Germany.

The paper is organized as follows. Section 2.2 gives an overview of the models that we use to obtain predictions as to the correlation between the history of labor market conditions and current earnings. As we briefly mentioned above, institutions are relevant to interpreting the results, and we briefly describe some institutional background in Section 2.3. In Section 2.4, we describe the data used. Section 2.5 describes the results obtained and some of the econometric issues relating to these. In Section 2.6, we test the robustness of the results from the previous section across different dimensions of worker and job characteristics. Section 2.7 concludes and offers an outlook to further analysis.

2.2 Theoretical background

The relationship between wages and unemployment has often been discussed in the literature. The robust empirical relationship between contemporaneous unemployment and wages (the "wage curve") has been amply documented and the possible models underlying this phenomenon discussed in Blanchflower & Oswald (1994). An IAB publication sums up recent results on German wage curves (*Mitteilungen aus der Arbeitsmarkt- und Berufsforschung* (1996), see also Pannenberg & Schwarze (1998) and Wagner (1994)).

A number of models have implications linking contemporaneous unemployment to wages, ranging from compensating differentials to incentive contracts. Others, predom-

inantly based on contracts, link past measures of labor market tightness to current earnings. We will expose here the salient characteristics of the most important ones.

Implicit contracts

The basic idea in the literature on implicit contracts is that risk-averse workers can only insure themselves with their risk-neutral employers against shocks to labor productivity. The resulting labor contracts contain an 'implicit' insurance clause, and will depend on the mobility assumptions for both workers and firms. Suppose that productivity follows an AR(1) with parameter α. Assume further that firms can commit to contracts, and that they compete for workers, for whom mobility will initially be assumed to be costly. Then it can be shown (Beaudry & DiNardo 1991) that wages are rigid during tenure, and will depend on the alternative wage $\underline{w}$ and expected productivity ϕ^*:

$$\log w_{t,t(0)} = \delta_1(\alpha, \beta, \mu) \log \underline{w}_t + \delta_2(\alpha, \beta, \mu) \log \phi^* + k \tag{2.1}$$

where δ_j are reduced form functions of the structural parameters α, discount rate β and the worker's survival probability μ, and $t(0)$ the point in time at which current tenure started. A general equilibrium argument relating the change in the worker's reservation wage to the participation wage establishes the link with unemployment, leading to a estimable form of (2.1):

$$\log w_t = X_t'\beta + \gamma u_t \tag{2.2}$$

with

$$u_t = U_{t(0)}, \tag{2.3}$$

where we denote by u_t the relevant measure of unemployment determining wages at time t, by U_t the level of the unemployment rate at time t. The vector X_{it} includes the usual human capital controls thought to affect a worker's productivity (in logs). Since workers are not mobile, their wages will be a function only of unemployment at the start of the

job, as denoted by (2.3).

Renegotiation-proof contracts

If workers are mobile but firms can still commit to the employment contract, then the optimal contract resulting from the above setup will be upward flexible, being renegotiated every time the worker's alternative utility becomes binding (Harris & Holmstrom 1982). Linking as before alternative utility to unemployment implies that the lowest level of unemployment since the start of the current contract will be the principal determinant of the current wage. Thus,

$$u_t = \min_{p \in [t(0),t]} U_p \tag{2.4}$$

replaces (2.3). Once renegotiated, the initial level of unemployment does not influence current wages anymore, and wages will be function only of unemployment rates at the time of renegotiation.

One critique of the above model is the lack of incentive compatibility for the employer. The employer is assumed to be able to commit to long-term contracts. If however the employer's outside option in a contractual relationship becomes binding, it is optimal to renegotiate. MacLeod & Malcomson (1989)[1] have pointed out that if one increases the contract space by allowing for discretionary bonuses, then any allocation of the surplus from a relation may be consistent with an equilibrium. The efficient contract will fix a wage at the beginning of a relationship according to a split of the surplus. Since this split is the result of some bargaining process between the two parties and thus Pareto-efficient, no party will want to renegotiate afterwards, except if one party's outside option is larger than the utility obtained from continuing the present contract.

[1] See also MacLeod & Malcomson (1993).

If this constraint becomes binding, both parties will renegotiate, and the new contract will reflect the split of the surplus at the time of renegotiation.

If the worker's outside options are a decreasing function of unemployment, then the wage in the current contract will reflect the best labor market conditions since the start of the contract as in the implicit contract model, with the supplementary condition that the employer's outside option was never binding in the meantime, and conditional on the value of the best labor market conditions, occurring say at time $t > t(0)$, having been higher not only than the value of the outside option at time $t(0)$, but also higher than the value of the contract at time t. Hence, the same conditions derived from the costless mobility version of the implicit contract model are consistent with the contract model here, but are neither a necessary nor a sufficient condition for this model. Thus, though we may find that our results are consistent with this model, we cannot test it formally, as our regressions cannot falsify its implications.

Efficiency wage

Turning to links between contemporaneous labor market conditions and wages, one model frequently drawn upon to explain such a correlation is the efficiency wage model. Efficiency wage models of the shirking type[2] suggest that incentives to furnish effort derive from the threat of losing a surplus extant in a relationship. This surplus may be generated by direct mobility costs, the presence of specific human capital or a number of other reasons. The link most commonly studied is the one proposed (not exclusively) by Shapiro & Stiglitz (1984). There, unemployment implies a loss in utility since the probability of immediate re-employment is less than unity. Thus, there is a benefit to the employee of staying with the current employer. The model thus directly links

[2]The most frequently cited paper is Shapiro & Stiglitz (1984). See Carmichael (1990) and Lang & Kahn (1990) for a critical look at efficiency wage models.

unemployment to effort levels and wages. Wages are the carrots and unemployment the stick to achieve an equilibrium in which no shirking occurs.

Effort e can be either high or low, and can be detected with probability q. If caught shirking, the employee is fired, in which case he receives unemployment benefits w_0 while unemployed. In every period that he is unemployed, he will be re-employed with probability a. The incentive compatible wage derived from the model is then

$$w = e + w_0 + e(a + b + r)/q \tag{2.5}$$

where r is the discount rate. In equilibrium, the flows out of unemployment $a(N - L)$ must be equal to flows out of employment bL, so that $a + b = b/u$. Substituting in (2.5) obtains

$$w = e + w_0 + \frac{e}{q}\left(\frac{b}{u} + r\right) \tag{2.6}$$

which shows a negative relationship between wages and unemployment. Note however that due to the forward-looking character of the incentive constraint, the appropriate measure u is the expected value of future unemployment. If unemployment follows a unit root process, the current unemployment rate is sufficient to form expectations of future unemployment rates. Thus, past values should not influence current wages once contemporaneous unemployment has been controlled for, and the efficiency wage model implies

$$u_t = U_t. \tag{2.7}$$

Union bargaining models

In models of collective bargaining, a union with a well-defined concave utility function is assumed to bargain over wages and possibly employment with a profit-maximizing firm. If the bargaining agenda only covers wages, the resulting contract locus will coin-

cide with the labor demand curve, implying a negative relationship between wages and employment, and thus a positive correlation between unemployment and wages.

If the bargaining agenda covers both elements and bargaining powers on each issue are equal,[3]the slope of the contract locus will depend on the union's risk aversion. If the union is risk-averse, the contract locus will have a positive slope in wage-employment space, thus implying a negative correlation between unemployment and wages. Heterogeneity in relative bargaining powers allows identification of this curve. The relative bargaining powers of union and firm are reflected in the position along this curve. If unions and/or firms differ in their relative bargaining powers, a cross-section of contracts will identify the slope of the contract curve.

Estimation

The model estimated is

$$\log w_t = X_t'\beta + u_t'\gamma \tag{2.8}$$

where u_t is now a vector with the three elements described by (2.3), (2.4) and (2.7). Conditional on the "right" unemployment rate, other measures of unemployment do not predict wages, and a test of the three alternative hypotheses resulting from the above theoretical models is equivalent to a test on the coefficients on the different measures of unemployment. Note however that not all the above models are mutually exclusive. If only one element of γ is significant, then we can exclude some, but not all models. As we will find, results are more equivocal.

[3]See Manning (1987) for an analysis of when bargaining powers are not equal on each issue.

2.3 Institutional background

Our aim in this paper is to characterize the contract structure of wages, and in this respect, labor institutions matter. The particular importance of trade unions in the German model has often been pointed out. This section describes some pertinent aspects of German labor market institutions.[4]

The German economy is characterized by a high degree of coverage by collective agreements. Although union membership is around 40 percent,[5] union coverage by either industry-wide or firm-level contracts lies at around 90 percent of the eligible population.[6] Most contracts are negotiated at the level of a regional industry. Thus, collective agreements on wages and earnings are defined for 1 200 region-industry cells in Western Germany and 250 in Eastern Germany (Bundesministerium für Arbeit und Sozialordnung 1994). The number of firms having individual contracts with unions outside of the industry-wide agreement has been slightly increasing in recent years, but it is unclear whether the number of workers covered by these contracts has increased.

Regional and cross-industry differences exist, but there is informal coordination by the German Federation of Unions (*Deutscher Gewerkschaftsbund, DGB*). Informal evidence for spill-over effects is widespread. Furthermore, the Minister of Labor can legally extend contracts to the whole industry under certain circumstances. Thus, in 1994, the wage and earnings contracts were actually extended in 75 of the above region-industry cells (Bundesministerium für Arbeit und Sozialordnung 1994, pg. 32)[7]. It has been shown that when firms face a high enough probability of extension, they will act as if

[4]For a good introduction see f.i. Flanagan, Soskice & Ulman (1983), for some recent developments in collective bargaining Thelen (1991).

[5]Author's tabulation from years in which this question was asked for in the GSOEP. See also Carruth & Schnabel (1993).

[6]Bundesministerium für Arbeit und Sozialordnung (1994). Industries with little coverage are predominantly in the services sector. Only workers contributing to the social insurance system are covered by collective agreements.

[7]These extensions occur predominantly in retail trade and in the cleaning industry.

they were actually covered by the collective bargaining agreement (Margolis 1992). For these reasons, our data does not distinguish whether or not workers are covered by collective bargaining agreements. Some variation nevertheless exists, and for many firms, the industry-wide agreement only acts as a wage floor (Bellmann 1995), allowing us to perform a more detailed analysis in Section 2.6.

The duration of collective agreements on wages and earnings is usually one year. However, in 1988 and 1989, a significant part of the collective agreements signed had minimum durations of up to three years. This was apparently a one-time phenomenon linked to the ongoing negotiations over hours reductions, and most of the three-year contracts expiring were followed by the usual one-year contracts.

A feature that Germany shares with other European economies is the severely restricted use of fixed-length contracts. Over most of the sample period, German law restricted fixed-length contracts to 6 months. Though the law allows for renewal, utilization seems to be quite low.[8] Only about 4.5 percent of workers declaring themselves as working full-time are on fixed length contracts, compared to 6.9 percent of part-time workers. Women are slightly more likely to be on fixed length contracts then men (7.2 and 4.5 percent respectively).

Finally, the relevant compensation variable we consider are earnings. Contrary to North America, where blue-collar workers tend to receive hourly compensation, German blue-collar workers tend to be compensated like white-collar workers, on a monthly or bi-weekly basis. Thus, 11 percent of workers (in Western Germany) are covered by contracts which do not differentiate between blue and white collar workers, and in which both categories are paid a monthly salary invariant in hours. For a further 40

[8]The degree of utilization in our sample is actually decreasing over the sample period, though this may be a result of the non-random sampling nature of the GSOEP. Hunt (1995) describes the changes and estimates the effect on employment. For the role of fixed-length contracts in France, see Abowd, Corbel & Kramarz (1996).

percent of blue collar workers, the collective agreement, though separate from that for white collar workers, specifies a fixed monthly salary (Bundesministerium für Arbeit und Sozialordnung 1994).

2.4 Data and estimation

The data used comes from the German Socio-Economic Panel (GSOEP). We will briefly describe some aspects this dataset that are of importance to the present study. Wagner, Burkhauser & Behringer (1993) and Burkhauser (1991) provide a more detailed description of the public use file available outside Germany.

The GSOEP is a longitudinal panel data set first created in 1984. Respondents are reinterviewed each year. Response rates are quite high. Children are followed separately once they leave the original household, providing for some non-random compensation for panel attrition. The questions asked are not restricted to economic questions, ranging from social to political subjects. Through the structure has varied from year to year, a great deal of homogeneity has been preserved, facilitating comparison over the years. A new, East German panel was started after German unification in 1990.

The survey instrument for the GSOEP was modeled after the PSID, and tries to avoid some of the problems the latter dataset had. Thus, the question on job tenure is fairly unambiguous,[9] asking respondents the month and year they started working for their current employer.[10] Some problems nevertheless occur. For example, in some waves, a number of questions relating to the job market and the current job were only asked of job changers. If this occurs, or data is missing, we carry forward information

[9]See Altonji & Shakotko (1987) and Topel (1991) for a treatment of the problems with the tenure data in the PSID.

[10]This author has worked with both the English translation and the original German questionnaires, and has found that in some waves, the English translation of the tenure question renders ambiguous what is not in German. More information is available from the author upon demand.

obtained in the previous wave conditional on the worker reporting no change in his job situation w.r.t. the previous year. Furthermore, if an individual reports conflicting data on the year she started working with the present employer, we use and carry forth the earliest report of a date. In this manner, we force tenure to be consistent across year.

Throughout, results are reported for net and gross real earnings. We would expect net earnings to be the variable of interest to workers, and thus the pertinent variable in union bargaining, though gross earnings are closer to the true cost of labor to the employer, and thus more appropriate in models imposing a zero profit condition. Hence, it is not clear which to use, and we avoid having to choose by using both variables.

We restrict our sample to blue and white collar workers with contracts of indeterminate length who are working full-time, and are German nationals living in Western Germany. We exclude workers with fixed length contracts at this stage due to ambiguity inherent to such contracts in the context of the theoretical models.[11] Due to the unavailability of data on contract duration in 1984, our sample is restricted to the years 1985 to 1994, and due to unavailability of regional unemployment rates prior to 1972, only workers having started their jobs since are included. As a glance at Figure 2 .1 on page 88 shows, though there is still considerable variation in the unemployment rates, there is a significant upward trend over this period. In Section 2.6, this will subject of discussion.[12] Excluding workers in agriculture and in the public sector as well as civil servants scattered in other industries leaves us with 10 349 observations on 2 459 individuals.[13] Of these, 10 268 have valid observations on net income. Finally, we eliminate individuals who have only one observation in the sample, since at least two observa-

[11] Separation for these contracts is exogenous, except if the contract is transformed into one of indeterminate length. It is unclear whether renegotiation will occur during the short duration of the contract. Furthermore, our data does not allow us to determine whether a fixed length contract in two consecutive years is with the same employer, and the tenure question may be ambiguous in these cases. Most previous studies seem to not have excluded these workers. Results obtained here when including them are not drastically different.

[12] An earlier version of this paper used only national unemployment data reaching back to 1958.

[13] About half of the eliminations for missing variables are due to missing initial experience.

tions are necessary to be able to eliminate individual-specific effects. Table 2.I gives a summary of the reductions made. Sample statistics are given in Table 2.II.

Contemporaneous unemployment rates are merged into the GSOEP using the month preceding the interview month, for which earnings are reported. Initial unemployment rate is taken from the quarter the current job is reported to have started if available. Otherwise, the average unemployment rate over the year in which the job started is used. Minimum unemployment is computed by searching between the starting date of the employment relation and the current date. For part of the analysis, regional unemployment rates were used, either in levels, in deviations from the national average, or in proportional deviation from the national average.[14] A more complete description of the data is given in a separate appendix.

2.5 Results

The main results of this paper are reported in Table 2.III on page 78. To estimate Equation (2.8), we have controlled for experience and tenure up to squared terms, education as measured in years, dummies for industry, marital status,[15] a linear trend.[16] Errors reported here are not corrected for heteroskedasticity, but results using the White

[14] See Figure 2 .1 on page 88.

[15] We use an indicator for the status of being single. Other dummy variables led to comparable results.

[16] Other specifications were tested, especially up to cubic terms in experience and tenure. Nested F-tests cannot reject a quadratic against the null of a cubic specification at the 5 percent level, but can reject a linear against a cubic specification. However, the linear specification cannot be rejected against the null of a quadratic specification at 1 percent level. It is thus not clear, based upon these tests, which specification to choose.

(1980)-correction yielded very similar results. Fixed effects were flushed out by using deviations from individual specific means.[17, 18]

Rows 1 to 3 show results obtained when including only measures of past labor market tightness. Note that unemployment is measured in percent, and thus the reported coefficient implies that an increase in the unemployment rate of one point reduces net monthly earnings by 1.1 percent (in specification (3)). When including each measure individually, the estimated coefficients have the predicted signs, but the effect of minimum unemployment is not very precisely estimated. Including both reveals a positive and significant effect of the lowest unemployment rate. In Rows 4 to 7, the current unemployment is included in various combinations with the two previous measures. Row 4 corresponds to the typical "wage curve" regression (Blanchflower & Oswald 1994). The estimated coefficient on current unemployment is stable across all specifications, and precisely estimated, suggesting that it is orthogonal to the other two measures.[19] When again including only one or the other measure of past labor market performance, the same scenario as before is produced.

The positive sign of the lowest unemployment rate since the start of a job is surprising, and does not fit any one of the models of Section 2.2. One possible explanation may be a selection model. Generally, higher unemployment is correlated with a higher separation rate. Employers have less liberty of laying off individual workers than in the United States, and it is in fact easiest to lay off a worker for "economic reasons".

[17] We tested fixed effects against random effects in a variety of specifications. A Hausman (1978) test always rejected the random effects specification by a large margin. For instance, for the regression reported in Table 2.V on page 79, Column (b), $\chi^2 = 242.13$ with 38 degrees of freedom, thus soundly rejecting the hypothesis that random effects are the correct specification.

[18] Note that we are not including regional effects. Geographical mobility is very low in our data - only 60 moves are observed -, and any regional effects are flushed out by the individual fixed effects.

[19] A well-know problem in the data as used here is aggregation bias. The results presented in the main text are not corrected for aggregation bias, but tests in a number of specifications showed that the results are not sensitive to a correction. For reasons of comparability with results presented in the literature, we present micro-level regression results throughout. Appendix 2.7 on page 75 gives more details.

If employers at that time select to keep only higher productivity workers, and if earnings reflect productivity, than those having experienced periods of high unemployment and are still unemployed will tend to be in more productive matches,[20] leading to the observed sign.

Row 7 captures the main result of this paper. There is a strong effect on current earnings through current unemployment. However, previous conditions in the labor market are also significantly correlated with current earnings. The elasticities corresponding to the point estimates in Row 7 are about 9.3 percent for contemporaneous unemployment and about 11 percent for initial unemployment. Neither a model that correlates only the contemporaneous unemployment rate with earnings, such as the simpler efficiency wage models as well as rent-sharing and spot market models, nor the implicit contract models are sufficient by themselves to explain the dynamics of earnings.

However, the fact that there is a strong correlation between initial and lowest unemployment in our sample may cast some doubt on the result that it necessarily be the initial unemployment rate that influences unemployment. The average time elapsed between start of the job and occurrence of the lowest unemployment rate in the sample is 15 months (27 months conditional on being strictly positive), and for only 40 percent of the observations, this value is larger than 12 months. Thus, it is possible that we cannot distinguish the two effects. Section 2.6 will pursue this point further.

Table 2.IV on page 79 reports equivalent results using gross instead of net earnings as dependent variable. Coefficients are generally of smaller magnitude, possibly due to the progressive German income tax schedule.

Turning our attention to Table 2.V on page 79, we report separate results for men. Our sample is disproportionately composed of men, furnishing 67 percent of sample

[20]Note that this must be match-specific effect, since individual effects have been flushed out. See f.i. Gibbons & Katz (1991).

observations and 61 percent of the sample population. Tenure for men is longer, the distribution across industries is different, and men are more likely to be blue-collar workers. Furthermore, since the participation decision is not modeled here, it is a standard result that coefficients may be biased.[21] Part of the results in Tables 2.III and 2.IV seems to be driven by the female part of the sample. Male earnings are more strongly correlated with current unemployment, whereas measures of past labor market performance are smaller in absolute values. In what follows, we concentrate only on the male subsample.[22] However, all previous patterns apparent in different specifications, in particular the positive sign on minimum unemployment, carry over to the analysis of the male sample.

Row (BD) at the bottom of Table 2.III reports the results obtained by (Beaudry & DiNardo 1991) for the United States. In PSID data, the effect of minimum unemployment rate clearly dominates the effect of contemporaneous and of initial unemployment rate, the latter not being significantly different from zero. The German case is more nuanced, lending support to a mixture or simultaneous presence of two mechanisms. The first affects current earnings through the current state of the labor market. A number of models can be consistent with this result, as we have expounded in Section 2.2. However, the institutional background outlined in Section 2.3 would lend credence to a rejection of a simple spot market model in favor of a model of rent-sharing. The negative effect of unemployment can then be interpreted as evidence of risk-averse unions bargaining over both employment and wages, if bargaining powers are heterogeneous across industries. The resulting bargaining outcome then feeds imperfectly into individual contracts, still

[21] Heckman (1976), Mroz (1987). Groot, Mekkelholt & Osterbeek (1992) show in the case of the Netherlands that estimates of the effect of contemporaneous unemployment for women may be severely biased if no self-selection correction is done, and that current unemployment affects not only the wage but also the participation decision. See also Strøm & Wagenhals (1991) on female labor supply in Germany.

[22] In regressions not reported here, female earnings are strongly correlated with the initial unemployment rate. It might be interesting to study the effect of local labor market conditions on long-term labor participation rates of women.

allowing for aspects of implicit contracts to have effect. It may be seen as corroborating evidence that unions have in recent years put a stronger emphasis on reduction of hours in order to maintain or increase employment. Our results are consistent with this.

In the next section we study the robustness of the above results before drawing a final conclusion.

2.6 Robustness of results

In this section, we consider different subsamples in order to test the robustness of the results obtained in the previous section. Since labor markets may function differently for individuals characterized by the size of the firm or the industry they work in, or by particular characteristics of their labor market history, not only may this characteristic influence his level of earnings, but may in fact alter the compensation structure. Furthermore, the results may be sensitive to the particular time period considered, picking up some time-specific artifact rather than a general phenomenon.

Firm size

A number of models have shown that the labor market may be segmented into tiers of jobs that function as a ladder, for a number of reasons.[23] Conceivably, progression up the ladder is associated with increasing firm size. On the other hand, these jobs may be within one large firm, and constitute an internal hierarchy of jobs. Furthermore, some studies have shown that firm size affects earnings and wages not only through worker quality - which we capture with fixed effects - but through firm-worker matches (Abowd, Kramarz & Margolis forthcoming). If firms are homogeneous within size categories, this will again be reflected in differences in the remuneration structure.

[23] A recent example is Jovanovic & Nyarko (1997), and see Soskice (1994) for such an explanation for the German market for apprentices.

It could be argued that firm size is a bad instrument for job ladders, as collective bargaining agreements cover all companies within an industry, irrespective of their size, as outlined in Section 2.3. However, variations do exist, and as we will see, are important.

Results are reported in Table 2.VII on page 80.[24] A dichotomy appears between very large firms (more than two thousand employees) and smaller ones. Whereas contemporaneous labor market tightness has no significant effect on wages in the former, smaller firms are remarkably homogeneous as to the effect of contemporaneous unemployment. Note that the coefficient decreases as firm size increases, reduced to insignificance for very large firms. Coefficients on previous labor market performance exhibit the same pattern as before, but the magnitude is much larger for very small firms. In large firms, net earnings are unaffected by any labor market conditions, but as Table 2.VIII on page 81 reports, there is a marginally significant effect in gross earning.

Thus, both with respect to variation in ongoing contracts as well as for initial conditions, smaller firms seem to be a lot more sensitive to market conditions than very large firms. It is worth noting that in results not reported here,[25] the return to initial experience is stronger relative to firm-specific experience for tiny firms. This fits well with the fact that firms with less than 10 employees are not subject to the fairly stringent German layoff regulations,[26] allowing them to participate more actively in search activities. Workers' tenure at these also is lower than elsewhere, also suggesting that job security is less than perfect, and that adjustment not only occurs on the wage margin.

At the other extreme, and markedly different from the intermediate categories, lie the very large companies. Initial experience finds no remuneration, whereas firm-specific experience is more highly remunerated. Again, one might find this consistent with the

[24] Firm size is reported in five categories. Table 2.VI on page 80 reports frequencies and the average tenure of jobs in each type of firm.

[25] Results available from the author upon request.

[26] See f.i. Hunt (1995) on the effects of layoff costs on employment in Germany.

view that large firms have a more strongly hierarchical structure, and provide for stable internal career paths. This finds support in the observation that average tenure is increasing and initial experience decreasing in firm size in Table 2.VI.

Blue vs. white collar

As pointed out earlier, blue-collar pay in Germany is much more similar to white-collar remuneration than in North America. However, since we cannot distinguish between hourly pay and monthly remuneration, the distinction according to status serves as a proxy. Alternatively, the method of remuneration may not be the only aspect affecting remuneration dynamics.

Accepting status as a proxy for remuneration methods, it is still not clear whether blue-collar pay should vary more or less with labor-market conditions. In an implicit contract model, earnings are consumption, and should, if perfectly insured, not vary with contemporaneous unemployment. However, if firms can adjust hours as well as pay, then wage rates may well change. The labor market institutions outlined in Section 2.3 seem to imply that for most white-collar workers, remuneration does not vary with hours, and this applies to a significant portion of blue-collar workers as well. However, given the extant discrepancies, we would expect more variance in blue-collar wage rates, rather than earnings, when compared to white-collar wage rates.

Table 2.IX on page 81 reports results on separate regressions for blue and white-collar workers. The results are qualitatively similar to those previously obtained. However, the effect of the contemporaneous unemployment rate is weaker for white collar workers than for blue collar workers, and the coefficients on previous labor market performance are larger (in absolute value) for white collar workers, though not significant for either worker type.[27] For both worker types, the impact of the implicit contract type measures

[27] In other regressions, results were not different when using wage rates instead of earnings.

is weakened, but the results suggest nevertheless that white collar earnings depend to a larger degree on labor market conditions at the start of the present employment, and to a lesser degree on contemporaneous fluctuations.

Regional and temporal variation

Nearly 90 percent of reported earnings refer to the first quarter of the survey year, and thus there is very little intra-year variation in the unemployment rates. Though a linear time trend is included in all regressions, capturing the upward trend over the time period of earnings (see Figure 2 .2 on page 89), the inclusion of year dummies mimics the path of the unemployment rate. In order to separate pure year effects from effects related to labor market conditions, some other dimension of variation is necessary.

In our case, we used two approaches to take this possibility into account. First, we use data on regional unemployment rates at the "Bundesland" (state) level instead of the national averages as the relevant unemployment measures. Results are reported in Table 2.X on page 82, Column (b), whereas Column (a) takes up the results from Table 2.V on page 79. The results are qualitatively the same as for the national measures, but the magnitude of the coefficient on contemporaneous unemployment is reduced, whereas those on contractual measures are increased. Thus, the addition of intra-year variation does not alter the general conclusion.

A second approach splits the sample into subperiods. A glance at the upper left panel of Figure 2 .1 on page 88 shows that although a substantial increase in unemployment followed the oil crisis of the early 70s, mean unemployment in the 70s lies below mean unemployment in the 80s. If in fact the coefficients on unemployment do not reflect pure year effects, than the estimates should not be (overly) sensitive to the period chosen. Column (c) in Table 2.X presents results for a sample of jobs starting after 1983 only, using regional unemployment rates. Note that for this subsample, average initial

unemployment will be higher, but there is still substantial variation in the unemployment rate over the period to identify any effect of contemporaneous unemployment on changes in earnings. Again, though point estimates are higher, the qualitative results do not change.

This approach can also be viewed from a different angle. Given the upward trend in unemployment in the data, a substantial percentage of persons in our sample will have experienced their best labor market conditions at the start of the job: their minimum unemployment rate is equal to their initial unemployment rate. In the sample used for Columns (a) and (b), this is the case for around 40 percent of all observations. If this is in fact driving our results, then curtailing the sample to those individuals in jobs that started within a period in which the unemployment rate does not have a (linear) trend, and thus this coincidence occurs less frequently, should yield very different results. For the sample in Column (c), the coincidence of initial and minimum occurs only for 24 percent of observations. The results can be taken as evidence against that hypothesis, since they remain qualitatively the same. This is also in line with the results presented in Tables 2.III on page 78 and 2.IV on page 79, when one or the other of the contractual measures is included.

Up until this point, a job contributed more than one observation to the analysis. All the models predictions collapse to the same one, that of a negative correlation between wages and alternative utilities, when we consider only starting wages, since this first observation is necessarily also current and minimal.[28]

This may serve as a check on our previous estimates. Column (d) reports results from a regression of starting wages on unemployment at the time of the contract start. The coefficient is negative and signficant. Furthermore, taking Column (c) as the comparison

[28]Since earnings are observed only once yearly in the GSOEP, this is not exactly true, but close enough.

group, the coefficient is on the same order of magnitude as the sum of all three coefficients in the more general specification.

Columns (e) and (f) split the sample again into blue and white collar workers, and the results are clearer than in Table 2.IX. For blue collar workers, the predominant influence is contemporaneous, whereas white collar workers are strongly affected by initial conditions, though some contemporaneous variation persists. It thus seems that the implicit contract model is a better description of the earnings evolution of white collar workers, whereas some contemporaneous model better explains the variation in the earnings of blue collar workers.

We had argued in Section 2.3 that there is substantial coordination or spill-over between regional bargaining units in Germany. If bargaining is what determines the contemporaneous correlation between earnings and unemployment, than one would expect that regional variations should not matter. On the other hand, regional mobility of Germans is low [ref here], and so local labor market may matter for *individual* decisions, such as are modeled in the implicit contracting models as well as in efficiency or spot market models. Thus, if efficiency wages determine contemporaneous correlation, one would expect regional variations to affect earnings. A similar argument goes for initial and lowest unemployment rate in the implicit contract models, where, if geographical mobility influences economic mobility, the relevant outside options are local.

Table 2.XI on page 83 reports results for several different specifications. Columns (a) and (b) are identical to the same columns in Table 2.X on page 82, where unemployment is measured in levels. In column (c), the appropriate regional measures are computed in deviations from national means. The only coefficient now significantly affecting earnings is the measure of initial unemployment. For workers starting jobs in regions with lower than average unemployment, wages stay lower for the rest of their careers, consistent with the implicit contract model. No contemporaneous variation at the regional level

affects their earnings. However, when we reintroduce the level of national unemployment measured at the same time in column (d), the level of contemporaneous unemployment affects earnings in much the same way as in the "original" model in column (a). Notice also that the point estimate on minimum unemployment is still about equal to the one in column (a), but is less precisely estimated.

The results in column (d) point to the simultaneous presence of and complicated interplay of several levels of wage and earnings determination. What it suggest is that at the micro level, employers give immobile workers insurance contracts against fluctuations in their idiosyncratic productivity. This wage guarantee is dampened by aggregate fluctuations in productivity (or whatever shock may translate into increased unemployment), which will affect anybody's earnings. This is consistent with circumstantial evidence on union influence in Germany. Note that what is often called profit sharing is consistent with a union model where bargaining occurs over wages and employment levels (efficient contracting), and this seems to be a plausible scenario for Germany.

2.7 Concluding Remarks

The main result of this paper is twofold. First, we have shown that earnings dynamics in Germany are influenced by both previous and current labor market conditions. This contrasts with findings for the American labor market indicating the preponderance of previous labor market conditions, and it puts a caveat to the analysis in the wage curve literature, where wages are only correlated to current unemployment. No single model is able to entirely explain microeconomic movements of earnings in Germany.

But, and this is the second result, a caveat applies. The above result is not universally valid in all parts of the labor market. A blue collar worker in a small firm will be much more affected by contemporaneous conditions than, say, a white collar worker in a large

firm, controlling for other aspects of productivity. Whereas the former's earnings move in a way consistent with spot market models, the latter's earnings behave if anything according to an implicit contract model.

Furthermore, and quite importantly, local labor markets matter for individual contracts, for which an implicit contract model or a contracting model seems consistent with the data, but national unemployment covaries with individual earnings, as might be expected in a union bargaining context if unions bargain over earnings and employment. Thus, different labor markets seem to vary substantially as to the sensitivity of earnings to labor market conditions.

We find that the elasticity of current earnings with respect to contemporaneous unemployment varies between 9.3 percent (full sample) and 15.6 percent (for men only, using regional and national unemployment rates), which is comparable in magnitude to coefficients found in previous studies.[29] Furthermore, we find the elasticity of current earnings with respect to initial unemployment to be between 6 percent (men only) and 10 percent (full sample), again depending on the specifications. White collar worker earnings are more sensitive to initial unemployment rates than blue collar earnings, and vice-versa with respect to contemporaneous correlation.

The positive sign obtained on the coefficient on the best labor market conditions since start of the job is puzzling in the present context, but disappears once both national and regional variations in unemployment rates are included in the model.

Comparing with previous results for the U.S. labor market (Beaudry & DiNardo 1991), we find that the elasticity of earnings with respect to the initial labor market conditions since the start of the current job are similar in both economies, if slightly higher in the United States. Elasticity with respect to current unemployment is higher

[29] Blanchflower & Oswald (1994), Wagner (1994).

in Germany, the U.S. value of approximately 4 percent being about a third to half of the corresponding German one. From this, it is fairly difficult to draw conclusions as to which labor market shows the "higher" flexibility. However, a tentative conclusion is that earnings in Germany seem to show no less flexibility with respect to labor market conditions than U.S. earnings.

Finally, though most of the above discussion is couched in the vocabulary of implicit contracts, it is important to point out that other models may well be consistent with the above findings. We have pointed out several in Section 2.2. Our findings as to the size of the firm seem to show that contracts in smaller firms are sensitive to market than those at very large firms. One possible interpretation is that small firms are too small to support internal labor markets, and thus substitute the marketplace for it. Large firms, on the other hand, offer a more stable environment in which internal labor market and hierarchical incentive systems may function. Support is also to be found in the observation that average tenure in our sample is higher for large firms, implying lower turnover.

Given the particular institutional structure of the German labor market, we hypothesize that some model superimposing collective bargaining agreements and individual contract models may be able to explain our results. We do not supply such a model, but establish stylized facts which such a model must be able to explain.

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Appendix

	OBSERVATIONS	PERSONS
Full GSOEP dataset used	107 252	18 185
Missing data	45 371	9 129
Starting year > 1972	33 197	7 172
Germans living in W Germany	23 035	4 896
Employed	21 386	4 587
Only FT working persons	17 288	3 834
Only unlimited contracts	12 621	2 833
Restricting to white/blue collar	11 399	2 643
Excluding agriculture, public sector	10 349	2 459
of which with:		
Valid gross income	10 100	2 432
and > 1 observation	9 537	1 869
Valid net income	10 268	2 452
and > 1 observation	9 698	1 882
of which:		
Men	6 524	1 160

Table 2.I: Sample reduction

Group effects

As initially pointed out by Moulton (1986), the standard errors reported by OLS for variables which are constant within groups are biased downwards. Their bias depends on the average group size and the intraclass correlation of the errors. Coefficient estimates themselves are unbiased. The aggregate variables in our case are the unemployment rates. Note however that whereas the coefficient on contemporaneous unemployment is estimated off fairly large cells, the minimum and initial unemployment rates vary widely across individuals, and cells are thus more numerous and smaller, diminishing the bias in the standard errors. For the period under consideration here (1972-1994), there are 22 years * 4 quarters per year * 10 regions = 880 possible cells for initial unemployment, with approximately 10 000 observations. On the other hand, since most interviews are completed within a three-month period, all contemporaneous unemployment observations come from within a single quarter per year, leaving 10 years x 10 regions =100 cells. The case for minimum unemployment will lie between these two extremes.

All regressions in the main test control for fixed effects. Given the low geographical mobility in the data (only 60 moves are observed), the fixed effects absorb any regional effects. The initial unemployment rate entering the typical regression is thus the deviation of the initial unemployment rate at the current job from the average initial unemployment rate for the individual. This measure thus depends on the entire employment history of an individual, further reducing the likelihood of aggregation bias. The same applies to minimum unemployment. On the other hand, contemporaneous unemployment is still subject to aggregation bias in a fixed effect specification, since all individuals in the panel will be subject to the same average over the observation period, and thus also to the same deviation from this average.

One possible correction for aggregation bias is to adjust the degree of aggregation on both sides of the regression equation. We have therefore regressed the dependent variable on the individual regressors (including minimum and initial unemployment rate), industry dummies, and a full set of 100 year-by-region dummy variables. The coefficient estimates on the year-region dummies are thus regression-adjusted regional means.

Note that this specification is also more general in its treatment of the time effects, which in the results reported in the main text are constrained to be linear.

An auxiliary regression of these coefficients on year dummies, region dummies, and on regional unemployment rates was then run. The coefficient on unemployment from this regression is not subject to the aggregation bias described above.

Column (c) in Table 2.XII on page 84 reports the relevant results from the two regressions. Column (a) of that table repeats the parameter estimates from Column (b) of Table 2.V on page 79, for the male sub-sample. Column (b) reports results when not controlling for fixed effects in the same sample. Coefficients are higher, indicating that part of the observed effects are actually due to individual heterogeneity, but the general pattern is fairly similar.

In all columns, the first two rows report parameter estimates from the micro-level regressions. The third row, the parameter estimate for the effect of contemporaneous unemployment, reports the estimate from the auxiliary regression in Column (c), but from the micro-level regression in Columns (a) and (b). The parameter estimates when correcting for aggregation bias lie between the fixed effect and cross-sectional estimates. The relative magnitudes of the estimates are similar in Columns (a) and (c), pointing to the fact that regional fixed effects may account for some of the variation otherwise captured by individual fixed effects. All parameter estimates are significantly different from zero irrespective of the specification chosen.

Correcting for aggregation bias and specifiying a more general treatment of time effects do not change the general results in this subsample. For reasons of parsimony and comparability, we therefore only report results from micro-level regressions throughout the main text.

VARIABLE	FULL SAMPLE		MEN	
Mininum national unemployment rate	5.61	(2.31)	5.50	(2.35)
Maximum national unemployment rate	9.02	(0.55)	9.04	(0.54)
Nat. unemployment rate, start of tenure	6.26	(2.61)	6.15	(2.66)
Nat. unemployment rate, time of interview	8.09	(1.01)	8.07	(1.01)
Mininum regional unemployment rate	5.50	(2.87)	5.42	(2.85)
Maximum regional unemployment rate	9.06	(2.40)	9.09	(2.38)
Reg. unemployment rate, start of tenure	6.18	(3.09)	6.09	(3.14)
Reg. unemployment rate, time of interview	7.99	(2.46)	7.99	(2.48)
Contractual hours	39.12	(2.84)	39.29	(2.73)
Actual weekly hours	42.61	(8.23)	43.72	(8.74)
Desired hours per week	37.35	(7.16)	38.93	(6.63)
Net Income in 1994 DM, 1000s	2.78	(1.20)	3.12	(1.24)
Gross Income in 1994 DM, 1000s	4.18	(1.76)	4.62	(1.83)
Blue-collar	0.45		0.55	
Years of education	11.44	(2.19)	11.55	(2.30)
Age	35.80	(10.34)	36.76	(10.02)
Single dummy (1/0)	0.38		0.31	
Female	0.33		1.0	
On-the-job Tenure (months)	79.55	(59.72)	83.83	(60.95)
Initial experience	11.54	(9.62)	12.07	(9.44)
Number of obs.	10268		6524	

Standard deviations in parenthesis.

Table 2.II: Means

NET EARNINGS

		Unemployment Rate at Start of Tenure (a)	Contemporaneous Unemployment Rate (b)	Minimum Rate over Tenure (c)
	Means	6.26	8.09	5.61
(1)	Fixed Effects	-0.0055** (0.0023)	–.–	–.–
(2)	Fixed Effects	–.–	–.–	-0.0010 (0.0022)
(3)	Fixed Effects	-0.0110** (0.0035)	–.–	0.0071* (0.0034)
(4)	Fixed Effects	–.–	-0.0115** (0.0020)	–.–
(5)	Fixed Effects	-0.0054* (0.0023)	-0.0115** (0.0020)	–.–
(6)	Fixed Effects	–.–	-0.0120** (0.0021)	0.0020 (0.0023)
(7)	Fixed Effects	-0.0175** (0.0037)	-0.0115** (0.0022)	0.0156** (0.0036)
(BD)	Fixed effects	-0.006 (0.007)	-0.007 (0.0025)	-0.029 (0.008)

Significance at ** 1% level and * 5% level. Standard errors in parentheses. All regressions include experience and tenure up to squared terms, education in years, hours in logs, dummies for industry, marital status (single), and a time trend. Row (BD) is taken from Table 2, row 10 in Beaudry & DiNardo (1991).

Table 2.III: Net earnings specifications

GROSS EARNINGS

		Unemployment Rate at Start of Tenure (a)	Contemporaneous Unemployment Rate (b)	Minimum Rate over Tenure (c)
	Means	6.26	8.09	5.61
(1)	Fixed Effects	-0.0068** (0.0024)	–.–	–.–
(2)	Fixed Effects	–.–	–.–	-0.0029 (0.0023)
(3)	Fixed Effects	-0.0107** (0.0036)	–.–	0.0050 (0.0035)
(4)	Fixed Effects	–.–	-0.0081** (0.0021)	–.–
(5)	Fixed Effects	-0.0067** (0.0024)	-0.0080** (0.0021)	–.–
(6)	Fixed Effects	–.–	-0.0079** (0.0022)	0.0009 (0.0024)
(7)	Fixed Effects	-0.0152** (0.0038)	-0.0102** (0.0022)	0.0101** (0.0037)

Significance at ** 1% level. Standard errors in parentheses. For regression details see footnote to Table 2.III.

Table 2.IV: Gross earnings specifications

	NET EARNINGS		GROSS EARNINGS	
UNEMPLOYMENT	ALL (a)	MEN (b)	ALL (c)	MEN (d)
First	-0.0175** (0.0037)	-0.0101* (0.0045)	-0.0152** (0.0038)	-0.0099* (0.0046)
Current	-0.0115** (0.0022)	-0.0169** (0.0026)	-0.0102** (0.0022)	-0.0126** (0.0027)
Minimum	0.0156** (0.0036)	0.0107* (0.0044)	0.0101** (0.0037)	0.0064 (0.0045)

Significance at ** 1% level, * 5% level. Standard errors in parentheses. For regression details see footnote to Table 2.III.

Table 2.V: Full sample and men

FIRMSIZE	Freq	Mean tenure	Mean initial exp
under 5	760	9.77	9.88
	12.36 %	(7.89)	(10.52)
5 to 20	1477	10.90	11.61
	24.03 %	(7.16)	(10.57)
20 to 200	1519	12.43	11.58
	24.71 %	(7.78)	(9.75)
200 to 2000	1595	13.92	9.08
	25.95 %	(8.04)	(9.05)
2000 and more	796	15.34	8.11
	12.95 %	(8.20)	(8.45)

Standard errors in parentheses. Men only.

Table 2.VI: Tenure and initial experience by firmsize

	FIRMSIZE				
	< 5	5 to 20	20 to 200	200 to 2000	> 2000
Initial	-0.0773**	-0.0462**	-0.0233*	-0.0308**	-0.0287
	(0.0157)	(0.0108)	(0.0108)	(0.0114)	(0.0207)
Current	-0.0248**	-0.0231**	-0.0160**	-0.0089*	0.0073
	(0.0093)	(0.0054)	(0.0044)	(0.0041)	(0.0054)
Minimum	0.0523**	0.0500**	0.0203*	0.0288*	0.0212
	(0.0150)	(0.0103)	(0.0102)	(0.0112)	(0.0173)

Significance at ** 1% level and * 5% level. Standard errors in parentheses. For regression details see footnote to Table 2.III.

Table 2.VII: Results by firm size, net earnings

	Firmsize				
	< 5	5 to 20	20 to 200	200 to 2000	> 2000
Initial	-0.0771**	-0.0443**	-0.0202+	-0.0222+	-0.0386+
	(0.0158)	(0.0108)	(0.0108)	(0.0115)	(0.0202)
Current	-0.0305**	-0.0238**	-0.0167**	-0.0110**	0.0059
	(0.0091)	(0.0054)	(0.0044)	(0.0041)	(0.0054)
Minimum	0.0521**	0.0488**	0.0193+	0.0249*	0.0253
	(0.0151)	(0.0103)	(0.0102)	(0.0114)	(0.0172)

Significance at ** 1% level, * 5% level and + at 10% level. Standard errors in parentheses. For regression details see footnote to Table 2.III.

Table 2.VIII: Results by firm size, gross earnings

	Net earnings		Gross earnings	
	Blue collar (a)	White collar (b)	Blue collar (c)	White collar (d)
Initial	-0.0058	-0.0103	-0.0046	-0.0107
	(0.0060)	(0.0067)	(0.0063)	(0.0066)
Current	-0.0215**	-0.0130**	-0.0183**	-0.0082**
	(0.0037)	(0.0037)	(0.0038)	(0.0036)
Minimum	0.0097	0.0073	0.0062	0.0024
	(0.0059)	(0.0064)	(0.0062)	(0.0064)
Observations	3575	2949	3497	2949

Significance at ** 1% level. Standard errors in parentheses. For regression details see footnote to Table 2.III.

Table 2.IX: Results by worker class

	NET EARNINGS, MEN					
	National	Regional				
		Job start		Starting wage	Blue collar	White collar
	≥ 1972 (a)	≥ 1972 (b)	≥ 1983 (c)	(d)	(e)	(f)
Initial	-0.0101*	-0.0126**	-0.0206*	n.a.	-0.0046	-0.0185**
	(0.0045)	(0.0040)	(0.0099)		(0.0055)	(0.0060)
Current	-0.0169**	-0.0108**	-0.0275**	-0.0209**	-0.0151**	-0.0072*
	(0.0026)	(0.0021)	(0.0048)	(0.0048)	(0.0030)	(0.0030)
Minimum	0.0107*	0.0110**	0.0312**	n.a.	0.0044	0.0144*
	(0.0044)	(0.0041)	(0.0091)		(0.0057)	(0.0060)
Observations	6524	6524	2847	712	3575	2949

Significance at ** 1% level and * 5% level. Standard errors in parentheses. For regression details see footnote to Table 2.III.

Table 2.X: Regional unemployment rates

	NET EARNINGS, MEN			
	Levels		Detrended	Detrend. reg. + nat. level
	(a)	(b)	(c)	(d)
Initial unemployment				
- Regional		-0.0126**	-0.0200**	-0.0146+
		(0.0040)	(0.0072)	(0.0076)
- National	-0.0101*			0.0044
	(0.0045)			(0.0084)
Current unemployment				
- Regional		-0.0108**	0.0003	0.0025
		(0.0021)	(0.0041)	(0.0039)
- National	-0.0169**			-0.0193**
	(0.0026)			(0.0047)
Minimum unemployment				
- Regional		0.0110**	0.0092	0.0003
		(0.0041)	(0.0090)	(0.0080)
- National	0.0107*			0.0103
	(0.0044)			(0.0085)

Significance at ** 1% level, * 5% level and + at 10% level. Standard errors in parentheses. For regression details see footnote to Table 2.III.

Table 2.XI: Regional and national effects

	Net earnings		
Unemployment	(a)	(b)	(c)
First	-0.0101*	-0.0235**	-0.0182**
	(0.0045)	(0.0050)	(0.0048)
Minimum	0.0107*	0.0232**	0.0158**
	(0.0044)	(0.0051)	(0.0059)
Current	-0.0169**	-0.0200**	-0.0226**
	(0.0026)	(0.0061)	(0.0047)
Corrected for Aggregation bias	No	No	Yes
Controlling for Fixed effects	Yes	No	No

Men only. Significance at ** 1% level, * 5% level. Standard errors in parentheses. For regression details see text.

Table 2.XII: Correcting for aggregation bias.

Data description

HOURS. Hours are reported as contractual hours (HRS_WK_C) and actual hours worked (HRS_WK) in the week preceding the interview. If available, actual hours are used, otherwise contractual.

EARNINGS. The earnings reported is the monthly amount received in the previous month (INCM_NET and INCM_GRS). Information is also reported on average monthly earnings in the previous year as computed by respondents, which we may use this in a later step to control for representativeness of the answer to INCM.

We computed average hourly earnings including overtime pay, where we used the reported actual hours in the week (HRS_WK) prior to the interview if overtime is paid (OVER_RUL), and contractual hours (HRS_WK_C) otherwise, multiplying it by the number of weeks in a month (4.5). Since wage contracts in Germany usually specify monthly earnings (for white-collar workers) or at least a monthly income w.r.t. hours, we use in our regressions a specification with monthly earnings. Unfortunately, though actual overtime last month is reported, overtime pay is not (except for 1986).

FIRM SIZE. is employee-reported, in 5 categories, referring to the firm, not the establishment.

YEARS OF EDUCATION. is calculated by the data providers from information given in the interview, translating into years of education. Information is also available on highest degree obtained.

FOREIGNER. The usual control for race in regressions on North American data does not make sense in Germany. We do have information about the immigration status (i.e. if the person is a foreigner or not) and residence according to the old political divisiono of the country (i.e. if the person resides in East or West Germany), SAMPLE. In all regressions, we exclude both foreigners and East Germans.

UNION STATUS. is available only for respondents present in the 1985, 1989 and 1993 waves. We construct the union status for years in between for those respondents who have not changed jobs as far back as this can be followed, but we run regressions without this constructed variable, since it is too incomplete.

MARRIED STATUS. We use a dummy for single status. Experiments for married status yielded similar results.

UNEMPLOYMENT RATES Unemployment rates are taken from yearly volumes of the *Statistisches Jahrbuch ((1957-1990),(1991-1996)*, which from 1972 on provides national and regional averages. Initial unemployment is taken from the quarter in which the present job started, and the lowest unemployment is computed by searching over the time between then and the interview date. Note that since jobs contribute multiple (yearly) observations, the minimum unemployment rate may differ from one year to the next for the same job.

EXPERIENCE. Wave 3 contained questions about the start of professional life, excluding apprenticeships. For respondents not present in Wave 3, or not having responded to these questions, we control for exit from school/apprenticeship and entry into the labor market when setting true experience.

The labor market history supplement in 1986 asked questions on earlier jobs. Specifically, the age when first gainful employment was entered (CP1001) and whether there any professional changes have occurred since (CP1301, CP1302: age) are reported. The second question may be misleading: The question pertains to "Beruf", meaning profession, which may very well be changed while remaining with the same employer. Though tempting to use as a measure for tenure, we opt for the cautious side, and only use CP1001 for potential labor market experience. (One possible use is to control for *occupation specific* tenure.) We retain this variable for all years, controlling in 1984 and 1985 if the start year thus set is later than the wave year. For job starters in subsequent years, we fill in this variable in the year they first report their job start (which may be for the *two* past years).

Nevertheless, the information is not always available, and in the paper we only use POTENTIAL EXPERIENCE. However, for those individuals for whom the comparison is possible, the two variables have a high degree of correlation.

TENURE. The respondents are asked the year and month when they started working for their current employer.[30] This gives a precise notion of tenure. However, the question was only asked of job changers in waves 3 and 4. For these, we construct the tenure variable as follows. In a first step, we fill in 'from below' for those that have not changed jobs, carrying forward the response given in wave 2. In a second step, if the respondents did not answer 'No' to the question 'Have there been changes in your professional circumstances?', we check to see if they have started a job for the first time. Though answering this question is an experience question rather than a tenure question, we use it to control for consistency in the tenure question. Specifically, if the tenure answer is missing, we fill in from the answer to the job start question. The year is also checked for consistency, the reply to the job start question serving to correct tenure in case of disagreement. This might conceivably overstate tenure, though not experience.

In a third step, we control for consistency of the variables thus filled in. If the respondent specifically replies that she has changed employers, we force the start year variable to be consistent with the response. Otherwise, if the job situation has not

[30]The version in all waves is

> Seit wann sind Sie bei Ihrem jetzigen Arbeitgeber beschäftigt? (Falls Sie Selbständiger sind, geben Sie bitte an, seit wann Sie Ihre jetzige Tätigkeit ausüben.)

to which the correct translation would be

> Since when have you been employed by your present employer? (If you are self-employed, please state since when you have been in your current occupation.)

Note that this is translated in some waves in the English version as "... been employed in your present job". The latter translation may be ambiguous.

changed,[31] we force the start year (SYEAR) to be consistent with the once-lagged value of this variable.

INDUSTRY. Earlier years of the survey only asked the industry variable of job changers. Later years filled it in for all respondents. We do this as well for the earlier years, conditional in all cases on no job change having occured.

DEFLATION. The inflation rate used is the official German index as reported by the BLS.[32] Inflation rates are only available on a monthly base starting in 1990. We use the year-end averages throughout to deflate net and gross earnings, expressing them in 1994 DM. Base year is 1982.

[31] The exact question is "Has your job situation/professional life ("*berufliche Situation*") changed since the beginning of [wave year-1]? Please enter if any of the following applies to you, and if yes, when." The categories are *(1) Took up a job for the first time in my life. (2) Gone back to work after a break. (3) Have a job with a new employer. (4) Became self-employed. (5) Have changed position within the firm. (6) No, none of these.* Possible answers to the first five are the month of the year preceding the interview and the month of the interview year in which that change occured.

[32] ftp://stats.bls.gov/pub/

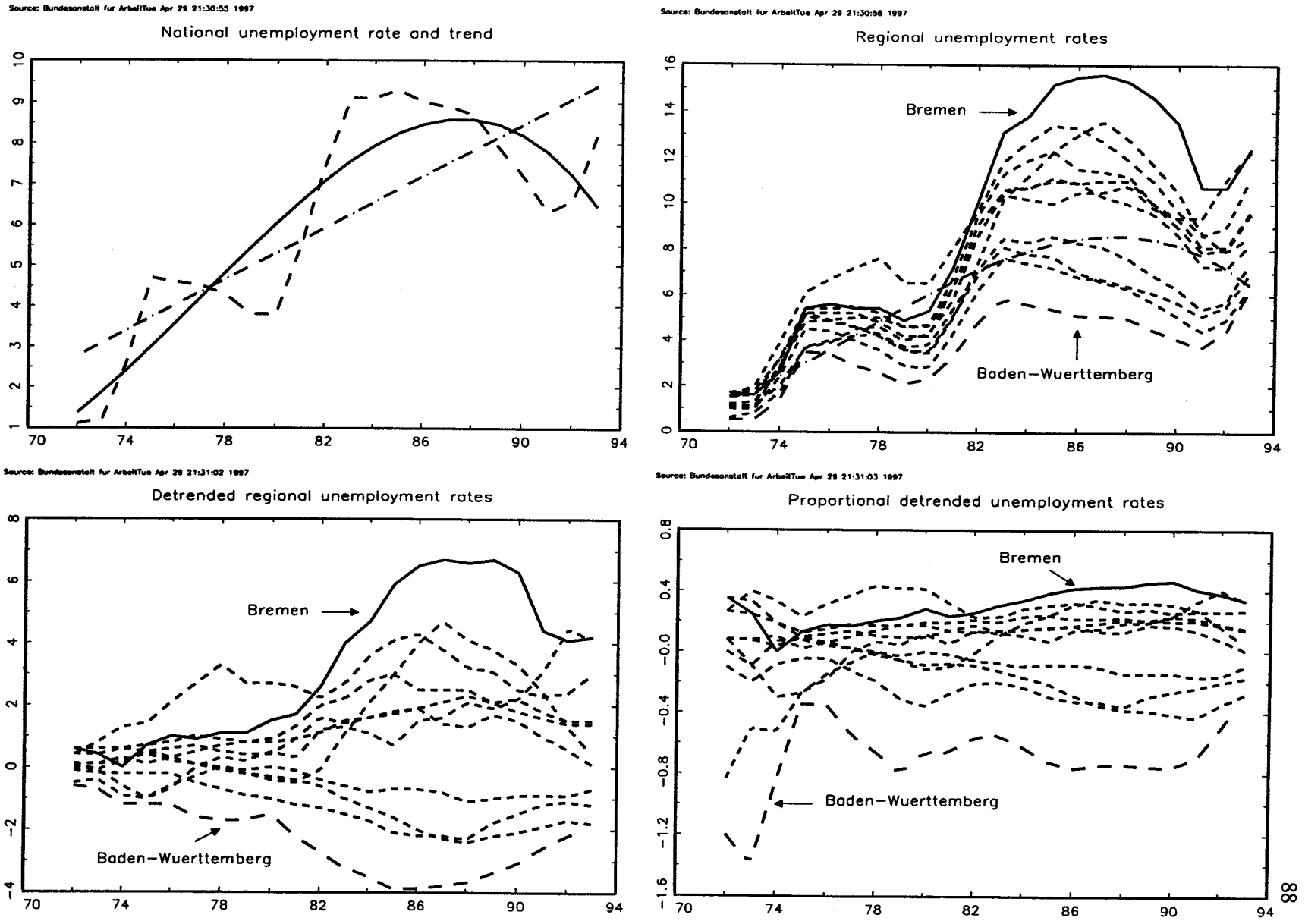


Figure 2 .1: Regional and national unemployment rates

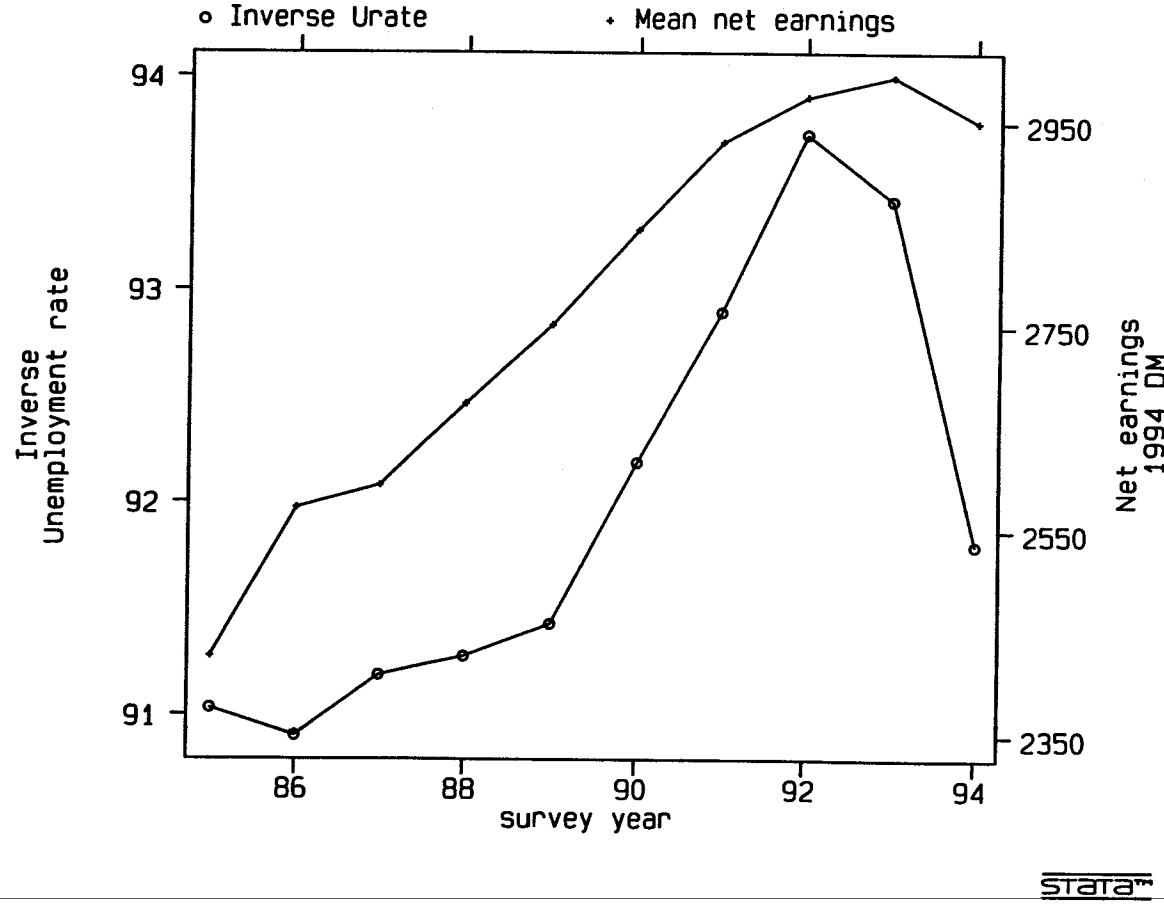


Figure 2 .2: Comovement of inverse unemployment and earnings

Chapitre 3

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

"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)

3.1 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 et al. 1997, Lynch 1991, Lynch 1992a, Parent forthcomingb).

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

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 forthcomingb). 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 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 3.3 describes the data and provides some descriptive statistics. Section 3.4 outlines the empirical model used and Section 3.5 presents the results. Section 3.6 concludes.

3.2 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

[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.

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

[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.

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 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 (forthcomingb) 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 (forthcomingb) 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 forthcomingb). Combined with the reported results on the wage effects of training, this is interpreted 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

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

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

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.*
>
> (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

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

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 $\alpha_1 = \alpha_2 = 1$.

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

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

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

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 the case of more general training. M_2 is now reinterpreted as the probability of a sectoral change,

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

Table 3.I: 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.

conditional on being employed in the next period. Table 3.I summarizes the testable hypotheses.

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.

3.3 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

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

Table 3.II: **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

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

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 3.II on the preceding page provides more details.[14] Sample means for the full

[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 (forthcomingb) 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

sample and the subsample with strictly positive training are given in Table 3.IV on page 107.

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 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.

---

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.

[15] See Table 3.XI on page 142 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.

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.

Table 3.III: 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.

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

[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.

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 forthcomingb, Loewenstein & Spletzer 1996). The results reported here should hence be interpreted with caution.

[17] See Table 3.X on page 142 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 (forthcomingb) 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 3 .1 on page 108 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.

The other panels of Figure 3 .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 3.III on page 104, 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.

Figure 3 .2 on page 141 in the Appendix 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

[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.

when enlargening the window size from one to five weeks, whereas enlargening 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.

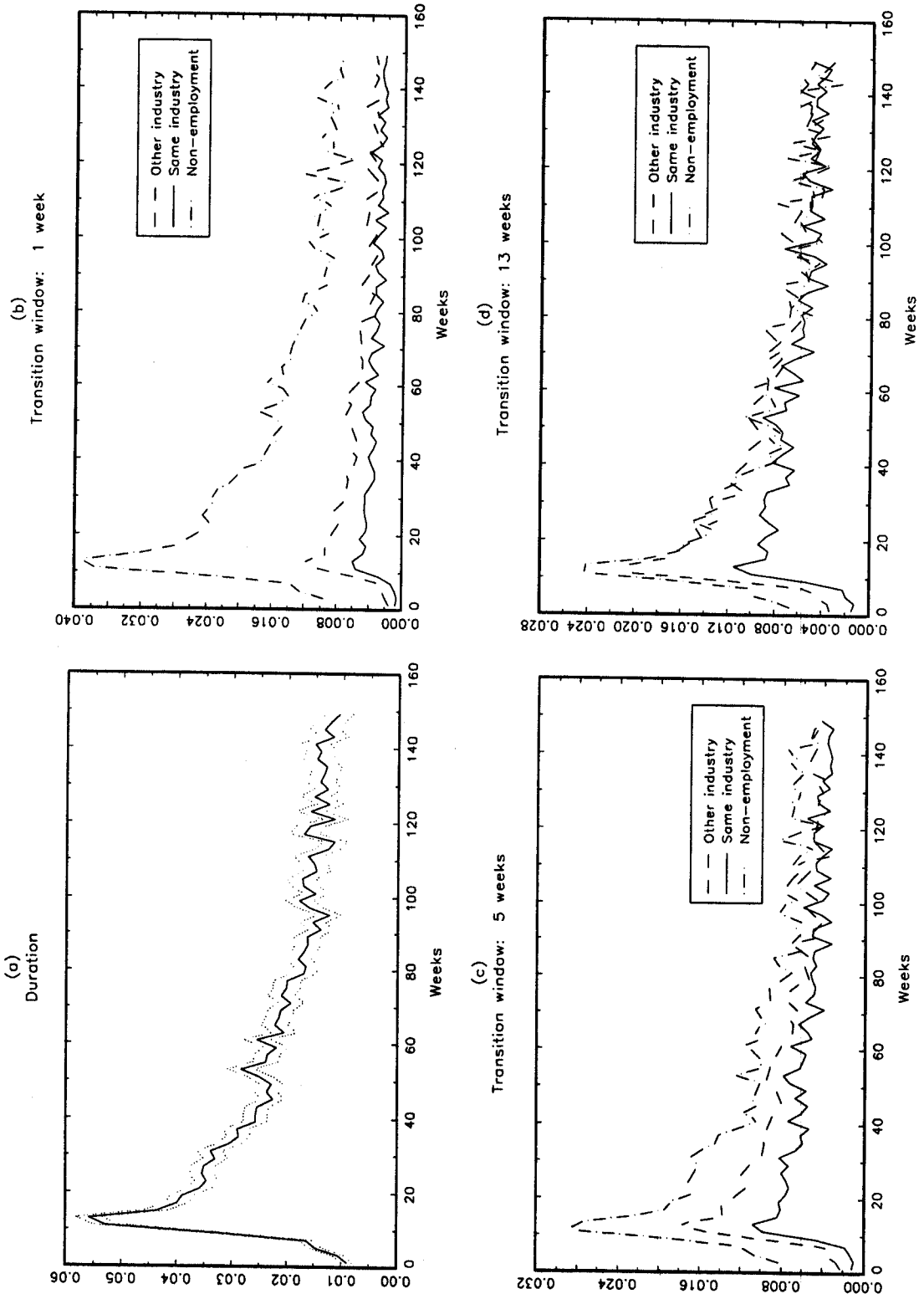
Table 3.IV: **Sample means**

	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 3.IV 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

Figure 3 .1: Transition intensities



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.

3.4 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 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.

[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)

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:

$$\lambda(t) = \lim_{dt \to 0} Prob(t < T \leq t + dt | T > t)/dt \tag{3.1}$$

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

$$S(t) = \exp(-\int_0^t \lambda(s)ds). \tag{3.2}$$

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:

$$T = k_1(t)k_2(x) \tag{3.3}$$

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 (3.3) as a linear regression model:

$$\log T - X\beta = u. \tag{3.4}$$

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:

$$\lambda(t;x) = \tilde{k}_1(t)\tilde{k}_2(x) \tag{3.5}$$

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

$$l = \sum_i (1 - c_i)\log\lambda(u_i) + \log S(u_i) \tag{3.6}$$

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 covariates, specifying the duration distribution is useful. Following

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

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

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

$$l = \sum_{j=1}^{J} \left[\log \tilde{k}_2(x(j)) - \log \sum_{k \in R_j} \tilde{k}_2(x(j)) \right] \tag{3.7}$$

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.

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

$$\theta_m(t;x) = \lim_{dt \to 0} Prob(t < T \leq t + dt, d_m = 1 | T > t; x)/dt \tag{3.8}$$

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

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

$$\lambda(t;x) = \sum_{m=1}^{M} \theta_m(t;x) \tag{3.9}$$

and the survivor function is defined as by (3.2).

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:

$$P(\text{left for } m \text{ at time } t) = \theta_m(t;x)S(t;x) \tag{3.10}$$

which can be rewritten as

$$p(d_1,\ldots,d_m,t;x) = \exp\left\{-\int_0^\infty \sum_{m=1}^{M} \theta_m(s)ds\right\} \prod_{m=1}^{M} \theta_m(t;x)^{d_m} \tag{3.11}$$

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 (3.10) over t yields

$$\pi_m = \int_0^\infty S(s)\theta_m(s)ds. \tag{3.12}$$

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 3.2, 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 (3.10), the former can be seen to be

$$M_2(t) = \frac{P(\text{left for sector 2 in period } t)}{P(\text{left for sector 2 in period } t) + P(\text{left for sector1 in period } t)} \text{ as } h \to 0$$

[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.

$$= \frac{\theta_2(t;x)}{\theta_1(t;x)+\theta_2(t;x)} \tag{3.13}$$

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:

$$\begin{aligned}\frac{\partial M_2(t)}{\partial x_j} &= \frac{\theta_2(t)\beta_{2j}\cdot(\theta_1(t)+\theta_2(t)) - \theta_2(t)\cdot(\theta_1(t)\beta_{1j}+\theta_2(t)\beta_{2j})}{[\theta_1(t)+\theta_2(t)]^2} \\ &= \frac{\theta_1(t)\theta_2(t)}{[\theta_1(t)+\theta_2(t)]^2}(\beta_{2j}-\beta_{1j})\end{aligned} \tag{3.14}$$

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 3.2 as

$$M_{job}(t) = \frac{\theta_1(t)+\theta_2(t)}{\lambda(t)} \tag{3.15}$$

where $\lambda(t)$ is defined as in (3.9) as the sum of the destination-specific transition intensities. The derivative of (3.15) with respect to a covariate x_j is

$$\frac{\partial M_{job}(t)}{\partial X_j} = \frac{\theta_3(t)}{[\sum_{i=1}^{3}\theta_i(t)]^2}\left[\theta_1(t)(\beta_{1j}-\beta_{3j})+\theta_2(t)(\beta_{2j}-\beta_{3j})\right]. \tag{3.16}$$

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 (3.14), which can be unambiguously signed.

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 (3.2) and the independence of t_m,

$$\begin{aligned} P(\text{exit to } m \text{ in period } t) &= \lambda_m(t)S_m(t)\prod_{j\neq m} S_j(t) \qquad (3.17)\\ &= \lambda_m(t)\exp\left\{-\int_0^t \lambda_m(s)ds\right\}\exp\left\{-\sum_{j\neq m}\int_0^t \lambda_j(s)ds\right\} \\ &= \lambda_m(t)\exp\left\{-\sum_{j}\int_0^t \lambda_j(s)ds\right\} \\ &= \lambda_m(t)S(t). \qquad (3.18) \end{aligned}$$

It can be seen that (3.18) is equivalent to (3.10) with $\lambda_m(t) = \theta_m(t)$. On the other hand, using (3.17) to write the likelihood of an individual observation,

$$\begin{aligned} p(d_1,\ldots,d_m,t;x) &= \prod_{m=1}^{M}\left\{\lambda_m(u)S_m(u)\prod_{j\neq m}^{M} S_j(u)\right\}^{d_m} \\ &= \prod_{m=1}^{M}\lambda_m(u)^{d_m}\prod_{m=1}^{M} S_m(u) \\ &= \prod_{m=1}^{M} L_m \qquad (3.19) \end{aligned}$$

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 (3.6), 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)

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.,

$$\theta_m(t) = k_1(t,x)k_{2m}(x;\beta_m) \tag{3.20}$$

Using (3.9) and cancelling out the common factor $k_1(t,x)$, we obtain a *proportional intensity model* with proportionality factor μ_m defined as

$$\frac{\theta_m(t;x)}{\lambda(t;x)} = \frac{k_{2m}(x;\beta_m)}{\sum_{j=1}^{M} k_{2j}(x;\beta_m)} = \mu_m \quad \forall k \tag{3.21}$$

Then the marginal probability of destination m as defined by (3.12) 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:

$$\mu_m = \pi_m = \frac{\exp(-x\beta_m)}{\sum_{j=1}^{K}\exp(-x\beta_j)} \tag{3.22}$$

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

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

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

$$\lambda_m(t;x,\beta) = k_1(t,x_1;\beta_1)k_{2m}(x_2;\beta_{2m}) \tag{3.23}$$

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

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

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 of (3.20), the logit model does not depend on tenure on the last job held. We present results for this test in the empirical section.

3.5 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).

Duration analysis

Panel (a) of Table 3.V on the following page 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.

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

[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.

Table 3.V: **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.

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 3.VI on the next page, 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 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

[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.

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

Table 3.VI:
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 3.V and text for raw coefficients and other details. 95 percent confidence intervals in square brackets.

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

[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.

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 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.

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 3.VII: Multinomial logit estimates

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.

Panel (a) of Table 3.VII on the preceding page 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.

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]

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[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 3.VII is set to five weeks. Results for windows of one and nine weeks do not differ significantly.

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

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 3.4. 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 .

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 3.VIII on the next page 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 when aggregating the two former categories into a category "job

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

[41] See appendix for complete results. Estimates using the accelerated failure-time models of Section 3.5 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.

Table 3.VIII:

Proportionality factor
Cox partial likelihood
Base specification

		Transition intensities			
		Other industry		*Non-employment*	
	Hazard	*Same industry*			*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 3.V. For full results, see Table 3.XIII. 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.

found" without distinguishing the industry in which the next job is located. The results were obtained assuming a transition window of five weeks.

The effects of other variables (reported in Table 3.XIII on page 144 in the appendix) 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 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.

[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.

Table 3.IX:

Proportionality factor
Cox partial likelihood
Heterogeneity

		Transition intensities			
		Other industry		*Non-employment*	
	Hazard	*Same industry*			*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 3.V. For full results, see Table 3.XIII. 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 3.IX 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 3.5, 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 3.XIV to 3.XXI on pages 145 to 152 provide the empirical counterparts to Table 3.I in Section 3.4. Column (a) in Table 3.XIV computes the approriate probabilities for the results reported in Table 3.VIII, and Column (b) for those in Table 3.IX. 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 3.XIV 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 3.IV on page 107, 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

[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.

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 3.XV to 3.XVII, we explore the impact of control for gender. A comparison of Panel (a) of Table 3.XV on page 146 with its theoretical counterpart, Table 3.I on page 99, 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 3.XVII on page 148 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.

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 3.XVII 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 prob-

abilities 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 3.2. 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 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

[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.

regressor. Results reported in Table 3.XVIII on page 149, 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 3.XVIII, 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 3.XVIII. 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 3.2. Tables 3.XIX to 3.XXI 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 3.XIX on page 150 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

[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 et al. (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.

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 3.XX on page 151, 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 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 3.XXI on page 152, 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.

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

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]

3.6 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.

[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.

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.

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Appendix

Figure 3 .2: Transition intensities and transition windows

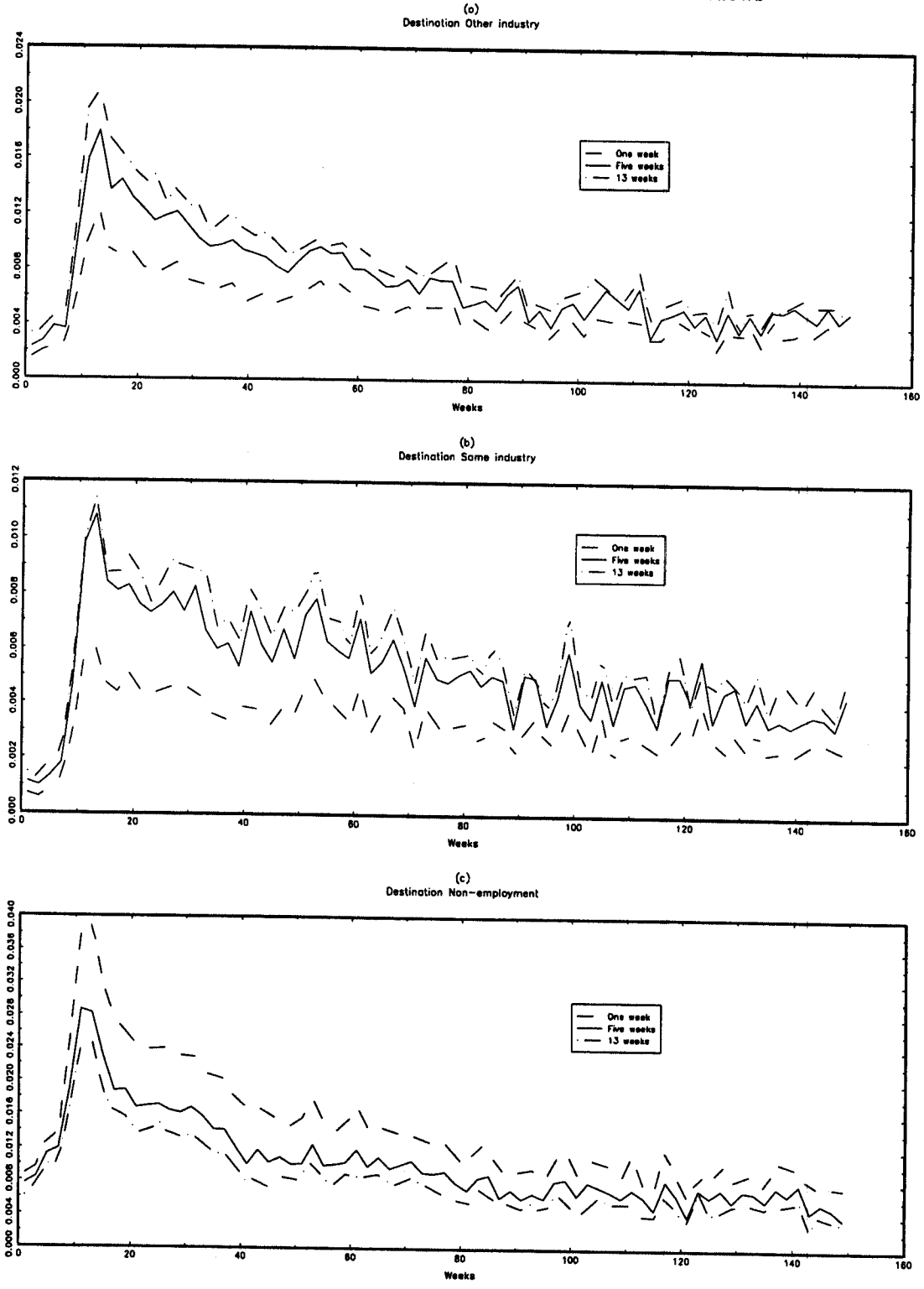

Table 3.X: **Training codes in NLSY**

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 3.XI:
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 3.XII:

Base specification
Effect on baseline hazard
Cox partial likelihood

Variable	*Parameter Estimate*	*Standard Error*
On-the-job training:		
current job	-0.0772	*0.00590*
previous, other industry	0.0080240	*0.00210*
previous, same industry	0.0032177	*0.00344*
Off-the-job training	-0.014197	*0.00204*
Years of education	0.000040732	*0.00244*
Initial experience	-0.000824	*0.0000652*
Jobs ever held	0.001008	*0.00181*
Hours per week	-0.000218	*0.0004719*
Hourly wage $	-0.003364	*0.0008368*
Dummies:		
Union	-0.171259	*0.01668*
Married	0.003914	*0.01200*
Female	-0.009124	*0.01208*
Race	0.043135	*0.01546*

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 change. All variables (except year) measured at start of job.

Table 3.XIII:

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 3.XIV:

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 3.V.

Table 3.XV:
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 3.V. 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 3.XVI:
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 3.V. 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 3.XVII:
Probability of sectoral move
Cox partial likelihood
by gender

Derivative of $M_2(t)$ with respect to:	*Men*		*Women*	
	(a)	(b)	(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 3.V.

Table 3.XVIII:
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 3.V.

Table 3.XIX:

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 3.V.

Table 3.XX:

Probability of employment attachment
Cox partial likelihood
by gender

Derivative of $M_{job}(t)$ with respect to:	*Men*		*Women*	
	(a)	(b)	(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 3.V.

Table 3.XXI:
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 3.V.

Chapitre 4

Sector-specific training and mobility: Evidence from Continuous Training in Germany

NOTE: *une version préliminaire de cet article sera publié dans le deuxième numéro de 1999 du* Vierteljahresheft zur Wirtschaftsforschung.

NOTE: *An earlier version of this article will be published in the 2nd issue of the 1999* Vierteljahresheft zur Wirtschaftsforschung.

4.1 Introduction

The "dual model" of German apprenticeship training is widely admired and often cited as a model of on-the-job training (Hilton 1991, Muszynski & Wolfe 1989). Less attention has been paid to continuous on-the-job training, received after the end of an apprenticeship. Nevertheless, post-apprenticeship training is quite common in Germany. In cross sectional analysis, 2.05 percent of all full-time workers are in some sort of non-apprenticeship training.[1] This compares to an incidence of 2.14 percent in the United States, based on data from the National Longitudinal Survey of Youth (NLSY), the data source most frequently used to study on-the-job training due to it's extensive questioning

[1] Author's computations based on 1984-1995 waves of German Socio-Economic Panel (GSOEP).

on the subject.[2] Thus, even after apprenticeships are absolved, Germans seem to train about as much as Americans.

Previous work has focused on apprenticeships, whether from an institutional perspective (Soskice 1994) or from a quantitative perspective (see f.i. Winkelmann (1994) and Werwatz (1996)). Some of the same authors have also looked at mobility after apprenticeship (Winkelmann 1996, Werwatz 1997). Work on continuous training in Germany is more seldom. Pischke (1996) has looked at continuous training in Germany using an earlier version of the German Socio-Economic Panel (GSOEP). He finds only small wage effects associated with continuous training, but did not consider mobility. Pannenberg (1997) has looked at the question of financing of training and found that sharing of returns does not occur in the case of German on-the-job training. Both Pannenberg (1997) and Büchel & Pannenberg (1994) find that continuous training is positively correlated with promotions, so that some of the returns may accrue in form of promotions rather than direct salary increases. The present paper complements the previous work by presenting results from an updated version of the GSOEP data, and extends the analysis to the industry mobility patterns associated with further training.

In previous work using the NLSY, I have shown that the mobility patterns associated with the stock of on-the-job training are consistent with the presence of industry-specific, but not firm-specific human capital. However, since apprenticeships are less prevalent in the United States, this conclusion may not carry over to Germany. Having acquired a higher initial stock of human capital through apprenticeships, the mobility decisions of German workers may be less affected by subsequent human capital acquisition. In this paper, I exploit the longitudinal nature of the GSOEP to study the transition patterns related to incidence and duration of on-the-job training. These patterns allow inference

[2] Author's computations based on 1979-1992 sample of NLSY. The NLSY sample has a lower average age, and the computed incidence number includes workers in apprenticeships.

as to the specificity of the human capital thus formed, whether firm-specific, sector-specific, or, as most previous authors have argued, general. Results are presented for Germany, and compared with the results obtained for North American workers.

4.2 A Model of Sectoral Capital and Mobility

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. Most work based on human capital theory has used a dichotomy between firm-specific and universally-general capital formation. Recent empirical work on the wage effects of industry tenure (Neal 1995, Parent 1995) has shown that this stark dichotomy may be too imprecise. Already Gary 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.*
>
> (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 it can truly be said to be completely general.

A model of sector-specific human capital

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.[3] 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 $\sigma = 1$.[4] 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

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

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

portable within the same sector, but not across sectors. Finally, general training is portable across sectors, hence $\alpha_1 = \alpha_2 = 1$.

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[5] 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.[6] 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.

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

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

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 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.

Related findings

Most previous empirical studies, most of which regarded U.S. workers, 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 (forthcomingb) 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, i.e. the employer finances training because he or she reaps the returns.

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 forthcomingb). Combined with the reported results on the wage effects of training, this is usually interpreted 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, Veum (1997) finds no effect of on-the-job training on tenure, and Vilhuber (1997) argues that any previously measured tenure effect is due to inference based on mismeasurement of the dependent variable, since measured tenure includes the non-productive time spent in (formal) training programs, but the variable of interest is productive time.

Few previous studies, and none in the training literature, have considered the distinction between intra-sectoral mobility and cross-sectoral mobility. As mentioned earlier, Neal (1995) and Parent (1995) have found evidence that the relevant distinction concerning *informal* human capital as conventionally measured by experience or tenure is sectoral. Neal (1995) comes closest in spirit to the present paper, estimating the wage returns to pre-displacement tenure for industry stayers and industry changers, but does not estimate the effect of human capital acquisition on the probability of a sectoral change. Parent (1995), uses wage regressions to show that industry tenure increases wages, and controlling for industry tenure, the wage effect of firm tenure is negligeable. 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. 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.

In Vilhuber (1997), I have estimated mobility models using the NLSY, and have found results consistent with the model of sector-specific training outlined earlier. These results will be reviewed in Section 4.6 in combination with the results of this paper.

The rest of the paper is structured as follows. Section 4.3 describes the structure of the training data in the GSOEP, and outlines the estimation strategy of the model just described. Section 4.4 describes the sample used, as well as some descriptive statistics as to training incidence and duration. Section 4.5 reports results from the estimation of the model. Last but not least, Section 4.6 briefly describes comparable results from U.S. data, and concludes.

4.3 Data and Estimation Strategy

In this paper, I use data from the German Socio-Economic Panel (GSOEP) for the years 1988-1990 and 1992-1994. In 1989 and 1993, the GSOEP asked a series of questions on "Fortbildung" (*further*, or *continuous* training) of its respondents. Here, I use information on training incidence, duration (in six increasingly broad categories), and training intensity (hours per week). This paper only considers West Germans, due in part to the particularities and differences in the former East German training system, and to the oversampling of the foreign-born population.[7] I merge this information with, on one hand, job market data relating to Period 1, defined as starting on January 1, 1988 (resp. 1992), and ending with the interviewee's date of interview in 1989 (1993), and on the other hand, labor market activity at the end of Period 2, at the time of the 1990 (1994)

[7]Weighting the foreign-born sample would have been an alternative.

interviews. In this section, I will briefly outline the available data and its constraints, and the ensuing estimation strategy.

The GSOEP questionnaire methodology puts a number of restrictions on the data. First, respondents are asked about training which occured in the last three years, but the questions are asked in two interviews separated by four years. Thus, even if information on every training spell within those three years were available, it would not be possible to construct a complete history of training.[8] Second, of those training spells having occured within the three-year time frame, only the three most recent spells are recorded. About 50 percent of respondents (35 percent in the selected subsample) who say they received some training in the last three years received more than three spells during that period. Thus, this is a major constraint.[9]

Third, information on financing and the organizing entity are only available for one of the training spells. Again, in the sample used here, the information is lacking for about half of all training spells. This is particularly important, since I am interested in on-the-job training. I circumvent this problem by assuming that training was on-the-job if it occurred concurrently with a job spell, as outlined below.

Sample construction

To take these restrictions into account, the following sample selection and estimation strategy is adopted in this paper. Any individual having worked within Period 1 is chosen for the present sample. For these workers, I consider only training having occured within the same time frame to aleviate the problem of incompleteness. This specification is chosen because the questions concerning employment changes use the same time frame.[10]

[8]This constraint affects the NLSY data to a lesser degree as well, see Vilhuber (1997). However, since the NLSY training questions are asked every year, the constraints are less restrictive.

[9]Again, this affects NLSY data as well. About 2.2 percent of respondents report receiving four training spells longer than a week in post-1987 NLSY data.

[10]JP23

Information from the second interview, at the end of Period 2, is merged with the sample, allowing identification of four possible states an individual can be observed in and three possible transitions. The four states are: employment with the same employer, employment with a different employer in the same industry (industry stayers), employment in a different industry with a different employer (industry switchers), and non-employment. Using the model put forward in Section 4.2, this data structure allows us to estimate the probability of a sectoral change in Period 2, conditional on employment in Period 1.

The final sample comprises male blue- and white-collar workers between 18 and 65 years of age having worked during Period 1 and still present in the data in Period 2. Workers are excluded if working in either in agriculture, fishing, or unclassified service industries, primarily because of small cell sizes. The resultant sample comprises about 5200 individuals, of which slightly more than a fifth have received some type of training in Period 1. However, 76 percent never change employers, and only slightly more than eight percent (245 men and 187 women) are with a different employer in period 2. The small sample sizes involved may thus prohibit generalizations.

As previously pointed out, information on who actually organizes and/or pays for the respective training is available for only a subset of spells (the "most important" one). After the above sample selection, the information is available for only about 50 percent of the sample. I thus circumvent this problem by verifying whether training occurred simultaneously with an employment spell, and if so, defining that training spell as being "on-the-job". To some degree, this strategy could include training spells undertaken in preparation of a career change, but outside of work or without sanction by the employer. Since of those stating an organizer, 70 percent quote their employer or an employers' association as the organizer of their training, and another 4 percent a manufacturer, the simplification adopted here may not be too restrictive. In fact, less than one percent of

all training spells in my sample (6 training spells) cannot be associated with a specific employer, and thus all are considered "formal on-the-job training".

Econometric specification

The econometric models are fairly straightforward.[11] The choice of destination as outlined in Section 4.2 is modeled as a multinomial logit, where the marginal probability of destination m, π_m is modeled as

$$\pi_m = \frac{\exp(-x\beta_m)}{\sum_{j=1}^{K} \exp(-x\beta_j)} \tag{4.1}$$

where x are covariates at their Period 1 values, and destinations $m = 1, 2, 3$ are a job in the same industry, a job in a different industry, and non-employment, respectively. As I show in Vilhuber (1997), this specification follows from a proportional intensity specification of a duration model with multiple destinations:[12]

$$\frac{\theta_m(t;x)}{\lambda(t;x)} = \frac{k_{2m}(x;\beta_m)}{\sum_{j=1}^{M} k_{2j}(x;\beta_m)} = \mu_m \quad \forall k \tag{4.2}$$

where the transition intensity to state m is defined as

$$\theta_m(t) = k_1(t,x)k_{2m}(x;\beta_m) \tag{4.3}$$

and the hazard $\lambda(t;x) = \sum_{j=1}^{M} \theta_j(t)$.

Note (after some manipulations) that the sign of M_2, the probability of a sectoral move conditional on separation, can be directly computed as the sign of the difference between the appropriate elements in vectors β_1 and β_2. The sign of M_{job} depends on the relative magnitudes of θ_1 and θ_2 in a three-state model (conditional on separation),

[11] For more details, see Maddala (1983).

[12] It implies that the time-dependent components of the hazard are common to all destinations. Vilhuber (1997) shows that this specification may be inappropriate, and proposes a test against the alternative of a competing risks model. In the present context, lack of data preempts that test.

but can be approximated by a two-state logit choice between non-employment and employment.[13]

4.4 Some basic results

Post-apprenticeship training is quite common in Germany. Column 1 in Table 4.I on page 183 reports tabulations from a series of cross-sections of workers taken from the GSOEP.[14] 2.05 percent of all workers are in on-the-job training while on full-time employment and not in apprenticeships. There is significant time-variation in this measure, varying from a low of 1.57 to highs of 2.56.[15] These numbers compare to an average rate of 2.19 percent in the U.S. National Longitudinal Survey of Youth, reported in Column 3. The NLSY sample, however, has a lower average age, and the above number includes workers on many type of training, not only more closely defined as on-the-job. In Columns 2 and 4, I adjust the sample selection and the definition of training to resemble each other even closer. In Column 3, only German workers who were between 19 and 27 at the time of the 1984 interview are included,[16], replicating the NLSY age structure of the same year. In Column 4, only American training spells that were organized at the workplace were counted. The difference is even more pronounced: 3.04 of the younger German population are in some sort of non-apprenticeship on-the-job training while working[17], whereas only 1.08 percent of the young Americans are in more closely defined on-the-job training. It would seem, contrary to expectations, that Germans train more than Americans, even after apprenticeships are excluded.

[13] For more details, see Vilhuber (1997).

[14] The definition of training here is not exactly comparable to the one used in the rest of the paper.

[15] The lows occur almost precisely in the years in which the more detailed training questions used in the rest of this paper were asked. If this is due to some response bias, then the other years will be over-estimates of true training activity.

[16] About 19 percent of the West German GSOEP sample satisfy this constraint.

[17] See Appendix 4.6 for more details on the questions used in both questionnaires.

Sample description

Table 4.II on page 184 reports means for the entire sample underlying this study, for those having left their Period 1 employer, and for those having received some training within Period 1. Movers tend to have lower income, but people having received training have higher income after training, though this is also true before training (see Table 4.IX on page 190). Movers also have a slightly lower incidence of training. In this sample, movers are younger and tend to have less labor market experience then the full sample. However, there are no signficant differences along these dimensions between the full sample and those having received some training.

Movers also have less family ties, whereas trained workers have more family ties as measured by family composition. This will also be discussed in more detail later.

With respect to the full sample and movers, trained workers have more (school) education and tend to have more professional degrees. This is also reflected in the higher percentage of trained workers who are still in the occupation they originally apprenticed in. To some extent, the jobs in which trained workers can be found are also more likely to require at least an apprenticeship as initial training, but require less training overall.

Turning to the characteristics of the training spells reported, I again distinguish between movers and the full sample in Table 4.III on page 185. The groups are primarily distinguished in who initiated training and when training took place. Movers have a small tendency to undertake training on their own initiative, and to participate outside of regular working hours. They tend not to undertake training in order to keep up with new job requirements, though this is still by far the most frequent reason. Notice however the small sample sizes involved, which will restrict the analysis I undertake here.

Opinions on continuous training

West German workers have on average favorable opinions concerning the utility of continuous training, though this is not universal (see Table 4.IV on page 186). 65 percent find some utility to continuous training. The predominant reason, expressed one way or another, is that continuous training is useful to update knowledge, be it by adding new knowledge or revising old knowledge, closely followed by the utility of gaining new knowledge for new jobs.

Opinions are fairly split when asked what reasons might hinder participation in training. Whereas 45 percent of respondents to question A state that they would participate in training in order to improve job prospects, 40 percent of those answering the next question dispute that training would increase job prospects.[18] Many seem to be either time-constrained or liquidity-constrained.

In participants' opinion, training is of a quite general nature. Of those workers having experienced some training within the last three years, nearly 67 percent state that the most important training received is either completely or to a large degree transferable to other jobs. As I will show in this paper, the subjective evaluation expressed here has objective foundings.

Financing and timing of training

Given this perceived generality of training, the extent to which employers financially contribute to continuous training appears surprising. Two thirds of all respondents report obtaining financing for the most important training spell, and the bulk of this financing comes from employers (Table 4.V on page 187). Interestingly enough, as

[18] Casting some doubt on the validity of these opinion polls, nearly 15 percent of those stating that they would participate in training in order to increase job prospects also state that they would not participate because training would not improve job prospects. Have these individuals participated in training and been disappointed?

Table 4.VI on page 187 reports, individuals who received financing from their employer do not seem to evaluate their training as less transferable than those that received no financing at all. Most of the variation comes from workers being financed through other sources, but workers who do not get any financing at all seem less sanguine about transferability than workers financed by their employer. This apparent mystery prompts a look at financial assistance by category of organizing entity in Table 4.6 on page 188. With the exception of adult education centers ("Volkshochschulen") and unclassified other institutions, it is among employer-organized training that the proportion of non-financing is highest, putting doubt on what workers perceive as financing. Among those entities most closely related with the present job, 26.60 percent of respondents state not being financed, but if financed, nearly 97 percent get financing from their employer. One possibility is that workers might state that they do not receive any financing if no direct costs were incurred by the worker, although the company may be paying directly for the cost of the course. It thus seems safe to say that the vast majority of continuous training is paid for by the employer.

If the employer covers any overt financing, what about foregone productive time? Do workers obtain training during working hours, presumably implying continued wage payments, and does this differ by organizing entity? Table 4.6 on page 189 shows that it is again the non-typical training spell, organized by adult education centers and other organizations, which does not occur primarily during working hours. Of those spells organized by employer-related entities, more than 80 percent took place at least partially during working hours. However, these two non-typical categories account for only 6 percent of reported training spells.

Hence, a typical training spell, independent of who organizes it, is financed by the employer directly, and includes continued receipt of wage payments.

Thus, at this point, the question arises as to how transferable training is. The workers' evaluation points to a large degree of transferability, but at the time of the interview, this evaluation is largely hypothetical. On the other hand, firms incur substantial direct and indirect costs through workers participating in training. If training has no effect on tenure, then the riddle of why German firms pay for apprenticeship training (Acemoglu & Pischke 1998, Harhoff & Kane 1993, Soskice 1994) is augmented by the riddle of why German firms pay for continuous training.

A step towards solving these questions lies in determining how "far" from the training firm separating trainees wander, given the wages they obtain. The model outlined in Section 4.2 provides a way to formalize this. In this paper, I define mobility along sectoral lines. Given the institutional structure of German industrial relations, if mobility is largely restricted to the training firm's industry, then an implicit coordination[19] may justify continuous training of workers paid for by firms.

Incidence of continuous training

As Table 4.II on page 184 showed, more than a fifth of all workers in our sample have experienced at least a day of training in Period 1. What determines the incidence of training? Results of logit analyses are reported in Table 4.IX on page 190.

For the full sample, the table shows that the probability of receiving on-the-job training increases in net income, decreases with initial (potential) experience, and increases in tenure with the present firm. Workers with more weekly hours are more likely to receive training, even after controlling for part-time. Workers without any further educational achievements are less likely to receive training, but receipt is otherwise unrelated to education, once blue-collar status has been controlled for.

[19] Soskice (1994) calls it the high skill-high education equilibrium.

Most of these measures, with the exception of labor-market experience, may be interpreted as indicators for the unobserved ability of the worker. Thus, Table 4.IX would indicate that more able workers receive more training.

The family background variables reported in Rows 9 through 12 are never individually significant, but for the full sample as well as for 1989, these variables are jointly significant, and generally have a positive signs. Presence of a partner and of children in school age increase the likelihood of receiving training, both variables being related to decreased family mobility.

None of the variables describing the training requirements on the job and past occupational mobility, are significant, neither individually nor jointly. In particular, workers in jobs which usually require an apprenticeship are not significantly more likely to receive training than others, even if these same workers already have an apprenticeship (the interaction term in Row 16).

Finally, the major distinction as to the incidence of training is blue-collar status. White-collar workers tend to train more frequently, a point already apparent in Table 4.II

Can these results be taken as evidence that sorting or selection, apart from occupational sorting, plays no role in the incidence of continuous training? To the extent that a higher salary proxies for higher ability, sorting by ability would seem to play a role. But the sorting criterion of interest in the case of training which is not firm-specific is sorting by inherent inter-firm mobility. Even if training were general, employers would be willing to pay for it if either the worker can be subsequently tied to the firm (through higher wages, promotion prospects, or other methods),[20] or the worker is inherently less mobile, giving the firm time to recoup its investmnet. It is far from clear that a high

[20]See f.i. Acemoglu & Pischke (1998), where the firm pays for general training because it enjoys an ex-post informational advantage.

salary need be correlated with an inherently lower mobility. A far better indicator of mobility would seem to be family background variables such as presence of children in school age or presence of partner, as these variables have been shown to negatively affect migration probabilities.[21,22] Here, these variables jointly increase the likelihood of training, lending support (albeit weak support) to the hypothesis that firms select workers based upon a worker's inherent probability of leaving.

A different but nonetheless interesting point in Table 4.IX is the strong relationship between income on the present job and the incidence of training. Remember that a fair number of workers cited financial reasons as an impediment to training. A higher income, if related to higher wealth, would alleviate this constraint, leading to the observed sign. Note that this is true even after controlling for blue-collar status, experience, and tenure, and thus to a certain degree controlling for career advancement. On the other hand, as indicated earlier, most training takes place during working hours, and with the sanction of the employer, thus presumably the worker continues to draw a salary, obliviating the need for any substantial financing on the worker side, but pointing towards companies willingness to incur substantial wage costs in order to provide training.

Industry mobility

Industry mobility conditional on changing jobs is high, as Table 4.X on page 191 reports. More than 54 percent of those changing employers between Periods 1 and 2 also change industry. Relating this to the incidence of training in the first period, a strong pattern appears: Only 47 percent of those having received some training in the first period change industry when changing employers, compared to 57 percent for those without

[21] Long (1972), Mincer (1978), Sandell (1977).

[22] Some results reported in the U.S. literature indirectly lend support to the mobility-based selection story. 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. See also Altonji & Spletzer (1991) and Royalty (1996).

training. Note also that the probability of non-employment is lower for those with training, indicating a higher employment attachment, be it with the training employer or some other employer. Most of the employment effect seems to come from an increase in jobs within the same industry with the old or new employers, rather than through an increase in employment in other industries. The rest of this paper will elaborate on this result in order to control for a variety of other factors which might affect incidence of training and mobility.

4.5 Mobility of trained workers

Table 4.X on page 191 reported frequency counts for the proportion of job separations that are either not employed, employed in the same sector, or employed in a different sector at the end of the second period, by incidence of training. The numbers indicate that workers having received training are less likely to be non-employed, and conditional on being employed, are more likely to be employed in the same sector. The following models will correlate these mobility patterns with a worker's or his job's observable characteristics.

Coefficients for training variables in the multinomial model of sectoral mobility are reported in Table 4.XI on page 192, Panel A, and the computed parameter $M_2 = \beta_2 - \beta_1$ is reported in Panel B. Note that as pointed out on page 163, although the coefficients reported in Panel A are used to compute M_{job}, the sign of M_{job} will also depend on the computed probabilities when the coefficients are of opposite sign. This is the case for a number of variables. However, one feature of Panel A is that none of the training variables significantly affect the probability of a job in a different industry vis-a-vis the base case of non-employment. Any action comes from changes in the probability of employment in the same sector.

Thus, a worker who at his last job was employed in an occupation corresponding to his apprenticeship had a higher probability of employment in the same sector with respect to non-employment (Panel A) as well as with respect to employment in a different sector (Panel B). However, given that the coefficients on this variable are of different signs for the two employment destinations, it is not clear that the probability of employment is increased overall.

Columns (a) through (c) explore the sensitivity of the coefficients to the inclusion of industry and occupational controls. The results do not seem to be particularly sensitive to the changes in specification despite the small number of observations.

The strongest effect, as was to be expected, is present for those workers still in the occupation they apprenticed in. Since apprenticed occupations are highly industry-specific, it comes to no surprise that a such a worker is highly likely to stick to his present industry, even though I do not control for time elapsed since his apprenticeship ended. Notice however that the positive employment is only present in his present industry, not for jobs ultimately taken up in different industries, where his employment likelihood is actually reduced, though not significantly so.

For the indicator of continuous training, the effect seems to be smaller. However, as Table 4.III on page 185 showed, spells of continuous training are significantly shorter than the usual two to three year long apprenticeship spell. Thus, it may seem at first glance astounding that the effect is on the same order of magnitude as that for the apprenticeship indicator.

Turning to Panel B, the industry specificity of apprenticeship is outlined by the a fact that the likelihood of industry mobility is significantly reduced. Continuous training reduces the likelihood of a sectoral move as well, though this parameter is never significant.

The interaction term captures any supplementary effect for those workers still in their apprenticed professions who receive further training. The sum of the coefficients on the interaction and on continuous training summarizes the *additional* effect of continuous training for these workers. Throughout, this sum is never significantly different from zero, even though the parameter itself is positive and significant.

Table 4.XI thus seems to indicate that if any mobility-reducing effects of continuous training are present, they are too small to be of statistical significance. However, it turns out that the likelihood of non-employment differs across time. Table 4.XII on page 193 reports coefficients from a logit model of employment conditional on separation, where the two employment destinations are grouped into one. As the first column reports, being an apprentice does not significantly increase the overall probability of employment when not distinguishing sectors, whereas continuous training increases overall employment probabilities significantly except for apprentices. However, as the dummy variable for 1994 indicates, employment probabilities conditional on separation are significantly lower in 1994. The next two columns apply the same model on each year separately, and as the selected other coefficients show, the structure of re-employment is changed only with respect to the training variables, not with respect to personal characteristics like labor market experience or marital status. To explore whether the two periods show different patterns as to the mobility effects of training, I ran separate regressions for the 1989-1990 and 1993-1994 periods, results for which are reported in Tables 4.XIII on page 194 and 4.XIV on page 195.

In 1990, both training variables increase employment probabilies in both destinations, significantly so for employment in the same industry by apprentices and by trainees if industry and occupational controls are included (Column (c)). In particular, the point estimates are large in both sectors for continuous training. This is reflected in Panel

B, where the probability of a sectoral move is not significanlty affected by continuous training. As before, there is no supplementary effect of training for apprentices.

This story changes in 1994. Then, employment probabilities in a different sector are *reduced* by training, though none of the coefficients are significantly different from zero. In Panel B, apprentices are as before less likely to leave their sector, but this is now also true for trainees, significantly in Column (a) and with marginal p-values in other specifications. The interaction term is significant, suggesting again that apprentices receiving supplementary training are no less mobile than apprentices without further training.

A glance at business cycle indicators (see Figure 4 .1 on page 197 in Appendix) shows that the two periods under consideration here were at opposite ends of a business cycle. 1990 was the year of the unification boom. Unemployment was declining, and manufacturing booming. In 1994, recession was well under way, unemployment increasing (only to decrease again in 1998), and production declining dramatically. This may explain why re-employment probabilities are so much lower in 1994.

The model presented in Section 4.2 can be extended to account for business cycle shocks Define an asymmetric shock to one industry as a shock to q, the proportion of job offers coming from industry 2. Redefine r such that $r = r_{12} = r_3$, and define a symmetric shock as a shock to r_{12}, the job offer arrival rate. The "offer arrival rate" from the household sector 3 is assumed unaffected by business cycles. Then it can be shown that $\partial^2 M_2(k)/\partial k \partial q$ is zero if training is completely general, and indeterminate otherwise, depending on the relative magnitudes of M_2, λ, and $F_1(k)$. M_2 is never affected by a shock which reduces the overall job offer rate, because it is measured conditional on having left the firm and having found an (acceptable) job. Furthermore, $\partial^2 M_{job}/\partial k \partial r_{12}$ is zero if training is firm-specific, and again indeterminate otherwise,

depending on the relative magnitudes of r_{12} and M_{job}. The sign of $\partial^2 M_{job}/\partial k \partial q$ is highly indeterminate in all cases.

This extension of the model gives few theoretical predictions towards an understanding of the observed changes in the parameters of interest, since it requires an understanding of the nature of the shock. If it is assumed that the shock is indeed asymmetric, then the reductions in both $\partial M_{job}/\partial k$ (Table 4.XII on page 193) and $\partial M_2/\partial k$ (Table 4.XI on page 192) would be in inconsistent with completely general capital. However, since the true nature of the shock is unknown, this would be highly speculative.

Up until this point, the length of training has not been used in the present analysis. Given the categorical character of this variable and the width of some of the intervals (see Table 4.III on page 185), using the mean of the interval would necessitate correction for substantial measurement error. Here, I split the indicator variable into two separate categories, indicating short training spells if duration was less than one month, and long training spells if duration was longer than one month. Figure 4 .2 on page 198 shows the distribution by destination. It is obvious that those workers who obtain a job in a different industry are those with *longer* spells.

The result from Figure 4 .2 is confirmed to a certain degree by the regression results in Table 4.XV on page 196. For both periods together, long spells increase sectoral mobility. However, this coefficient is neither significant nor stable across time. The pattern found in the previous tables is replicated by the indicator for short training spells, which now is significant for 1994 as well as for the full sample.

At a first glance, this would seem contrary to a human capital model, since the amount of acquired human capital is usually assumed to increase with the time spent on training. One explanation may lie in the worker's motivation for training. If training is undertaken to improve job opportunities, then these job opportunities may occur

outside of the present firm. One way to take this into consideration would be to look at the reason the worker stated for the job separation.[23] Unfortunately, the responses to these questions are missing for about half our sample, and concentrated among those non-employed in Period 2. Sample sizes drop dramatically, and inference is not feasible.

4.6 Conclusion

This paper has proposed a model to test for the sector-specificity of continuous training. Applying this model to West German data from 1989-90 and 1993-94, the ability of the data to distinguish between competing assumptions of specificity is at most tepid. When an apprenticeship in the last held occupation is controlled for, continuous training seems to weakly increase employment in both sectors, and decreases sectoral mobility, but only for short training spells. When separating the sample into two subperiods, the effect of continuous training on sectoral mobility is present only in the recession year 1994, but not in 1990. The increase in employment, sign of both sector-specific and general human capital, is only present in 1990. Furthermore, continuous training has no effect on the sectoral mobility for workers who still work in the occupation they apprenticed in, more than half the present sample.

Note that firm-specific continuous training would complement the Soskice (1994) model of apprenticeship training as non-firm specific human capital. In the present analysis, workers in their apprenticed occupation are less mobile across sectors than other workers, but are as affected in their re-employment probabilities by a recessionary period.

It is not clear whether the absence of strong results is an artefact of the small sample sizes or constitutes a negation of the sector-specifity, or even the generality of continuous

[23] Questions GP24 and KP23.

training in Germany. In related analysis using American data, I constructed complete job histories for young workers and showed that on-the-job training (which includes American apprenticeships) is sector-specific (Vilhuber 1997). Employment attachment increases with the quantity of training, whether or not acquired in the immediately preceding sector, and sectoral mobility is reduced by the quantity of training (total hours) acquired with the present employer or other employers. This is also true for the subset of workers who had been selected by previous employers to receive training, and who have to a certain degree revealed their type, if such selection mechanisms are at work. That type of analysis is not feasible here except to the degree that apprenticed workers are such a subset.

The temporal instability of the coefficient here may be related to the business cycle, though such inference from only two points in time would be premature. It would however extend Becker's (1964,1993) insight that "training may be useful [...] in a set of firms defined by product, type of work, or geographical location"(Becker 1964,1993, pg. 49) to the case where the state of the economy defines the number of firms as "buyers" of a worker's human capital. Though not feasible with the present dataset, it remains an avenue to be pursued further.

Thus, weak evidence presented here for Germany suggests that training as dispensed or sanctioned by firms, because occuring concurrently with employment and possibly during working hours, is correlated with mobility patterns consistent to some degree with the presence of sector-specific or general human capital. Training would appear to confer industry-specific human capital, of use not to all firms in the economy, but at least to a larger number of firms producing similar outputs as the training firm. This result obviously does not directly answer the question why firms would finance training which could be of use to other firms ("general" human capital). And it does not preclude the simultaneous presence of firm specific capital. If trained employees stay long enough

with the training firm, the return on investment for the firm may be positive. Results in Vilhuber (1997) suggest that this effect may be minor for U.S. workers, and results not reported here for the GSOEP suggest that the probability of leaving the training employer by the time of the Period 2 interview is not significantly affected by the presence of training.

One explanation for Germany may lie in the high degree of unionization and the subsequent homogeneity of wage scales within an industry. If reasons of separation are not related to training itself, then firms may gain trained employees in the same measure as they lose them, and benefit from a high industry-wide incidence of training. To be feasible, firms must perceive it in their own interest not to shirk, and this is tricky, but the mechanism may be the same that allows for such widespread apprenticeship training.[24] This explanation would seem not to work in the United States, but different equilibria with different degrees of training incidence may well exist.

[24]See Soskice (1994) for more details in the case of apprenticeships.

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Appendix

Table 4.I: Cross-sectional incidence

	GSOEP		NLSY	
	Full	Young	Train	OJT
all	2.05	3.04	2.19	1.08
1984	2.45	3.10	2.25	1.07
1985	2.47	3.65	1.84	0.78
1986	2.10	3.39	2.25	1.12
1987	2.56	4.12	.	.
1988	1.74	3.99	2.34	1.22
1989	1.57	2.70	2.15	1.19
1990	1.68	2.83	1.65	0.97
1991	1.73	3.11	1.57	1.03
1992	2.30	3.69	1.71	1.07
1993	1.70	2.35	2.22	1.32
1994	2.23	2.83	.	.
1995	2.09	1.62	.	.

GSOEP: Percentage of full-time workers in on-the-job training (excl. apprenticeships) at time of interview. The definition of on-the-job training only includes "career retraining" and "vocational advanced training". See Questions FP09 and JP14. Young sample is between 19 and 27 in 1984.
NLSY: Overall average is for 1979-1993, all workers with a job, possibly not at work, and currently in an unfinished (OJT: on-the-job) training spell. NLSY respondents are between 19 and 27 in 1984.

Table 4.II: Sample means

	All	Moved	Trained
Proportion 1993	0.456	0.364	0.495
Incidence of training within last year	0.237	0.210	1
Left Period 1 employer	0.177	1	0.157
Job:			
Net income (DM 1993)	2713.373	2331.132	3228.242
Contractual hours	39.521	39.268	39.428
Actual hours	42.946	42.698	44.225
Part-time job	0.012	0.043	0.010
Potential experience at start of Period 1	21.928	18.669	19.265
Tenure (years)	10.870	8.400	10.042
Blue-collar	0.523	0.586	0.261
Family:			
Partner present	0.761	0.629	0.793
Married	0.692	0.539	0.711
Partner works	0.375	0.314	0.385
Kids < 16 yrs old	0.403	0.319	0.486
Age	39.358	36.049	37.577
Education:			
Years of education	11.430	11.379	12.312
No professional degree	0.119	0.167	0.050
Necessary training:			
Some	0.289	0.279	0.171
Apprenticeship	0.563	0.564	0.597
Currently in apprenticed prof	0.569	0.568	0.642
Number of obs:	2755	508	654

For sample selection criteria, see text.

Table 4.III: Sample means of training variables

	All		Moved	
Certificate received	646	0.664	106	0.641
Hours per week	641	21.57	104	19.86
(min/max)		1/90		2/80
Number of courses:				
in last 3 yrs	641	3.84	106	3.89
(min/max)		1/36		1/20
in Period 1:				
1	320	48.93	53	49.53
2	178	27.22	34	31.78
3	156	23.85	20	18.69
Respondents with more than three spells in last three years		*0.376*		*0.387*
of which: exactly three in past year		*0.054*		*0.045*
Reason for training:				
on-the-job training	651	0.070	106	0.084
promotion qualification	651	0.351	106	0.386
new demands	651	0.683	106	0.594
other	651	0.107	106	0.169
On whose initiative:				
own initiative	523	0.399	80	0.462
company. initiative	523	0.321	80	0.300
both	523	0.279	80	0.237
Occured during working hours:				
Entirely	653	0.739	106	0.660
Partially	653	0.076	106	0.066
Not at all	653	0.180	106	0.254
Length of training:				
only one day	654	0.151	107	0.177
up to one week	654	0.584	107	0.542
up to one month	654	0.107	107	0.056
up to 3 months	654	0.100	107	0.130
up to one year	654	0.025	107	0.056
up to two years	654	0.016	107	0.009
more than 2 yrs	654	0.013	107	0.028

Sample of trained persons only. Sample selection criteria are described in the text. Note that number of observations are not constant due to missing data on some training spells.

Table 4.IV: Opinions on training

A. *Further training can be done for different reasons. Which of the following reasons is applicable in your case? (More than one choice is possible.)*

1. complete the final examinations for your degree	5.0%
2. retrain for a different job	10.3%
3. update and review your job knowledge	31.5%
4. keep up-to-date with the latest developments	42.2%
5. become qualified for a better job	34.6%
6. expand field of knowledge for greater range of job opportunities	26.8%
7. none of these, no intention to obtain further training	37.6%
Update knowledge (3 and/or 4)	49.2%
Better qualifications for job opportunities (5 and/or 6)	44.9%

B. *You may also have specific reason not to undertake further training. Which of the following reasons is applicable in your case?*

1. Further training will not improve my job prospects	40.3 %
2. No time for further training	39.8 %
3. Cannot afford to give up my income or pay for further training	47.9 %

C. *How well could you use this training in case you changed jobs?*

1. Not at all	9.6%
2. In a limited way, only a small part	22.7%
3. To a large extent	34.5%
4. Completely	33.3%

Frequency of respondents giving a positive answer to the question. Question C. was only asked in 1993 and refers to the most important training of the respondent.

Table 4.V: Financial assistance

68. *Do you get financial assistance or continued payment of wages from your employer, employment office, or from somewhere else during your further training?*

No	33.6%
Yes	66.4%
Of those receiving assistance:	
from the employer	88.5%
from the employment office	11.4%
from somewhere else	1.6%

Note: Question refers to the respondent's most important training.

Table 4.VI: Financing and transferability

Usefulness of training on other job	*Financial support*			
	Employer	Other	None	Total
none	9.51	2.78	11.29	9.67
small	22.33	8.33	26.34	22.76
large extent	34.29	40.28	33.60	34.45
completely	33.86	48.61	28.76	33.13
Observations	694	72	372	1138

Note: Questions refers to the respondent's most important training. The questions were asked in 1993 only.

Table 4.VII: Financial support and organizing entity

Organizer	*Financial support from:* Employer	Other	None	Obs
1. Employer	69.81	0.00	30.19	212
2. Company institute	77.78	1.39	20.83	72
Directly employer-related (1+2)	*71.83*	*0.35*	*27.82*	*284*
3. Employer of prof. association	66.29	10.11	23.60	89
4. Manufacturer	77.78	0.00	22.22	18
Employer-related (1+2+3+4)	*70.84*	*2.56*	*26.60*	*391*
5. Adult education center (VHS)	20.83	0.00	79.17	24
6. Trade union, university, church	100.00	0.00	0.00	8
7. Private school or institute	68.82	5.38	25.81	93
8. Other	57.14	0.00	42.86	7
Total	*68.45*	*2.87*	*28.68*	*523*

Note: Question on financing refers to most important training spell only. For comparability with Table 4.VI, only 1993 data reported.

Table 4.VIII: Timing and organizing entity

Organizer	*Training occurs partially or fully during working hours*	
	No	Yes
1. Employer	10.57	89.43
2. Company institute	14.62	85.38
Directly employer-related (1+2)	*11.51*	*88.49*
3. Employer or prof. association	41.57	58.43
4. Manufacturer	13.89	86.11
Employer-related (1+2+3+4)	*19.58*	*80.42*
5. Adult education center (VHS)	87.61	12.39
6. Trade union, university, church	38.61	61.39
7. Private school or institute	42.51	57.49
8. Other	48.24	51.76
Non-employer-related (5-8)	*50.07*	*49.93*
Total	*70.59*	*29.41*

Note: Percentages of training spells reported. Respondents could report up to three training spells.

Table 4.IX: Incidence of training

		All	1989	1993
1	1993 dummy	-0.594 (*-1.64*)		
2	Net income *(in 1000 DM)*	0.244** (*4.78*)	0.131 (*1.565*)	0.312** (*4.553*)
3	Weekly hours	0.015* (*2.25*)	0.015 (*1.555*)	0.015 (*1.545*)
4	Part-time	0.166 (*0.35*)	-1.128 (*-1.022*)	0.756 (*1.318*)
5	Initial experience	-0.055** (*-7.24*)	-0.057** (*-5.280*)	-0.057** (*-5.228*)
6	Tenure	0.015* (*1.99*)	0.008 (*0.769*)	0.025* (*2.423*)
7	Absence of degree	-0.546** (*-2.58*)	-0.725* (*-2.225*)	-0.429 (*-1.476*)
8	Education (in years)	-0.009 (*-0.31*)	-0.016 (*-0.362*)	-0.010 (*-0.228*)
9	Married	-0.089 (*-0.49*)	0.240 (*0.858*)	-0.353 (*-1.372*)
10	Presence of kids < 16 yrs	0.158 (*1.31*)	0.022 (*0.125*)	0.267 (*1.544*)
11	Partner works	0.097 (*0.82*)	-0.067 (*-0.388*)	0.263 (*1.560*)
12	Partner present	0.327 (*1.63*)	0.471 (*1.505*)	0.164 (*0.607*)
13	Some training necessary	-0.060 (*-0.28*)	-0.206 (*-0.677*)	0.055 (*0.168*)
14	Apprenticeship necessary	0.323 (*1.36*)	0.403 (*1.241*)	0.183 (*0.496*)
15	Currently in appr. profession	0.101 (*0.50*)	0.063 (*0.219*)	0.159 (*0.533*)
16	Interaction term	-0.228 (*-0.91*)	-0.377 (*-1.066*)	-0.084 (*-0.224*)
17	Blue Collar	-1.134** (*-8.99*)	-1.214** (*-6.732*)	-1.006** (*-5.533*)
	Observations	2831	1496	1335

Logit of incidence of training in Period 1 based on characteristics at start of Period 1. All regressions also include a constant, 12 industry controls, 8 regional controls, and controls for length of Periods 1 and 2.

z-statistics in parentheses. ** denotes significance at 1 percent level, * at 5 percent level, + at 10 percent level.

Table 4.X: Sectoral mobility and training

	Destination			
	Same Employer	Not Employed	Different Employer, *Switched industry*	
			No	*Yes*
No training	75.52	16.51	3.44	4.53
			43.21	*56.79*
Received training	78.16	12.09	5.14	4.60
			52.78	*47.22*
Total	**76.09**	**15.56**	**3.81**	**4.54**
			45.60	*54.40*

Rows sum to 100 percent.

Table 4.XI: Mobility conditional on separation

	A: Employment effects					
	(a)		(b)		(c)	
Industry employed:	same	other	same	other	same	other
In appr. prof.	1.104** (*2.716*)	-0.284 (*-0.779*)	1.143** (*2.729*)	-0.231 (*-0.621*)	1.165** (*2.718*)	-0.281 (*-0.731*)
Continuous training	0.763 (*1.491*)	0.157 (*0.320*)	0.947+ (*1.772*)	0.219 (*0.435*)	0.943+ (*1.744*)	0.286 (*0.554*)
Interaction	-0.715 (*-1.156*)	0.396 (*0.645*)	-0.964 (*-1.491*)	0.272 (*0.436*)	-0.932 (*-1.652*)	0.211 (*0.334*)
Year dummy	-0.033 (*-0.115*)	0.158 (*0.568*)	-0.099 (*-0.330*)	0.160 (*0.556*)	-0.133 (*-0.440*)	0.173 (*0.592*)
Blue Collar	-0.434 (*-1.454*)	-0.106 (*-0.345*)	-0.545+ (*-1.638*)	-0.014 (*-0.042*)	-0.709+ (*-1.652*)	-0.495 (*-1.147*)
B: Effect on sectoral mobility conditional on employment						
In appr. prof.	-1.388**		-1.374**		-1.446**	
Continuous training	-0.606		-0.728		-0.657	
Interaction	1.111+		1.236+		1.143+	
Year dummy	0.191		0.259		0.306	
Blue Collar	0.328		0.531		0.214	
Industry controls	*No*		*Yes*		*Yes*	
Occupation controls	*No*		*No*		*Yes*	
Observations	508		508		508	
Log-Likelihood	-432.05		-413.22		-409.15	

z-statistics in parentheses. Multinomial logit of sectoral mobility. Base category is non-employment. All regressions include net monthly income, hours worked, an indicator for part-time status, and tenure on the last job worked, experience and its square, years of education, an indicator for the absence of a degree, and marital status at the end of Period 1, and controls for length of Periods 1 and 2.
** denotes significance at 1 percent level, * at 5 percent level, + at 10 percent level.

Table 4.XII: Employment attachment

	All	1990	1994
Employment effects			
In appr. prof.	0.418 (*1.395*)	0.985* (*2.268*)	-0.276 (*-0.577*)
Continuous training	0.914* (*2.075*)	1.220* (*2.056*)	-0.058 (*-0.075*)
Interaction	-0.807 (*-1.555*)	-1.539* (*-2.194*)	0.337 (*0.375*)
Year dummy	-1.747* (*-2.373*)		
Pot. Experience	0.111* (*2.497*)	0.115* (*1.978*)	0.113 (*1.465*)
Experience squared	-0.003**(*-3.559*)	-0.004**(*-2.854*)	-0.004* (*-2.151*)
Married	0.563* (*2.296*)	0.450 (*1.332*)	0.607 (*1.535*)
Observations	508	323	185
Log-Likelihood	-317.59	-176.51	-129.08

z-statistics in parentheses. Logit of employment attachment. All regressions include net monthly income, hours worked, an indicator for part-time status, 10 industry controls, control for blue-collar status, and tenure on the last job worked, experience and its square, years of education, an indicator for the absence of a degree, and marital status at the end of Period 1, and controls for length of Periods 1 and 2. ** denotes significance at 1 percent level, * at 5 percent level, + at 10 percent level.

Table 4.XIII: Mobility conditional on separation: 1990

	A: Employment effects					
	(a)		(b)		(c)	
Industry employed:	same	other	same	other	same	other
In appr. prof.	1.331** (*2.564*)	0.116 (*0.234*)	1.563** (*2.858*)	0.278 (*0.528*)	1.570** (*2.787*)	0.261 (*0.479*)
Continuous training	0.950 (*1.505*)	0.701 (*1.161*)	1.070 (*1.600*)	0.671 (*1.060*)	1.162+ (*1.685*)	0.770 (*1.172*)
Interaction	-1.202 (*-1.574*)	-0.625 (*-0.818*)	-1.614* (*-1.992*)	-0.661 (*-0.837*)	-1.690* (*-2.036*)	-0.796 (*-0.981*)
Blue Collar	-0.568 (*-1.542*)	-0.152 (*-0.373*)	-0.655 (*-1.594*)	-0.093 (*-0.204*)	-0.618 (*-1.184*)	-0.645 (*-1.081*)
	B: Effect on sectoral mobility conditional on employment					
In appr. prof.	-1.215+		-1.285+		-1.309+	
Continuous training	-0.249		-0.399		-0.392	
Interaction	0.577		0.953		0.894	
Blue Collar	0.416		0.562		-0.027	
Industry controls	*No*		*Yes*		*Yes*	
Occupation controls	*No*		*No*		*Yes*	
Observations	323		323		323	
Log-Likelihood	-274.44		-258.37		-254.72	

For details, see Table 4.XI .

Table 4.XIV: Mobility conditional on separation: 1994

A: Employment effects						
	(a)		(b)		(c)	
Industry employed:	same	other	same	other	same	other
In appr. prof.	0.639 (*0.878*)	-0.654 (*-1.093*)	0.347 (*0.447*)	-0.703 (*-1.128*)	0.474 (*0.580*)	-0.797 (*-1.236*)
Continuous training	0.823 (*0.823*)	-1.049 (*-1.010*)	0.658 (*0.607*)	-0.860 (*-0.793*)	0.645 (*0.575*)	-0.850 (*-0.755*)
Interaction	-0.519 (*-0.437*)	2.145 (*1.781*)	-0.672 (*-0.496*)	1.619 (*1.287*)	-0.798 (*-0.552*)	1.732 (*1.319*)
Blue Collar	-0.254 (*-0.440*)	-0.196 (*-0.379*)	-0.646 (*-0.963*)	-0.168 (*-0.284*)	-0.985 (*-1.161*)	-0.432 (*-0.569*)
B: Effect on sectoral mobility conditional on employment						
In appr. prof.	-1.293*		-1.050		-1.271+	
Continuous training	-1.872+		-1.518		-1.495	
Interaction	2.664*		2.291+		1.733+	
Blue Collar	0.058		0.478		0.553	
Industry controls	*No*		*Yes*		*Yes*	
Occupation controls	*No*		*No*		*Yes*	
Observations	187		187		187	
Log-Likelihood	-147.47		-135.59		-133.92	

For details, see Table 4.XI.

Table 4.XV: Mobility conditional on separation, (cont)

	A: Employment effects					
	All		1990		1994	
Industry employed:	same	other	same	other	same	other
In appr. prof.	1.011** (*2.656*)	-0.236 (*-0.647*)	1.543** (*2.829*)	0.273 (*0.519*)	0.320* (*0.408*)	-0.711 (*-1.131*)
Continuous training:						
Short duration	0.998+ (*1.835*)	-0.061 (*-0.114*)	1.113 (*1.637*)	0.457 (*0.493*)	0.895 (*0.796*)	-1.267 (*-1.115*)
Long duration	0.331 (*0.370*)	1.029 (*1.462*)	0.199 (*0.879*)	1.258 (*1.418*)	0.007 (*0.002*)	0.034 (*0.024*)
Interaction	-0.895 (*-1.371*)	0.152 (*0.236*)	-1.153+ (*-1.880*)	-0.721 (*-0.898*)	-0.402 (*-0.289*)	1.146 (*1.115*)
Year dummy	-0.062 (*-0.206*)	0.133 (*0.644*)				
Blue Collar	-0.551+ (*-1.650*)	-0.037 (*-0.111*)	-0.641 (*-1.554*)	-0.109 (*-0.240*)	-0.719 (*-1.041*)	-0.268 (*-0.435*)
	B: Effect on sectoral mobility conditional on employment					
In appr. prof.	-1.247**		-1.270*		-1.031	
Continuous training:						
Short duration	-1.059+		-0.656		-2.162+	
Long duration	0.698		1.059		0.027	
Interaction	1.047		0.432		1.548	
Year dummy	0.195					
Blue Collar	0.514		0.532		0.451	
Observations	508		323		185	
Log-Likelihood	-410.00		-256.98		-143.04	

All regressions include 10 industry controls. Short training spells are shorter than 1 month. For further details, see Table 4.XI.

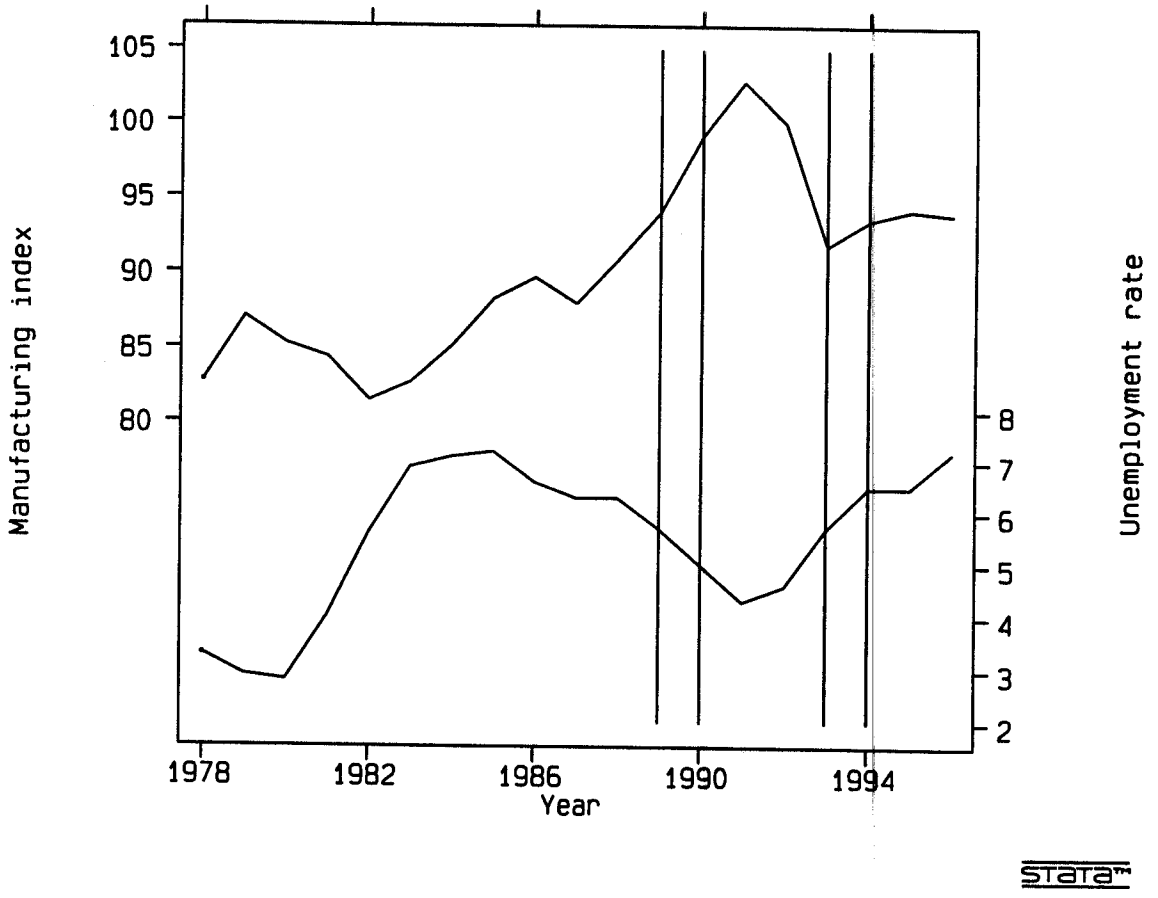
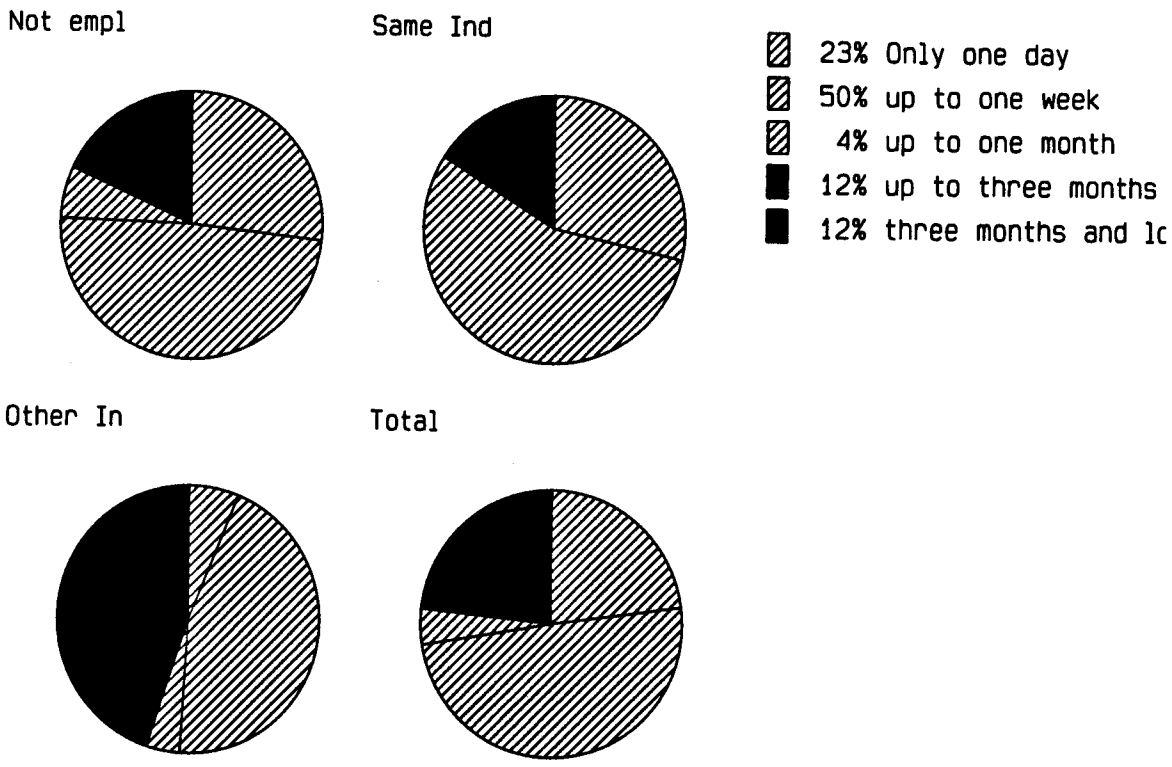


Figure 4 .1: German unemployment and manufacturing index
Data source: BLS.

Figure 4 .2: Length of training by destination



GSOEP questionnaires

Current training in GSOEP and NLSY

GSOEP (1993)

13. [JP13] Are you receiving education at the moment? In other words are you in school, career training, or are you attending a further education course?

yes _____
no (proceed to question 15) _____

14. What sort of education or training is this?

[JP1401] GENERAL SCHOOLING
short-course secondary school _____
intermediate type of secondary school _____
academically-oriented secondary school _____
comprehensive secondary school _____
night school-secondary _____
technical high school _____

[JP1402] HIGHER EDUCATION
technical college _____
university/college _____

ADVANCED TRAINING COURSES

[JP1403] vocational retraining _____
[JP1404] advanced vocational training _____
[JP1405] vocational rehabilitation _____
[JP1406] general or political training _____
[JP1407] other _____
fill in here _____

BASIC VOCATIONAL TRAINING

[JP1408] basic vocational training year, vocational preparation year _____
[JP1409] vocational school (not including apprenticeship) _____
[JP1410] apprenticeship _____
[JP1411] specialized vocational school, business school _____
[JP1412] public health school _____
[JP1413] specialized schools such as master-schools or technicians' schools _____
[JP1414] training for the civil service _____
[JP1415] other _____
fill in here _____

NLSY (1989)

The NLSY questions on training are all retrospective for the time period between interviews. To obtain a cross-sectional estimate of training incidence, it is necessary to combine information on all possible training spells with the information on the end date of the training spell. The latter is coded as a zero if training is ongoing at the time of the interview.

(R29391.) ANY OTHER VOCATIONAL/TECHNICAL TRAINING BEGAN SINCE LAST INT?

Record type: TRAINING Question number: Q1439 Survey year: 89
Variable name: TRN0825

(BESIDES THE TRAINING PROGRAMS WE'VE ALREADY TALKED ABOUT), SINCE (DATE OF LAST INTERVIEW), DID YOU ATTEND ANY (OTHER) TRAINING PROGRAM OR ANY ON-THE-JOB TRAINING DESIGNED TO HELP PEOPLE FIND A JOB, IMPROVE JOB SKILLS, OR LEARN A NEW JOB?

1551	1 YES
9054	0 NO

(...)

(R29405.) YEAR COMPLETED/LEFT 1ST VOCATIONAL/TECHNICAL PGM ENROLLED IN SINCE LAST INT

Record type: TRAINING Question number: Q1466 Survey year: 89
Variable name: TRN0839

0 STILL ENROLLED

Training questions in GSOEP

62. (JP62) *In the last three years how many courses or classes for occupational advancement did you take? Please include courses that began earlier if they ended sometime within the last three years.*

Number of courses or classes _____

63. *We would like some additional information about the three most recent courses or classes.*

	Most recent or current course	1	2	3
a)	*In what year and month did this course/class begin?*			
	year	**(JP6301)**	**(JP6313)**	**(JP6325)**
	month	**(JP6302)**	**(JP6314)**	**(JP6326)**
b)	*What was/is the length of this course or class?*	**(JP6303)**	**(JP6315)**	**(JP6327)**
	just one day	_____	_____	_____
	up to one week	_____	_____	_____
	up to one month	_____	_____	_____
	up to three months	_____	_____	_____
	up to one year	_____	_____	_____
	up to two years	_____	_____	_____
	more than two years	_____	_____	_____
c)	*How many hours of class time per week were there?*			
	number of hours	**(JP6304)**	**(JP6316)**	**(JP6328)**
	correspondence course	**(JP6305)**	**(JP6317)**	**(JP6329)**
d)	*What was your reason for taking these courses or classes? (More than one reason possible.)*			
	retraining for another job	**(JP6306)**	**(JP6318)**	**(JP6330)**
	on-the-job training at a new place of work	**(JP6307)**	**(JP6319)**	**(JP6331)**
	become qualified for advancement	**(JP6308)**	**(JP6320)**	**(JP6332)**
	keep up-to-date with new developments in your field	**(JP6309)**	**(JP6321)**	**(JP6333)**
	other	**(JP6310)**	**(JP6322)**	**(JP6334)**
e)	*Was the course/class given during working hours?*	**(JP6311)**	**(JP6323)**	**(JP6335)**
	yes	_____	_____	_____
	partially	_____	_____	_____
	no	_____	_____	_____
	does not apply, was not employed at that time	_____	_____	_____
f)	*Did you receive a certificate when you completed your course/class and you can later use when job hunting.*	**(JP6312)**	**(JP6324)**	**(JP6336)**
	yes	_____	_____	_____
	no			

64. *If in the previous question you answered that you took more than one course or class, which one was most important to your career?*

		Freq.	Value
(JP6401) course number	_____	413	1
		222	2
		101	3

		Freq.	Value
(JP6402) took only one course	_____	793	1
(JP6402) all of equal importance	_____	761	2

Synthèse

Au chapitre 2, nous analysons la corrélation entre les revenus de travail contemporains et les conditions sur le marché du travail à différents moments pendant la durée du contrat. Différentes théories sont associées à ces mesures, et les coefficients estimés par régression linéaire sont interprétés sur le plan de la pertinence de chaque modèle pour la dynamique des salaires. Les données utilisées proviennent du Panel Socio-Économique Allemand pour les années 1984-1994. Contrairement aux résultats obtenus pour le marché américain, nous trouvons que l'état actuel du marché du travail a un impact majeur sur les revenus de travail contemporains, même en contrôlant pour des valeurs passées du taux de chômage. Plus révélatrices de la structure contractuelle allemande, des différences quant à l'impact apparaissent lorsqu'on différencie les régions. Tandis que le taux de chômage régional affecte le niveau des revenus d'emploi par l'intermédiaire du niveau du taux de chômage en vigueur en début de contrat, le taux national influence la variation contemporaine des revenus. Ces résultats permettent de conclure à la superposition de contrats individuels et collectifs affectant tous deux les revenus des travailleurs. Ainsi, les syndicats négocieraient sur les niveaux de salaires et d'emploi et ce, sur le plan national, mais les firmes conserveraient une marge pour pouvoir varier les salaires d'entrée en fonction des conditions sur le marché du travail local, tout en assurant les travailleurs contre des fluctuations de revenu idiosyncratiques.

De plus, nous étudions l'hétérogenéité des contrats selon certaines caractéristiques des travailleurs et des emplois. En particulier, nous constatons que les contrats de tra-

vail diffèrent selon la taille de l'entreprise. De plus, ils changent en fonction du type d'activité du travailleur et ce, en dépit de la convergence des modalités de paiement dans les conventions collectives allemandes. Un travailleur dans une grande entreprise est remarquablement plus isolé des fluctuations du marché de travail qu'un travailleur dans toute autre type d'entreprise, et il en est de même pour les travailleurs col blanc.

L'étude met en relief plusieurs points. D'abord, elle confirme l'importance des marchés de travail internes dans la détermination des salaires. Deuxièmement, elle révèle que le système de relations industrielles allemand, affichant une forte centralisation des négociations collectives, est assez flexible pour permettre la réponse des employeurs à des conditions locales sur le marché du travail. Enfin, la flexibilité des salaires allemands face aux conditions sur le marché du travail, à savoir le taux de chômage, n'est pas moindre que celle aux États-Unis. Les élasticités estimées varient entre 9 et 15 pourcent pour l'élasticité des revenus de travail par rapport au taux de chômage contemporain, et entre 6 et 10 pourcent par rapport au taux de chômage en début de contrat. Les estimations américaines correspondantes sont deux fois plus petites quant au taux de chômage contemporain, et de même ordre de grandeur pour l'élasticité par rapport au taux de chômage initial. Les résultats ne permettent pas de conclure à une plus grande rigidité salariale en Allemagne. Les relations de travail sont plus longues, et de surcroît plus stables et moins flexibles en Allemagne, mais la plus grande flexibilité des salaires par rapport aux conditions contemporaines pourrait être compensatoire.

Ayant constaté au chapitre 2 que les salaires allemands, pour un emploi donné, ne sont pas moins flexibles que les salaires américains, les chapitres 3 et 4 se concentrent sur la mobilité des travailleurs et sa relation avec la formation de la main-d'œuvre. Tandis que beaucoup d'écrits portent sur l'effet de la formation sur le salaire des travailleurs - recensés au chapitre 1 -, peu connu est son effet sur la mobilité. De plus, si la littérature est abondante au regard du système d'apprentissage en Allemagne, il n'en est pas de

même quant à la formation fournie après la fin de l'apprentissage ou de la scolarité en entreprise.

En partant du fait que les Allemands ont des relations d'emploi plus longues que les Américains, on pourrait conclure que les travailleurs allemands reçoivent moins fréquemment une formation plus longue, cette dernière correspondant alors à l'apprentissage. Les données analysées au chapitre 4 nous informent autrement. Il découle de l'analyse du Panel Socio-Économique Allemand que la formation formelle post-apprentissage en entreprise est plus fréquente en Allemagne qu'une formation équivalente aux États-Unis. Au lieu de réduire la formation tardive, l'apprentissage semblerait fournir une base pour un processus d'accumulation continuelle de connaissances.

Comparant les résultats américains sur l'effet de mobilité de la formation avec leur pendant allemand, la formation en elle-même ne semble guère être plus spécifique dans un pays que dans l'autre. Dans les deux cas, et ce résultat est nouveau dans cette littérature, la formation semblerait fournir un capital humain spécifique non à une seule firme, mais spécifique à un secteur. Un travailleur ayant reçu une formation par l'intermédiaire de son employeur ne resterait pas nécessairement plus longtemps avec sa firme formatrice. Par contre, il est plus probable de trouver un emploi dans le même secteur lors d'une séparation de son employeur formateur.

La formation formelle en milieu de travail est alors un outil économique à double tranchant. D'une part, elle n'entrave pas la mobilité des travailleurs entre les firmes dans un même secteur. L'allocation de la main-d'œuvre peut se faire sans encombrement vers les firmes les plus productives, à condition d'être dans le «bon »secteur. D'autre part, dans la même logique, un travailleur dans un secteur en déclin serait moins susceptible de trouver un emploi ailleurs, et l'allocation sectorielle se verrait alors freiné.

Est-ce que ces résultats nous aident à comprendre les différences de performance des marchés du travail, au moins celles entre l'Allemagne et les États-Unis ?

Deux conclusions générales peuvent être tirées à ce stade. Premièrement, la similitude des résultats concernant la flexibilité des salaires et les effets de la formation sur la mobilité de la main-d'œuvre pour deux pays aux institutions économiques très différentes permet de conclure que les mécanismes sous-jacents ne sont pas particuliers à chaque pays. Deuxièmement, la performance visiblement différente à l'heure actuelle peut quand même être la conséquence de politiques économiques qu'ignorent ces mécanismes, à leur péril d'ailleurs. Ainsi, soutenir la formation de la main-d'œuvre en période de changement structurel peut nuire davantage à l'économie que ça ne rapporte en bénéfice aux travailleurs.

La flexibilité des salaires et la mobilité de la main-d'œuvre ne sont que deux constituantes d'un marché du travail flexible. Parmi les nombreux autres paramètres pouvant être ajustés, ne mentionnons que les heures de travail et l'allocation des tâches à l'intérieur d'une firme. La réduction des heures de travail ayant eu lieu en Europe dans les années 1980 était souvent - mais pas toujours - accompagnée d'une flexibilisation des règles régissant l'allocation intertemporelle du travail. La simple arithmétique, à savoir que la réduction des heures de travail, pour une quantité de travail donnée, augmenterait le nombre de travailleurs, est certainement fausse, puisqu'elle néglige l'ajustement suite aux nouveaux prix. Par contre, il est également trop simple d'affirmer que la réduction des heures augmente le chômage puisqu'elle augmente les coûts fixes. L'allègement des règles d'allocation des heures de travail pourrait bien être l'avantage crucial, et non la réduction totale des heures. Quant à l'allocation des tâches, une des mesures observables aux économistes sont les changements de position à l'intérieur de l'entreprise, que ce soit des changements horizontaux ou verticaux. Lié aux études présentées ici, un capital

humain plus général peut faciliter les réallocations dans la mesure où celles-ci sont fréquemment nécessaires. À notre avis, ceci constitue une avenue intéressante de recherche.

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