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Time Out of Work and Skill Depreciation

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Abstract
This paper investigates the role of skill depreciation in the relationship between work interruptions and subsequent wages. Using Swedish data from two waves (1994 and 1998) of the International Adult Literacy Survey, which included results of tests gauging respondents' ability to read and make practical use of printed information, the authors are able to analyze changes in individuals' skills as a function of time out of work. They find statistically strong evidence of a negative relationship between work interruptions and skills. The analysis suggests that depreciation of general skills was economically important. A full year of non-employment, for example, was associated with a 5-percentile move down the skill distribution.

Keywords
skill depreciation, unemployment, time out of work

TIME OUT OF WORK AND SKILL DEPRECIATION

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This paper investigates the role of skill depreciation in the relationship between work interruptions and subsequent wages. Using Swedish data from two waves (1994 and 1998) of the International Adult Literacy Survey, which included results of tests gauging respondents' ability to read and make practical use of printed information, the authors are able to analyze changes in individuals' skills as a function of time out of work. They find statistically strong evidence of a negative relationship between work interruptions and skills. The analysis suggests that depreciation of general skills was economically important. A full year of non-employment, for example, was associated with a 5-percentile move down the skill distribution.

Economists have long 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 in these issues, however, goes far beyond potential effects at the individual level. Questions about unemployment's possible negative effects on human capital formation figure prominently in many discussions of the persistence of unemployment, or "unemployment hysteresis" (see, for example, Phelps 1972; Blanchard and Summers 1986; Pissarides 1992). In these models, unemployment, by arresting skill formation, gives rise to further unemployment. Similarly, potential detrimental effects of unemployment loom importantly in discussions about the role of active labor market policies in fighting unemployment (for example, Calmfors 1994). The existence and magnitude of skill depreciation have important implications for designing anti-unemployment policies.

Empirical studies of how work interruptions affect individuals can roughly be divided into two main strands. One strand has been concerned with women's participation 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 (see, for example, Jacobson et al. 1993).[1] This literature is mainly concerned

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[1]A related research field has focused on scarring, or state-dependence, effects of unemployment among youths; see, for example, Heckman and Borjas (1980) and Ellwood (1982).

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with wage penalties associated with loss of firm- or industry-specific human capital (Neal 1995). In general, empirical studies show that work interruptions have negative effects on wages; that is, time out of work induces a wage loss larger than can be explained by forgone experience alone.

The negative wage effects of time out of work have normally been interpreted as due to human capital depreciation. This interpretation, however, has never been put to direct empirical test. There are other potential explanations for the negative association between work interruptions and wages, perhaps the most obvious being various signaling hypotheses. Gibbons and Katz (1991) found that part of the wage (and employment) consequences of displacement may be due to signaling effects.[2] Also, Albrecht et al. (1999) found that the sign and magnitude of the wage effect depend on gender and the reason for time out, a pattern that is not consistent with the simple human capital depreciation story.

In this paper we directly investigate whether time out from the labor market actually leads to human capital depreciation. Though such a relationship has long been a standard textbook explanation for the wage penalty associated with time out of work, our study represents the first explicit test of that mechanism. Such a test will both enhance our understanding of the consequences of employment interruptions and assist in policy design.

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 in 1994 and 1998. Several studies have demonstrated that the skills captured by the IALS have important effects on a worker's wage (Blau and Kahn 2005; Leuven, Oosterbeek, and van Ophem 2004; Green and Riddell 2003; Devroye and Freeman 2001; OECD and Statistics Canada 1995). Using the Swedish panel, we are thus able both to relate changes in individual skill levels to time out of work and to infer the economic significance of skill depreciation.

Data

The 1994 International Adult Literacy Survey[3]

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, the United States, Netherlands, Poland, and Sweden. The purpose of the IALS was to measure adult literacy within each country by means that would also allow for cross-country comparisons. The success of the initial survey inspired two later waves, conducted in 1996 and 1998. In total, 21 countries participated in the IALS.

Literacy, commonly understood as the ability to read and write, is specially defined by the IALS as the ability to understand and practically employ printed information of the kind frequently encountered in real life. Survey participants were therefore tested for a broad set of information-processing competencies, with special weight given to skills of the kind that are needed in the labor market.

An IALS coordinator in each country was directed to assemble a representative sample of the country's adult, non-institutionalized, population. The sample size varied by country, ranging from 1,500 to 8,000. For Sweden, the target population was all persons at least 15 years of age who were permanent residents of Sweden on October 1, 1994, and not living abroad or in institutions, including military service. The response rate for Sweden was 60%. Darcovich et al. (1998) performed a non-response follow-up study for Sweden and found no evidence of systematic or statistically significant differences between respondents and non-respondents.

The participants were given tasks that belonged to three broad domains of literacy:

[2]There is also convincing evidence from survey data that employers use unemployment as a negative signal. See, for example, Blinder and Choi (1990) and Agell and Bennmarker (2007).

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

prose literacy, the ability to understand and use information from texts; *document literacy,* the ability to understand and locate information contained in various formats, including maps, tables, and graphics; and *quantitative literacy,* the ability to apply arithmetic operations to numbers embedded in printed materials.

The tasks in each domain built on real-life documents. Three examples of documents respondents were asked to interpret were a quick copy printing requisition form of the kind that might be found in the workplace, a medicine label, and instructions for adjusting a bicycle.[4]

The respondent's literacy in the three domains was measured on a scale from 0 to 500 with the use of Item Response Theory (IRT) scaling.[5] Five skill levels, based on test scores, were defined for each domain, with level 1 indicating the lowest proficiency and level 5 the highest.

An important question for our purpose is, of course, whether the test scores constitute a good measure of general work-related skills. The relevance of these scores might be challenged, for example, on the grounds that required literacy skills vary greatly across occupations. Keep in mind, however, the broadness of the IALS's definition of *literacy,* which captures the ability to understand and employ written information. Although different occupations obviously require different specific skills, our prior is that virtually all occupations in today's labor market involve information-processing skills, and that individuals with fluent understanding and use of written information are rewarded for this ability. We will return to this question in a later discussion ("Are Skills Priced?"), where we look at the connection between wages and the IALS skill measure.

The Swedish Panel

The Swedish micro data for 1994 cover 3,038 individuals ranging in age from 15 to 94.[6] A follow-up study in early 1998 reexamined 759 of these participants, chosen by random draw.[7] In the follow-up, respondents completed both a new test, equivalent to the earlier one, and a new background questionnaire.[8] Besides the two sets of test scores, we have information on the respondents' employment status. For those not employed, we have the date they last worked, which tells us the length of the unemployment spell. We also observe (self-reported) annual earnings for 1993 and 1997, as well as background characteristics such as age, country of birth, education level, and parents' education level. In addition, we know whether respondents completed any kind of formal education between 1994 and 1998.

Our data have two main limitations. 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 observe time out of work only for those currently out of work in 1998. It is not possible to observe time out of work 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 information gap. Table 1 describes the variables and samples used in our estimations.

For both 1994 and 1998, the scores from the three parts of the test are highly correlated, with a correlation coefficient of 0.95 between the document and quantitative scores, 0.90

[4]The tests, designed to take about 45 minutes to complete, consisted of a selection of tasks, mostly with open-ended answers, from a pool of 114 tasks that had had been field-tested in a pilot study and found to be valid across countries. The exact selection of tasks varied from one individual to the next. The tasks were created from news articles, product instructions, business forms, and other documents provided by IALS coordinators in the respective countries, and care was taken to ensure that the finished items were free of cultural and language bias.

[5]See Yamamoto (1998) for a description of the IRT method used in IALS.

[6]The data contain 2,450 individuals in the age range 19–64—the typical age range of persons in the Swedish labor force.

[7]No non-response study is available for the 1998 sample. We do, however, know that immigrants are under-represented in the data.

[8]Both of these studies show that literacy rates across the Swedish population are high by international standards; see, for example, OECD and Statistics Canada (1995).

Table 1. Definitions of Variables and Sample Characteristics.
(standard deviations in parentheses)

Variable	*Description*	*1994 Earnings Sample*[a]	*1998 Earnings Sample*[b]	*1998–1994 Longitudinal Earnings Sample*[c]	*1998–1994 Longitudinal Time Out Sample*[d]	*1998–1994 Longitudinal Time Out Sample, Level 1–3*[e]
Skills	Individual test score	312.63 (43.27)	316.41 (36.92)			
Δ*Skills*	*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, 1994–98, 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)	41.97 (10.31)	43.46 (9.05)	38.50 (11.83)	43.70 (11.92)
Age29	1 if age in 1994 is lower than 30	0.13	0.15	0.08	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
Δ*Ed*	1 if higher educational attainment level in 1998 than in 1994		0.01	0.01	0.05	0.05
EdDiff	1 if any formal training or course, 1994–98		0.07	0.04	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		1,018	312	207	622	307

[a]Used in the Mincerian wage equations in Table 2, columns (2)–(4).
[b]Used in the Mincerian wage equations in Table 2, columns (5)–(7).
[c]Used in the fixed effect wage equations in Table 2, columns (8)–(9).
[d]Used in the time out equations in Table 4, columns (2) and (4).
[e]Used in the time out equations in Table 4, columns (3) and (5).

between the document and prose scores, and .90 between the prose and quantitative scores. 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 Riddell (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% of the variance.[9] The second principal component, which accounts for 4% of the variance, does not add any information to the analysis of earnings or time out of work. We therefore conclude that it is appropriate to use the simple average of the three literacy scores as a measure of an individual's literacy. This average test score is henceforth simply called skills.

Are Skills Priced?

Several studies have included test scores from IALS in earnings equations for different countries and have found that these scores have a positive and statistically significant association with earnings. Devroye and Freeman (2001) used data from the 1994 IALS to estimate earnings equations for Germany, the Netherlands, Sweden, and the United States. To measure literacy skills, they averaged the prose, document, and quantitative test scores. Controlling for sex, immigrant status, and (a quadratic in) age, they found that a 100-point increase in the test score raised earnings the most in the United States (a 48% increase) and the least in Sweden (a 13% increase).[10] When a control for years of schooling was added to the equation, the effect of skills became statistically insignificant for Germany, but it was statistically significant for the three other countries, with a 100 point increase being associated with an earnings increase of 23% for the Netherlands, 7% for Sweden, and 32% for the United States.

Similar results for slightly different sets of countries were obtained by Leuven, Oosterbeek, and van Ophem (2004) and Blau and Kahn (2005). The pattern found in these studies is broadly consistent with the differences in overall wage inequality between the countries (see, for example, 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 question using the Swedish panel. In addition, we estimate cross-section earnings equations for the 1994 and 1998 data using a standard human capital earnings equation of the form

$$(1) \qquad \ln(w_{it}) = \alpha + \beta_1 \mathit{Skills}_{it} + \beta_2 \mathit{Age}_{it} + \beta_3 \mathit{Age}^2_{it} + \beta_4 \mathit{Female}_{it} + \beta_5 \mathit{Immigrant}_{it} + \beta_6 \mathit{Education}_{it} + \varepsilon_{it}, \quad t = 1994, 1998.$$

Assuming that the error term in (1) may be described as $\varepsilon_{it} = \nu_i + \eta_{it}$, where ν_i is an unobserved person-specific component fixed over time and η_{it} is an independent random term, taking first differences of the variables in (1) will eliminate ν_i and produce unbiased estimates of the effect of skills on earnings (that is, a "fixed effects estimation").

It is important to note that first differences will give unbiased estimates of β_1 only if other first differences of exogenous variables that potentially should be included in (1) are uncorrelated with changes in skills. Another problem is that some measurement error is always attached, by nature, to test scores, our metric for skills. 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 effects estimation is used as long as the true values of the independent variable are correlated over time (see Griliches and Hausman 1986).

As noted above, our measure of earnings is based on annual data and we do not have

[9]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.

[10]The increases for Germany and the Netherlands are 16% and 32%, respectively.

Table 2. Earnings Equation Estimates.
(White's [1980] robust standard errors in parentheses)

Variable	*1994*	*1994*	*1994*	*1998*	*1998*	*1998*	*Fixed Effect*	*Fixed Effect*	*High School Diploma or Less, 1994*	*University Education, 1994*
Skills/100	0.152	0.153	0.075	0.174	0.234	0.107	0.093	0.085	0.101	0.092
	(0.018)***	(0.016)***	(0.018)***	(0.044)***	(0.041)***	(0.044)**	(0.046)**	(0.047)*	(0.017)***	(0.035)***
Age		0.031	0.028		0.044	0.047			0.024	0.047
		(0.005)***	(0.005)***		(0.013)***	(0.013)***			(0.005)***	(0.010)***
Age^2/100		–0.030	–0.026		–0.042	–0.046		–0.010	–0.023	–0.046
		(0.006)***	(0.006)***		(0.015)***	(0.015)***		(0.009)	(0.006)***	(0.012)***
Female		–0.217	–0.237		–0.198	–0.207			–0.226	–0.257
		(0.014)***	(0.014)***		(0.030)***	(0.029)***			(0.015)***	(0.027)***
Immigrant		–0.042	–0.057		–0.058	–0.105			–0.025	–0.117
		(0.026)	(0.025)**		(0.031)*	(0.046)**			(0.024)	(0.061)*
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								0.020		
								(0.041)		
Constant	11.739	11.060	11.269	11.821	10.647	10.898	0.184	0.218	11.370	11.003
	(0.056)***	(0.110)***	(0.106)***	(0.134)***	(0.323)***	(0.320)***	(0.014)***	(0.031)***	(0.114)***	(0.236)***
Adj. R^2	0.056	0.272	0.353	0.042	0.206	0.307	0.014	0.0087	0.273	0.301
Observations	1,018	1,018	1,018	312	312	312	207	207	661	357

Note: Dependent variables are log annual earnings in 1994, log annual earnings in 1998, and the difference between log annual earnings in 1998 and 1994, respectively.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

information on hours worked.[11] The age interval is therefore set to 20–64 in order to minimize the likelihood of including people who just entered the labor market.[12] However, a large proportion of the earnings of those sampled still came from part-time work. One way to mitigate this problem is to truncate the earnings variable, eliminating observations with earnings below some threshold.[13] Earnings below the 10th percentile for full-time earnings for all sectors, for men and women respectively, have therefore been excluded.[14] This leaves us with 1,018 and 312 individuals for 1994 and 1998, respectively, and 207 individuals who participated in both surveys. The first three columns of Table 1 provide descriptive statistics for these earnings samples.

The estimates from the earnings equations are shown in Table 2. Due to the presence of heteroskedasticity, White's (1980) standard errors have been estimated for all earnings equations. The variable *Skills* is highly statistically significant in all specifications when cross-section data are used. For 1994, a 100-point increase in the test score is associated with a 15-log-point increase in earnings when no other regressors are included. Adding *Age, Female,* and *Immigrant* changes the results only slightly. Adding controls for education causes the effect to decrease to 8 log points. When no controls are added, the effect of literacy skills is approximately the same across the two test years, but in the specifications with controls, the effect in 1998 exceeds that in 1994; when education is controlled for, the literacy skills effect is 3 log points higher in 1998 than in 1994. 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 yields results similar to those for 1998.

For the 1994–98 panel and the fixed effect estimations in the next two columns, the effect of Δ*Skills* when no other controls are included is significant at the 5% level even though the sample size is only 207 individuals. When controls are added, the effect of Δ*Skills* is significant at the 10% level (p-value = 0.074) and a 100-point increase in skills is associated with an 8.5 log-point increase in earnings, which is close to the cross-section estimates when education is included.[15] Thus the cross-section association between skills and earnings holds true also in a fixed effect specification.[16]

Finally, as discussed in the data section, there is the question of whether the IALS test scores measure skills that are useful throughout the labor market. The evidence in OECD and Statistics Canada (1995) and Green and Riddell (2003) strongly suggests that the answer is yes, and also highly supportive of that conclusion are data in the last

[11]Although an indicator for part-time work is included in the 1994 IALS, we do not use it, for two reasons. First, the question about part-time work was asked in October 1994, whereas earnings were measured in 1993. Second, there is no corresponding indicator in the 1998 IALS; hence, using this indicator for 1994 would result in two inconsistently structured samples. (As it turns out, however, trials in which the indicator for 1994 was used produced results differing only slightly from the results of the main analysis.)

[12]An analysis using individuals aged 20–60 in order to reduce the impact of individuals exiting the labor market yields very similar results. Because Swedish youths enter the labor market later, on average, than youths in most other countries, we also performed the analysis with a sample restricted to persons aged 25–64. Again, there were only minor changes in the results.

[13]We also tried using various alternative robust estimators along the lines suggested by Hamilton (1992). The results were qualitatively similar to those presented in the text.

[14]For 1994 and 1998, respectively, the income cut-offs for women are 137,124 and 152,400 SEK; for men, 146,736 and 163,200 SEK. Using information from the register-based database LINDA for the year 1998 (see Edin and Fredriksson 2000 for a description of the data), we investigated the effect of our earnings cut-offs by comparing results from basic Mincerian wage equations for hourly wages and annual earnings, where earnings smaller than our cut-offs were excluded. Overall, the estimates for wages and earnings turned out to be very similar.

[15]Both because literacy skills are only one of many individual characteristics that affect earnings and because these skills are measured with error in our data (test scores being a plainly imperfect measure), the low R-squares associated with the skills variable are not surprising.

[16]Fixed effects estimations performed on samples excluding individuals who participated in different forms of education between the two test dates altered none of our conclusions.

two columns of Table 2.

The second-to-last and last columns in Table 2, respectively, present estimates for those individuals in the 1994 sample who had no more than a high school education (corresponding to the variables *Ed1–Ed4* in Table 1) and those with a university education (corresponding to the variables *Ed5* and *Ed6* in Table 1). Since fluent understanding and use of written information is needed for admission to, and success in, most universities in Sweden, one might expect differences in the IALS test scores to be unimportant for highly educated individuals, or at least less important for them than for the less-educated. Such is not the case, however. The two estimates for *Skills* in the last two columns of Table 2 are very similar in magnitude and are not significantly different (based on a single regression model with interactions between all variables and a dummy for those with a university education). The conclusion that our skill measure is equally important for individuals of all education levels remained unaltered in trials that employed various other ways of splitting the data, as well as in a trial that included detailed controls for education (the *Ed1–Ed6* variables) in the estimates for these partitioned samples.

Time Out and Skill Depreciation

Having shown that the labor market appears to be responsive to skills as we have measured them—in other words, that these measures of skill appear to be priced in the labor market—we can turn to our main objective: investigating whether time away from employment leads to skill depreciation. To do so, we first discuss how our estimates should be interpreted in terms of forgone experience versus skill depreciation. We then obtain empirical estimates of time out and skill depreciation.

Forgone Experience versus Skill Depreciation

Our estimates of the effect of time out on skills are based on a simple "value added" specification in which the changes in individual skills are regressed on time out of work and a set of controls. A 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, as this negative effect could instead be due to forgone experience associated with the jobless spell.

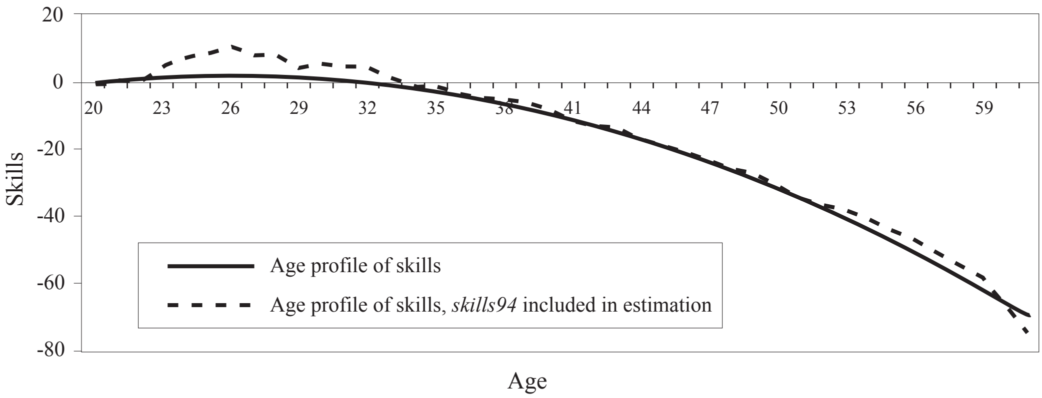
As we do not have data on individuals' whole labor market history, there is no way to explicitly estimate the connection between experience and skills. What we instead do is exploit the longitudinal nature of our data to estimate how skills varied with age conditional on uninterrupted employment between the two test dates. However, since we cannot observe whether an individual actually worked full-time and without interruption between the two test dates, we look at the connection between age and the 1994–98 change in skills for those individuals with a high probability of having been employed throughout this four-year period. (See Appendix A for detailed information on the empirical set-up.) This approach will provide us with an (admittedly biased) estimate of the life-cycle curvature of the experience-skill profile that can be used to assess the relative importance of forgone experience.

In Figure 1 we show the estimated age profile of skills for workers who were uninterruptedly employed between the two test dates. The solid line represents the implied age profile from an estimation in which $\Delta Skills_i$ is regressed on a continuous age variable and a constant.[17] Both because this regression only gives us information about age-specific changes in skills and in order to abstract from age and cohort-specific means, we normalize the initial level of skills in Figure 1 to zero. The dashed line shows the age profile from a value-added specification in which the initial level of skills is included. These two sets of estimates give a similar, and somewhat surprising, picture: skills increased until the age of 26, and then decreased.[18]

[17]The estimations underlying Figure 1, as well as alternative specifications with age dummy variables, are reported in Appendix A.

[18]The cross-sectional association between age and skills differs in that it is linear, indicating that skills decreased over the whole age-span.

Figure 1. Predicted Evolution of Skills Conditioned on Uninterrupted Employment.



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 exhibit an increasing profile in early years, but the pattern is very muted. Also, net depreciation of skills started at much younger ages than would be expected based on earnings profiles. This pattern may be an artifact of our particular measure of skills, which is constructed to capture general information-processing abilities. Our results imply that the curvature of standard age-earnings profiles is to a large extent driven by other factors, such as specific skills. Still, the bottom line, for our purposes, is that the effects of forgone experience on our estimates are most likely limited. Even for 20-year-olds—the age group among whom experience most strongly affected skills—we find that the effect of forgone experience was minor. We invoke these findings in the next subsection to deduce the impact of forgone experience on our estimated connection between time out and skills.

Are Skills Affected by Time Out?

We now turn to the question of whether time out of work affected individuals' skill levels. Following our removal from the 1998–1994 panel of those who, by their report, were retired, students, or participating in the government adult education initiative ("kunskapslyftet"), the sample for this investigation consists of 622 individuals.[19] For individuals who were not employed at the time of the 1998 test, Table 3 shows the length of time out of work (in increments defined by the pertinent questionnaire items—15 months, 16–27 months, 28–39 months, and so on) and the main current activity. As can be seen, those with time out were mainly unemployed.

To investigate whether changes in skills are affected by time out, we apply OLS estimation to the equation

$$(2) \qquad \Delta Skills_i = \phi + \gamma\, TimeOut_i + \delta_1 EdDiff_i + \delta_2 \boldsymbol{x}_i + \delta_3 Skills94_i + \eta_i,$$

where *TimeOut* is either a dummy variable capturing those with time out of work between the two test dates or a continuous variable capturing the length of the spell. The exact specification, and the reasons for our choice of these variables, are discussed below. The variable *EdDiff* is a dummy for those who completed some formal education between the two test dates, and *Skills94* is the test score for 1994. The vector $\boldsymbol{x}_i$ captures individual characteristics in 1994, including age, schooling, and immigrant status.

The model that most resembles (2) is the so-called value-added model (see, for ex-

[19]Including students in the sample, and controlling for them with a dummy, does not affect the final results.

Table 3. Reason for Not Being Employed, by Months Since Last Worked.

Reason	*<16 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%

ample, Hanushek 1979). There are several reasons for adopting a model that includes lagged skills. First, we find that those not employed in 1998 generally performed more poorly on the 1994 test than did their counterparts who were working in 1998.[20] We might therefore underestimate any negative change in skills following time out of work if floor and ceiling effects were present in the test scores, since those who had time out might be close to the floor in 1994. However, controlling for lagged skills will reduce this problem. A second argument for using a model that includes lagged skills is the possibility of regression to the mean—high scores might be the result of luck and therefore be more likely to fall than to rise, and low scores might be the result of bad luck and thus be more likely to rise than to fall.

As a secondary check of our results, we also present results based on the following fixed effects specification:

$$(3) \quad \Delta Skills_i = \tilde{\phi} + \tilde{\gamma}\, TimeOut_i + \tilde{\delta}_1 EdDiff_i + \tilde{\eta}_i.$$

Equation (3) is the special case of (2) where $\delta_3 = 0$ and $\boldsymbol{\delta_2} = 0$. Though this is unlikely to be true in practice, we estimate (3) since the inclusion of lagged skills in (2) may bias our results due to measurement errors in the test scores. If equations (2) and (3) produce the same conclusion for the time out variables, then it is at least unlikely that measurement errors in lagged skills alone drive our results. On the other hand, if the results for (2) and (3) differ, this could be due to measurement errors in (2), omitted variable bias in (3), or both.

Equations (2) and (3) are estimated for the total sample as well as for a sample restricted to those who scored no higher than level 3 on both the 1994 and the 1998 tests. There are two reasons for this strategy. First, changes in test scores due to time out could differ across skill groups. Because the general information processing skills measured by these test scores might be more easily maintained by individuals with higher skills, the effect of time out could be non-linear across skill groups. Second, we expect more contamination by measurement errors in the upper part of the test score distribution than in the lower part. Very few tasks were graded at level 5, since low-skilled individuals were the main focus of the IALS (see OECD and Statistics Canada [1995] for details). Columns (4) and (5) in Table 1 show the mean characteristics of individuals who were included in the analysis.

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. Also complicating the analysis is the fact that some individuals in the sample had been out of work for periods exceeding the approximately 42 months between the tests (October 1994 and March 1998). These individuals should be coded as having been out of work for 42 months if skill loss is linear; but what if the rate of skill loss increases (or

[20]This pattern is revealed by performing OLS on the test scores for 1994 with a dummy for those not employed in 1998 and with controls for age, years of schooling, immigrant status, and gender. The dummy coefficient becomes negative and statistically significant, with a value of –12.89. Those with more than 39 months' time out were excluded from the regression. Robust regression yields the same results.

Table 4. Skill Equation Estimates.
(standard errors in parentheses)

Variable	*Total Sample*	*Level 1–3*	*Total 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)***
Adj. R^2	0.254	0.173	0.256	0.185
Observations	622	307	622	307

Note: Dependent variable is changes in test scores.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

decreases) with time out of work? Mindful of this possibility, we have explored various empirical specifications that allow for a distinction between short and long out-of-work spells. The picture that emerges from all of these estimates is that skill loss was more severe for those with relatively long time out. The results also indicate that loss of skills can be considered approximately linear, since a specification that controls for the length of time out using both dummy variables for each of the time out intervals and a linear term does not provide a significantly better fit than a specification that includes only a linear term (based on an F-test).[21] This linear approximation is used in the analysis in order to overcome the statistical difficulties associated with our limited sample size.

The results for two specifications of equation (2) are shown in Table 4.[22] The first specification involves a single dummy variable for those with time out of work. The estimates show a statistically significant negative effect of time out for the total sample as well as for the restricted sample. The estimate is, however, more negative for the restricted sample, though there is no statistically significant difference across samples. The second specification employs a (quasi) continuous measure of months of time out, using the

[21]The result from the F-test can be explained in part by the lower precision of the estimates for the dummy variables; see Appendix B.

[22]The null of homoskedasticity cannot be rejected for these estimates, which is why regular standard errors are used in Table 4.

Table 5. Skill Equation Estimates, Fixed Effects Specification.
(standard errors in parentheses)

Variable	*Total Sample*	*Level 1–3*	*Total Sample*	*Level 1–3*
TimeOut	–2.100	–8.051		
	(4.065)	(3.985)**		
Spell			–0.171	–0.377
			(0.163)	(0.149)**
EdDiff	8.780	10.652	8.756	9.632
	(3.607)**	(4.212)**	(3.567)**	(4.107)**
Constant	–6.383	–3.380	–6.239	–3.296
	(1.458)***	(1.599)**	(1.443)***	(1.574)**
Adj. R^2	0.006	0.021	0.008	0.029
Observations	622	307	622	307

Note: The dependent variable is changes in test scores.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

midpoints of the categorical variables; that is, it takes the value zero for individuals without time out, 7.5 for those with less than 16 months of time out, 21 for those in the interval 16–27, and so forth, with those who were out of work longer than 40 months being assigned the value 42.[23] The estimates for the continuous variable are highly statistically significant and are not significantly different across samples.[24]

In Appendix B we also present results of specifications in which time out of work is captured by four dummy variables corresponding to the time out intervals in Table 3. These results, together with two additional dummy variable specifications also presented in Appendix B, provide further evidence of a positive relationship between the length of time out of work and skill depreciation.[25]

[23]Using the Swedish longitudinal dataset LINDA, we investigated actual unemployment spells for a large sample of adults for the period between the two tests (see Edin and Fredriksson 2000 for details). We found the distribution within these categories to be approximately uniform, with a mean and median very close to the midpoints.

[24]The results were unchanged when we used samples excluding individuals who participated in different forms of education between the two test dates, and were changed only slightly when we used samples with the age range set to 25–64.

[25]We also tried various specifications to investigate whether the effect of time out varied by the age, gender, or type of time out of the group. We found no statistically significant differences across groups, however, possibly due to the fairly small sample sizes.

As a robustness check on the results in Table 4, Table 5 presents the estimates of the fixed effects specification in equation (3). Time out of work now has a statistically significant effect only for the restricted sample. Since the effect of *EdDiff* does not differ across the samples, the difference does not seem likely to be entirely attributable to measurement errors in the dependent variable. Instead—and consistent with some evidence in Table 4—nonlinear effects of time out across skill groups probably are at work. It should also be remembered here that the results shown in Table 4 strongly reject the hypothesis that $\delta_3 = 0$ and $\delta_2 = 0$, an assumption underlying equation (3). The results in Table 5 should hence be interpreted with care.

One concern is the possibility of reverse causality: it could be that loss of skills leads to non-employment rather than vice-versa. However, we find that of the dummy variables we used to capture time out of work, the one indicating over 42 months out of work—identifying those whose non-employment began before the 1994 test and continued unbroken until at least the 1998 test—is the most negative and statistically significant (Appendix B). We interpret this as evidence that our results are not driven by one-time shifts in skills leading to non-employment. We cannot, however, rule out the possibility that negative *trends* in skills lead to non-employment. To be able to investigate this issue, we would need a third wave of data.

As previously mentioned, the variables for time out of work do not capture individuals who had non-employment spells between the two test dates but were working at the time of the 1998 survey. Based on additional data sources, we have tried to estimate the bias associated with this omission.[26] The main finding is that the estimated skill depreciation may well be too low; the results suggest that both the dummy variable and the continuous variable are biased toward zero and that they should be corrected upward by 36% and 4.6%, respectively.

How much of our estimated skill loss from time out is then due to skill depreciation? To give the forgone experience hypothesis the greatest 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), we find that an individual aged 20 in 1994 gained 2.26 test score points in 3.5 years (10.424 – 20*0.408). This means that each month out of work results in the skill decrement represented by a 0.054-point test score reduction (2.26/42) due to forgone experience. From Table 4, our lowest estimated skill loss from one month of time out is 0.414 points. Hence, the minimum value of skill depreciation should be around 0.36 test score points a month (0.414 – 0.054). At 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 almost solely 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 examine this question in two 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 these estimates 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. To minimize the measurement error in skills, for this estimate we exclude the highest skill groups from the sample. We find that a 12-month spell of non-employment would have moved an individual at the median of the 1994 skill distribution to percentile 44.5. Similarly, an individual at the 25th percentile would have fallen to percentile 20.5 after a year of non-employment. In terms of the cross-sectional association between skills and education, this is equivalent to losing 69% of a full year of schooling.[27] 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 out of work resulted in a wage decrease of 0.52%. 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% for one year out of work. The "baseline" numbers of between 0.52% and 0.95% can be compared with the average of the estimated wage penalties of 3.24% found in the panel data analysis of Albrecht et al. (1999), Table 2. Thus, our estimates would account for between 16% and 29% of the wage penalty.

Concluding Remarks

Using a unique longitudinal dataset, the Swedish IALS database, we have presented

[26]This analysis is described in an appendix available from the authors on request.

[27]This is based on a regression with skills in 1994 as the dependent variable and with years of schooling, age, and dummy variables for women and immigrants as regressors. We use the cross-sectional association because only a small number of individuals in the panel had changed their educational attainment level.

the first direct test of the proposition that time out from employment leads to human capital depreciation. That such depreciation effects exist is a premise at the heart of human capital–based explanations of individual consequences of career breaks and is essential to many macro-oriented theories of the functioning of the labor market.

Our empirical analysis supports the notion that time out of work leads to skill depreciation. In general, we find statistically strong evidence of a negative relationship between work interruptions and skills: longer time out is associated with larger loss of skills. Further, the possibility that this pattern of results could be due to reverse causation, whereby one-time shifts in skills cause unemployment, seems small given our finding that the most negative effects were for individuals with uninterrupted spells of non-employment between the two test dates.

Our evidence does not, however, close the door on questions and doubt. We cannot wholly disregard the possibility of reverse causation, since we are unable to test whether negative trends in skills lead to unemployment; in order to investigate this issue, we would need a third wave of data. Our analysis is also restricted by the rather modest sample size.

Our primary contribution in this paper has been to present the first explicit test of whether time out of work actually leads to human capital depreciation. More research will be needed before more definitive conclusions can be drawn. We therefore hope that future researchers will be able to conduct similar analyses but with more waves of data or with different measures of worker skills.

Appendix A
Measuring the Effect of Forgone Experience on Skills

To maximize the proportion of individuals in the sample with uninterrupted work experience between the two tests, we only include those who had a job at both dates, who engaged in no formal education between those dates, and who at the time of the 1998 test had worked for at least 50 weeks, including vacation, during the previous 12 months. This leaves us with 385 observations, and with individuals ranging in age between 20 and 60 years at the time of the 1994 test.

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—that is, uninterrupted employment during the approximately four-year period we observe—is displayed as the solid line in Figure 1. Skills increase until the age of 26, and then decrease. As the specification in the first column of Table A1 is quite restrictive, the next column contains a specification with four age dummies, with the dummy for age 20–29 being omitted. Here, the intercept (that is, the variable *Age2029*) and the *Age3039* and *Age4049* variables are not significantly different from zero at the 5% level. Based on the coefficient estimates, this model shows a positive relation between age and skills for those under age 30, 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 happen if, for example, regression to the mean in the test scores affects the estimates in the first and second columns of Table A1. The third and fourth 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, which could result in some irregular predictions due to extreme values on the *Skills94* variable. Another problem is that the value of *Skills94* is a function of past labor market experience, about which we know nothing; we therefore cannot 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 fourth column yields similar results. As can be seen, the age pattern is the same as when *Skills94* is omitted from the regression.

Table A1
Estimates of Changes in Skills for Individuals without Time Out
(standard errors in parentheses)

Variable	*(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)***
Adj. R^2	0.014	0.008	0.188	0.183
Observations	384	384	384	384

Note: The dependent variable is changes in test scores. Age is measured in 1994.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Appendix B
Alternative Specifications of Time Out of Work

The first specification in Table B1 captures time out of work with four dummy variables corresponding to the time out intervals presented and discussed in connection with Table 3 in the main text. The point estimates are consistent with the hypothesis that longer time out results in larger depreciation of skills. The variables capturing those with less than 16 months and 28 to 39 months of time out are never statistically significant, however. For the latter category, this is likely due to the very small number of observations, with only 5 and 3 individuals in this category for the whole and restricted samples, respectively.

Next in Table B1 is a specification that simultaneously includes the variables used in the main analysis, that is, a dummy variable for those with any observed time out of work between the two test dates (*TimeOut*) and the quasi-continuous measure of time out of work (*Spell*). Neither of these two variables gains statistical significance when the total sample is used, while the continuous measure becomes statistically significant for the restricted sample. As the restricted sample is expected to be less contaminated by measurement errors in the test scores, this offers some evidence in favor of the hypothesis that longer time out leads to larger loss of skills.

As discussed in the main text, one could speculate that some individuals experience a one-time loss of skills, which in turn leads to non-employment, and that this drives our results, that is, that we have reverse causation. The last two columns of Table B1 investigate this possibility by using two dummy variables, one for those with less than 42 months of time out and one for those with at least 42 months of time out. The latter category corresponds to those who were out of work at the time of the 1994 test and whose non-employment remained unbroken up to the 1998 test. Thus, if negative one-time shifts in skills are what drive our estimates, one would not expect any skill loss for these individuals, as they had uninterrupted spells between the test dates. However, Table B1 strongly contradicts such a story, as the estimate for those with at least 42 months of time out turns out to be both more negative and more statistically significant than the estimate for those with less than 42 months of time out.

Table B1
Alternative Specifications of Time Out of Work
(standard errors in parentheses)

Variable	*Total Sample*	*Level 1–3*	*Total Sample*	*Level 1–3*	*Total Sample*	*Level 1–3*
Time Out <16 Mths.	–5.757 (4.680)	–4.554 (4.998)				
Time Out 16–27 Mths.	–10.341 (8.594)	–15.552 (8.847)*				
Time Out 28–39 Mths.	–16.705 (12.644)	–6.958 (13.383)				
Time Out >39 Mths.	–15.398 (7.026)**	–22.300 (6.616)***				
TimeOut			–3.719 (5.692)	–1.294 (5.934)		
Spell			–0.298 (0.229)	–0.480 (0.224)**		
Time Out <42 Mths.					–7.252 (3.886)*	–8.195 (4.126)**
Time Out ≥42 Mths.					–19.758 (7.942)**	–21.333 (7.116)***
EdDiff	7.246 (3.571)**	5.052 (4.507)	7.297 (3.558)**	5.358 (4.413)	7.314 (3.557)**	5.871 (4.413)
Ed2	11.425 (4.772)**	7.230 (4.280)*	11.372 (4.761)**	7.355 (4.264)*	11.177 (4.762)**	7.283 (4.278)*
Ed3	9.591 (5.172)*	4.898 (4.945)	9.527 (5.156)*	4.948 (4.928)	9.496 (5.154)*	4.971 (4.943)
Ed4	19.653 (4.792)***	5.156 (4.634)	19.593 (4.774)***	5.060 (4.602)	19.567 (4.772)***	5.092 (4.617)
Ed5	19.302 (5.022)***	6.538 (5.092)	19.185 (4.996)***	6.851 (5.061)	19.057 (4.998)***	6.787 (5.081)
Ed6	28.376 (5.248)***	15.442 (6.359)**	28.260 (5.221)***	15.722 (6.286)**	28.108 (5.222)***	15.043 (6.297)**
Age	–0.245 (0.113)**	–0.193 (0.133)	–0.245 (0.113)**	–0.178 (0.131)	–0.247 (0.112)**	–0.190 (0.132)
Immigrant	–2.047 (8.582)	2.493 (8.766)	–1.956 (8.561)	2.301 (8.732)	–1.596 (8.572)	2.419 (8.780)
Skills94	–0.442 (0.031)***	–0.360 (0.047)***	–0.442 (0.031)***	–0.357 (0.047)***	–0.443 (0.031)***	–0.355 (0.047)***
Constant	126.986 (11.069)***	102.624 (14.068)***	127.145 (11.018)***	101.098 (13.928)***	127.496 (11.014)***	101.074 (13.980)***
Adj. R^2	0.253	0.180	0.255	0.183	0.256	0.178
Observations	622	307	622	307	622	307

Note: The dependent variable is changes in test scores.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

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