Human Capital Specificity: Direct and Indirect Evidence from Canadian and US Panels and Displaced Worker Surveys

by

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HUMAN CAPITAL SPECIFICITY: DIRECT AND INDIRECT EVIDENCE FROM CANADIAN AND US PANELS AND DISPLACED WORKER SURVEYS

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Abstract

Recent papers by Neal (1995) and Parent (2000), using different methods, provided evidence in support of the hypothesis that previously estimated firm tenure effects are, in fact, capturing industry specific human capital investments due to a correlation between firm and industry tenure. This paper uses both methods applied to both US and Canadian data sets to provide evidence in support of an alternative hypothesis that human capital is, for the most part, not narrowly specific to firm or industry. An analysis using either the indirect method of Neal, or the direct approach of Parent, provides evidence against the importance of industry specific capital and in favor of broad skill based specificity.

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1 Introduction

In the extensive literature that studies the determination of earnings and earnings inequality, human capital theory has played a central role.[1] There is by now quite general agreement that human capital plays a major role in the determination of earnings. One aspect of human capital that was emphasized at a very early stage was the importance of assessing the degree of specificity. Becker's (1964) initial discussion focussed on the dichotomy between firm specific and general capital. This distinction was important because while workers had a full incentive to invest optimally in general human capital, there were potential incentive problems in financing firm specific human capital. Subsequent authors also studied the implications for turnover.[2] This initial focus on firm specific capital also led to attempts to measure the relative importance of specific capital by examining the effects of firm tenure on earnings profiles. This literature produced conflicting evidence on the magnitude of tenure effects.[3]

More recently, the whole issue of the source of the specificity has been examined. In particular it has been argued that the tenure effect may be capturing industry specific capital rather than firm specific capital. Two recent papers taken together, Neal (1995) and Parent (2000), provide evidence to suggest that industry specificity is much more important than firm specificity. The most basic measures of general human capital usually studied in the human capital earnings function literature are years of education and years of labour market experience. Analyses of firm or industry specific human capital then typically add tenure with a firm and years of experience in the current industry to capture these types of human capital. This is the

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[1]Seminal works include Becker (1964), Ben Porath (1967), Mincer (1974). See Willis (1986) for a survey.

[2] See Parsons (1986), section 4, and the references therein.

[3] See, for example, Abraham and Farber (1987); Altonji and Shakotko (1987); Topel (1991); Abowd, Kramarz, and Margolis (1999).

approach directly taken in Parent (2000), and indirectly in Neal (1995).

There is accumulating evidence that mobility has increased in the new economy.[4] Knowledge of the source of human capital specificity is increasingly important in an environment of increased mobility. The labor markets of North America are often characterized as more flexible than those of Europe. This higher degree of flexibility is one of the explanations offered for the lower unemployment rates in North America compared to Europe following the emergence of the new economy. However, the mobility of workers that creates this flexibility can also have a cost in the form of destruction of specific human capital. If a large part of a country's human capital was industry specific, then some of the gain that would occur from the reallocation of workers in a flexible labor market from declining industries to growth industries would be offset by the destruction of industry specific human capital. Large industry specific investments would in fact tend to "lock in" labor to particular industries making adjustment difficult.

In this paper we investigate the hypothesis that human capital may not be narrowly specific, but may instead by specific only to a small number of broad basic skills. If specific human capital is specific to broad basic skills rather than narrowly specific to industry, then mobility across industries would not involve human capital destruction unless it was accompanied by mobility across broad skill categories. There are a variety of ways of defining broad skill categories. In this paper we build on the research of Ingram and Neumann (2000). Ingram and Neumann develop a measure of skill using information in the Census Population Survey (CPS) and the Dictionary of Occupational Titles (DOT) on characteristics such as verbal and mathematical ability, motor skills and strength requirements to attach to each worker in the CPS the skill level required to perform the job that he or she occupies. The DOT actually provides information on 53 characteristics. Ingram and Neumann (2000) use factor analysis to

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[4]See Kambourov and Manovskii (2003) for evidence for the United States.

combine similar characteristics into broader skill characteristics. [5]

In earlier work (Poletaev and Robinson, 2003) we re-examined the evidence from displaced worker surveys developed by Neal (1995). We used the same basic methodology that Neal employed to provide evidence for industry specificity, and showed that the evidence for industry specificity is very weak once a skill measure is included. Instead, the evidence is more consistent with a general skill concept of human capital, based on the Ingram/Neumann measure, that we employed. In fact Neal (1995) anticipates this possibility in the concluding section of his paper. "I must acknowledge the possibility that results outlined above reflect the importance of skills that are not truly specific to given industries but rather specific to a set of jobs that are associated with the intersection of certain occupations and industries." He concludes that "[f]uture research in this area must confront the task of defining job categories that directly capture important skill specificities"[6]

In this paper, we extend our earlier analysis of the indirect approach to a general evaluation of human capital specificity based on data from both the U.S. and Canada. First we update our analysis based on Neal's methodology using the most recent US displaced worker survey data and compare it to results from the 1986 Canadian Displaced Worker Survey. We characterise Neal's methodology applied to displaced worker survey data as an indirect approach to assessing industry specific capital since the displaced worker survey data set contains no direct measure of industry tenure. Second, we extend the analysis to examine evidence from direct approaches to estimating specificity that use panel data sets that contain direct measures of industry, occupation and skill tenure measures. The two most popular U.S. panel data sets, the Panel Study of Income Dynamics (PSID) and the National Longitudinal Survey of Youth (NLSY) were used by Parent (2000). Kambourov and Manovskii (2002) re-examine Parent's results with the PSID using the newly available PSID Retrospective File, and allowing for the possibility of

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[5]See Ingram and Neumann (2000) for more a detailed discussion.

[6]Neal (1995), pp. 669-670.

occupation specific capital. Parent uses a methodology that we characterise as the direct approach and presents evidence in favour of a major, if not exclusive role for industry specificity compared to firm specificity. We re-examine this U.S. evidence from the direct approach using skill measures and compare it with results obtained from the Canadian Survey of Labor and Income Dynamics (SLID).

The plan of the paper is as follows. In Section 2 we present the results from the U.S. and Canadian Displaced worker surveys. The data from both countries show consistent evidence of the importance of basic skill specificity. Evidence for industry specificity largely disappears when basic skill tenure is incorporated into the analysis. In section 3 we turn to the evidence from the panel data sets. Again, the evidence from both countries provides substantial confirmation of the importance of basic skill tenure and the sensitivity of estimated returns to industry tenure to the inclusion of basic skill tenure. In many specifications, industry tenure returns are insignificantly different from zero or quite small. There is evidence of modest returns to firm tenure, largely confined to the early years, with somewhat stronger results for the Canadian data. Section 4 presents some conclusions and an outline of future work.

2 Evidence of Specificity from Displaced Worker Surveys

Neal's (1995) analysis of industry specific capital is an indirect one. A direct approach would correspond to estimating Neal's equations (1)-(3):

$$w_1 = \alpha \text{ experience} + \theta \text{ industry tenure} + \gamma \text{ firm tenure} + X\beta + \epsilon_1 \tag{1}$$

$$w_2 = \alpha \text{ experience} + \theta \text{ industry tenure} + X\beta + \epsilon_2 \tag{2}$$

$$w_3 = \alpha \text{ experience} + X\beta + \epsilon_3 \tag{3}$$

where w_1 is the wage on the pre-displacement job, w_2 is the new wage for workers whose industry does not change after displacement (stayers), w_3 is the wage for workers who do change industry (switchers), and X is a vector of unchanging worker characteristics such as education. In Neal's analysis, the mean zero, independent, error terms capture match specific effects on productivity. If workers were moved from and to jobs exogenously, and the relevant data were available, estimates of the parameters of equations (1) - (3) would provide the required evidence on human capital specificity. For stayers the wage loss after displacement is less than the wage loss for switchers by:

$$\theta \text{ industry tenure}$$

for workers with the same X characteristics and the same firm tenure prior to displacement. Thus, if industry specific human capital is important the wage loss for switchers will be more than for stayers and the amount will be proportional to industry tenure.

Neal does not test this hypothesis directly because of possible problems of endogenous job changes and the absence of data on industry tenure in the Displaced Worker Surveys. Instead, he estimates the following equation separately for switchers and stayers:

$$\Delta \ln w = \beta_0 + \beta_1 \text{ experience} + \beta_2 \text{ experience}^2 + \beta_3 \text{ tenure} + \beta_4 \text{ tenure}^2 + Z\zeta + \epsilon \qquad (4)$$

where Δlnw is the change in the log wage between post- and pre-displacement jobs for a worker and the experience and tenure variables both refer to pre-displacement values. The vector Z is a set of controls.[7] Neal argues that since the model (1) - (3) implies that the wage cost of switching industry should vary positively with pre-displacement industry tenure, in "the absence of direct controls for industry tenure, we expect to observe positive correlations between the wage cost of switching industries and pre-displacement measures of both experience and firm tenure." (Neal,

[7] See Neal (1995), pp. 656-57, for more details.

1995: p. 657.)

The US Evidence Using the Indirect Approach

The U.S. Displaced Worker Surveys (DWS) for the years 1984, 1986, 1988 and 1990 are the data sources used in Neal (1995). Neal establishes evidence for the importance of industry specificity by showing that switchers with more pre-displacement experience or tenure will suffer a larger wage loss than switchers with less, and that this relative loss (by experience or tenure) will be smaller for stayers. In his specific example for males, one worker is displaced after working 10 years for the same employer; the other is displaced during the first year of his career. If both workers switched industry, the more experienced worker's losses are 27% greater than the less experienced. If neither worker switched industry, the more experienced worker's losses are only 13% greater than those of the less experienced.[8]

In our first test of the relative importance of industry specific capital and basic skill specific capital we re-estimated Neal's equation (4), subdividing his sample of industry switchers into those who also switched basic skill categories and those who did not. The Ingram and Neumann skill measures permit the identification of the main basic skill used in an occupation from a group of four basic skills - intelligence, fine motor skills, gross motor skills and strength. A worker switches basic skill category when he/she changes three-digit occupation in such a way that the new occupation has a different main basic skill. If industry is the important source of specificity then the subdivided sample should yield similar results for the skill switchers and

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[8] Neal categorizes the results for females as "not so dramatic". In fact, the point estimates yield relative losses for the more experienced worker of 19% for the switchers and 7% for the stayers which shows a very similar percentage point gap between switchers and stayers to that for the males. Neal's concern is with the statistically insignificant coefficients on the experience variables for females which he conjectures may be a noisier measure for females and hence less correlated with industry tenure. It may also be due to a different relationship between experience and tenure for females than for males. These two variables are generally positively, and often highly correlated, so that estimates of the partial effect of experience, holding tenure constant may be very sensitive to the particular reasons for the "independent" variation.

stayers in the sense that both should show larger relative losses than the industry stayers. If industry is relatively unimportant and basic skill specificity matters, the industry switchers who also switched skill should have greater relative losses for the more experienced workers than the industry switchers who did not switch skills. The data set was constructed to exactly replicate Neal's data set.[9]

The results comparing industry switchers with industry stayers are as reported in Neal. However, the estimates from the subsamples within industry switchers are not what would be expected if industry specificity was the dominant feature, but instead show a pattern that favours the relative importance of basic skill specificity. Following Neal's method of comparing the relative wage losses for the more and less experienced workers, within industry switchers the skill switchers have a much larger loss than the skill stayers. The results are given in the top half of Table 1.[10] In the last column we replicate the experiment reported in Neal (1995) that compares a worker who is displaced after 10 years with the same firm (experience = 10, tenure = 10) with a worker who is displaced in the first year of employment (experience = 0, tenure = 0). In the data the average displaced worker has about 15 years of experience and 5 years of tenure. This leads us to prefer a comparison of an inexperienced worker with the more typical hypothetical worker presented in the first column. An intermediate case is presented in the middle column.

The results are quite striking. Comparing industry switchers and stayers, the relative losses for the more experienced worker are at least twice as large for the switchers as for the stayers. In the first column, for example, the loss for the industry switchers is -.2369 compared to -.1237 for the industry stayers. This is Neal's basic result. However, the large relative loss for the

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[9] We are grateful to Derek Neal who provided us with his original code to make this possible.

[10] The sample is the same as Neal (1995) except for the exclusion of 2.8% of the observations which were top-coded. The results are almost identical if these observations are included.

industry switchers is largely due to the larger losses of the industry switchers who also switch skill. This is especially so for the more representative comparisons in the first and second column. In the first column, for example, the loss is doubled (-.3306 vs. -.1647) if there is a skill switch. Similarly, the low relative loss for the average industry stayer is almost entirely due to the small losses of the industry stayers who are also skill stayers. Indeed, the industry stayers who switch skill have the same losses as the average industry switcher (-.2651 vs. -.2369).

To pursue these differences further, the sample was re-divided into basic skill switchers and stayers and this time the basic skill switchers are subdivided into industry switchers and industry stayers. The estimates are given in the bottom half of Table 1. The magnitude of the difference between skill switchers and skill stayers is substantially larger than the difference between industry switchers and stayers in the more representative first and second columns. In the first column, for example, the loss for the skill switcher is -.3070, while the loss for the skill stayer is -.1209. Whereas the industry stayer who switched skill had the same losses as the average industry switcher, the skill stayer who switched industry suffers a much smaller loss than the average skill switcher. In the first and second columns the losses of the average skill switcher are about double those of the skill stayer than switches industry. The evidence in Table 1 shows a consistent ranking of losses - skill switchers losses are largest, irrespective of whether they switch industry, and skill stayers are the smallest, irrespective of whether they switch industry. These conclusions continue to hold if the data set is updated to 2000. The results are reported in Table 2. They show very similar estimates with the same pattern and relative loss rankings as Table 1.

Neal subjected his basic OLS results to various robustness checks including modelling the selection into industry switcher or stayer status. Using the same selection model for industry switching, we re-estimated the wage equations as in Neal (1995) with the further subdivision into skill switcher or skill stayer. We used Neal's selection model for industry, but did not specify any selection process for occupation switches.[11] The results are presented in Table 3. The results are

[11]Neal's selection model uses primarily the level and growth rate of the predisplacement industry as selection variables, though in this section of the paper (Table 3) predisplacement

broadly similar to those in Table 1. Again the large relative losses for the average industry switcher come primarily from the worker who also switches skill, and the small relative losses for the industry stayers again come largely from those who also stay in the same skill. The relative loss rankings are also broadly similar, although the typically larger relative losses for the skill switchers are less exaggerated. Overall, our analysis using Neal's basic indirect methods suggests that the evidence advanced by Neal (1995) to support the hypothesis of important industry specificity is more supportive of skill specificity with industry specificity playing a more minor role.

The coefficient estimates from which the results in Table 1 were derived are reported in Poletaev and Robinson (2003). These show an interesting difference in the results for the simple comparison of switchers and stayers of basic skill compared to industry. The larger losses for the switcher who is more experienced shows up in the individual coefficients for both firm tenure and general experience for the industry switcher, but only through general experience for the skill switcher. To the extent that basic skills survive across firm switches more frequently than industry, and that firm tenure and experience are themselves correlated, this result is not surprising. In fact, there arise some general difficulties in interpreting the differences in the firm tenure and general experience coefficients separately. Workers of the same experience with shorter firm tenure may be more mobile or less stable workers that differ in other ways. In addition, in the indirect approach, firm tenure and experience are both imperfect but correlated proxies for unobserved industry tenure. Neal's experiment considers the joint movement in firm tenure and general experience where the more "experienced" worker has more of both. Our analysis uses the same experiment.

Neal's paper concluded with a final piece of evidence of the importance of industry

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industry employment growth is also added to the wage equation. Our re-analysis was conducted under the null hypothesis that industry specificity was important and skill was not. The group of industry switchers who also switch skill are then a random sample of all industry switchers. Neal's selection model is then applied to a data set consisting of all the industry stayers in the DWS sample and a random sample of the industry switchers in the DWS sample.

specificity by showing that for both industry stayers and switchers, post-displacement wages are positively correlated with pre-displacement tenure but that this link is stronger among displaced workers who stay in their pre-displacement industry. This result is replicated in Parent (2000) using the NLSY and the PSID. As occurs with Neal's main source of evidence, incorporating the basic skill measure into the alternative approach changes the picture. Again, the evidence for industry specificity is weakened, though this approach provides a less clear picture of the relative roles of industry and skill.

Table 4a presents our re-examination of this form of evidence using our basic skill measure. The first column presents a standard log wage regression using the pre-displacement wage as the dependent variable to establish a benchmark. This regression shows significant returns to both firm tenure and experience. The remaining columns use the post-displacement wage as the dependent variable but splits the sample into industry switchers and industry stayers. Neal argues that if you stay in the industry you were displaced from you should receive a return from your industry specific capital. Since there is no direct measure of this in the regression the effect will be picked up by pre-displacement job tenure and experience. Those that switch industry will not have this effect so the coefficients on pre-displacement tenure and experience for the industry stayers will be higher. The second and fifth columns show that this is in fact the case. Both pre-displacement experience and job tenure have larger effects for the industry stayers.

However, subdividing the industry stayers into skill switchers and stayers casts some doubt on this evidence of industry specific effects. In particular, the large effect of tenure for the industry stayer is due mainly to the sub-group who are also skill stayers. Similarly, the small effect of experience for industry switchers is largely due to the subgroup who also switch skill. The overall effects are given in Table 4b. The pattern of results is the same at the three points of evaluation. For the most representative group (first column), the estimates clearly show the larger effect for the industry stayer (.3878 vs. .2088). However, the breakdown by skill indicates that the large effect for the industry stayer is primarily from the sub-group of skill stayers (.3959 vs.

.2905). Similarly, the small effect for industry switchers is primarily due to the sub-group of skill switchers (.1358 vs. .2226).

Neal emphasizes the importance of the large effect of pre-displacement tenure on post-displacement wages for industry stayers, noting that it is almost as large as the current firm tenure return in the cross section regression in Table 4a, column 1. He notes that this is difficult to reconcile with models of firm specific investment. However, since the effect is primarily through the sub-group of skill stayers, it is not clear that this should be taken as convincing evidence of industry specific capital even if it casts doubt on substantial firm specific capital. Given that the sub-divided analysis was carried out under the null hypothesis of no importance for skill change status given industry status, (i.e. no model was specified for skill status changes) and the imprecision of some of the estimates, it is difficult to assess the relative role of skill and industry. However, it again appears clear that industry effects are probably picking up at least some, if not primarily, skill effects.

The Canadian Evidence Using the Indirect Approach

The displaced worker survey data used in Neal (1995) was obtained by pooling the January 1984, 1986, 1988 and 1990 surveys that were conducted as supplements to the monthly Current Population Surveys in those months. The displacement data come from the response to the question asked of all persons 20 years and older if they had "lost or left a job because of plant closings, and employer going out of business, a layoff from which [the worker] was not recalled or other similar reasons" in the 5 years preceding the survey date. The pooled data set resulted in a sample size of 2641 for men and 1491 for women. The data set for Canada is the Survey of Displaced Workers, 1986, obtained as a supplement to the monthly Canadian Labor Force Survey. As with the U.S. data, the survey month is January and the target population is persons 20 years and over. There were a sequence of questions that use similar wording to the U.S. question to obtain the displacement information. Three questions have the same form. The person is asked if in the past 5 years he/she: " has ... been laid off from a job from which he/she was not recalled"; "has lost or left a job because of an employer going out of business"; "has lost or left a job because of a plant closing or moving." Thus the U.S. and Canadian data appear closely comparable.

Unfortunately, however, unlike the U.S., the Canadian data is restricted to the 1986 survey since no further surveys were carried out. This results in a male sample size for the Canadian analysis that is smaller than can be obtained from United States data where a series of displaced worker surveys have been carried out. The smaller sample size, makes it impossible to construct an identical sample to that used by Neal. In particular, it was necessary to use all the displaced workers (except those who had an own business failure) rather than those displaced by plant closure or moving. As a check, we repeated Neal's analysis of the US data using the broader definition of displaced worker and found his results to be insensitive to the alternative definitions.

Table 5 reports the results of the indirect approach applied to the Canadian data. The Neal analysis, contrasting industry switchers and stayers is straightforward to carry out on the Canadian data. However, the subdivision into skill switchers and stayers is more complicated. It was not possible to construct the equivalent skill measure in the DWS for Canada since the skill measure is based on (U.S. 3-digit) occupation codes. As an approximation, we substitute skill switchers and stayers by occupation switchers and stayers, measured at the Canadian two-digit level. The results for industry status are presented in the top half of Table 5. Contrasting industry switchers and stayers, the results of Neal (1995) are confirmed in the Canadian data. The relative loss of the more experienced worker analysis is much greater for industry switchers than for industry stayers. For the comparison closest to the data means (E = 15, T = 5) the relative loss is more than double for the industry switcher (.2061 vs. .0872).

A parallel analysis of the relative losses of occupation switchers and stayers is reported in the lower part of Table 5. The losses for all groups are much larger for the occupation switcher than for the occupation stayer. If the occupation switchers are split into industry switchers and industry stayers they both lose more than the occupation stayer and using the preferred group (E = 15; T = 5), the loss is the same whether industry is switched or not. Splitting the occupational stayers, losses are negligible, and for all groups are unrelated to whether industry is switched or not.

The coefficients from which the results in Table 5 were derived are presented in Tables 6 and 7. Given the smaller sample size and lack of precision in the estimates in the quadratic specification for the Canadian data, the more precisely estimated linear approximations are presented in the lower half of the tables. In contrasting industry switchers and stayers alone (Table 6, cols. 1 & 4), the greater losses for the more experienced workers at the mean appear primarily through the firm tenure coefficients, in contrast to the U.S. data where it appears more evenly through both experience and tenure. In contrasting skill switchers and stayers the effect occurs more evenly through experience and tenure, unlike the U.S. where it occurs only through experience. This may be due to the use of occupation as a proxy for the skill measure in the Canadian data.

Overall, the evidence from the Canadian data reinforces the conclusions of Poletaev and Robinson (2003) that the evidence for industry specificity from the indirect approach is substantially weakened when skill is taken into account. The data are much more consistent with the hypothesis that human capital is specific to broad skill groups rather than to industry. This is directly evident from the U.S. data, where a direct measure of basic skill is used, and indirectly from the Canadian data where 2-digit occupation is used as a proxy for basic skill.

3 Evidence for Industry Specificity from Panel Data

Parent (2000) takes a direct approach to identifying sources of specificity, similar to Neal's equations (1) - (3). His basic statistical model is:

$$\ln w_{ijkt} = \beta_0\, OJ_{ijt} + \beta_1\, T_{ijt} + \beta_2\, Exp_{it} + \beta_3\, Expind_{ikt} + \alpha_i + \theta_{ij} + \gamma_{ik} + \epsilon_{ijt} \qquad (5)$$

where w_{ijkt} is the real hourly wage of person I in job j in industry k at time t, T is firm tenure, Exp is experience, and Expind is experience in the current industry. Parent adds the additional variable, OJ, equal to one if firm tenure is greater than one to allow for expected nonlinearities in the firm tenure effect. He also includes higher order polynomial terms in the experience and tenure variables as well as other “controls”. Unlike Neal’s data set, measures of all the tenure and experience variables are directly available. However, selection effects, while possibly mitigated to some degree in Neal’s displaced worker data if the displacements can be viewed as exogenous, are very likely to be present in the NLSY and PSID given the endogenous nature of job choices envisaged by standard models of worker behaviour. Parent attempts to deal with these problems via an instrumental variables (IV) methodology.[12]

An additional problem for the direct approach, stemming from the panel data nature of the data used, is the bias induced by measurement error in the job histories. It is well recognized in the literature that the data sets employed by Parent - the PSID and NLSY - have problems in this regard that complicate the computation of accurate firm, industry or occupation tenure variables.[13] Parent employs a procedure (Partition T) based on Brown and Light (1992) to identify true firm tenure in the PSID. In essence this involves identifying a firm switch when the reported length of employment at a given firm is smaller than the time elapsed since the last interview. This is not necessary with the NLSY. However, both data sets suffer from possible miscoding of switches in industry or occupation because there may be some variation in how coders employed at different times code similar job descriptions. Neal (1999) examines job mobility in the NLSY and argues in favour of considering industry switches unaccompanied by changes in employer to be spurious. Parent (2000) follows this procedure for both the PSID and the NLSY.

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[12] See Parent (2000) for exact details and discussion of the limitations of the approach.

[13] See, in particular, Brown and Light (1992).

Parent's strategy is to estimate (5) with and without the industry experience variables and note the changes to the effect of firm tenure (β_1). Having obtained a preferred specification for equation (5), returns to firm and industry tenure are derived from the estimated coefficients. In all cases of estimation technique and industry definition, and across both the NLSY and PSID data sets, Parent shows that inclusion of the industry experience variables greatly reduces the firm tenure effect which, in the IV estimates, becomes insignificantly different from zero. Two forms of industry tenure are calculated - continuous, which is set to zero as soon as an industry switch occurs, and non-continuous which is cumulative.[14] The industry tenure variables which are added to the equation (5) are always highly significant. Parent concludes that the significant results for firm tenure are spurious and result from the correlation between firm tenure and industry tenure.

Analysis of the NLSY

Table 8 reproduces the key results from Parent's analysis of the NLSY. The results in column 1 show that without industry tenure, firm tenure has a significant effect, though the marginal effect is essentially zero after 5 years and the 5 year cumulative return is a reasonably modest 6%. The remaining columns show that including one or three digit industry tenure, either in continuous or non-continuous form eliminates the firm tenure effect and results in a significantly positive industry tenure effect ranging from 8.18% to 15.46% over five years, depending on the tenure definition and degree of industry aggregation. The industry tenure cumulative returns and standard errors are taken directly from Parent (2000), Table 7. The firm tenure returns are those implied by the coefficients in Parent (2000), Table 4. The standard errors on the coefficients suggest that the firm returns are significantly positive when industry is excluded, but zero or significantly negative when industry is included.

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[14]The continuous measure counts only the current tenure spell in the industry, while the non-continuous measure includes earlier spells in the same industry. See Parent (2000) for discussion of the two measures.

Exact replication of Parent's analysis was impossible because of the different sources of the raw NLSY data available now and those used by Parent. In addition, the sample selection criteria are complicated. We obtained a sample from the NLSY as close as possible to that used by Parent.[15] Using this sample, and imposing Parent's condition that industry switches cannot occur without employer switches, we were able to approximate the results of Parent for the firm and industry comparisons. The cumulative returns for firm and industry are presented in the first five columns of Table 9.[16] Overall the results are very similar to those in Parent (2000). In the absence of industry tenure there is a significant, positive effect of firm tenure, though the effect is quite modest. At five years the cumulated returns are 6.59% in Table 9 compared to 5.99% implied by the coefficient estimates in Parent (2000). When industry tenure is added, firm tenure effects become zero or significantly negative, while five year industry tenure cumulative returns are significantly positive and range from 7.10% to 13.56% compared to 8.18% to 15.46% in Parent (2000), Table 7.

The remaining columns of Table 9 report the effects of including skill tenure in the analysis. Skill tenure is defined analogously to industry tenure, representing the time spent in an occupation or occupations that use the same main basic skill. The same criteria as used for a valid industry switch is applied to occupation switches. First, when skill is included in place of industry (columns 6 & 7) the effect is very similar to including industry. The significantly positive firm tenure returns are greatly reduced and skill tenure returns themselves are significantly positive with magnitudes similar to those estimated for industry tenure. Five year continuous industry tenure cumulative returns are estimated as 13.56% and 12.24% when one- and three-digit industry is used compared to 12.16% for skill. Five year non-continuous industry

[15]We are grateful to Daniel Parent for supplying details not available in Parent (2000) that allowed us to come as close as possible to his sample. This resulted in a sample for us of 2579 individuals compared to 2816 in Parent (2000). In our data set, however, we obtain more observations per individual.

[16]The coefficients from which the returns are obtained are presented in Tables 9a (continuous tenure) and 9b (non-continuous tenure) in the Appendix.

tenure cumulative returns are 11.14% and 7.10% compared to 7.69% for skill. One difference is that the somewhat puzzling significantly negative returns at ten years for firm tenure apparent when industry tenure is included in Parent (2000) and in the approximate replication in Table 9 is substantially reduced when skill is substituted.[17]

The final columns of Table 9 report the results when industry and skill tenure are both included. Using the continuous tenure measures skill and industry, the returns obtained from using industry or skill alone are each reduced when they are both included, though the skill returns are typically somewhat less affected. The skill returns when only skill is included are 12.16% at five years; these are reduced to 9.27% or 10.72% depending on which aggregation of industry is used. At ten years the skill return estimate by itself is 17.77% which is reduced to 14.30% or 15.77%. For one-digit industry the five and ten year return estimates by themselves are 13.56% and 17.19% which are reduced to 11.00% and 12.98%. For three-digit industry the five and ten year return estimates are reduced from 12.24% and 16.21% to 9.32% and 11.67%. However, in all cases, the inclusion of both skill and industry measures results in significantly negative returns to firm tenure, even at five years. This suggests that industry and/or skill returns may be overstated at the expense of firm tenure due to a high correlation between the underlying true firm, industry and skill tenures in the continuous measure case.

The problem of a high underlying correlation between firm, industry and skill tenure may be mitigated in a variety of ways. In the lower half of Table 9, the results are presented when industry and skill tenures are constructed on a non-continuous basis, breaking the link in many cases with firm tenure. The effect on firm tenure is clear. The puzzle of significantly negative returns to firm tenure is essentially eliminated. When industry tenure is included without skill tenure, firm tenure effects are small or insignificantly different from zero. Compared to the continuous case, the estimated industry returns are somewhat smaller at the one-digit level and

[17]One possibility for the negative result is that it represents an approximation error from extrapolating the effects based in the quadratic coefficients to a point significantly beyond the mean firm tenure in the sample.

halved at the three-digit level. When skill tenure is included without industry, firm tenure effects are reduced, but are never negative and the skill returns are reduced. When industry and skill tenure are included together, firm tenure effects are insignificantly different from zero in all cases at the two and five year points, with only the one-digit industry case estimated at the ten year point showing a significantly negative estimate for firm tenure. This estimate itself is quite small (-4.25%) compared to the large effect estimated in the continuous case (-12.84%). The three-digit industry case may lower the correlation further since it shows a reduction from the very puzzling -15.03% effect for firm tenure at the ten year point in the continuous case to a small value, -2.79%, that is insignificantly different from zero in the non-continuous case.

The estimated returns to industry and skill tenure in the non-continuous case depend on the degree of industry aggregation. At the one digit level, industry and skill return estimates when each are included alone are both reduced, with skill reduced marginally more. At the three digit level, there is a substantial reduction in both with industry reduced more. At the five year point, without including skill the industry return estimate would be 36.54% too high at 7.10% instead of 5.20%; without including industry tenure the skill return would be 19.22% too high at 7.69% instead of 6.45%. At the ten year point, the industry return estimate would be 52.72% too high and the skill return estimate would be 15.97% too high. The correlation between firm, industry and skill tenure obviously means that the results should be interpreted with caution. To the extent that this correlation is reduced in the three-digit case, which eliminates any negative estimates for firm tenure, the results suggest that a substantial part of previously estimated industry returns may be due to a downward bias in firm tenure returns and a correlation with skill tenure.

Parent (2000) argues that the model should apply primarily to individuals with a full commitment to the labor market. Time spent with a firm, industry or skill is obviously an imperfect proxy for the amount invested in any specific capital associated with the firm, industry or skill. Individuals with similar levels of labor market “commitment” as assumed to have a similar level of investment. An alternative definition of general and specific human capital that incorporates this idea was employed as follows. Individuals are assumed to accumulate general or specific human capital in proportion to time spent only after three consecutive interviews showing full time employment. In the absence of this commitment level, human capital is not accumulated. The estimates of cumulative returns to firm, industry and skill tenure obtained using this definition are reported in Table 10.[18] Estimates of large negative returns to firm tenure in Table 9 were primarily a problem when continuous tenure was used. The use of non-continuous tenure largely eliminated the problem. The same is true of Table 10. The results in Tables 9 & 10 are in fact quite similar. The main systematic difference between Tables 9 and 10 is a modest increase in the relative returns to skill compared to industry.

The overall results in Table 10 reinforce the evidence from Table 9 that industry tenure returns are over-estimated when skill is excluded, especially for industry at the three-digit level. At the three-digit level, when both industry and skill are included, the cumulative five year industry returns are insignificantly different from zero when a non-continuous tenure definition is used compared to 5.06% when skill is excluded. When a continuous measure is used the estimated industry returns are significant but over-estimated by almost 40% if skill is excluded. The estimated returns to skill tenure are about 50% larger than industry tenure returns at the three digit level for both forms of tenure definition. At the one digit level the returns are about equal when a continuous tenure is used. Industry returns are larger only when the non-continuous measure is used in the one-digit case.

As noted above, Parent's measure of industry tenure does not permit a change of industry to occur without a change of employer. Parent notes that "the data show that it may not always be the case: there are workers who declare that they are changing industries while not changing employers. Whether these are true industry changes or just mis-classification errors is not clear."[19] However, the insignificant results that Parent obtains for firm tenure depend on the

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[18]The coefficient estimates from which the returns are calculated are reported in Tables 10a & 10b in the Appendix.

[19]Parent (2000), p. 310. For a discussion of the issue, Parent refers the reader to Neal (1999) who does not present any direct evidence of mis-coding but because there are also

validity of this assumption. If the restriction is lifted, and workers are permitted to change industries within an employment spell, the cumulative 5 returns to firm tenure always remain significant and the magnitude is less sensitive to the inclusion of industry tenure. Simple simulations (available from the authors) show that the firm tenure effect can be strongly biased towards zero, especially when the true effect is small relative to the true industry effect. There is thus a tradeoff that is difficult to evaluate when Parent's assumption is imposed: it may avoid possible measurement error in industry/occupation tenure which would otherwise bias their coefficients towards zero, but it may also bias the firm tenure effect towards zero.

Since the NLSY results for firm tenure are dependent on the validity of the assumptions imposed to deal with spurious coding changes in industry or occupation, it is worthwhile pursuing the issue further. The PSID Retrospective Files provide some evidence on the issue. Kambourov and Manovskii (2002) - hereafter, KM - conduct a detailed analysis of invalid industry and occupation switches in the PSID. Since different coders may be used in different years, there is an obvious risk for similar industry or occupation descriptions given a year apart by someone who has changed neither industry or occupation may nevertheless be coded differently in the two years. The PSID Retrospective Files eliminate this problem by having a single coder code all of the observations across the years for any given individual in the sample. KM argue that the Retrospective Files provide the means to distinguish valid from invalid switches in industry or occupation. They provide evidence to suggest that the codes in the Retrospective Files may be treated as the true codes. Under this assumption, KM show that of the "switches" in industry or occupation identified by using the "raw" industry or occupation codes without any adjustment only about half of the switches are valid even at the one-digit level. In view of this, and the fact that the number of valid industry switches within an employment spell is relatively small, Parent's procedure appears reasonable. However, because of the correlation

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missing industry codes adopted the following strategy "to address the problems of coding errors and missing data. In an effort to minimize the number of false transitions implied by the industry codes, I edited codes that imply a change in industry affiliation within a continuous employment spell associated with a single firm. In the edited data, the first industry code reported for a given employer is always the industry code associated with that employer." Neal (1999), p. 244.

between industry and firm tenure, simple simulations can show that the imposition of Parent's restriction even with a small number of valid industry switches can cause substantial downward biases for firm tenure and upward biases for industry tenure.[20]

Analysis of the PSID

Parent also estimated his model on the PSID for the period 1981-1991. In contrast to the NLSY, this presents a problem of measurement for industry tenure for workers who have been employed before 1981. Parent's solution is to assign the reported firm tenure in 1981 as equivalent to industry tenure. As with the NLSY, industry codes that changed over time without a change of employer were considered spurious changes. The PSID data replicate the qualitative effects of adding industry tenure variables obtained from the NLSY in that again the firm tenure effect is substantially reduced and often becomes insignificant.

The PSID has also been analysed by KM, using the same time period as Parent, but also utilizing the newly available retrospective PSID data which were obtained by having industry and occupation recoded by a single person across all observations for the same individual for the period 1968-1980. This recoding dramatically reduced the number of switches especially for occupation. Use of the retrospective data eliminates the need to assign the reported firm tenure in 1981 as equivalent to industry tenure. The results of KM support Parent's conclusion of insignificant firm tenure effects but cast substantial doubt on the importance of industry tenure in favour of occupation tenure. KM examine various methods of identifying true occupation (industry) switches. Brown and Light (1992) proposed a method called partition T to identify an employer switch whenever the reported length of the present employment is smaller than the time elapsed since the last interview date. KM use this approach to calculating firm tenure and follow Parent in not permitting an industry change unaccompanied by an employer change. In addition, they apply the same rule for occupations. While applying the rule for industry changes appears in

[20]Examples are available from the authors on request.

the literature to be relatively uncontroversial there has not previously been made a case for applying it to occupations since it rules out career ladders within firms. KM make the case that without this restriction, occupation tenure will remain too noisy. However, as noted above, even the industry restriction in the case where there are only a small number of cases where the is a true industry change without an employer change, can cause substantial biases. In the case of occupation where there may be a larger number of true occupation changes without an employer change, the potential for bias is increased.

Neal (1999) argues that workers first seek a career match, then seek an employer match. Young workers thus exhibit “complex” job changes and older workers just have simple employer changes without changing careers. He examines the NLSY. He notes the problem of possible invalid industry/occupation switches and does not permit an industry switch to occur within a continuous spell with the same employer. He does *not* do this with occupation. He considers that job changes within an employer are ones that involve the new job being able to use most of the skills of the old - i.e. he assumes there can be no search within employers across what we call basic skills. He argues that in any case these could not be identified in the data because of errors in coding occupation. He notes that two thirds of the one digit occupation changes with the same employer were cyclic patterns of a worker alternating 2 codes. Among white collar workers this is in and out of a management aspect rather than a change in a line of work. Neal concludes that the NLSY occupation codes are quite noisy and that he knows of no satisfactory way to clean them.

Our analysis uses the PSID data of KM. These data are similar to those of Parent and can replicate the basic features of Parent’s results. KM identify industry and occupation switches up to 1980 using the Retrospective File information. After 1980 switches are identified from applying variations on Partition T to identify firm tenure and requiring genuine industry or occupation changes to be accompanied by employer changes. The availability of the Retrospective File data permit a more accurate calculation of industry or occupation tenure prior to 1981 than was possible for Parent who had to assign industry tenure for all those present in the panel in 1981 as identically equal to firm tenure.[21] Despite this change in the initial industry tenure measure, as reported in KM it is possible to replicate the basic features of the results of Parent (2000) , especially for the one-digit industry codes.

The estimated cumulative returns to firm, industry and skill tenure using the PSID sample are reported in Table 11.[22] Without any industry or skill tenure variables a significant positive firm tenure effect is estimated (Table 12, column 1). The coefficients on the dummy variable equal to one if firm tenure is greater than one year and on the quadratic terms for firm tenure are in fact quite similar to those of Parent (2000).[23] Adding the continuous industry tenure variables reduces the firm tenure effect - halving it in the one-digit case - and results in significant industry tenure returns of a very similar magnitude to those of Parent (2000) for the one digit case, though substantially smaller for the three-digit case.[24] The industry tenure is calculated from the retrospective file data, but subsequent calculations proceed in the same way as in Parent (2000) which does not permit an industry change without a firm change. The main difference in the results is that, unlike Parent (2000), the firm tenure returns remain significantly positive.

[21]See Kabourov and Manovskii (2002) for a detailed discussion of the sample construction and the relation to Parent's sample.

[22]The estimated coefficients of the earnings functions from which the returns are derived are given in Appendix Tables 11a (one-digit) & 11b (three-digit).

[23]See Tables 11a & 11b in the Appendix. Compared to the NLSY, the PSID has a much larger coefficient on the dummy variable indicating more than one year of firm tenure. This may be due to the fact that the range of firm tenure over which the functional form is estimated is much smaller in the NLSY - a range over which firm tenure effects are believed to be increasing at their fastest rate - than in the PSID. If the true profile for firm tenure effects becomes flat at some point, the quadratic form that has to fit this will not turn down "fast enough" in the PSID data at the beginning so that more of the effect will be captured by the dummy variable.

[24]For the one-digit result, the coefficients in Parent (2000) for the linear, quadratic and cubic terms for industry tenure are .0211, -.11 and .0021 versus .0191, -.10 and .0014 in Table 11a results. The cumulative returns for 2, 5 and 10 years are .0348, .0742 and .1077 in Table 11 compared to .0378, .0792 and .1144 in Parent's Table 7.

The remaining columns of Table 11 report the returns when the basic skill tenure measure is included. If basic skill tenure is substituted for industry tenure it has a similar effect on firm tenure.[25] The skill tenure itself is positive and significant and the firm tenure returns are substantially reduced, though remain significantly different from zero. If firm, industry and skill tenure are all included, the firm tenure effect is reduced further but in many cases remain significantly different from zero. Industry and skill tenure returns follow a similar pattern to the NLSY results. Skill tenure returns are always significant. They are reduced relative to when industry tenure is excluded, but the reductions are relatively modest, in the range of 11-33%. Industry tenure returns are insignificant at the 3-digit level and are generally reduced when skill tenure is included. The reduction is more dramatic than for skill - in the range of 21-73%. These results are broadly consistent with KM who compare firm, industry and occupation tenure, though in their preferred specification neither firm nor industry tenure remain significant if occupation tenure is included except at the one-digit level.

Analysis of the SLID

The closest equivalent panel for Canada is the SLID. Given the age structure of SLID, it is closer to the PSID than to the NLSY. The major difference is the much reduced length of the SLID panel relative to the US panels. Using the SLID it is straightforward to follow the approaches of Parent and KM using Canadian industry and occupation codes which are constructed in a manner similar to those of the US. Thus the relative importance of firm, industry and occupation specific capital can be examined for Canada in this way.[26]

It is more difficult to assess the role of the broad skill measure because of the difficulty of comparing occupational codes across countries. The Neumann-Ingram measure is based on US

[25]The skill measure is derived from occupation codes that, as in KM, are not permitted to change without a firm change.

[26]Full results on this comparison are available in Poletaev and Robinson (2003).

census codes up to 1990. A preliminary concordance between US and Canadian SOC codes for 2000 was provided to us by the Standards Division of Statistics Canada.[27] We have then attempted to construct the skill measure based on this concordance and our own mapping of codes from 2000 to 1990. For precise counts of detailed occupations these methods would be subject to large errors because of the substantial changes in SOC classification in the US. This classification had remained unchanged between 1980 and 1998. However, for the main skill measure the errors are likely to be much smaller as many "equivalent" occupations produced by our method, that are not strictly equivalent because of the classification changes, will tend to be similar in the skills they use.

Table 12 reports the cumulative returns to firm, industry and occupation estimated on the first two waves of overlapping SLID panels covering 1993-2000 using an aggregated industry coding consisting of 21 industries.[28] This is an intermediate level between the one and two digit industry codes used in the PSID which have 12 and 34 industries respectively. The industry tenure variable is constructed in the same way as Parent. The SLID , like the PSID, records the start date of the current job at the first interview but not the start date of the industry or occupation of the current job. Following Parent's strategy to overcome this missing data, industry tenure is assigned equal to firm tenure for the first interview. Thereafter, industry and occupation are recorded and, provided invalid recorded changes in industry or occupation can be eliminated, industry and occupation tenure for the remainder of the job history can be accurately constructed. The shorter length of the SLID panel may thus result in less accurate measures of industry or occupation tenure because of the larger fraction of the job history for which industry and occupation tenure have to be assigned equal to firm tenure.

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[27]We are grateful to Paul Johanis, Director of the Standards Division for making the preliminary concordance available to us. No responsibility attaches to Paul Johanis or Statistics Canada for the use we have made of their preliminary work.

[28]The industry coding used in the SLID analysis is the North American Industrial Classification System (NAICS).

Finally, the same functional form as in Parent is used. This consists of a third order polynomial for industry tenure and general experience and a combination of a dummy variable to indicate more than one year and a second order polynomial for the firm tenure variable. This form for firm tenure is generally employed in the literature and is used here for easy comparison.[29] Two definitions of tenure are used. The "continuous" measure accumulates tenure in the industry (occupation) provided almost all work was done in the same industry (occupation) in the period between interviews - i.e. the tenure in the industry/occupation was almost unbroken. This is close to Parent's measure. The non-continuous measure accumulates all work in an industry (occupation) in the worker's job history.[30]

Firm tenure returns are significantly positive effect when industry and occupation tenure are excluded (first column of Table 12). The magnitudes at the five and ten year points are quite similar to the PSID, but start out higher. For firm tenure by itself the 2 year return is .0629 and the 5 year return is less than a percentage point higher at .0717, and reaches .0857 after 10 years which is only 2.3 percentage points higher than the 2 year return. Thus while there is some growth in cumulative firm tenure, the big effect occurs early on.[31] The inclusion of continuous or non-continuous industry tenure (second and third columns) flattens the profile of the firm tenure returns marginally, but does not have a major impact. Moreover, the industry tenure effect itself is quite small compared to firm tenure, only in the range of 3.11% to 3.54% for ten year cumulative returns compared to 6.73% to 7.42% for firm tenure.

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[29]See Parent (2000) for more discussion of this functional form.

[30]Parent (2000) measures continuous industry experience as the consecutive number of years a worker has been in the same industry. If the worker ever leaves the industry to go to a new job, industry tenure is reset to zero. For non-continuous tenure it is reset to the prior level reached in the same industry as the new job. The definition used here for continuous tenure is that the level is reset to zero if less than 90% of the year was spent working in the current occupation.

[31]This is clearly apparent in the estimated coefficients in the earnings equation (Appendix Table 12a) where there is a large coefficient on the "old-job" dummy variable.

The remaining columns of Table 12 introduce occupation tenure. This is constructed in the same way as KM. Occupation coding changes are considered valid only if accompanied by employer changes. As with industry tenure, this could result in a downward bias for firm tenure. The introduction of occupational tenure by itself (columns 4 - 5) reduces the firm tenure returns marginally more than was the case for industry tenure and shows a significantly positive effect of occupational tenure that is substantially larger than the industry effect. The ten year returns for occupation are double those of industry, ranging from 6.71% to 6.92% for occupation compared to 3.11% to 3.54% for industry. If both industry and occupation tenure are entered together (columns 6 - 8), the occupation returns are either strengthened or stay the same. The industry returns, however, are generally reduced and often insignificantly different from zero. The ten year returns to occupation range from 6.05% to 10.20% compared to industry returns with point estimates ranging from -4.45% to 5.41%.

The analysis was repeated for less aggregated industry and occupation codes and the results are reported in Tables 13 & 14.[32] Table 13 uses a medium level of aggregation: the industry codes cover 105 industries and the occupation codes cover 52 occupations. Table 14 uses the most disaggregated codes of around 600 industries/occupations. Introducing industry or occupation tenure typically has increasingly larger negative effects on the firm tenure returns as the codes become more detailed, though the change remains modest. The SLID data continue to show strong initial firm tenure effects after industry and/or occupation tenure is included, and the magnitude of the industry returns remains small. Introducing occupation at the intermediate grouping level produces similar results to Table 12. Occupational tenure by itself has significant positive returns, and when entered together with industry, the occupation returns remain the same but the industry returns disappear. The results are a little different at the most detailed coding level where all the returns are generally imprecisely estimated, and there is no longer evidence particularly favoring occupation returns over industry returns.

[32]The coefficient estimates from which the cumulative returns are derived are given in Appendix Tables 13a and 14a.

A basic skill measure was also analyzed using SLID, though as noted above, the construction of this measure for SLID is more complicated than for the US data sets and the basic skill tenure variables may be subject to more measurement errors. Table 15 presents the estimated cumulative returns when the basic skill tenure is included.[33] The first two columns of Table 15 report the returns for the specification which includes just firm and skill tenure. Skill tenure by itself completely flattens the firm tenure profile. The cumulative returns for 2 or 10 years are the same. This is in contrast to the results in Tables 12-14 for industry and occupation where in many cases the firm tenure profile remains increasing. The cumulative returns to skill are increasing and in the non-continuous case surpass the firm tenure returns around 7 years.

The remaining columns of Table 15 report the returns for the specification that includes firm, industry and skill tenure together. The returns are presented for three levels of industry aggregation. The firm returns remain similar, though in the continuous tenure case, evidence of an increasing cumulative return to firm tenure re-appears. Comparing industry and skill tenure, using the continuous measure industry returns are insignificantly different from zero and the skill returns are significantly positive, increasing, and larger than firm returns after the first 5-7 years. Using the non-continuous measure, skill returns are again significantly positive and large relative to firm effects after 5-7 years. Industry returns remain insignificantly different from zero except for the most disaggregated case (penultimate column) where the magnitude is surpassed by the skill return at about 8 years. Comparing the skill results with those for occupation in Tables 12-14, the skill returns are generally much steeper and typically by 10 years show larger returns than for occupation. The skill return patterns are consistent across all levels of aggregation for industry tenure.[34]

[33]Appendix Table 15a reports the coefficients from which the returns are derived.

[34]The skill definition is based on a single occupational aggregation level - an approximation to the 3-digit U.S. codes, while occupational tenure depends on the level of aggregation of the codes. Skill returns are therefore less likely to vary across levels of aggregation than occupation returns.

Overall, the SLID results show an important role for the skill measure. They are consistent with the results based directly on the Canadian occupation codes. Thus for Canada, the evidence for the importance of basic skill specific human capital over industry specific human capital is strong.[35] The estimates suggest a magnitude of ten year returns to skill tenure around 10%. In addition, the Canadian evidence shows evidence of firm specific capital even when industry, occupation or skill measures are taken into account, though the magnitude is modest and the investment is largely confined to the first year with a firm.

Comparison of Canadian and US Results

The United States panel most closely related to the SLID, given the age ranges covered, is the PSID. Comparison of the PSID results with those from the SLID shows considerable qualitative similarity, though there are interesting differences, especially in the behavior of the firm tenure effect. In the SLID data, when firm tenure is considered by itself the cumulative returns are 6.29% at 2 years, 7.17% at 5 years and 8.57% at 10 years. The magnitude of the 10 year return is similar to the PSID estimates obtained by both Parent and KM, but the time path of the return is different. In particular, the SLID estimates always show a much larger return to the first year, and then slower growth in the cumulative returns thereafter compared to the PSID.

The introduction of industry tenure in the SLID results in most cases reduces the firm tenure effects only modestly and shows quite low cumulative returns to industry tenure even after 10 years. In the SLID estimates the results are obtained at three different levels of industry aggregation and for both continuous and non-continuous definitions of industry tenure. The industry aggregation levels are not strictly comparable across the data sets. Our analysis of the SLID data used three levels of industry aggregation. The first (high) aggregation consists of 21

[35] To the extent that the estimates are subject to the same sensitivity regarding the assumptions used to identify valid industry and occupation switches, and given the difficulty of constructing the skill measure for Canada, the results should be interpreted with some caution.

industries. This is an intermediate level between the one and two digit industry codes used in the PSID which have 12 and 34 industries respectively. The second is a medium level of aggregation: the industry codes cover 105 industries and the occupation codes cover 52 occupations. The industry code is thus between the 2 and 3 digit codes used in the PSID which contain 34 and 213 industries, respectively. The occupation codes are also between the PSID 2 and 3 digit codes which contain 26 and 428 occupations, respectively. The third level uses the most disaggregated codes of around 600 industries/occupations - a more disaggregated level than even the 3 digit level used by the PSID.

In all levels of aggregation with continuous definition of industry tenure, and in all but the lowest level of aggregation with the non-continuous definition of industry tenure the introduction of industry tenure in the SLID results reduces the firm tenure effects only modestly and shows quite low cumulative returns to industry tenure even after 10 years. This contrasts with the PSID results reported by Parent, though it is closer to the PSID results obtained using the KM data set. In Parent (2000) the firm effect is always at least halved by the introduction of industry and the industry effect itself is always more than double the SLID estimates. Using the KM data set the firm effect is reduced by nearly one half in the one-digit case, but much less in the two-digit case and only marginally in the three digit case. In addition, in KM the 10 year industry returns are smaller - as low as 5% in the three-digit case. Only in the lowest aggregation with the non-continuous definition of industry tenure is the firm effect substantially changed by the introduction of industry tenure in the SLID.

The introduction of occupation or skill tenure in the SLID analysis eliminated much of the evidence of significant industry specific capital. KM report the same for their analysis of the PSID using occupation. Our analysis of the KM data using the skill measure has a similar result. In most cases industry tenure is insignificant, or if statistically significant it is of small magnitude. Only the results using the 1-digit codes produce a ten year cumulative return for industry above 5%. By contrast, consistently in both the SLID and PSID results, the ten year cumulative returns to skill are substantially above 5%. Apart from eliminating any significant evidence of industry specific human capital, the other consistent result of introducing a skill measure in the SLID analysis is the flattening of the estimated firm tenure profile. This is not the case for the PSID results. However the United States results from the PSID do show the same relative ranking of firm and skill tenure and a similar 10 year cumulative returns across the two countries.

4 Discussion and Future Work

The recent literature has provided evidence for the importance of industry tenure in a variety of ways. In addition, it has been argued that previous findings of the importance of firm specific capital were due to the neglect of incorporating industry specific capital. In this paper, the alternative methods of providing evidence on specificity have been examined using data sets for both Canada and the U.S. The hypothesis examined in the paper is that human capital, to the extent it is not completely general, is specific to a limited number of basic skills that can be used in a wide variety of contexts. There is considerable evidence that favours this hypothesis relative to one emphasizing industry specificity. Firm specific investments appear to be relatively small and confined to a short period following a worker starting with a new firm. Indirect approaches provide little evidence of industry specific capital if basic skill is taken into account. Direct approaches are typically consistent with at most a small role for industry specific capital. Strong evidence of basic skill specificity is found in all approaches and both countries.

It is important to establish the degree of specificity of human capital from a policy point of view given the evidence of increased mobility of workers in the new economy. An interesting question for any country is the extent to which its human capital is more or less specific, and what costs are associated with the human capital loss that accompanies worker turnover. For both Canada and the U.S. it appears that skill specificity is important, but industry specificity less so. An efficiently functioning labor market is continuously reallocating labor across industries in response to changing demands for different industry outputs. To the extent that only a negligible amount of human capital is industry specific, these re-allocations could take place without any major destruction of human capital, and therefore without serious negative wage consequences for the average worker, provided basic skills did not have to be switched. Although the evidence suggests that industry specific capital may be relatively unimportant, it does not mean that it is unimportant for all workers. While for the average worker the wage consequences of involuntarily moving industry may be small, the effect for a minority of individuals could be more substantial. A more disaggregated analysis would be necessary to identify such minorities.

KM's analysis of the PSID concludes with the finding that if occupational tenure is taken into account, there is little importance for industry tenure in explaining wages. They argue that this conclusion has intuitive appeal. The industry codes were not designed to reflect primarily the tasks performed by workers. "While it is true that the work setting (industry) can affect the job one performs, it seems implausible that the human capital of these workers is specific to the industry they work in rather than to the type of work they do (their occupation).. ...it appears natural to expect that when a truck driver switches industries (say, from wholesale trade to retail trade)....he loses less of his human capital generated by the truck-driving experience than when he switches his occupation and becomes a cook."[36] The argument for the importance of basic skill takes this argument one step further. The occupation codes include a wide range of distinct occupations some of which use very different skills, say heavy lifting occupations versus manual dexterity occupations, and others that use the same or very similar skills, say managerial occupations that appear in the classifications as different occupations. They also include, in some cases, progression levels in a given skill such as automobile mechanic apprentice and automobile mechanic. Presumably, the human capital learned as an automobile mechanic apprentice should not all be lost when a worker changes occupation to become an automobile mechanic.

There are problems with all the approaches to estimating specificity that are well known in the literature, primarily due to the endogenous nature of job changes. Although various statistical techniques have been used to minimize the problems, there must remain some doubt as

[36]Kambourov and Manovskii (2002), p 2.

to the validity of the estimates. The problem is compounded by the fact that the measures of the alternative forms of specificity are correlated with one another and are all measured with error. Future work should refine the measure of basic skill and to address the endogeneity directly. For the indirect approach, the use of the main skill only was sufficient for the purposes at hand. However, for estimating returns in the direct approach, it has some obvious drawbacks. For example, there may be some jobs that use approximately equal “amounts” of two skills. A worker who changes our measure of main skill in changing such jobs may in fact be using almost the same amount of both skills in a slightly different proportion, and should not be expected to lose specific human capital. A more refined measure should take this into account. Moreover, the main skill measure will also be inappropriate for some career ladders. A direct approach to the endogenity problem requires a model that incorporates the worker’s decision to invest in specific capital and to change jobs and investigates the links between them.

Empirical research on skill based specificity is still in an early stage. There remain many gaps in our knowledge. We would argue that the skill specificity is unlikely to be as narrowly defined as the United States 3-digit occupations codes as studied in KM. Not only would this rule out the skills of the automobile mechanic apprentice being useful to him/her when he/she changes 3-digit occupation to become an automobile mechanic, it would also rule out career paths across occupations where the skills acquired along the way are precisely those required for the terminal point of the path. The former is to some extent accounted for in our basic skill measure, but the latter is not. Future work that studied career paths and their evolving skill mixes in a model of endogenous worker mobility could usefully increase our knowledge of the labor market’s ability to adapt to future changes without major negative wage consequences for workers.

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Table 1

Relative Losses for More Experienced Workers After Displacement:

Neal Sample 1984-1990, United States Males

By Industry Status

	E=15; T=5	E=10; T=5	E=10; T=10
INDUSTRY SWITCHER	-.2369 (.0341)	-.1975 (.0282)	-.2639 (.0340)
Industry & Skill Switcher	-.3306 (.0504)	-.2597 (.0414)	-.3035 (.0504)
Industry Switcher & Skill Stayer	-.1647 (.0472)	-.1497 (.0391)	-.2332 (.0466)
INDUSTRY STAYER	-.1237 (.0400)	-.1050 (.0326)	-.1419 (.0389)
Industry & Skill Stayer	-.0831 (.0457)	-.0724 (.0370)	-.1070 (.0440)
Industry Stayer & Skill Switcher	-.2651 (.0906)	-.2198 (.0751)	-.2709 (.0914)

By Skill Status

SKILL SWITCHER	-.3070 (.0437)	-.2440 (.0360)	-.2901 (.0439)
Skill & Industry Switcher	-.3306 (.0504)	-.2597 (.0414)	-.3035 (.0504)
Skill Switcher & Industry Stayer	-.2651 (.0906)	-.2198 (.0751)	-.2709 (0914)
SKILL STAYER	-.1209 (.0328)	-.1086 (.0269)	-.1681 (.0321)
Skill & Industry Stayer	-.0831 (.0457)	-.0724 (.0370)	-.1070 (.0440)
Skill Stayer & Industry Switcher	-.1647 (.0472)	-.1497 (.0391)	-.2332 (.0466)

Note: E is experience in years; T is firm tenure in years; standard errors in parentheses

Table 2

Relative Losses for More Experienced Workers After Displacement

Full Sample 1984-2000, United States Males

By Industry Status

	E=15; T=5	E=10; T=5	E=10; T=10
INDUSTRY SWITCHER	-.2361 (.0330)	-.1887 (.0263)	-.2434 (.0305)
Industry & Skill Switcher	-.3240 (.0510)	-.2521 (.0407)	-.2981 (.0475)
Industry Switcher & Skill Stayer	-.1771 (.0436)	-.1471 (.0347)	-.2096 (.0398)
INDUSTRY STAYER	-.1378 (.0387)	-.1159 (.0306)	-.1572 (.0350)
Industry & Skill Stayer	-.1136 (.0445)	-.0953 (.0350)	-.1290 (.0399)
Industry Stayer & Skill Switcher	-.2427 (.0842)	-.2054 (.0674)	-.2817 (.0789)

By Skill Status

SKILL SWITCHER	-.2907 (.0437)	-.2293 (.0349)	-.2817 (.0408)
Skill & Industry Switcher	-.3240 (.0510)	-.2521 (.0407)	-.2981 (.0475)
Skill Switcher & Industry Stayer	-.2427 (.0842)	-.2054 (.0674)	-.2817 (0789)
SKILL STAYER	-.1457 (.0311)	-.1216 (.0246)	-.1702 (.0281)
Skill & Industry Stayer	-.1136 (.0445)	-.0953 (.0350)	-.1290 (.0399)
Skill Stayer & Industry Switcher	-.1771 (.0436)	-.1471 (.0347)	-.2096 (.0398)

Note: E is experience in years; T is firm tenure in years; standard errors in parentheses

Table 3

Relative Losses for More Experienced Workers After Displacement
Neal Sample 1984-1990, with selection correction, United States Males

	E=15; T=5	E=10; T=5	E=10; T=10
INDUSTRY SWITCHER	-.2728 (.0432)	-.2249 (.0345)	-.3025 (.0408)
Industry & Skill Switcher	-.4037 (.0689)	-.3114 (.0522)	-.3617 (.0614)
Industry Switcher & Skill Stayer	-.1679 (.0563)	-.1543 (.0451)	-.2491 (.0526)
INDUSTRY STAYER	-.1022 (.0469)	-.0864 (.0371)	-.1211 (.0434)
Industry & Skill Stayer	-.0674 (.0518)	-.0608 (.0409)	-.1014 (.0476)
Industry Stayer & Skill Switcher	-.2264 (.1051)	-.1834 (.0842)	-.2360 (.0976)

Note: E is experience in years; T is firm tenure in years; standard errors in parentheses.

Table 4a

Pre-Displacement Job Tenure and Experience and Wages:

Selectivity Corrected Estimates: US Males 1984-1990

	Full Sample	Industry Switcher			Industry Stayer		
		All	Skill Sw	Skill St	All	Skill Sw	Skill St
Experience	.0311	.0155	.0080	.0195	.0239	.0252	.0231
	(.0029)	(.0036)	(.0052)	(.0050)	(.0059)	(.0108)	(.0066)
Experience^2 *100	-.0596	-.0307	-.0199	-.0388	-.0389	-.0620	-.0331
	(.0079)	(.0101)	(.0145)	(.0140)	(.0155)	(.0304)	(.0173)
Tenure	.0306	.0113	.0147	.0043	.0280	.0124	.0299
	(.0035)	(.0045)	(.0065)	(.0061)	(.0073)	(.0135)	(.0082)
Tenure^2 *100	-.0765	-.0446	-.0539	-.0179	-.0952	-.0374	-.1028
	(.0129)	(.0162)	(.0240)	(.0218)	(.0267)	(.0549)	(.0290)
Schooling	.0710	.0714	.0484	.0829	.0709	.0574	.0766
	(.0037)	(.0046)	(.0081)	(.0061)	(.0075)	(.0132)	(.0085)
White	.1734	.1229	.1447	.1014	.1589	.1659	.1422
	(.0298)	(.0358)	(.0527)	(.0475)	(.0648)	(.0972)	(.0780)
Married	.0605	.0852	.0206	.1404	.1662	.0652	.1752
	(.0195)	(.0241)	(.0350)	(.0330)	(.0410)	(.0674)	(.0468)
Years since displacement	-	.0214	.0153	.0327	.0384	.0511	.0422
		(.0092)	(.0138)	(.0124)	(.0153)	(.0264)	(.0173)
Weeks Unemployed	-	-.0039	-.0039	-.0040	-.0024	-.0010	-.0028
		(.0005)	(.0007)	(.0006)	(.0007)	(.0014)	(.0009)
Selection Term	-	-.0238	.0128	.0707	.5346	.1500	.4777
		(.0931)	(.1173)	(.0939)	(.1237)	(.1544)	(.1403)
N	2575	1660	772	888	915	224	691

Notes: The specification is as in Neal (1995); it includes year of displacement and region of residence dummies, and industry employment growth. For the full sample the dependent variable is the pre-displacement wage. For the remaining columns the dependent variable is the post-displacement wage. Standard errors are given in parentheses.

Table 4b

Effects of Pre-displacement Tenure on Post-displacement Wages:

Selectivity Corrected Estimates, United States Males 1984-1990

	E=15; T=5	E=10; T=5	E=10; T=10
INDUSTRY SWITCHER	.2088 (.0332)	.1697 (.0275)	.1928 (.0338)
Industry & Skill Switcher	.1358 (.0505)	.1206 (.0409)	.1538 (.0500)
Industry Switcher & Skill Stayer	.2226 (.0452)	.1735 (.0376)	.1816 (.0458)
INDUSTRY STAYER	.3878 (.0565)	.3167 (.0461)	.3854 (.0559)
Industry & Skill Stayer	.3959 (.0618)	.3218 (.0502)	.3942 (.0610)
Industry Stayer & Skill Switcher	.2905 (.0998)	.2422 (.0831)	.2762 (.1004)

Note: E is experience in years; T is firm tenure in years; standard errors in parentheses

Table 5

Relative Losses for More Experienced Workers After Displacement: Canada Males

By Industry Status

	E=15; T=5	E=10; T=5	E=10; T=10
INDUSTRY SWITCHER	-.2061 (.0564)	-.1681 (.0461)	-.2348 (.0589)
Industry & Occ Switcher	-.2351 (.0645)	-.1968 (.0525)	-.2879 (.0673)
Industry Switcher & Occ Stayer	-.0266 (.1108)	-.0039 (.0963)	.0253 (.1196)
INDUSTRY STAYER	-.0872 (.0708)	-.0642 (.0577)	-.0761 (.0729)
Industry & Occ Stayer	-.0234 (.0731)	-.0162 (.0596)	-.0030 (.0749)
Industry Stayer & Occ Switcher	-.2281 (.1623)	-.1286 (.1345)	-.0900 (.1630)

By Occupation Status

OCCUPATION SWITCHER	-.2310 (.0589)	-.1861 (.0479)	-.2598 (.0609)
Occ & Industry Switcher	-.2351 (.0645)	-.1968 (.0525)	-.2879 (.0673)
Occ Switcher & Industry Stayer	-.2281 (.1623)	-.1286 (.1345)	-.0900 (.1630)
OCCUPATION STAYER	.0183 (.0576)	.0129 (.0470)	.0048 (.0596)
Occ & Industry Stayer	-.0234 (.0731)	-.0162 (.0596)	-.0030 (.0749)
Occ Stayer & Industry Switcher	-.0266 (.1108)	-.0039 (.0963)	.0253 (.1196)

Note: E is experience in years; T is firm tenure in years; standard errors in parentheses.

Table 6

Determinants of Changes in Log Wages for Displaced Male Workers by Industry Status: Canada

	Industry Switcher			Industry Stayer		
	All	Occ Sw	Occ St	All	Occ Sw	Occ St
Experience	-.0115	-.0101	-.0083	-.0031	-.0150	.0057
	(.0053)	(.0062)	(.0086)	(.0066)	(.0175)	(.0065)
Experience^2 *100	.0154	.0096	.0151	-.0059	-.0197	-.0170
	(.0116)	(.0141)	(.0171)	(.0152)	(0439)	(.0144)
Tenure	-.0140	-.0226	.0163	-.0069	.0084	-.0058
	(.0082)	(.0094)	(.0215)	(.0089)	(.0268)	(.0083)
Tenure^2 *100	.0046	.0289	-.0698	.0303	-.0048	.0213
	(.0331)	(.0367)	(.1131)	(.0327)	(.1258)	(.0289)
R^2	.09	.11	.21	.05	.20	.04
N	517	425	92	278	93	185
	Linear Approximation					
Experience	-.0050	-.0061	-.0011	-.0055	-.0222	-.0013
	(.0020)	(.0023)	(.0037)	(.0026)	(.0065)	(.0025)
Tenure	-.0132	-.0162	.0038	.0001	-.0087	-.0015
	(.0038)	(.0044)	(.0068)	(.0044)	(.0124)	(.0040)
R^2	.09	.11	.20	.05	.20	.03

Notes: The specification is comparable to Neal (1995). The regressions all include education, marital status, years since displacement, weeks without work following displacement and year dummies. Standard errors are given in parentheses.

Table 7

Determinants of Changes in Log Wages for Displaced Male Workers by Occupation Status: Canada

	Occupation Switcher			Occupation Stayer		
	All	Ind Sw	Ind St	All	Ind Sw	Ind St
Experience	-.0109	-.0101	-.0150	.0034	-.0083	.0057
	(.0059)	(.0062)	(.0175)	(.0050)	(.0086)	(.0065)
$Experience^2$ *100	.0078	.0096	-.0197	-.0094	.0151	-.0170
	(.0134)	(.0141)	(0439)	(.0109)	(.0171)	(.0144)
Tenure	-.0180	-.0226	.0084	-.0027	.0163	-.0058
	(.0087)	(.0094)	(.0268)	(.0070)	(.0215)	(.0083)
$Tenure^2$ *100	.0219	.0289	-.0048	.0075	-.0698	.0213
	(.0347)	(.0367)	(.1258)	(.0263)	(.1131)	(.0289)
R^2	.09	.11	.20	.05	.21	.04
N	518	425	93	277	92	185
	Linear Approximation					
Experience	-.0077	-.0061	-.0222	-.0006	-.0011	-.0013
	(.0022)	(.0023)	(.0065)	(.0020)	(.0037)	(.0025)
Tenure	-.0134	-.0162	-.0087	-.0014	.0038	-.0015
	(.0041)	(.0044)	(.0124)	(.0034)	(.0068)	(.0040)
R^2	.11	.11	.20	.04	.20	.03

Notes: See notes to Table 6.

Table 8

Cumulative Returns to Firm and Industry: Parent's Analysis , NLSY

		Firm & One-digit Industry Tenure		Firm & Three-digit Industry Tenure	
	Firm	Firm	Industry	Firm	Industry
	Continuous Tenure Definitions				
2 years	.0383	-.0084	.0823	-.0082	.0650
			(.0121)		(.0155)
5 years	.0599	-.0231	.1546	-.0250	.1218
			(.0208)		(.0298)
10 years	.0239	-.0836	.1950	-.0890	.1511
			(.0272)		(.0361)
	Non-Continuous Tenure Definitions				
2 years	.0383	.0151	.0439	.0116	.0386
			(.0090)		(.0135)
5 years	.0599	.0148	.0925	.0065	.0818
			(.0180)		(.0268)
10 years	.0239	-.0297	.1309	-.0460	.1191
			(.0257)		(.0373)

Notes: Cumulative returns for industry tenure are reported directly in Parent (2000), Table 7, with standard errors in parentheses. Cumulative returns for firm tenure are based on the coefficients in Parent (2000), Table 4.

Table 9

Cumulative Returns to Firm, Industry and Skill Tenure: NLSY

		Firm & Ind(1dgt)		Firm & Ind(3dgt)		Firm & Skill		Firm, Industry(1dgt) & Skill			Firm, Industry(3dgt) & Skill		
	Firm	Firm	Ind1	Firm	Ind3	Firm	Skill	Firm	Ind1	Skill	Firm	Ind3	Skill
	Continuous Tenure Definitions												
2 years	.0421	-.0023	.0725	-.0078	.0631	.0103	.0609	-.0193	.0603	.0448	-.0261	.0483	.0536
	(.0062)	(.0083)	(.0084)	(.0135)	(.0146)	(.0078)	(.0077)	(.0089)	(.0089)	(.0081)	(.0134)	(.0153)	(.0084)
5 years	.0659	-.0182	.1356	-.0318	.1224	.0013	.1216	-.0541	.1100	.0927	-.0702	.0932	.1072
	(.0087)	(.0132)	(.0144)	(.0242)	(.0269)	(.0121)	(.0132)	(.0144)	(.0150)	(.0134)	(.0241)	(.0275)	(.0137)
10 years	.0507	-.0709	.1719	-.0905	.1621	-.0539	.1777	-.1284	.1298	.1430	-.1503	.1167	.1577
	(.0120)	(.0185)	(.0187)	(.0352)	(.0371)	(.0171)	(.0175)	(.0202)	(.0193)	(.0177)	(.0349)	(.0375)	(.0176)
	Non-Continuous Tenure Definitions												
2 years	.0421	.0164	.0579	.0162	.0396	.0294	.0405	.0108	.0501	.0268	.0114	.0288	.0341
	(.0062)	(.0073)	(.0090)	(.0102)	(.0121)	(.0069)	(.0102)	(.0076)	(.0097)	(.0109)	(.0101)	(.0128)	(.0111)
5 years	.0659	.0172	.1114	.0198	.0710	.0400	.0769	.0048	.0977	.0504	.0081	.0520	.0645
	(.0087)	(.0113)	(.0165)	(.0178)	(.0224)	(.0107)	(.0186)	(.0121)	(.0172)	(.0196)	(.0176)	(.0232)	(.0199)
10 years	.0507	-.0168	.1461	-.0052	.0785	.0038	.1082	-.0425	.1271	.0750	-.0279	.0514	.0933
	(.0120)	(.0163)	(.0223)	(.0269)	(.0318)	(.0157)	(.0257)	(.0179)	(.0228)	(.0265)	(.0267)	(.0327)	(.0269)

Notes: Standard errors in parentheses.

Table 10

Cumulative Returns to Adjusted Firm, Industry and Skill Tenure: NLSY

		Firm & Ind (1dgt)		Firm & Ind(3dgt)		Firm & Skill		Firm, Industry(1dgt) & Skill			Firm, Industry(3dgt) & Skill		
	Firm	Firm	Ind	Firm	Ind	Firm	Skill	Firm	Ind	Skill	Firm	Ind	Skill
	Continuous Tenure Definitions												
2 years	.0376	-.0054	.0694	-.0060	.0563	.0054	.0639	-.0219	.0570	.0484	-.0242	.0406	.0581
	(.0064)	(.0084)	(.0087)	(.0142)	(.0156)	(.0079)	(.0083)	(.0090)	(.0094)	(.0089)	(.0141)	(.0164)	(.0095)
5 years	.0611	-.0229	.1306	-.0261	.1108	-.0056	.1262	-.0574	.1054	.0987	-.0648	.0806	.1158
	(.0090)	(.0133)	(.0149)	(.0255)	(.0286)	(.0123)	(.0141)	(.0147)	(.0157)	(.0145)	(.0253)	(.0293)	(.0153)
10 years	.0517	-.0782	.1721	-.0837	.1563	-.0614	.1849	-.1345	.1304	.1517	-.1458	.1081	.1732
	(.0123)	(.0186)	(.0192)	(.0369)	(.0391)	(.0173)	(.0183)	(.0205)	(.0200)	(.0186)	(.0366)	(.0397)	(.0191)
	Non-Continuous Tenure Definitions												
2 years	.0376	.0172	.0439	.0175	.0292	.0276	.0383	.0128	.0363	.0258	.0143	.0202	.0307
	(.0064)	(.0073)	(.0096)	(.0101)	(.0123)	(.0073)	(.0115)	(.0077)	(.0102)	(.0118)	(.0101)	(.0132)	(.0121)
5 years	.0611	.0202	.0844	.0243	.0506	.0409	.0720	.0104	.0726	.0461	.0162	.0363	.0561
	(.0090)	(.0114)	(.0169)	(.0177)	(.0224)	(.0114)	(.0207)	(.0123)	(.0177)	(.0209)	(.0178)	(.0236)	(.0213)
10 years	.0517	-.0149	.1182	-.0018	.0569	.0078	.1076	-.0391	.1024	.0694	-.0213	.0369	.0845
	(.0123)	(.0163)	(.0223)	(.0267)	(.0317)	(.0165)	(.0275)	(.0180)	(.0229)	(.0274)	(.0269)	(.0330)	(.0277)

Notes: Standard errors in parentheses.

Table 11

Cumulative Returns to Firm, Industry and Skill Tenure: PSID

	Firm	Firm & Industry		Firm & Skill		Firm, Industry & Skill		
		Firm	Industry	Firm	Skill	Firm	Industry	Skill
1-digit								
2	.0351	.0186	.0348	.0213	.0403	.0113	.0280	.0266
	(.0089)	(.0098)	(.0063)	(.0093)	(.0078)	(.0098)	(.0067)	(.0083)
5	.0583	.0323	.0742	.0379	.0793	.0219	.0594	.0519
	(.0101)	(.0119)	(.0141)	(.0101)	(.0161)	(.0121)	(.0145)	(.0168)
10	.0925	.0526	.1077	.0641	.0998	.0418	.0853	.0659
	(.0140)	(.0173)	(.0224)	(.0152)	(.0240)	(.0174)	(.0227)	(.0246)
2-digit								
2	.0319	.0189	.0274	.0173	.0429	.0112	.0166	.0324
	(.0088)	(.0100)	(.0064)	(.0092)	(.0078)	(.0097)	(.0065)	(.0082)
5	.0553	.0341	.0576	.0337	.0846	.0237	.0344	.0626
	(.0100)	(.0128)	(.0141)	(.0108)	(.0161)	(.0124)	(.0140)	(.0167)
10	.0898	.0586	.0820	.0596	.1064	.0450	.0478	.0756
	(.0139)	(.0191)	(.0224)	(.0152)	(.0241)	(.0184)	(.0217)	(.0245)
3-digit								
2	.0316	.0222	.0210	.0183	.0413	.0158	.0091	.0352
	(.0087)	(.0100)	(.0065)	(.0092)	(.0078)	(.0101)	(.0067)	(.0081)
5	.0532	.0407	.0417	.0347	.0816	.0333	.0162	.0701
	(.0098)	(.0137)	(.0143)	(.0108)	(.0161)	(.0137)	(.0144)	(.0166)
10	.0850	.0718	.0509	.0606	.1028	.0632	.0136	.0911
	(.0137)	(.0211)	(.0225)	(.0152)	(.0240)	(.0211)	(.0222)	(.0245)

Notes: standard errors in parentheses.

Table 12

SLID Cumulative Returns to Firm, Industry and Occupation Tenure: high aggregation

	Firm	Firm & Industry		Firm & Occupation		Firm, Industry & Occupation		
		Firm	Industry	Firm	Occup	Firm	Industry	Occup
Continuous								
2	.0629	.0528	.0152	.0431	.0279	.0459	-.0257	.0497
	(.0055)	(.0082)	(.0072)	(.0080)	(.0069)	(.0083)	(.0146)	(.0143)
5	.0717	.0583	.0285	.0464	.0548	.0445	-.0467	.0937
	(.0063)	(.0148)	(.0155)	(.0176)	(.0150)	(.0150)	(.0306)	(.0297)
10	.0857	.0673	.0311	.0537	.0671	.0462	-.0445	.1020
	(.0089)	(.0238)	(.0244)	(.0289)	(.0234)	(.0240)	(.0476)	(.0455)
Non-Continuous								
2	.0629	.0559	.0154	.0531	.0253	.0487	.0077	.0267
	(.0055)	(.0062)	(.0055)	(.0069)	(.0059)	(.0073)	(.0089)	(.0088)
5	.0717	.0646	.0293	.0615	.0509	.0504	.0229	.0508
	(.0063)	(.0093)	(.0117)	(.0103)	(.0125)	(.0115)	(.0173)	(.0171)
10	.0857	.0742	.0354	.0709	.0692	.0498	.0541	.0605
	(.0089)	(.0138)	(.0181)	(.0154)	(.0190)	(.0175)	(.0227)	(.0220)

Notes: standard errors in parentheses

Table 13

SLID Cumulative Returns to Firm, Industry and Occupation Tenure: medium aggregation

	Firm	Firm & Industry		Firm & Occupation		Firm, Industry & Occupation		
		Firm	Industry	Firm	Occup	Firm	Industry	Occup
Continuous								
2	.0629	.0510	.0178	.0393	.0323	.0416	-.0231	.0520
	(.0055)	(.0094)	(.0084)	(.0085)	(.0074)	(.0092)	(.0167)	(.0153)
5	.0717	.0550	.0343	.0325	.0635	.0351	-.0351	.0926
	(.0063)	(.0178)	(.0184)	(.0155)	(.0162)	(.0172)	(.0356)	(.0322)
10	.0857	.0630	.0401	.0326	.0772	.0313	-.0055	.0790
	(.0089)	(.0287)	(.0294)	(.0249)	(.0256)	(.0279)	(.0566)	(.0503)
Non-Continuous								
2	.0629	.0555	.0145	.0497	.0279	.0468	.0007	.0327
	(.0055)	(.0065)	(.0058)	(.0069)	(.0060)	(.0076)	(.0088)	(.0088)
5	.0717	.0657	.0256	.0556	.0549	.0481	.0066	.0620
	(.0063)	(.0103)	(.0122)	(.0108)	(.0126)	(.0128)	(.0173)	(.0172)
10	.0857	.0790	.0232	.0641	.0696	.0501	.0250	.0718
	(.0089)	(.0154)	(.0185)	(.0163)	(.0192)	(.0195)	(.0228)	(.0224)

Notes: standard errors in parentheses

Table 14

SLID Cumulative Returns to Firm, Industry and Occupation Tenure: low aggregation

	Firm	Firm & Industry		Firm & Occupation		Firm, Industry & Occupation		
		Firm	Industry	Firm	Occup	Firm	Industry	Occup
Continuous								
2	.0629	.0489	.0200	.0452	.0253	.0446	-.0148	.0399
	(.0055)	(.0101)	(.0092)	(.0093)	(.0082)	(.0097)	(.0201)	(.0189)
5	.0717	.0517	.0378	.0464	.0480	.0441	-.0224	.0704
	(.0063)	(.0197)	(.0203)	(.0176)	(.0181)	(.0187)	(.0434)	(.0405)
10	.0857	.0613	.0416	.0537	.0534	.0497	-.0091	.0641
	(.0089)	(.0321)	(.0326)	(.0289)	(.0293)	(.0306)	(.0696)	(.0650)
Non-Continuous								
2	.0629	.0455	.0292	.0512	.0201	.0449	.0149	.0134
	(.0055)	(.0070)	(.0062)	(.0070)	(.0062)	(.0075)	(.0084)	(.0083)
5	.0717	.0447	.0581	.0596	.0362	.0448	.0322	.0225
	(.0063)	(.0116)	(.0132)	(.0117)	(.0131)	(.0133)	(.0166)	(.0165)
10	.0857	.0472	.0746	.0716	.0350	.0479	.0491	.0164
	(.0089)	(.0173)	(.0199)	(.0179)	(.0200)	(.0205)	(.0219)	(.0220)

Notes: standard errors in parentheses.

Table 15

SLID Cumulative Returns to Firm, Industry and Skill Tenure.

	Firm & Skill		Firm, Industry & Skill			Firm, Industry & Skill			Firm, Industry & Skill		
			high aggregation			medium aggregation			low aggregation		
	Firm	Skill	Firm	Ind	Skill	Firm	Ind	Skill	Firm	Ind	Skill
Continuous											
2	.0537	.0114	.0521	-.0224	.0416	.0539	-.0086	.0268	.0561	-.0093	.0258
	(.0071)	(.0051)	(.0083)	(.0137)	(.0111)	(.0099)	(.0147)	(.0101)	(.0109)	(.0156)	(.0097)
5	.0508	.0253	.0569	-.0540	.0907	.0614	-.0239	.0582	.0681	-.0291	.0584
	(.0120)	(.0115)	(.0151)	(.0309)	(.0251)	(.0191)	(.0334)	(.0230)	(.0218)	(.0353)	(.0221)
10	.0534	.0400	.0667	-.0947	.1363	.0749	-.0486	.0867	.0910	-.0710	.0965
	(.0190)	(.0191)	(.0245)	(.0524)	(.0425)	(.0314)	(.0568)	(.0392)	(.0361)	(.0598)	(.0374)
Non-continuous											
2	.0556	.0188	.0515	.0084	.0185	.0512	.0091	.0179	.0420	.0237	.0160
	(.0079)	(.0065)	(.0067)	(.0064)	(.0056)	(.0073)	(.0067)	(.0056)	(.0079)	(.0073)	(.0058)
5	.0537	.0450	.0538	.0148	.0418	.0548	.0142	.0413	.0354	.0461	.0374
	(.0117)	(.0147)	(.0104)	(.0137)	(.0126)	(.0118)	(.0144)	(.0127)	(.0133)	(.0156)	(.0130)
10	.0549	.0830	.0557	.0156	.0690	.0596	.0079	.0708	.0298	.0567	.0662
	(.0174)	(.0250)	(.0157)	(.0210)	(.0208)	(.0179)	(.0217)	(.0212)	(.0201)	(.0234)	(.0219)

Appendix Tables

Table 9a
NLSY Industry vs Firm vs Skill Effects, Continuous tenure IV-GLS

	Firm	Firm/Ind1	Firm/Ind3	Firm/Skill	Firm/Ind1/Skill	Firm/Ind3/Skill
OldJob	.0125	.0018	.0034	.0063	-.0002	.0016
	(.0072)	(.0077)	(.0084)	(.0077)	(.0078)	(.0083)
Firm	.0175	-.0008	-.0047	.0040	-.0088	-.0135
	(.0029)	(.0038)	(.0064)	(.0036)	(.0040)	(.0063)
Firm^2	-.0014	-.0007	-.0005	-.0010	-.0004	-.0002
	(.0002)	(.0003)	(.0004)	(.0002)	(.0003)	(.0004)
Industry		.0437	.0372		.0367	.0284
		(.0054)	(.0090)		(.0058)	(.0096)
Industry^2		-.0040	-.0030		-.0035	-.0022
		(.0007)	(.0011)		(.0008)	(.0012)
Industry^3		.0001	.0001		.0001	.0001
		(.0000)	(.0000)		(.0000)	(.0000)
Skill				.0354	.0255	.0312
				(.0050)	(.0054)	(.0056)
Skill^2				-.0027	-.0017	-.0024
				(.0007)	(.0008)	(.0008)
Skill^3				.0001	.0001	.0001
				(.0000)	(.0000)	(.0000)
Experience	.1435	.1395	.1396	.1198	.1304	.1356
	(.0080)	(.0083)	(.0082)	(.0082)	(.0085)	(.0084)
Experience^2	-.0061	-.0050	-.0057	-.0049	-.0044	-.0049
	(.0007)	(.0007)	(.0007)	(.0008)	(.0008)	(.0008)
Experience^3	.0001	.0001	.0001	.0001	.0001	.0001
	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)

Notes: The specification is as in Parent (2000). All regressions include education, marital and union status, as well as year dummies. Ind1 and Ind3 indicate one- and three-digit industry, respectively. Standard errors are given in parentheses.

Table 9b
NLSY Industry vs Firm vs Skill Effects, IV-GLS Non-Continuous Tenure

	Firm	Firm/Ind1	Firm/Ind3	Firm/Skill	Firm/Ind1/Skill	Firm/Ind3/Skill
OldJob	.0125	.0070	.0060	.0088	.0056	.0059
	(.0072)	(.0074)	(.0080)	(.0073)	(.0074)	(.0079)
Firm	.0175	.0065	.0067	.0130	.0045	.0042
	(.0029)	(.0034)	(.0050)	(.0033)	(.0036)	(.0049)
Firm^2	-.0014	-.0009	-.0008	-.0013	-.0009	-.0008
	(.0002)	(.0002)	(.0003)	(.0002)	(.0002)	(.0003)
Industry		.0343	.0242		.0293	.0174
		(.0058)	(.0075)		(.0062)	(.0080)
Industry^2		-.0028	-.0024		-.0022	-.0016
		(.0007)	(.0009)		(.0008)	(.0010)
Industry^3		.0001	.0001		.0001	.0000
		(.0000)	(.0000)		(.0000)	(.0000)
Skill				.0244	.0164	.0206
				(.0063)	(.0068)	(.0070)
Skill^2				-.0023	-.0016	-.0020
				(.0008)	(.0009)	(.0009)
Skill^3				.0001	.0001	.0001
				(.0000)	(.0000)	(.0000)
Experience	.1435	.1360	.1416	.1476	.1419	.1498
	(.0080)	(.0086)	(.0083)	(.0089)	(.0090)	(.0089)
Experience^2	-.0061	-.0053	-.0057	-.0051	-.0048	-.0051
	(.0007)	(.0008)	(.0007)	(.0008)	(.0008)	(.0008)
Experience^3	.0001	.0001	.0001	.0001	.0001	.0001
	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)

Notes: See notes to Table 9a.

Table 10a
NLSY Industry vs Firm vs Skill Effects, Continuous Adjusted Tenure IV-GLS

	Firm	Firm/Ind1	Firm/Ind3	Firm/Skill	Firm/Ind1/Skill	Firm/Ind3/Skill
OldJob	.0098	-.0002	.0013	.0033	-.0026	-.0004
	(.0074)	(.0076)	(.0087)	(.0077)	(.0078)	(.0086)
Firm	.0163	-.0013	-.0024	.0029	-.0087	-.0112
	(.0030)	(.0039)	(.0067)	(.0037)	(.0042)	(.0067)
$Firm^2$	-.0012	-.0007	-.0006	-.0009	-.0004	-.0003
	(.0002)	(.0003)	(.0004)	(.0003)	(.0003)	(.0004)
Industry		.0418	.0330		.0345	.0235
		(.0056)	(.0097)		(.0062)	(.0103)
$Industry^2$		-.0038	-.0026		-.0032	-.0017
		(.0008)	(.0012)		(.0009)	(.0013)
$Industry^3$		.0001	.0001		.0001	.0000
		(.0000)	(.0000)		(.0000)	(.0001)
Skill				.0376	.0279	.0340
				(.0054)	(.0059)	(.0063)
$Skill^2$				-.0030	-.0020	-.0027
				(.0008)	(.0009)	(.0010)
$Skill^3$				.0001	.0001	.0001
				(.0000)	(.0000)	(.0000)
Experience	.0909	.1003	.0856	.0776	.0801	.0719
	(.0054)	(.0062)	(.0056)	(.0060)	(.0063)	(.0059)
$Experience^2$	-.0049	-.0039	-.0045	-.0034	-.0030	-.0033
	(.0006)	(.0006)	(.0006)	(.0006)	(.0006)	(.0007)
$Experience^3$	.0001	.0001	.0001	.0001	.0000	.0001
	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)

Notes: See notes to Table 9a.

Table 10b
NLSY Industry vs Firm vs Skill Effects, IV-GLS Non-Continuous Adjusted Tenure

	Firm	Firm/Ind1	Firm/Ind3	Firm/Skill	Firm/Ind1/Skill	Firm/Ind3/Skill
OldJob	.0098	.0051	.0037	.0049	.0030	.0029
	(.0074)	(.0074)	(.0079)	(.0076)	(.0074)	(.0079)
Firm	.0163	.0080	.0088	.0141	.0072	.0077
	(.0030)	(.0035)	(.0050)	(.0035)	(.0037)	(.0050)
$Firm^2$	-.0012	-.0010	-.0009	-.0014	-.0011	-.0010
	(.0002)	(.0002)	(.0004)	(.0002)	(.0003)	(.0004)
Industry		.0261	.0184		.0210	.0123
		(.0061)	(.0077)		(.0065)	(.0083)
$Industry^2$		-.0023	-.0020		-.0015	-.0012
		(.0008)	(.0010)		(.0009)	(.0011)
$Industry^3$		.0001	.0001		.0000	.0000
		(.0000)	(.0000)		(.0000)	(.0000)
Skill				.0234	.0163	.0191
				(.0072)	(.0075)	(.0076)
$Skill^2$				-.0023	-.0019	-.0021
				(.0009)	(.0010)	(.0010)
$Skill^3$				.0001	.0001	.0001
				(.0000)	(.0000)	(.0000)
Experience	.0909	.0990	.1112	.0778	.0967	.1009
	(.0054)	(.0065)	(.0063)	(.0068)	(.0073)	(.0072)
$Experience^2$	-.0049	-.0042	-.0046	-.0037	-.0035	-.0037
	(.0006)	(.0006)	(.0006)	(.0007)	(.0007)	(.0007)
$Experience^3$	.0001	.0001	.0001	.0001	.0001	.0001
	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)

Notes: See notes to Table 9a.

Table 11a
PSID Industry vs Firm vs Skill Effects, IV-GLS (One-Digit)

	(1)	(2)	(3)	(4)
OldJob	.0184	.0097	.0098	.0048
	(.0094)	(.0100)	(.0096)	(.0100)
Firm	.0083	.0044	.0057	.0032
	(.0018)	(.0022)	(.0019)	(.0022)
$Firm^2$	-.0001	.0000	-.0000	.0000
	(.0001)	(.0001)	(.0001)	(.0001)
Industry		.0191		.0154
		(.0034)		(.0036)
$Industry^2$		-.0010		-.0008
		(.0002)		(.0002)
$Industry^3$ x 100		.0014		.0011
		(.0004)		(.0004)
Skill			.0231	.0155
			(.0044)	(.0047)
$Skill^2$			-.0018	-.0012
			(.0004)	(.0004)
$Skill^3$ x 100			.0042	.0032
			(.0011)	(.0011)
Experience	.0798	.0467	.0712	.0499
	(.0086)	(.0054)	(.0087)	(.0060)
$Experience^2$	-.0012	-.0008	-.0011	-.0008
	(.0002)	(.0002)	(.0002)	(.0002)
$Experience^3$ x100	.0011	.0004	.0010	.0007
	(.0002)	(.0003)	(.0003)	(.0003)

Notes: See notes to Table 9a.

Table 11b
PSID Industry vs Firm vs Skill Effects, IV-GLS (Three-Digit)

	(1)	(2)	(3)	(4)
Old Job	.0162	-.0099	.0071	.0041
	(.0091)	(.0096)	(.0095)	(.0097)
Firm	.0077	.0061	.0056	-.0057
	(.0018)	(.0026)	(.0019)	(.0026)
Firm^2	-.0001	-.0000	-.0000	-.0000
	(.0001)	(.0001)	(.0001)	(.0001)
Industry		.0120		.0055
		(.0035)		(.0037)
Industry^2		-.0008		-.0005
		(.0002)		(.0002)
Industry^3 x100		.0013		.0008
		(.0004)		(.0004)
Skill			.0237	.0202
			(.0044)	(.0046)
Skill^2			-.0018	-.0015
			(.0004)	(.0004)
Skill^3 x100			.0043	.0037
			(.0011)	(.0011)
Experience	.0815	.0586	.0652	.0670
	(.0082)	(.0058)	(.0073)	(.0077)
Experience^2	-.0012	-.0010	-.0011	-.0010
	(.0002)	(.0002)	(.0002)	(.0002)
Experience^3 x100	.0012	.0009	.0010	.0008
	(.0002)	(.0003)	(.0003)	(.0003)

Notes: See notes to Table 9a.

Table 12a
SLID Earnings Function Estimates: high aggregation

	1	2	3	4	5	6	7
		C	NC	C	NC	C	NC
Old Job	.0569	.0492	.0488	.0473	.0464	.0478	.0467
	(.0062)	(.0064)	(.0063)	(.0064)	(.0068)	(.0064)	(.0070)
Firm	.0030	.0018	.0038	-.0024	.0036	-.0012	.0012
	(.0014)	(.0035)	(.0024)	(.0033)	(.0026)	(.0035)	(.0029)
Firm^2	-.0000	-.0000	-.0001	.0002	-.0001	.0001	-.0001
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Industry		.0090	.0091			-.0154	.0033
		(.0039)	(.0031)			(.0082)	(.0052)
Industry^2		-.0007	-.0007			.0013	.0003
		(.0002)	(.0002)			(.0006)	(.0004)
Industry^3 x100		.0015	.0017			-.0023	-.0009
		(.0003)	(.0003)			(.0012)	(.0009)
Occupation				.0162	.0145	.0293	.0158
				(.0038)	(.0033)	(.0081)	(.0052)
Occupation^2				-.0011	-.0010	-.0023	-.0013
				(.0002)	(.0002)	(.0006)	(.0004)
Occupation^3 x100				.0019	.0022	.0041	.0030
				(.0003)	(.0004)	(.0012)	(.0009)
Experience	.0454	.0410	.0384	.0399	.0474	.0392	.0476
	(.0041)	(.0042)	(.0043)	(.0042)	(.0040)	(.0042)	(.0041)
Experience^2	-.0016	-.0016	-.0016	-.0015	-.0013	-.0016	-.0014
	(.0001)	(.0001)	(.0001)	(.0001)	(.0002)	(.0001)	(.0002)
Experience^3 x100	.0018	.0017	.0016	.0016	.0013	.0016	.0014
	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)

Notes: See notes to Table 9a; C denotes continuous tenure; NC denotes non-continuous.

Table 13a
SLID Earnings Function Estimates: medium aggregation

	1	2	3	4	5	6	7
		C	NC	C	NC	C	NC
Old Job	.0569	.0487	.0477	.0467	.0454	.0477	.0458
	(.0062)	(.0065)	(.0063)	(.0064)	(.0067)	(.0064)	(.0070)
Firm	.0030	.0011	.0041	-.0043	.0022	-.0034	.0005
	(.0014)	(.0042)	(.0026)	(.0036)	(.0027)	(.0040)	(.0032)
$Firm^2$	-.0000	.0000	-.0001	.0003	-.0000	.0002	-.0000
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Industry		.0105	.0089			-.0148	-.0004
		(.0046)	(.0032)			(.0093)	(.0051)
$Industry^2$		-.0008	-.0008			.0017	.0004
		(.0002)	(.0002)			(.0006)	(.0004)
$Industry^3$ x100		.0015	.0019			-.0027	-.0011
		(.0003)	(.0003)			(.0011)	(.0008)
Occupation				.0187	.0161	.0314	.0193
				(.0041)	(.0033)	(.0086)	(.0052)
$Occupation^2$				-.0013	-.0012	-.0028	-.0016
				(.0002)	(.0002)	(.0006)	(.0004)
$Occupation^3$ x100				.0020	.0024	.0046	.0034
				(.0003)	(.0004)	(.0012)	(.0009)
Experience	.0454	.0438	.0390	.0407	.0458	.0391	.0478
	(.0041)	(.0041)	(.0042)	(.0041)	(.0040)	(.0042)	(.0041)
$Experience^2$	-.0016	-.0015	-.0015	-.0015	-.0013	-.0015	-.0013
	(.0001)	(.0001)	(.0001)	(.0001)	(.0002)	(.0001)	(.0002)
$Experience^3$ x100	.0018	.0016	.0015	.0015	.0013	.0016	.0013
	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)

Notes: See notes to table 9a; C denotes continuous tenure; NC denotes non-continuous.

Table 14a
SLID Earnings Function Estimates: low aggregation

	1	2	3	4	5	6	7
		C	NC	C	NC	C	NC
Old Job	.0569	.0483	.0470	.0457	.0451	.0467	.0457
	(.0062)	(.0065)	(.0065)	(.0065)	(.0064)	(.0065)	(.0063)
Firm	.0030	.0001	-.0009	-.0005	.0031	-.0013	-.0006
	(.0014)	(.0046)	(.0029)	(.0040)	(.0029)	(.0043)	(.0034)
Firm^2	-.0000	.0001	.0001	.0001	-.0000	.0002	.0001
	(.0001)	(.0002)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Industry		.0118	.0168			-.0097	.0081
		(.0050)	(.0035)			(.0111)	(.0049)
Industry^2		-.0009	-.0011			.0012	-.0003
		(.0002)	(.0002)			(.0007)	(.0004)
Industry^3 x100		.0016	.0021			-.0034	.0003
		(.0003)	(.0003)			(.0014)	(.0007)
Occupation				.0149	.0121	.0244	.0084
				(.0045)	(.0034)	(.0105)	(.0048)
Occupation^2				-.0012	-.0011	-.0023	-.0009
				(.0002)	(.0002)	(.0007)	(.0004)
Occupation^2 x100				.0021	.0023	.0052	.0020
				(.0003)	(.0003)	(.0014)	(.0007)
Experience	.0454	.0439	.0426	.0423	.0391	.0411	.0382
	(.0041)	(.0041)	(.0041)	(.0041)	(.0042)	(.0042)	(.0043)
Experience^2	-.0016	-.0015	-.0014	-.0015	-.0015	-.0015	-.0015
	(.0001)	(.0001)	(.0001)	(.0001)	(.0002)	(.0001)	(.0002)
Experience^2 x100	.0018	.0016	.0015	.0015	.0015	.0016	.0016
	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)

Notes: See notes to Table 9a; C denotes continuous tenure; NC denotes non-continuous.

Table 15a
SLID Earnings Function Estimates with Basic Skill Measure

	1	2	3	4	5	6	7	8	9
		C	NC	C	NC	C	NC	C	NC
Industry Aggregation				high		medium		low	
Old Job	.0569	.0576	.0579	.0494	.0495	.0491	.0485	.0487	.0477
	(.0062)	(.0062)	(.0083)	(.0064)	(.0067)	(.0066)	(.0070)	(.0066)	(.0072)
Firm	.0030	-.0023	-.0014	.0013	.0011	.0023	.0014	.0035	-.0031
	(.0014)	(.0029)	(.0030)	(.0035)	(.0026)	(.0044)	(.0030)	(.0050)	(.0034)
Firm(2)	-.0000	.0002	.0001	.0000	-.0000	.0000	-.0000	.0001	.0001
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0002)	(.0001)	(.0002)	(.0001)
Industry				-.0113	.0052	-.0039	.0059	-.0037	.0138
				(.0073)	(.0036)	(.0078)	(.0037)	(.0083)	(.0041)
Ind(2)				.0000	-.0005	-.0003	-.0007	-.0005	-.0010
				(.0003)	(.0002)	(.0003)	(.0002)	(.0003)	(.0002)
Ind(3)x100				.0017	.0018	.0017	.0020	.0017	.0021
				(.0003)	(.0004)	(.0003)	(.0004)	(.0003)	(.0004)
Skill		.0061	.0097	.0226	.0098	.0146	.0094	.0137	.0083
		(.0027)	(.0034)	(.0059)	(.0030)	(.0054)	(.0030)	(.0052)	(.0031)
Skill(2)		-.0002	-.0001	-.0009	-.0003	-.0006	-.0002	-.0004	-.0002
		(.0001)	(.0001)	(.0002)	(.0001)	(.0002)	(.0001)	(.0002)	(.0001)
Skill(3)x100		.0000	-.0000	-.0000	-.0000	-.0000	-.0000	-.0000	-.0000
		(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)	(.0000)
Experience	.0454	.0440	.0483	.0389	.0384	.0439	.0393	.0441	.0411
	(.0041)	(.0041)	(.0052)	(.0042)	(.0045)	(.0040)	(.0046)	(.0040)	(.0046)
Exp(2)	-.0016	-.0016	-.0016	-.0015	-.0015	-.0015	-.0015	-.0015	-.0015
	(.0001)	(.0001)	(.0002)	(.0001)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)
Exp(3)x100		.0017	.0018	.0016	.0015	.0015	.0015	.0015	.0015
		(.0002)	(.0003)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)	(.0002)

Notes: See notes to Table 9a. C denotes continuous tenure; NC denotes non-continuous tenure.