Time Out of Work and Skill Depreciation*

Per-Anders Edin and Magnus Gustavsson

Department of Economics, Uppsala University

This version: 28 September 2004

Abstract

This paper investigates the role of skill depreciation in the relationship between work interruptions and subsequent wages. Using a unique longitudinal dataset, the Swedish part of the International Adult Literacy Survey, we are able to analyze changes in literacy skills for individuals as a function of time out of work. In general, we find statistically strong evidence on a negative relationship between work interruptions and skills. Our analysis suggests that depreciation of general (literacy) skills is economically significant. Our estimates imply that a full year of non-employment is associated with skill losses that are equivalent to moving 5 percentiles down the skill distribution.

JEL Classification: J13, J24, J31, J69.

Keywords; Work interruptions, Skill depreciation, Unemployment, Wage differentials.

* We are grateful to Sebastian Arslanogullari, Peter Fredriksson, Daniel Hamermesh, Patrik Hesselius, Alan Krueger, Mats Myrberg and participants at seminars at FIEF, Stockholm University (SOFI and the Department of Economics) and Uppsala University, as well as conferences in Braga (LOWER), Stockholm (EEA) and Seville (EALE) for useful comments. This research has been partly funded by SMIF.

1. Introduction

Economists have for a long time been interested in the labor market consequences of work interruptions of various types. One of the main questions is how work interruptions affect human capital formation and thereby future outcomes in the labor market. The interest for these issues goes far beyond potential effects at the individual level though. The existence of negative effects of unemployment plays an important role in many discussions of the persistence of unemployment and hysteresis, e.g. Phelps (1972), Blanchard and Summers (1986), and Pissarides (1992). In these models, unemployment will result in future unemployment through the skill formation process. Similarly, potential detrimental effects of unemployment play a central role in discussion about the role of active labor market policies to fight unemployment, e.g. Calmfors (1994). The existence and magnitude of skill depreciation has important implications for designing policies against unemployment.

The empirical studies of the individual effects of work interruptions can roughly be divided into two main strands. One strand has been concerned with the participation of women in the labor market. A large number of empirical studies, starting with Mincer and Polachek (1974), have estimated standard human capital wage equations with the inclusion of variables that capture time out of work to investigate the effect on women's careers. The other strand of the literature deals with the consequences of unemployment, in particular the effects of job loss due to displacement, e.g. Jacubson *et al* (1993). This literature is mainly concerned with wage penalties associated with loss of firm- or industry-specific human capital, e.g. Neal (1995). In general, empirical studies show that work interruptions have negative effects on wages; that is, time out induces a wage loss larger than can be explained by forgone experience solely.

The negative wage effects of time out of work have normally been interpreted as due to human capital depreciation. This interpretation has, however, seldom been put to direct empirical tests. There are other potential explanations to this negative association between work interruptions and wages – various forms of signaling stories are perhaps the most obvious alternative. Gibbons and Katz (1991) find that part of the wage (and employment) consequences of displacement may be due to signaling effects.[1] Also, Albrecht *et al* (1999) find that the sign and magnitude of the wage effect depends on gender and the reason for time out. This finding is not consistent with the simple human capital depreciation story.

In this paper we will investigate more directly whether time out from the labor market actually leads to human capital depreciation. This is an issue that has to be understood in order to assess the consequences of unemployment and how to mitigate these. It is also important for understanding the gender wage gap since women are more likely to spend time out of the labor force, e.g. to take parental leave. We use a unique dataset, the Swedish part of the International Adult Literacy Survey (IALS), which contains individual test scores from two literacy tests taken 1994 and 1998. These test scores provide measures of *general* skills at the individual level. Using these data, we are able to study changes in individual skill levels and relate those to time out of work.

This paper starts with a description of the Swedish longitudinal data in the IALS. The next section contains a short investigation of whether literacy skills, in the form of test scores, matter for a worker's wage. Like earlier studies, we find that test scores are significantly related to earnings. Furthermore, we find that these results remain in panel data - changes in test scores are significantly related to changes in earnings. We then turn to the analysis of how time out of work affects skills. Our results suggest that being out of work is associated with depreciation of worker skills. Test scores drop for individuals that are out of work, in particular for those who are long term non-employed.

[1] There is also convincing evidence from survey data that employers use unemployment as a bad signal, e.g. Blinder and Choi (1990) and Agell and Bennmarker (2002).

2. Data

The 1994 International Adult Literacy Survey[2]

Seven governments, the OECD, the European Union, and UNESCO, collaborated in the making of the complete 1994 IALS. The participating countries were Canada, Switzerland, Germany, USA, Netherlands, Poland, and Sweden. Its purpose was to measure the literacy ability of the adult population in each country and to be able to permit a cross-country comparison of the results. Due to its success, two later waves of the survey were conducted in 1996 and 1998. In total, 21 countries have participated in the IALS.

Each country was assigned to draw a representative sample (ranging from 1,500 to 8,000 per country) of its adult, non-institutionalized, population aged 16-65 years using a similar sampling frame. For Sweden, the target population was all persons aged 16 years who were permanent residents of Sweden on 1 October 1994 and not living abroad or in institutions, including military service. The response rate for Sweden was 60 percent. Darcovich *et al* (1998) performs a non-response follow up study for Sweden and find no evidence of systematic or significant differences between respondents and non-respondents.

The IALS test consisted of three domains. *Prose literacy* - the ability to understand and use information from texts including editorials, news stories, poems and fiction. *Document literacy* - the ability to understand and locate information contained in various formats, including job applications, payroll forms, transportation schedules, maps, tables and graphics. *Quantitative literacy* – the ability to apply arithmetic operations to numbers embedded in printed materials, such as balancing a check account, calculating a tip or completing an order form.

The tasks at each domain test skills needed in everyday activities. Some typical tasks require the respondent to be able to understand a medicine label, to understand an instruction

[2] For a comprehensive description and detailed results for different countries, see OECD and Statistics Canada (1995).

of how to adjust a bicycle, to calculate the total amount of money received on an investment with given interest rate, and to understand a quick copy printing requisition form that might be found in the workplace.[3]

The respondent's literacy ability in the three domains was measured on a scale from 0 to 500 with the use of Item Response Theory (IRT) scaling.[4] The collectors of the data also classified the test scores on each domain into five skill levels, with level 1 being the lowest and level 5 the highest.

The Swedish panel

The Swedish micro data for 1994 contains 3038 individuals. Based on a random draw of these, 759 individuals also participated in a follow up study in early 1998.[5] In the follow up, a new equivalent test was given together with a new background questionnaire.[6] Besides the test scores for the two occasions, we have information on the respondents' employment status. For those not employed it is possible to observe when they last worked, i.e. time out of work. We also observe (self reported) annual earnings for 1993 and 1997, as well as background characteristics such as highest completed education, parents' highest education, age and country of birth. We are also able to observe if the respondents have completed any form of formal education between 1994 and 1998.

There are two main limitations with our data. First, earnings are reported on an annual basis and we do not have information on hours worked. Thus, we are not able to compute hourly earnings. Second, we only observe time out of work for those currently out of work in

[3] Each respondent were given a selection out of a pool of tasks, mostly with open-ended answers, designed to take about 45 minutes to complete. The pool of tasks consisted of 114 tasks that had had been field tested in a pilot study and found to be valid across countries. The tasks had been created from material such as news articles and documents sent in by each country's study manager as a part in the work to avoid cultural and language bias.
[4] See Yamamoto (1998) for a description of the IRT-method used in IALS.
[5] The follow up sample is not representative of the Swedish adult population due to the fact that too few immigrants wanted to participate.
[6] Both these studies show that the level of literacy skills in the Swedish population is high by international standards, e.g. OECD and Statistics Canada (1995).

Table 1: Definitions of variables and sample characteristics. Standard deviations are in parentheses.

Variable	Description	1994 earnings sample	1998 earnings sample	1998-1994 longitudinal earnings sample	1998-1994 longitudinal time out sample	1998-1994 longitudinal time out sample, level 1-3
skills	individual test score	312.63 (43.27)	316.41 (36.92)			
Δskills	skills98 - skills94			-4.36 (30.81)	-5.26 (32.32)	-3.18 (25.01)
ln(w)	log of annual earnings	12.21 (0.27)	12.37 (0.30)			
Δln(w)	ln(w98)-ln(w94)			0.18 (0.21)		
spell	time out in months 1998-94, max value=42.				2.27 (7.96)	3.19 (9.49)
timeout	1 if spell >0				0.117	0.156
not unemployed	1 if time out for other reason than unemployment				0.040	0.055
age		42.77 (10.24)	45.78 (10.38)	43.46 (9.05)	38.50 (11.83)	43.70 (11.92)
age29	1 if age in 1994 is lower than 30				0.257	0.218
schooling	years of schooling	12.42 (3.30)	12.81 (3.42)	12.84 (3.52)	12.26 (3.36)	11.04 (3.09)
ed1	1 if completed some secondary (max 8 years)	0.11	0.09	0.09	0.08	0.15
ed2	1 if completed lower secondary (min 9 years)	0.18	0.16	0.16	0.20	0.28
ed3	1 if completed secondary, vocational	0.14	0.11	0.10	0.14	0.16
ed4	1 if completed secondary, academic	0.22	0.26	0.27	0.25	0.21
ed5	1 if studied at university, less than three years	0.17	0.17	0.15	0.17	0.13
ed6	1 if studied at university, three years or more	0.18	0.21	0.23	0.16	0.06
Δed	1 if higher level of education in 1998 than in 94			0.01		
eddiff	1 if completed any form of education 1994-98				0.16	0.14
female	1 if female	0.41	0.46	0.41	0.53	0.55
immigrant	1 if born outside of Sweden	0.06	0.01	0.01	0.02	0.02
Observations		1018	312	207	622	307

1998. It is not possible to observe time out of work in-between 1994 and 1998 for those who were employed at the time of the follow up survey. In the empirical analysis we investigate the measurement error bias associated with this. Table 1 contains a description of the variables used as well as descriptive statistics. We will return to a discussion of the various samples used below.

For both 1994 and 1998, the tests scores from the prose, document and quantitative part are highly correlated. The correlation of the document and quantitative test scores is 0.95, while the correlation of the prose test score and the document and quantitative test scores is 0.90. The high correlations make it impossible to identify the separate effects of the three types of literacy on earnings. A similar reasoning applies to the relationship between time out of work and the three measures of literacy. We therefore carried out a principal components analysis to evaluate how best to aggregate the three individual literacy scores. The results from this analysis were clear and very similar to those obtained by Green and Ridell (2001) based on the Canadian part of the IALS. The first principal component places almost equal weights on the three literacy scores and accounts for 95 percent of the variance.[7] The second principal component, which accounts for 4 percent of the variance, does not add any information to the analysis of earnings or time out of work. Like Green and Riddell (2001), we draw the conclusion that it is appropriate to use the simple average of the three literacy scores as a measure of an individual's literacy ability. This average test score is henceforth simply called skills.

3. Are skills priced?

Several studies have included test scores from IALS in earnings equations for different countries and found that they have a positive and significant effect. Devroy and Freeman

[7] For the 1994 test scores, the weights associated with the first eigenvector are 0.57, 0.59 and 0.58 respectively. Almost identical values are obtained for 1998.

(2001) use data from the 1994 IALS to estimate earnings equations for Germany, the Netherlands, Sweden, and the US. They apply the average test score from prose, document, and quantitative as their measure of literacy skills. Controlling for sex, immigrant status, and (a quadratic in) age, they find that a 100-point increase in the test score raises earnings the most in the US with a 48 percent increase, while the smallest number is found for Sweden with a 13 percent increase.[8] Adding years of schooling to the equation gives an insignificant effect of skills for Germany, while the effect is significant for the 3 other countries with a 100 point increase associated with an increase in earnings of 23 percent for the Netherlands, 7 percent for Sweden, and 32 percent for the US.

Similar results were obtained by Blau and Kahn (2001) for a slightly different set of countries (Canada, the Netherlands, Sweden, Switzerland, and the US) and by Green and Riddell (2001) (for Canada) using instrumental variables procedures where schooling and literacy skills are treated as endogenous. The pattern found in these studies is broadly consistent with the differences in overall wage inequality between the countries; see e.g. Freeman and Katz (1996).

Since previous studies have been restricted to using cross section data, they have not been able to examine whether changes in literacy skills actually lead to changes in earnings. We are able to investigate this using the Swedish panel. Besides this, we also estimate cross-section earnings equations for the 1994 and 1998 data using a standard human capital earnings equation of the form:

$$(1) \quad \begin{aligned} \ln(w_{it}) &= \alpha + \beta_1 skills_{it} + \beta_2 age_{it} + \beta_3 age_{it}^2 + \beta_4\, female_{it} + \beta_5 immigrant_{it} \\ &+ \beta_6 education_{it} + \varepsilon_{it} \qquad t = 1994,\ 1998 \end{aligned} .$$

Assuming that the error term in (1) may be described as $\varepsilon_{it} = v_i + \eta_{it}$, where v_i is an unobserved person specific component fixed over time and η_{it} is an independent random

[8] The numbers for Germany and the Netherlands are 16 and 32 percent, respectively.

term, taking first differences of the variables in (1) will eliminate v_i and produce unbiased estimates of the effect of skills on earnings.

It is important to notice that first differences will only give unbiased estimates of β_1 if other first differences of exogenous variables that potentially should be included in (1) are uncorrelated with changes in skills. It is however difficult to see which variables this might be. A bigger problem is the fact that skills are measured with test scores that, by nature, always consists of some measurement error. This will bias the estimate toward zero in the cross section analysis even if the error is random. This bias will be aggravated when fixed effect estimation is used as long as the true values of the independent variable is correlated over time; see Griliches and Hausman (1986).

As noted above, our measure of earnings are based on annual data and we do not have information on hours worked. The age interval is therefore set to 20-64 years in order to minimize the probability of including people who just entered the labor market.[9] However, the sample still consists of a large proportion of earnings that apparently originated from part time work. One way to partly solve this is to truncate the earnings variable, that is, throw away observations with earnings lower than some predetermined number.[10] Earnings lower than the 10th percentile for full time earnings for all sectors, for men and women respectively, have therefore been excluded.[11] This leaves us with 1018 and 312 observations for 1994 and 1998 respectively and 207 and observations for both years. The first three columns of Table 1 provide descriptive statistics for these earnings samples.

The estimates of the earnings equations are contained in Table 2. The standard errors for the coefficients in all earnings equations have been estimated with White's (1980) standard

[9] An analysis using individuals aged 20 – 60 to reduce the impact of individuals exiting the labor market yields very similar results.
[10] Alternatively, we have tried various robust estimators along the lines suggested by Hamilton (1992). These results are qualitatively similar to those presented in the text.
[11] The income cut-offs for women are 137124 and 152400 SEK for 1994 and 1998 respectively. The cut-offs for men are 146736 and 163200 SEK.

Table 2: Earnings equation estimates

	1994	1994	1994	1998	1998	1998	1st difference	Fixed effect
skills/100	0.152	0.153	0.075	0.174	0.234	0.107	0.093	0.0846
	(0.018)	(0.016)	(0.018)	(0.044)	(0.041)	(0.044)	(0.046)	(0.0471)
age		0.031	0.028		0.044	0.047		
		(0.005)	(0.005)		(0.013)	(0.013)		
$age^2/100$		-0.030	-0.026		-0.042	-0.046		-0.0099
		(0.006)	(0.006)		(0.015)	(0.015)		(0.0086)
female		-0.217	-0.237		-0.198	-0.207		
		(0.014)	(0.014)		(0.030)	(0.029)		
immigrant		-0.042	-0.057		-0.058	-0.105		
		(0.026)	(0.025)		(0.031)	(0.046)		
ed2			0.021			-0.029		
			(0.027)			(0.043)		
ed3			0.068			-0.045		
			(0.029)			(0.046)		
ed4			0.081			0.125		
			(0.028)			(0.057)		
ed5			0.162			0.093		
			(0.029)			(0.054)		
ed6			0.256			0.273		
			(0.032)			(0.061)		
Δed								0.0199
								(0.0412)
constant	11.739	11.060	11.269	11.821	10.647	10.898	0.184	0.218
	(0.056)	(0.110)	(0.106)	(0.134)	(0.323)	(0.320)	(0.014)	(.031)
Adjusted R^2	0.056	0.272	0.353	0.042	0.206	0.307	0.014	0.0087
Observations	1018	1018	1018	312	312	312	207	207

Note: Dependent variables are log annual earnings in 1994, 1998, and the difference between log annual earning in 1998 and 1994, respectively. White's (1980) robust standard errors are in parentheses. The fixed effect model is in first difference form.

errors due to the presence of heteroskedasticity. The variable *skills* is highly significant in all specifications when cross section data is used. For 1994, a 100-point increase in the test score is associated with a 15 logpoints increase in earnings when no other regressors are included. Adding *age*, *female* and *immigrant* causes only minor changes. Adding controls for education causes the effect to decrease to 8 logpoints. For 1998, the effect of literacy skills is approximately the same as for 1994 when no controls are added but is noticeable higher in the other specifications, especially when education is controlled for, now being 10 logpoints. The difference between 1994 and 1998 seems mainly to be driven by the different (smaller) sample in 1998. Estimating the 1994 earnings equation using the 1998 sample produces estimates similar to the 1998 results.

For the 1994–1998 panel the effect of $\Delta skills$, when no other controls are included, is significant at the 5 percent level even though the sample size is only 207 individuals. Last are the fixed effect estimates. The effect from $\Delta skills$ is significant at the 10 percent level (p-value=0.074) and a 100-point increase in skills is associated with an 8.5 logpoints increase in earnings, which is close to the cross-section estimates when education is included. Thus the cross section association between skills and earnings holds true also in a fixed effect specification.

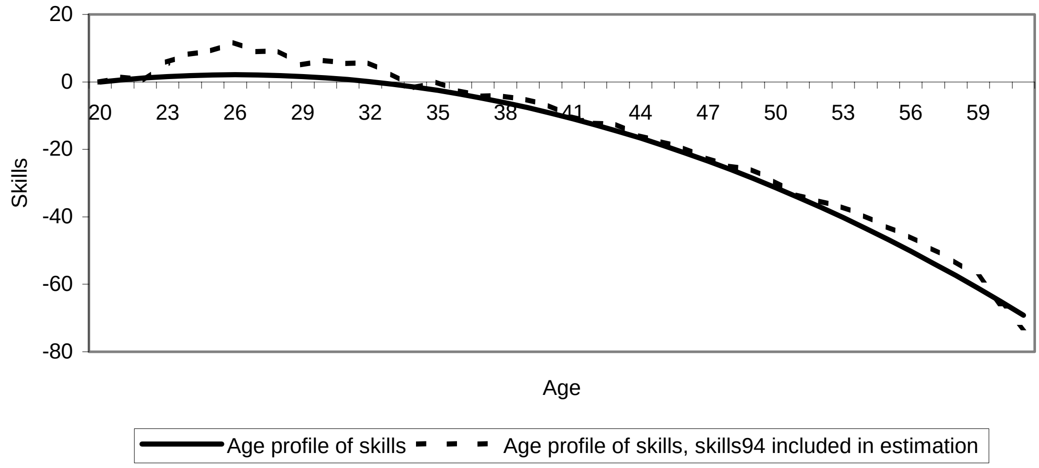
4. Time out and skill depreciation

After having established that our measures of skills seem to be priced in the labor market, we will now turn to our main objective: to investigate whether time out of the labor market leads to skill depreciation. In order to do this we first discuss how our estimates should be interpreted in terms of forgone experience versus skill depreciation. We then turn to our empirical estimates of time out and skill depreciation.

4.1 Forgone experience versus skill depreciation

Our estimates of the effect of time out on skills are based on a simple "value added" specification where the changes in individual skills are regressed on time out of work and a set of controls. Finding that time out of work has a negative effect on skills in this framework does not by itself imply that time out of work causes human capital to depreciate. If labor market experience has a sufficiently positive effect on literacy skills, our estimates could be due to forgone experience solely. As we do not have data on individuals' whole labor market history, there is no explicit way to estimate the connection between experience and skills. What we instead do is make use of the longitudinal aspect of our data and estimate how skills vary with age conditioned on full labor market experience, i.e. that an individual has no time

Figure 1: Predicted evolution of skills conditioned on full labor market experience



out during the whole work career. This will provide us with an (admittedly biased) estimate of the curvature of the experience-skill profile that can be used to assess the relative importance of forgone experience.

In Figure 1 we show the age profile of skills for workers without labor market interruptions. The solid line represents the implied age profile from a regression where $\Delta skills_i$ is regressed on a continuous age variable and a constant.[12] The dashed line shows the age profile from a value added specification where the initial level of skills is included. Both these set of estimates give a similar, and somewhat surprising, picture. Skills increase until the age of 26, and then decreases.

We are used to thinking about labor market experience as producing skills that generate "Mincerian" wage profiles. The pattern in Figure 1 does not fit well with this story. Our measure of skills does only to a small extent exhibit the increasing profile in early years. Also, net depreciation of skills starts at much younger ages than what would be implied by earnings profiles. The explanation for this pattern may have to do with our particular measure of skills that is constructed to measure basic general skills. Our results implicate that the curvature of

[12] Details of the estimates, including alternative specifications with age dummies, are reported in Appendix A.

standard age earnings profiles seems to a large extent be driven by other factors, e.g. specific skills. Still, the bottom line for our purposes is that the effects of foregone experience on our estimates are most likely limited. The (positive) effect of experience on skills is very small implying that the effect of foregone experience for individuals out of work also will be very small.

4.2 Are skills affected by time out?

We are now turning to the question of whether time out of work affects the level of skills of the individual. We are using the 1998–1994 panel that consists of 622 individuals after removing individuals who reported to be retired, full time students, or participating in the government adult education initiative ("kunskapslyftet").[13] Table 3 displays the number of individuals non-employed at the time of the 1998 test together with their current main activity in percent and the number of months since they last worked. The time since last worked is shown as detailed as we are able to observe it, that is, we are able to observe if they worked in the last 1-15 months, 16-27 months, 28-39 months and so forth; this is due to the layout of the questionnaire. As can be seen, those with time out are mainly unemployed.

The following equation will be estimated with OLS to investigate whether changes in skills are affected by time out:

$$(2) \qquad \Delta skills_i = \phi + \gamma \; timeout_i + \delta_1 eddiff_i + \boldsymbol{\delta_2} \mathbf{x}_i + \delta_3 skills94_i + \eta_i ,$$

where $timeout$ is either a dummy variable capturing those with time out of work in between the two test occasions or a continuous variable capturing the spell of the time out – the exact specification and why we use these variables will be discussed below. The variable $eddiff$ is a dummy for those who completed some formal education between the two test occasions,

[13] Including individuals who are currently students in the sample and controlling for these with a dummy causes no change in the final results.

Table 3: Reason for not being employed by months since last worked

	1-15 months	*16-27 months*	*28-39 months*	*>39 months*	Percent
Unemployed, looking for work	29	6		5	54.8%
Unemployed, employment training	1		1	6	11.0%
Long-term illness	2	1	1	3	9.6%
Homemaker		1		2	4.1%
Child care	3	2	1		8.2%
Other	5	1	2	1	12.3%
Total	40	11	5	17	73/100%

and *skills94* is the test score for 1994. The vector $\mathbf{x}_i$ captures individual characteristics in 1994 and includes age, schooling, and immigrant status.

The model that most resembles (2) is the so-called value-added model, see e.g. Hanushek (1979). There are several reasons for assuming a model where lagged skills are included. Investigating those not employed in 1998 gives at hand that they generally performed worse on the 1994 test than their equivalents that are working 1998.[14] This together with the fact that floor and ceiling effects in the test scores probably are present is one argument. Another, combined with the fact that time out may not be independent of past skills, is that regression to the mean might be present, that is, it is easier for individuals with low skills to improve their results due to their low initial value.[15]

Equation (2) is estimated for the whole sample as well as on a restricted sample where only those who scored no higher than level 3 on both the 1994 and the 1998 tests are included. The reason for this is that there seems to be a bigger uncertainty, or measurement error, in the upper part of the test score distribution due to very few tasks graded at level 5. This is explained by OECD and Statistics Canada (1995) as due to the focus on the low

[14] This is based on performing OLS on the test scores for 1994 with a dummy for those not employed 1998 and with controls for age, years of schooling (only available for 1994), immigrant status, and gender. The dummy coefficient becomes negative and significant with a value of -12.89. Those with time out longer than 39 months have been excluded in the regression. Robust regression gives the same results. Results are available upon request.
[15] The drawback with including the previous test score is that it leads to bias in all the estimated coefficients if the test result is measured with error.

skilled; they also combine those at level 5 and level 4 in their analysis of the test results. Columns 4 and 5 in Table 1 contain mean characteristics for the included individuals.

Economic theory offers no guide to the functional form of the loss of skills due to time out, and there are no previous studies on the subject. Another thing complicating the analysis is that there was approximately 42 months between the tests (October 1994 and March 1998) but some have not worked for a longer period than that. These should be coded as being out of work for 42 months if skill loss is linear. It may on the other hand be that the rate of skill loss increases (or decreases) with time out of work. We have therefore explored various specifications of equation (2) that allows for a distinction between short and long time out of work. The picture that emerges in all of these is that skill loss is more severe for those with relative long time out and that the loss of skills seems to be linear.[16]

The results for two of these specifications are shown in Table 4. The first specification involves a single dummy variable for those with time out. The estimates show a significant negative effect of time out of employment for the whole sample as well as for the restricted sample. The effect is, however, stronger for the restricted sample. The second specification uses a (quasi) continuous measure of months of time out, using the midpoints of the categorical variables, i.e. it takes the value zero for individuals without time out, 7.5 for an individual with 1-15 months of time out, 21 for those in the interval 16-27, and so forth, while individuals with time out of work longer than 40 months receive the value 42.[17] The continuous variable is highly significant for both samples and more negative for the restricted sample.[18]

---

[16] We have also tried various specifications where the effect of time out varies across groups, i.e. age, gender and type of time out. We were however not able to find significant differences across groups, possibly due to the fairly small sample sizes. These estimates are reported in Appendix B.
[17] We have investigated actual unemployment spells for a large sample of adults for the period between the two tests by using the Swedish longitudinal dataset LINDA (see Appendix C for a description) and found that the distribution within these categories to be approximate uniform with a mean and median very close to the midpoints.
[18] We have also tested for an occurrence effect from time out by including a dummy variable for time out in the continuous specification. This dummy variable is never statistically significant.

Table 4: Skill equation estimates

	Whole sample	Level 1-3	Whole sample	Level 1-3
timeout	-9.486	-11.243		
	(3.570)	(3.706)		
spell			-0.414	-0.518
			(0.143)	(0.139)
eddiff	7.395	6.209	7.049	5.169
	(3.559)	(4.422)	(3.536)	(4.321)
ed2	11.392	7.398	11.314	7.339
	(4.764)	(4.290)	(4.758)	(4.257)
ed3	9.745	5.111	9.450	4.948
	(5.156)	(4.957)	(5.152)	(4.920)
ed4	19.765	5.427	19.501	5.002
	(4.775)	(4.626)	(4.770)	(4.586)
ed5	19.415	7.250	19.085	6.834
	(4.996)	(5.089)	(4.992)	(5.053)
ed6	28.381	15.060	28.189	15.759
	(5.224)	(6.316)	(5.218)	(6.273)
age	-0.261	-0.202	-0.240	-0.177
	(0.112)	(0.132)	(0.112)	(0.131)
immigrant	-2.544	0.942	-1.703	2.396
	(8.554)	(8.761)	(8.549)	(8.707)
skills94	-0.442	-0.350	-0.441	-0.357
	(0.031)	(0.047)	(0.031)	(0.047)
constant	127.344	99.719	126.403	100.981
	(11.023)	(13.997)	(10.954)	(13.895)
Adjusted R^2	0.254	0.173	0.256	0.185
Observations	622	307	622	307

Note: Dependent variable is changes in test scores. Standard errors are in parentheses.

It could be that loss of skills leads to non-employment, and that this drives our results, i.e. that we have reverse causality. However, we have also used dummy variables for each of the time out intervals that we observe and not been able to reject the hypothesis that the implied skill loss from these differs from that implied by the continuous variable. Specifically, the dummy variable for those with time out of work longer than 42 months, i.e. that capture those who were non-employment by the time of the 1994 test and then have been so up until the 1998 test, is the most negative and the most significant. We interpret this as evidence against a story where one time shifts in skills leads to unemployment. We cannot, however, rule out the case where negative *trends* in skills lead to unemployment. To be able to investigate this issue we would need a third wave of data.

As previously mentioned, the time out variables do not capture those with time out in between the two test occasions that worked at the time of the 1998 survey. The bias associated with this is discussed and estimated in Appendix C. The main finding is that the dummy variable is biased toward zero and should be corrected upward by 36 percent. The bias for the continuous variable appears to be small and toward zero; our estimate indicate that its effect should be corrected upward by 4.6 percent.

How much of our estimated skill loss from time out is then due to skill depreciation? To give the forgone experience hypothesis the most possible weight, let us say that the effect of experience is the same over the life cycle. Using the estimates underlying Figure 1 (reported in Appendix A), an individual aged 20 in 1994 gains 2.26 points of skills in 3.5 years (10.424-20*0.408). This means that each month of time out of work results in 0.054 points lower skills (2.26/42) due to forgone experience. From Table 4, our lowest estimated skill loss from one month of time out is 0.414. Hence, the minimum value of skill depreciation should be around 0.36 points a month (0.414-0.054). On the other end, if experience mainly affects skills before the age of 30, the estimated effect of time out of work in Table 4 is approximately only due to skill depreciation. The correct estimate is probably somewhere between these estimates, but nevertheless, they both point to the conclusion that the main force captured in Table 4 is skill depreciation.

A natural question is whether the estimated skill depreciation effects are economically significant. We illustrate this in two different ways. First, we ask how a spell of unemployment affects the individual's position in the skill distribution. Second, we calculate the implied wage losses from our analysis and compare those to estimated wage losses from time out in previous studies.

In order to assess the effect of time out on the individual's position in the overall skill distribution we use the estimate in column 4 of Table 4. This estimate is based on the sample where the highest skill groups are excluded in order to reduce the measurement error in skills. Using this estimate we find that a 12 month spell of non-employment would move an individual at the median of the 1994 skill distribution to the 44.5th percentile. Similarly, an individual at the 25th percentile would fall to the 20.5th percentile after a year of non-employment. Thus, our estimates imply fairly large effects of non-employment on relative skills.

To assess the pecuniary effects of work interruptions, we use the wage equation estimated with fixed effects in Table 2 and the skill equation with months of time out for the low skilled sample in Table 4. These estimates imply that 12 months of time out of work results in a wage decrease of 0.52 percent. Since the fixed effects estimates may be affected by measurement errors, we also calculate the same number using the largest cross section wage estimate, the estimate for 1998 with included controls for age, gender and immigrant status. In this case we get a wage decrease of 0.95 percent for a year of time out of work. The "baseline" numbers of between 0.52 and 0.95 percent can be compared with the average of the estimated wage penalties of 3.24 percent found in the panel data analysis of Albrecht *et al* (1999), Table 2. Consequently, our estimates would account for between 16 and 29 percent of the wage penalty.

5. Concluding remarks

This paper investigates the role of skill depreciation in the relationship between work interruptions and subsequent wages. Using a unique longitudinal dataset, the Swedish IALS database, we are able to analyze changes in skills for individuals as a function of time out of work. In an initial analysis we first look at the relationship between our measure of skills and earnings. We confirm the cross section association between test scores and earnings, and show that the relationship holds also in longitudinal data.

In the main analysis we analyze the effect of work interruptions on changes in test scores. In general, we find statistically strong evidence of a negative relationship between work interruptions and skills. Also, it seems like skill depreciation is economically relevant. Our estimates imply that one year out of work will move an individual 5 percentile points down the skill distribution. The implied wage reduction due to depreciation of literacy skills accounts for 15–30 percent of the overall wage penalty for work interruptions.

References

Agell, J and H Bennmarker (2002), “Wage Policy and Endogenous Wage Rigidity: A Representative View from the Inside”, IFAU Working Paper 2002:12.

Aigner, D (1973), “Regression with a Binary Independent Variable Subject to Errors of Observations”, *Journal of Econometrics*, Vol. 1, pp. 49-60.

Albrecht, J, P-A Edin, M Sundström and S Vroman (1999), “Career Interruptions and Subsequent Earnings: A Reexamination Using Swedish Data, *Journal of Human Resources*, Vol. 34, pp. 294-311.

Angrist, J and A Krueger (1999), “Empirical Strategies in Labor Economics”, in Ashenfelter, O and D Card, ed., *Handbook of Labor Economics*, Vol. 3, Elsevier Sience B.V.

Blau, F and L Kahn (2001), “Do Cognitive Test Scores Explain Higher US Wage Inequality?”, NBER Working Paper no. 8210.

Blinder, A and D Choi (1990), “A Shred of Evidence on Theories of Wage Stickiness”, *Quarterly Journal of Economics*, Vol. 105, pp. 1003-1015.

Calmfors, L (1994), “Active Labour Market Policy and Unemployment – A Framework for the Analysis of Crucial Design Features”, OECD Economic Studies 22.

Darcovich, N, M Binkley, J Cohen, M Myrberg and S Persson (1998), “Non-response Bias” in U.S. Department of Education, National Center for Education Statistics (NCES), *Adult literacy in OECD Countries: Technical Report on the First International Adult Literacy Survey*, NCES.

Devroye, D and R Freeman (2001), “Does Inequality in Skills Explain Inequality of Earnings Across Advanced Countries?” NBER Working Paper no. 8140.

Edin, P-A and P Fredriksson (2000), “LINDA – Longitudinal Individual Data for Sweden” Working Paper 2000:19, Department of Economics, Uppsala University.

Freeman, R and L Katz (1995), *Differences and Changes in Wage Structures,* University of Chicago Press.

Gibbons, R and L Katz (1991), “Layoffs and Lemons”, *Journal of Labor Economics,* Vol. 9, pp. 351-380.

Green, D and C Riddell (2001), “Literacy, Numeracy and Labour Market Outcomes in Canada”, Discussion Paper No. 01-05, Department of Economics, University of British Columbia.

Griliches, Z and J Hausman (1986), “Errors in Variables in Panel Data”, *Journal of Econometrics*, Vol. 31, pp. 93-118.

Hamilton, L (1992), *Regression with Graphics: A Second Course in Applied Statistics*, Duxbury Press.

Hanushek, E (1979), “Conceptual and Empirical Issues in the Estimation of Educational Production Functions”, *Journal of Human Resources*, Vol. 14, pp. 351-388.

Jacubson, L, R LaLonde and D Sullivan (1993), *The Cost of Worker Dislocation,* Upjohn Institute.

Kaestner, R, T Joyce and H Wehbeh (1996), “The Effect of Maternal Drug Use on Birth Weight: Measurement Error in Binary Variables”, *Economic Inquiry*, Vol. 34, pp. 617-629.

Kirsch, I and T Murray (1998), “Introduction”, in U.S. Department of Education, National Center for Education Statistics (NCES), *Adult literacy in OECD Countries: Technical Report on the First International Adult Literacy Survey*, NCES.

Mincer, J and S Polachek (1974), “Family Investments in Human Capital: Earnings of Women”, Journal of Political Economy, Vol. 82, pp.76-108.

Neal, D (1995), “Industry-Specific Human Capital: Evidence from Displaced Workers”, *Journal of Labor Economics* 13, 653-677.

OECD and Statistics Canada (1995), *Literacy, Economy and Society*, OECD Publications.

Phelps, E (1972), *Inflation Policy and Unemployment,* Norton.

Pissarides, C (1992), “ Loss of Skill During Unemployment and the Persistence of Unemployment Shocks”, *Quarterly Journal of Economics,* Vol. 107, pp. 1371-1391.

White, H (1980), “A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity”, *Econometrica*, Vol. 48, pp. 817-830.

Wiklund, F (1995), “Unemployment and Subsequent Wages: Does Gender Matter?”, Working paper 1995:5, Department of Economics, Uppsala University.

Yamamoto, K (1998), “Scaling and Scale Linking”, in U.S. Department of Education, National Center for Education Statistics (NCES), *Adult literacy in OECD Countries: Technical Report on the First International Adult Literacy Survey*, NCES.

Appendix A: Measuring the effect of foregone experience on skills

To get a sample where as many as possible has full experience between the two tests, we only use individuals with a job at both time points (1994 and the 1998) who did not participate in any form of formal education between the tests, and who at the time of the 1998 test had worked for at least 50 weeks, including vacation, during the last 12 months. This leaves us with 385 observations, and the age in 1994 range between 20 to 60 years in this sample.

In the first column of Table A1, $\Delta skills_i$ is regressed on a continuous age variable and a constant. Based on these estimates, the implied average age profile of skills conditioned on full labor market experience, i.e. no time out from the labor market during the age range studied here, is displayed as the solid line in Figure 1. Skills increase until the age of 26, and then decreases. As the specification in the first column of Table A1 is quite restrictive, the next column contains a specification with 4 age dummies, where the dummy for age 20 to 29 is omitted. Here, the intercept (i.e. the variable *age2029*) and the *age3039* and *age4049* variables are not significantly different from zero at the five percent level. Based on the coefficient estimates, this model shows a positive relation between age and skills before the 30s, and after that a negative effect, and the predicted age pattern is very similar to that from the continuous age variable; this also holds for various other models examined.

As we use a "value added model" to investigate the effect of time out of work, which includes *skills94*, it is important to see if our age profile of skills changes if we also control for *skills94*. This could for example happen if regression to the mean in the test scores affects the estimates in the first and second columns of Table A1. The third and forth columns contain the relevant regression results. In obtaining the average age profile, we predict $\Delta skills_i$ and use the average predicted value for each age. One problem here is that there are few observations at the youngest and oldest ages, four individuals is the lowest number, which could result in some irregular predictions due to extreme values on the *skills94*

Table A1: Estimates of changes in skills for individuals without time out

	(1)	(2)	(3)	(4)
age	-0.408		-0.412	
	(0.160)		(0.145)	
age3039		-8.261		-8.548
		(5.187)		(4.706)
age4049		-9.823		-10.349
		(5.057)		(4.589)
age5060		-13.844		-13.403
		(5.308)		(4.816)
skills94			-0.333	-0.334
			(0.037)	(0.037)
constant	10.424	2.258	116.304	108.747
	(6.828)	(4.281)	(13.201)	(12.340)
Adjusted R^2	0.014	0.008	0.188	0.183
Observations	384	384	384	384

Note: Dependent variable is changes in test scores. Standard errors are in parentheses. Age is measured in 1994.

variable. Another problem is that the value of *skills94* is a function of past labor market experience, which we do not know anything about; we can therefore not formally say that the predictions are conditioned on full experience. Based on the estimates in the third column of Table A1, the dotted line in Figure 1 is the predicted age profile of skills; using the estimates in the forth column gives similar results. As can be seen, the age pattern is the same as when *skills94* is omitted from the regression.

Appendix B: Differences in skill depreciation

Table B1: Skill equation estimates, allowing for differences in the rate of depreciation

	Whole sample	Level 1-3	Whole sample	Level 1-3	Whole sample	Level 1-3
spell	-0.342	-0.386	-0.603	-0.472	-0.408	-0.254
	(0.159)	(0.153)	(0.184)	(0.170)	(0.245)	(0.243)
spell*age29	-0.426	-0.825				
	(0.360)	(0.364)				
age29	4.614	8.508				
	(4.174)	(5.198)				
spell*not unemployed			0.451	-0.132		
			(0.277)	(0.276)		
spell*female					-0.003	-0.392
					(0.300)	(0.297)
female					-1.109	0.994
					(2.396)	(2.783)
eddiff	6.856	5.040	7.309	4.989	7.203	5.313
	(3.541)	(4.299)	(3.535)	(4.342)	(3.560)	(4.329)
ed2	11.667	7.578	11.350	7.263	11.297	7.172
	(4.764)	(4.227)	(4.752)	(4.265)	(4.766)	(4.263)
ed3	9.687	5.229	9.736	4.880	9.602	4.653
	(5.154)	(4.892)	(5.148)	(4.928)	(5.174)	(4.941)
ed4	19.763	5.991	19.988	4.848	19.684	4.411
	(4.772)	(4.571)	(4.772)	(4.604)	(4.802)	(4.631)
ed5	19.443	6.947	19.385	6.753	19.396	6.048
	(4.998)	(5.019)	(4.988)	(5.062)	(5.059)	(5.131)
ed6	28.553	15.430	28.419	15.780	28.316	15.160
	(5.223)	(6.237)	(5.213)	(6.282)	(5.238)	(6.300)
age	-0.140	-0.021	-0.242	-0.175	-0.236	-0.187
	(0.161)	(0.189)	(0.112)	(0.131)	(0.113)	(0.132)
immigrant	-1.409	1.812	-2.671	2.870	-1.578	3.276
	(8.570)	(8.652)	(8.558)	(8.774)	(8.571)	(8.736)
skills94	-0.442	-0.351	-0.444	-0.356	-0.442	-0.356
	(0.031)	(0.047)	(0.031)	(0.047)	(0.031)	(0.047)
constant	121.558	91.098	127.318	100.664	127.119	100.804
	(12.151)	(14.883)	(10.954)	(13.929)	(11.076)	(13.914)
Adjusted R^2	0.256	0.198	0.258	0.183	0.254	0.185
Observations	622	307	622	307	622	307

Note: Dependent variable is changes in test scores. Standard errors are in parentheses.

Appendix C: The effect of measurement errors in observed time out

The first test was taken in early October 1994 and the second in late March 1998. At each occasion the respondents were asked about their current labor market status. If they reported to be anything else than "Employed" or "Self employed" they were also asked when they last worked at a job or business. Potential time out spells in-between the tests for individuals employed at the second test are therefore not observed. Also, for individuals not employed at the time of the second test we only observe the current spell of time out, i.e. we never observe multiple spells in-between the tests. The used time out variable may then be described as:

(C1) $t_i = T_i - u_i$,

where t_i is observed time out, T_i is true time out, and u_i is measurement error. Given true responses to the relevant questions, u_i is non-negative and less than or equal to T_i for the binary as well as the continuous variable. This corresponds to non-classical measurement error in the sense that the error is not mean zero nor uncorrelated with the true value. Inserting the true time out variable in equation (2) and using (C1) gives:

(C2) $$\begin{aligned}&\Delta skills_i = \phi + \gamma t_i + \gamma u_i + \delta_1 eddiff_i + \delta_2 education94_i + \delta_3 age94_i + \delta_4 immigrant_i \\ &+\delta_5 skills94_i + \eta_i\end{aligned}.$$

With only observed time out included in (C2), we get an omitted variable bias resulting in the following estimate of γ:

(C3) $\hat{\gamma} = \gamma(1+\theta)$,

where θ is equal to the partial correlation between observed time out and the measurement error holding constant all of the other variables, i.e.:

(C4) $$\begin{aligned}&u_i = \psi_1 + \theta t_i + \psi_1 eddiff_i + \psi_2 education94_i + \psi_3 age94_i + \psi_4 immigrant_i \\ &+\psi_5 skills94_i + \varepsilon_i\end{aligned}.$$

In the case of a binary variable, Aigner (1973) shows that $\hat{\gamma}$ is biased toward zero. For a continuous variable, however, the bias can go in either direction depending on the sign and magnitude of the correlation between the true variable and the error conditioned on the other regressors; e.g. Kaestner *et al* (1996) and Angrist and Kruger (1999). To see this, assume that time out was the only regressor (with a constant), θ would then equals:

(C5) $\theta = Cov(t,u)/Var(t) = [Cov(T,u) - Var(u)]/[Var(T) + V(u) - 2Cov(T,u)]$.

If we mainly miss time out spells for those with long true time out, $Cov(T,u)$ will be greater than zero but the direction of the bias is indeterminate because the sign of the term $Cov(T,u) - Var(u)$ is unknown. On the other hand, if we mainly miss time out spells for individuals with shorter true time out, $Cov(T,u)$ will be less than zero and $\hat{\gamma}$ is an underestimate of the true coefficient.

As suggested by Aigner (1973), we use outside information to estimate (C4). The estimated θ is used to adjust our estimated effect of time out. Although the adjusted effect will only have "a sort of 'approximate' consistency", Aigner (1973 p.55), it still provides an idea of the sign and the size of the bias. The variable *skills94* is, of course, unique for the IALS-panel; the consequence of omitting this variable in (C4) is discussed below.

We use the Swedish register-based longitudinal database LINDA, described in Edin and Fredriksson (2000). It contains a random representative sample of 3.35 percent of the Swedish population.[19] Besides individual characteristics, it also contains information from the Swedish National Labour Market Administration (AMS). We are therefore able to observe whether, why, and for how long, an individual has been registered at an unemployment office in Sweden. We use the individual characteristics information in LINDA for the years 1994-1998 and the information about unemployment for the period 1994-09-30 to 1998-03-31, corresponding to the period between the two literacy tests, to replicate our IALS-panel, the

[19] This corresponds to 300,000 individuals.

observed time out variables therein, and the true value of the time out variable. The observed and the true value of the binary variable, *timeout*, is created from LINDA by using the same definitions as in (C1):

(C6) $timeout_i = TIMEOUT_i - u_i$,

where $TIMEOUT_i$ contains the true value. Those unemployed 1998-03-31 correspond to those observed being unemployed in the IALS-panel and therefore receive a value of $timeout_i$ equals to one, while all individuals unemployed at least once between 1994-09-30 to 1998-03-31 receive a value of $TIMEOUT_i$ equals one. The variable u_i is then created as the difference between the true variable and the observed variable. For our continuous variable, *spell*, we have:

(C7) $spell_i = SPELL_i - e_i$,

where $SPELL_i$ contains the true value. An individual unemployed 1998-03-31 receive a value of $spell_i$ corresponding to the duration in months of the current spell, while all individuals unemployed at least once 1994-09-30 to 1998-03-31 receive a value of $SPELL_i$ equals the total number of months of time out in-between these two dates. The variable e_i is then created in the same manner as for the binary variable.[20]

Table C1 contains the mean values of the time out variables from LINDA and the IALS-data. In order for these means to be comparable the IALS variables now only captures those unemployed. Individuals in LINDA 1998 who are retired, students, or not in the age interval 20-64, have been removed, all in order to replicate our IALS-sample. As can be seen, the observed values for the IALS-panel is about half the size of the corresponding numbers

[20] In order to get the same type of variable as in the IALS-panel, the time out durations have first been placed in the intervals 1-15 months, 16-27 months, and so forth. The midpoints in these intervals have then been used. Whether one use this variable or the ‘raw’ variable has no consequence; the results are very close to one another.

Table C1: Sample means for the time out variables

Variable	LINDA	IALS-panel
spell	2.99 (9.28)	1.36(6.17)
SPELL	6.20 (11.79)	--
timeout	0.14	0.12
TIMEOUT	0.31	--
Observations	135,614	622

Note: Standard deviations are in parentheses.

for LINDA. This could indicate that the IALS- panel is not representative for the Swedish population in that too few unemployed individuals are included.[21]

The estimates of (C4) are presented in Table C2. The estimates of θ are –0.266 and –0.044 for the binary and continuous variable, respectively. This indicates a non-negligible bias toward zero for the binary variable. For the continuous variable, however, the bias seems to be small and towards zero.

One possible explanation for the estimated small bias for the continuous variable could be the omission of *skills94* in the estimated equation. According to the standard omitted variable framework, the consequence of omitting *skills94* for the estimated θ is:

(C8) $\hat{\theta} = \theta + \psi_5 \rho_{st}$,

where ψ_5 is the coefficient for $skills94$ in (C4) and ρ_{st} belongs to the following regression:

(C9) $t_i = \alpha + \rho_{st} skills94 + \rho_1 eddiff_i + \rho_2 education94_i + \rho_3 age94_i + \rho_4 immigrant_i + \upsilon_i$.

An estimate of ρ_{st} is straightforward to obtain from the IALS-data. The result is displayed in Table C3. As can be seen, $\hat{\rho}_{st}$ is close to zero in both cases indicating that the effect of omitting $skills94$ probably is small.

[21] As previously mentioned we know for a fact that to few immigrants are included in the IALS-panel. However, excluding the immigrants in the LINDA sample causes no dramatic changes. However, we have excluded too many individuals from the LINDA-sample in that we identify e.g. students by observing if an individual has received student grants some time during 1998, i.e. we are not able to observe whether he or she actually was a student at the exact time of the second IALS-test.

Table C2: OLS estimates of equation (C4)

	Binary	Continuous
timeout	-0.266	
	(0.003)	
spell		-0.044
		(0.002)
ed2	-0.043	-0.669
	(0.004)	(0.089)
ed3	-0.046	-0.570
	(0.004)	(0.078)
ed4	-0.023	-0.466
	(0.004)	(0.087)
ed5	-0.081	-1.738
	(0.004)	(0.087)
ed6	-0.111	-2.057
	(0.004)	(0.088)
immigrant	0.089	2.241
	(0.003)	(0.065)
eddiff	0.113	-0.411
	(0.004)	(0.085)
age94	-0.010	-0.158
	(0.000)	(0.002)
constant	0.613	9.983
	(0.006)	(0.118)
Adjusted R^2	0.149	0.072
Observations	135,614	135,614

Note: Dependent variables are measurement errors. Standard errors are in parentheses.

Overall, the analysis of the effect of measurement errors in reported time out indicates that the estimate for the time out dummy variable should be corrected upward with 36 percent. For the continuous variable, the estimate appears to be biased toward zero, although the bias appears to be small; according to Table C2 the estimated effect of time out from the continuous variable should be corrected upward by 4.6 percent.

Table C3: Equation (4) estimated with IALS data.

	Binary	Continuous
skills94	-0.00122	-0.02651
	(0.00035)	(0.00868)
ed2	0.034	0.597
	(0.054)	(1.342)
ed3	-0.005	-0.839
	(0.058)	(1.452)
ed4	0.017	-0.254
	(0.054)	(1.345)
ed5	0.007	-0.631
	(0.057)	(1.407)
ed6	0.016	-0.108
	(0.059)	(1.471)
immigrant	-0.015	1.676
	(0.097)	(2.410)
eddiff	0.144	2.468
	(0.040)	(0.992)
age94	0.0003	0.059
	(0.0013)	(0.032)
constant	0.454	8.125
	(0.123)	(3.072)
Adjusted R^2	0.038	0.024
Observations	622	622

Note: Dependent variable is observed time out. Standard errors are in parentheses.