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UNEMPLOYMENT AND SUBSEQUENT WAGES: DOES GENDER MATTER?*

by

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ABSTRACT

Are there any differences in how men and women fare from unemployment in terms of the wages they receive on a new job? This paper addresses that question using the 1991 wave of the Level of Living Survey. The results suggest that men who experience unemployment will suffer a reduction of subsequent wages while no such effect could be found for women. These findings support the interpretation that women invest more in *general* rather than *specific* human capital which make them less exposed to career interruptions, at least those of a short duration. Due to the favourable labour market at the time, average unemployment duration was rather short, which may have prevented general capital from depreciating. However, the presence of large negative occurrence effects for men suggests that unemployment, even of a short duration, is associated with considerable loss of human capital.

Keywords: Unemployment, Gender, Human Capital, Wages

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1. Introduction

It is often believed that long-term unemployment (high duration) entails larger individual costs than shorter spells do since well-being and skills are more likely to decline with time spent out of work. An individual who for some reason ceases to work will loose the possibility of accumulating new skills relative to those who continue to work. Instead, the individual's human capital may depreciate during unemployment or any other form of intermittency. Many empirical studies have found that individuals who become unemployed receive lower wages upon reemployment than those who have been in continuos employment and that the (relative) loss of wages increases with unemployment duration. Other studies have found that people who experience unemployment in early stages of their labour market career are more likely to face future unemployment risks (see e.g. Blomskog, 1997). Consequently, preventing long spells of unemployment is one of the primary objectives of active labour market policy in Sweden and elsewhere. However, shorter spells may involve large costs as well, for example if those spells are associated with labour market relocation which forces people to take on jobs in other firms or industries which require other types of know-how or skills. In sum, the costs of unemployment in terms of subsequent labour market performance depend intimately on, respectively, the cause of unemployment and the interaction between unemployment and human capital investments. Furthermore, since the labour market is a patchwork of different institutions, norms and preferences, it is likely that the mechanisms that propagate unemployment into future pay and employment patterns will differ across socio-demographic groups, even across groups with small differences in unemployment as registered in official statistics.

The purpose of this study is to investigate whether men and women can expect to receive the same *wages* upon reemployment as their employed counterparts. Prior estimates of unemployment effects on subsequent wages have often focused solely on men or have simply assumed that unemployment experience has the same impact on subsequent wages for men and women, respectively.[1] There are no (economic) reasons to believe that men and women would differ in terms of intrinsic earnings capacity and hence the effect on subsequent wages of a spell of unemployment of given length should not be different either. That is, if men and women have the same background and faced the same length of a spell of unemployment, depreciation of human capital should not depend on gender *per se*. However, there are good reasons to believe that men and women do in fact differ with respect to both the *amount* and the *kind* of human capital investments they undertake. Various explanations have been suggested for why one should expect an association between human capital investments and the cost of unemployment that is non-neutral with respect to gender. These will be discussed in more detail in the subsequent section. It may therefore be important to separate men and women when one estimates the effects of unemployment on human capital and subsequent wages.

I will adopt the common empirical framework used in these studies, namely to augment a standard Mincerian wage equation by measures of individual unemployment experience. When estimating the effect of unemployment on subsequent wages, a distinction will be made between a 'occurrence-effect' that captures the loss of specific human capital, and a 'duration-effect' that captures the loss of general human capital. The presence of large effects of the former type would imply that even short spells of unemployment may have serious effects on labour market prospects. The wage equations are estimated separately for men and women. Briefly, the main findings are that unemployment is associated with a large loss in subsequent wages for men while no significant effect could be established for women. These results are consistent with the theoretical predictions.

The remainder of the paper is organised as follows. In the next section conventional theoretical explanations of a gender-biased relationship between unemployment and subsequent wages are discussed. Some of the related research is also reviewed. Section three provides a description of the data and sample. The fourth section presents the empirical specification and in

[1] Swedish studies that do not separate the impact of unemployment on subsequent wages by gender include Björklund (1981), Edin (1988) and Ackum Agell (1991). Björklund's and Edin's results suggest negative and persistent wage-effects of unemployment, while Ackum Agell found no significant effects.

section five I report the least squares estimates of the wage-level equations for men and women respectively. Section six considers the possibility that unemployment experience may not be a random event, but rather reflecting a selection based on some, unobserved, productivity or ability. The final section concludes.

2. Theory and related research

In general, one should expect that if men and women have the same characteristics and face the same time in unemployment they should, on average, receive the same wages upon reemployment. That is, the expected value of the next job should not depend on the prospective holder of that job being a man or a woman. However, there are several reasons why unobservable differences between men and women, with respect to job preferences, investments in human capital etc., may cause this prediction to be violated in the data.

First, men and women may, on average, have different job preferences which involve different trade-offs between wages and non-pecuniary aspects of a labour market career. Of course, these preferences may derive from social norms or conventions and not from an inherent predisposition for certain jobs. One such trade-off that is likely to be an important determinant of differences in labour market outcomes between men and women is that women can be expected to choose jobs that allow more time spent out of the labour force due to childbearing or household work. According to human capital theory there is a close relationship between (expected) labour market participation and human capital investments. A high rate of intermittency would imply that, all else equal, human capital investments are less profitable since there will be less time to reap the benefits from such investments (Mincer and Polachek, 1974). Thus women can be expected to invest less in overall human capital than men since women, on average, spend less time working.[2]

Another implication, which is central to the present study, is that women, who anticipate to be frequently intermittent, may choose to make *human capital investments that facilitate movements in*

and out of the labour market. It is therefore important to recognise differences in the *type* of human capital investments made. As noted in Polachek (1981) the penalty for intermittency is likely to be higher in some occupations, such as managerial jobs
or the crafts, than in service or clerical work. The 'high-penalty' jobs are typically associated with large investments in *firm-specific* human capital as opposed to the 'low-penalty' jobs that to a larger extent only require investments in *general* human capital. For an individual with a large portion of general human capital the consequences of loosing a job will be less severe than for an individual who has a large portion of firm-specific human capital.[3] The reason is that while investments in general human capital do increase productivity in other firms as well, firm-specific skills are not transferable to other firms and hence such investments do not increase the individual's productivity outside a particular firm. Owing to such differences in the penalty to time out of work, women can be expected to choose industries or occupations where the penalty for intermittency is low or, correspondingly, which require investments in human capital are general rather than specific. Men, on the other hand, foreseeing a much higher participation rate, can be expected to invest heavily in firm specific human capital since such investments, due to the higher 'atrophy rate', yield higher wages.[4]

The second argument for why there may be gender differences in human capital investments focuses on the occupational segregation of men and women that is due to discrimination. Since women can be expected to be intermittent more frequently and/or for longer time than men are, employers find it less profitable to invest in women's training or careers. Hence, women are typically less likely to be promoted and are more likely to be found in professions that require relatively less investment in human capital. The implication of this argument, in terms of gender-differences in wage losses due to unemployment, is basically the same as the implications of

[2] Figures in Albrecht et. al. (1997) suggest that female nonwork is about twice as high as male nonwork.
[3] For a discussion of the distinction between specific and general human capital, see Becker (1975).
[4] It should be noted that the human capital explanation may be equally valid in the context of unemployment effects even though the primary reason for women to choose a career which facilitates labour force movements without significant wage loss is not be the anticipation of long or frequent *unemployment* spells. There might still be similarities between timeout due to child-bearing or household work and timeout due to unemployment in the sense that the penalty for intermittency may be

human capital theory, i.e. that women can be expected to incur less losses due to unemployment because they have 'less to loose'. However, in addition to employers *perceiving* women to yield lower returns on human capital investment, women might not be hired for certain traditionally high-paying male jobs simply because employers have a *preference* for a male dominance at the top of the payrolls.

There are some empirical studies that explicitly compare the wage effects of unemployment for men and women respectively. For example, there are a number of studies using data sets on displaced workers.[5]

Maxwell and D'Amico (1986), using data from the young men and young women's panel of the National Longitudinal Survey (NLS) found a weak indication that women incurred greater wage losses after displacement. Podgursky and Swaim (1987) using data from the Displaced Worker Survey (DWS) found that being reemployed in the *same* industry was associated with a wage gain for blue-collar males compared to changing industry. This effect was not found for either for women or white-collar and service. Their result is consistent with men loosing specific human capital when they are changing industry. Madden (1987) also using DWS-data found larger wage losses due to displacement for women which she interprets as an indication of women being subject to labour market discrimination.[6] Jacobsson et. al. (1993) using an administrative data set from the state of Pennsylvania, found that earnings losses of displaced workers are large and persistent for both men

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low in some professions irrespectively of the cause.

[5] According to Hamermesh (1989) displacement can be defined as "...job loss among workers with significant labour force attachment...". However, there is no consensus about the precise definition of displacement. For a brief discussion, see Fallick (1996).

[6] Madden (1987), argues that women who earns the same pre-unemployment wage, who engage in equal amount of search and who are equipped with the same background characteristics as their male counterparts may be forced to choose their subsequent jobs from a smaller set, resulting in lower expected wages. Thus, women should expect *greater* losses to unemployment as far as subsequent wages are concerned. However, if women are subject to this kind of discrimination, the adverse effects of discrimination in terms of less labour market opportunities must apply not only to the post-unemployment situation, but also to the pre-unemployment situation. In general, then, if men and women are not choosing from the same opportunity set, it is difficult to rationalise that their choices are made from an identical pre-unemployment position.

and women, but they do not find that gender is an important predictor of the size of displacement costs. Finally, Crossley et. al. (1994) using a administrative data set from the province of Ontario found that wage losses due to displacement were, if anything, negatively correlated with pre-displacement tenure for both men and women but women incurred greater losses for given amount of tenure. Since the effect of tenure on pre-displacement wages turned out to be similar to men and women, they conclude that the larger negative effect for women cannot be associated with women accumulating more firm-specific training. Contrary to Madden, Crossley et. al. argue that one should look for differences in search behaviour between men and women in order to find an explanation of this result rather than turning to explanations focusing on specific capital or (wage) discrimination.

There are, at least to my knowledge, two Swedish studies that explicitly compare effects of unemployment on reemployment earnings across gender. Albrecht et. al. (1997), using a sample of Swedish men and women, found a smaller, although negative, effect of unemployment for women; one week of unemployment decreased subsequent wages by .35 percent for men and .1 percent for women. Moreover, they report gender-stratified estimates of unemployment effects by sector affiliation and educational level respectively; in all cases, unemployment was associated with a reduction in subsequent wages. As far as educational background is concerned, both men and women with at least some university education fared worse from unemployment than did those at lower educational levels. Joyce (1997) also found negative effects for both men and women using a sample from the 1991-1996 Labour Force Surveys (AKU); one week of unemployment lowers subsequent hourly wages by .17 percent for men and by .18 percent for women while subsequent weekly income was estimated to be .19 percent lower for men and .23 percent lower for women.[7] He could not reject the hypothesis that the (negative) effect was the same for both men and women.

[7] Joyce regresses the wage/income of a particular year on the cumulated duration during a two-year period prior to that year. For example, 1993 wages/income are regressed on total weeks of unemployment during 1991-1992. He conducts this regression for each year 1992-1995. The estimates reported here is the *average* duration effect for these years.

3. The Data and Sample

The data set used in this study is a cross-section of individuals from the Level of Living Survey (LNU) conducted in 1991.[8] The data is based on a representative sample of the Swedish population between 18 and 75 years of age and nearly 7,000 individuals are included. Data on age, sex and place of birth are available for all individuals in the sample but information on labour market experience, education and wages etc. are only available for those 5,306 individuals actually interviewed.[9] The sample used below is submitted to a number of restrictions. The rationale for these sample restrictions as well as the effects on the variables used are documented in Appendix B.

In addition to a heap of individual characteristics and human capital variables, the LNU data contains information on hourly gross wages. The wage variable is calculated from information on wage earnings and hours worked. The advantage of using hourly wages, rather than weekly income, is that possible influences of labour supply decisions are mitigated. There are a number of different variables that reflect the unemployment experience of individuals. This information is based on a subjective criteria, that is, the individual is recognised as unemployed if he/she *regards himself/herself as unemployed*. In order to estimate effects of unemployment on subsequent wages, two measures of unemployment during the preceding year (i.e. 1990) are available, (i) whether the individual has been unemployed at all during the year and (ii) if that is the case, the total number of weeks the individual has been unemployed during the same year.[10]

Regarding the unemployment information in the LNU-survey, some caveats are in order. First, it should be pointed out that both measures of unemployment duration might include several

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8. The survey was also conducted in 1968, 1974 and 1981.

9 The number of drop-outs according to category are: not interviewed in 1981 (i.e. between 18 and 25 years of age) (151), immigrants (65), interviewed in all previous surveys (927) and interviewed in 1974 and/or 1981 but not 1968 (324). All in all, there are 1,467 individuals not interviewed. These can also be grouped according to the cause of dropping out; refusal to participate (1,185), could not be reached (129), prevented from participating (e.g. due to sickness) (89), emigrated (52), deceased (11) and participated partially (1). The LNU data set as well as some empirical applications are presented in Fritzell and Lundberg (1994).

10 The other unemployment variables available are total numbers of unemployment spells and total duration in months of unemployment over the life cycle, any spell of unemployment that lasted at least two months, and, finally, the year during which the latest spell of unemployment that lasted at least one month occurred.

spells of different lengths. That is, it is impossible to distinguish one long continuos spell from several short spells. As far as unemployment during the preceding year is concerned, the estimated effect of unemployment on wages might be influenced by 'catch-up effects' insofar that wages refer to the first *year* and not to the first *job* after unemployment. That is, it is possible that individuals accept a job in order to escape unemployment and then move on to another, higher-paying, job after acquiring experience and/or on-the-job training. These human capital investments may reduce a negative effect of unemployment on wages at the first job. Second, information regarding the *cause* of unemployment is not available. That is, it is impossible to discriminate between effects arising due to layoffs, discharge for cause, quits, entrance to the labour force etc. It is likely that different causes of unemployment have different effects on subsequent wages. It is often argued (e.g. see Björklund 1981) that negative welfare effects may be stronger for permanently laid-off workers or displaced workers - workers who lost their jobs because of plant closure or relocation - than for workers that were temporarily separated, for example due to seasonal factors. Further, Björklund and Holmlund (1989) find evidence that individuals who voluntarily separate find new jobs quicker, have greater access to permanent jobs and get higher pay on the new job compared to individuals who are laid off, i.e. because of an employer-induced separation. However, the distinction between the different causes for separations may be relevant for the results in this study to the extent that men and women differ with respect to the cause of unemployment. The Labour Force Survey (AKU) provides information about the nature of the stock of unemployment. In Table 1, the unemployment stock is divided into four categories; ”new entrants or re-entrants”, ”permanent layoffs” due to cutback in production or personnel, ”termination of engagement” and ”other reasons”, preferably quits. [11]

[11] Formally, Swedish employers have a limited possibility to lay off workers temporarily without pay (see Edebalk and Wadensjö (1995). However, Harkman and Jansson (1995) find that many workers return to their previous employer after a spell of unemployment, without being formally registered as temporarily laid off. Temporary laid offs are not classified as unemployed in the Labour Force Survey, but as absent from work.

Table 1. Causes of unemployment in 1990. Percent

	New entrant/ re-entrant	Perm. layoffs	Termination of engagement	Other reasons
Men	23.5	15.1	26.3	26.0
Women	28.9	11.8	29.0	22.2

Source: SCB (1990)

According to these figures, permanent layoffs seem to be a rather rare cause of unemployment in 1990 while one out of four men and one out of five women seem to have entered unemployment voluntarily.[12] Women are more likely than men to be unemployed due to entry or re-entry into the labour force as well as due to the termination of an engagement. Men, on the other hand have a much higher likelihood of being unemployed due to permanent layoff than do women. To the extent that these figures carry over to my sample as well, permanent layoffs or 'displacements' do not seem to be very likely causes of unemployment, although more likely for men than for women.

As a final caveat it should be mentioned that measures of the respondent's unemployment experience are based on retrospective information. This raises the question of so called recall bias. At least three types of bias are relevant here. First, it is obvious that respondents are more likely to forget past events the longer time that has elapsed since these events occurred. However, recollection need not necessarily decrease linearly with time. It is not unlikely that respondents recall events that occurred for the first time, e.g. their first job, better than more recent events. Second, individuals tend to report events as occurring more recently than they in fact did. Finally, if respondents are asked to describe or classify certain events or states further back in time, it may be more difficult to check the accuracy of the description or classification. Since it is impossible to make an assessment of either the direction or the importance of these biases in the data, a cautious attitude towards the results is warranted.[13]

[12] There is an important cyclical dimension to be added to these observations. Quits tend to be pro-cyclical whereas layoffs tend to be counter-cyclical. As unemployment decreases, quits become more profitable (or less risky) since the probability of finding a new job increases. On the contrary, firms become reluctant to lay off people when unemployment decreases since future recruitment is likely to be costly. The year 1990 was a year with very low unemployment rate, only 1.7 percent of the male labour force and 1.6 percent of the female labour force were unemployed.

[13] The reliability of the retrospective data in LNU is also discussed in Blomskog (1997).

4. Empirical Implementation

A straightforward, and often used way to estimate the unemployment effects on subsequent wages is to augment a standard Mincerian wage equation with some measure of the individual's unemployment experience along with other human capital variables and personal characteristics known to influence the wage. The results presented in the next section are obtained using this framework. In the introduction it was argued that unemployment may cause a depreciation of the individual's human capital resulting in a lower wage compared to those who have been employed. Moreover, it was argued that the reason why men and women might be asymmetrically hit by unemployment in terms of subsequent wages might be differences in human capital investments. This suggest that an empirical investigation of the association between unemployment and wages should take these matters into account. Following Björklund (1981), the basic cross-section equation is augmented with two measures of unemployment experience. Unemployment experience enters both as a dummy variable, un, which is equal to one if the individual has experienced any spell of unemployment during the previous year and as a continuos variable, unw, which is the total number of weeks the individual has been unemployed during the previous year. Thus, unw will capture duration given that unemployment occurred. The empirical specification can then be written as

(1) $$\mathrm{lnW_i} = \boldsymbol{b}_1\mathrm{exp_i} + \boldsymbol{b}_2\mathrm{exp_i^2} + \boldsymbol{b}_3\mathrm{educ_i} + \boldsymbol{b}_4\mathrm{un_i} + \boldsymbol{b}_5\mathrm{unw_i} + \boldsymbol{e}_i$$

where exp is labour market experience and educ is some measure of education. ε is a disturbance term. Human capital theory predicts that the wage should rise with experience at a decreasing rate, i.e. $\beta_1>0$ and $\beta_2<0$, and with education, i.e. $\beta_3>0$. The coefficient of the dummy, β_4, will capture the *occurrence effect* of unemployment. That is, the (net) depreciation associated with losses of human capital specific to the previous firm, industry or occupation where as the coefficient of the continuos variable will capture the *duration effect* due to loss of general human capital.

According to the discussion in the introduction, one would expect men to undertake more total human capital investments - general and specific - than women and that the male human capital portfolio contains a larger *share* of *firm-specific* capital. Thus, the representative male is likely to have a larger *stock* of firm-specific human capital than the representative female. Whether men would have a larger stock of *general* human capital as well is less clear. If general and specific human capital are strong complements, men will tend to have more of both types of capital while if they are substitutes, men may very well have less general human capital. Consequently it is reasonable to expect a larger occurrence effect (in absolute value) for men than for women while it is less clear how, if at all, the duration effect should differ between men and women.[14]

The human capital variables used are, apart from experience and its square, years of schooling and educational level.

The reason for including both years of schooling and educational level in the regression is that there is a reason to expect a wage premium associated with the completion of a specific education level when the number of years spent in education are controlled for. In other words, workers are rewarded not only for the productivity-enhancing contributions of schooling, but also for obtaining the certificate or diploma that comes with completing a certain level of schooling. Educational levels enter as dummy variables in the regression equations. The left-out category are individuals who have not enrolled in senior-level compulsory school (ed2) and thus - completed (nine-year) compulsory school - is the lowest, included educational category.[15] The other categories are completed upper secondary (ed3) and some academic degree (i.e. degree from university-college or

[14] Some of the wage losses assumed to be captured by the occurrence variable may reflect loss of non-competitive rents on the old job, for example due to the presence of efficiency wages. Insofar that men are concentrated in jobs where such rents are more common, this might account for differences in the magnitude of the occurrence effect.

[15]The left-out category includes individuals with (i) uncompleted elementary school, completed elementary school and (iii)

university) (ed4).[16] The categories ed1-ed3 also include individuals with additional vocational training.

Several individual background variables are also used: (i) a dummy variable equal to one if the interviewed individual was married or cohabiting, (ii) a dummy variable equal to one if both parents are/were of Swedish origin, and, finally, (iii) an index-variable indicating physical and psychical status. Dummies capturing industry affiliation are also included as they may account for some omitted heterogeneity that is correlated with the other regressors. For example, 'better' workers may be concentrated in certain high-wage industries and hence adding industry controls to equation (1) may capture these characteristics. Second, industry affiliation may also pick up some of the compensating differentials, arising due to non-pecuniary aspects associated with particular kinds of jobs. A more detailed variable description is given in Appendix A.

Before turning to the results, one should bear in mind that they are calculated for the sub-sample of individuals *employed* at the time of the survey. In general, it will not be valid to extrapolate the regressions to the entire sample, since the effect of the exogenous variables on potential reemployment wages for those individuals who are not employed in 1991 may differ systematically from the effects on workers who were either reemployed or continuously in employment. Moreover, as pointed out by Maxwell and D'Amico (1986), the reservation wage of an unemployed individual may approach the shadow wage in home production *before* reaching the market wage, thereby causing the individual to drop out of the labour force rather than being reemployed. That is, rather than remaining in the labour force until the reservation wage falls enough to make it profitable to accept a job offer at the going wage, the individual finds the drop-out alternative more attractive. Since women can be expected to specialise in home production and men in market work, unemployed women are more likely to have higher shadow wage of home production and hence they are more likely to withdraw from the labour force than men for a given time spent in

completed elementary school and at least one year of vocational training.

[16] LNU records seven different levels of education. I also tried running regressions with dummies for each of these levels but that did not add any valuable information.

unemployment. Thus if women's wage equation is estimated separately, potential selection bias due to labour force withdrawal should be taken into account.[17] However, since the group of nonemployed in the sample consists of individuals engaged in various gainful activities (farming, family businesses, artists etc.) as well as in education or household work, it is difficult to give any sufficient structural interpretation of the underlying selection mechanisms and hence I will not pursue this question any further.

5. Estimates of the Impact of Unemployment on Subsequent Wages

In this section, I estimate equation (1) together with the human capital and individual background variables discussed above. Each cross-section is separately estimated for men and women. The results are presented in Table 2. A comparison of the human capital and individual background variables reveals that men receive higher returns to experience compared to women, but the (negative) coefficient on experience squared is larger for men indicating that their returns are falling at a greater rate. The coefficients of the experience variables also measure the returns to human capital accumulation accruing during time spent working. Thus, if unemployed, men suffer from larger wage losses due to forgone accumulation than women do (for given duration). Men also receive a higher return to years of schooling than women but women are better rewarded for increasing their educational standard to a college or a university level (ed4). Moreover, married or cohabiting men receive a wage premium of around five percent compared to single men. This could reflect either a positive selection into marriage/cohabitation based on earnings (i.e. that high-wage earners are overrepresented among the married men) or that marriage/cohabitation does in fact increase wages.[18] Women who are either married or cohabiting, do not get this premium, which is usually explained by the argument that spouses tend to specialise in either household work or

[17] Maxwell and D'Amico found that a displaced woman indeed are more likely to leave the labour force than men. The female reemployment rate converges, but not fully, to the male rate as time elapses.
[18] Support for the former explanation is found by Richardson (1995). Using the panel structure of LNU, she found no

market work. Due to this specialisation one spouse can develop a larger human capital stock and hence receive higher wages.[19] The origin of the parents is more important for men that for their female counterparts and, finally, women's wages are less affected by bad mental or psychical health.

Table 2. Cross-section Wage Equations based on Sample I

	(1)	(2)	(3)	(4)
	men	men	women	women
const	3.7600 (53.271)	3.5799 (53.269)	4.0418 (19.150)	4.0419 (19.147)
un90	-.0600 (1.331)	-.0984 (3.184)	-.0090 (.268)	.0037 (.128)
unw90	-.0032 (1.276)		.0083 (.610)	
exp	.0226 (11.636)	.0226 (11.607)	.0160 (9.970)	.0160 (9.994)
$exp^2/100$	-.0331 (7.973)	-.0330 (7.956)	-.0248 (6.899)	-.0002 (6.916)
schy	.0316 (9.471)	.0315 (9.458)	.0212 (8.327)	.0212 (8.326)
ed2	.0838 (3.602)	.0845 (3,624)	.0145 (.745)	.01440 (.738)
ed3	.1224 (3.859)	.1227 (3.871)	.0948 (4.952)	.0947 (4.942)
ed4	.1382 (3.853)	.1378 (3.842)	.1966 (6.941)	.1963 (6.931)
married	.0530 (3.811)	.0526 (3.778)	.0133 (1.162)	.0134 (1.168)
sscale	-.0052 (3.481)	-.0053 (3.558)	-.0027 (2.798)	-.0027 (2.805)
swpar	.0739 (3.624)	.0759 (3.759)	.0412 (3.028)	.0410 (3.015)
R^2 adj.	.3730	.3730	.3089	.3092
n	1,565	1,565	1,559	1,559

Notes: Sample I is described in Appendix B. The standard errors are corrected using the White-estimator (see White (1980)). All regressions also include dummies for industry affiliation. t-values in parentheses.

Turning to the unemployment variables in column (1) and (3), the estimated coefficients suggest that men's subsequent wages are negatively affected by both the occurrence and the duration of unemployment. The point estimates of the unemployment variables indicate that wages will drop by six percent due to the occurrence effect and by .3 percent per week due to the duration effect.

marriage/cohabitation premium when unobserved heterogeneity were removed.

For women standard errors are large and none of the coefficients are significant at conventional probability levels which prevents any firm conclusions.[20] In column (2) and (4), the duration argument is dropped. The primary reason for omitting one of the unemployment arguments is that they are highly collinear which mightresult in imprecise estimates of the coefficients. The pairwise correlation between the two measures of unemployment experience is .74 in the male cross-section and .69 in the female cross-section. A simple F-test of the hypothesis that the two coefficients are simultaneously being zero is rejected for the males but not for the females. The computed F-values were F(2,1545)=4.89 and F(2,1539)=0.15 respectively. Thus, the null hypothesis that previous unemployment is unrelated to current wages cannot be rejected for women. The omission of the duration argument changes the interpretation of the coefficient. It is no longer possible to discriminate between the two types of depreciation effects. Instead, the unemployment dummy now captures the net of the occurrence and the duration effects. When the duration variable is omitted, the coefficients of the human capital and individual background variables are essentially unchanged while the unemployment dummy is highly significant and more negative for men but positive and insignificant for women. These results suggest that unemployment does in fact reduce subsequent wages for men but not for women.

6. Unobserved Heterogeneity Bias

In specifying equation (1), no account was taken of the possible effects of unmeasured individual characteristics such as 'ability' or 'motivation' that might be associated with the proneness to unemployment. Consequently, it cannot be ruled out that these individual characteristics are correlated with the included regressors that affect wages. In other words, the reason why one find negative estimates of previous unemployment may not be because unemployment actually decreases wages, but because 'low-wage individuals' are more likely to end up in unemployment. If these individual effects are not properly accounted for, the estimates will suffer from omitted

[19] For a more rigorous treatment of this issue, again see Richardson (1995).

variable bias and then the negative effects of unemployment on subsequent wages will be overstated.

I control for individual heterogeneity in two different ways. First, by including lagged wages as an additional regressor allowing the coefficient of the lagged wage variable to be freely estimated. More specifically, I use (hourly) wages from the 1981 wave of the LNU-survey. Second, I condition on the total number of unemployment spells during the labour market career.[21] These approaches impose further restrictions on the sample. Due to the inclusion of the 1981 wage rate the total sample observations (Sample II) is reduced from 3,124 to 2,265 since wage observations for both periods are needed. When the total number of unemployment spells is used, the total sample is reduced by 574 observations to 2,550 (Sample III).[22] The effects on the means of these restrictions can be seen in Table B2.

6.1 Estimates with lagged wages

The first approach is to estimate the following equation

$$(2) \qquad \ln \mathrm{W}_{i91} = b_1 \exp_i + b_2 \exp_i^2 + b_3 \mathrm{educ}_i + b_4 \mathrm{un}_i + b_5 \ln \mathrm{W}_{i81} + e_i$$

The rationale for including past wages is the notion that if there are any unobservable variables that govern the proneness to unemployment, they may influence the wage rate as well. Including the past wage rate will thus make it possible to take such characteristics into account. The estimates are reported in Table 3. For men, the effects of human capital and personal background variables in the 'benchmark column' (1) are still significant and with the expected signs with the sole exception of

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[20] Henceforth, 'conventional' levels of significance refers to probability values in the neighbourhood of .90 or above.

[21] A Two-Stage Least Square (2SLS) procedure was also tried, but the instruments that were used (the aggregate cohort unemployment rate for the period 1988-90 reported in the Labour Force Survey failed to capture the variations in the occurrence variable.

[22] Both restrictions eliminates individuals that are 24 years old or younger in 1991. The 1981 wave of the LNU included individuals between 15-75 years of age and hence individuals 24 years old or younger could not earn a positive wage in 1981. Further, questions about the respondent's total unemployment experience, included in the so-called "employment biography" of the 1991 wave, were not posed to those who (i) were interviewed by telephone (when the questions were not posed), (ii) were born before 1925 or after 1965,

the coefficient of parental nationality, which is insignificant. However, the university wage premium (i.e. the relative wage difference between a university degree and uncompleted compulsory schooling) has increased from around 6 percent to 10 percent and the marriage wage premium has risen by one percentage point to more than six percent. For females, the estimates show a similar pattern as for men when compared to the estimates based on the unrestricted sample in Table 2. The magnitudes of the coefficients of the human capital variables are slightly lower with the exception of the coefficient of the schooling variable, which is increased by almost one half of a percentage point. Further, the marriage premium is much lower than in column (4) of Table 2 but still insignificant.

In column (2) and (4), respectively, the lagged wage rate enters as an additional regressor. The coefficient of lagged wages is highly significant in both samples where the male coefficient is around .27 and for females the coefficient is around .08. With $0<\beta_5<1$, individuals with relatively high wages in 1981 enjoyed smaller proportionate wage gains than the average individual. Thus, there is a tendency for high wage earners to converge to the average wage level and that tendency is much lower for women than for men. To interpret the changes in the other coefficients, consider the case when a particular explanatory variable has the *same* effect on the wage level in both 1981 and 1991. lnW81 will then capture some of the explanatory power of this particular variable and the estimated coefficient will be lower than the true effect on the 1991 wages. Thus, it is not surprising that the coefficients for the human capital variables is reduced for both men and women. The coefficient of the linear experience term is lower and the wage experience profile has become less steep. Both measures of the returns to education (i.e. schy and ed) are also lower.

Table 3. Cross-section Wage Equations based on Sample II

	(1)	(2)	(3)	(4)
	men	men	women	women
const	3.781 (46.225)	3.1267 (27.388)	3.9921 (18.765)	3.7385 (18.438)

(iii) have had more than 15 jobs or (iv) have never had a job that lasted more than six months. Thus, using total number of unemployment spells as an additional regressor will also eliminate the 25 year olds.

un90	-.1208 (2.203)	-.1188 (2.161)	.0750 (1.132)	.0710 (1.117)
exp	.0230 (8.319)	.0098 (3.197)	.0153 (6.888)	.0133 (5.937)
$exp^2/100$	-.0327 (6.095)	-.0146 (2.629)	-.0218 (4.857)	-.0192 (4.327)
schy	.0303 (8.287)	.0225 (6.235)	.0259 (9.502)	.0236 (8.676)
ed2	.1211 (4.233)	.1037 (3.695)	.0111 (.452)	.0068 (.277)
ed3	.1603 (4.418)	.1390 (4.057)	.0853 (3.771)	.0832 (3.715)
ed4	.2028 (5.149)	.1784 (4.765)	.1898 (5.926)	.1765 (5.531)
married	.0630 (3.571)	.0516 (3.033)	.0023 (.169)	.0027 (.206)
sscale	-.0056 (3.363)	-.0052 (3.235)	-.0026 (2.772)	-.0026 (2.865)
swpar	.0323 (1.227)	.202 (.816)	.0388 (2.348)	.0419 (2.566)
lnW81	0	.2673 (8.490)	0	.0811 (4.109)
R^2 adj.	.3547	.4116	.3130	.3225
n	1,139	1,139	1,126	1,126

Notes: Sample II is described in Appendix B. The standard errors are corrected using the White-estimator (see White (1980)). All regressions include dummies for industry affiliation. t-values in parentheses.

The coefficient of the unemployment dummy in the benchmark regressions is still significant for men and has increased substantially in magnitude. The point estimate is around three percentage points higher than the corresponding estimate in Table 2. The positive coefficient of the unemployment dummy for women is substantially larger in magnitude and more precisely estimated, but still insignificant. It is worth noting that the unemployment rate is much lower for both men and women if one compares the means between Sample I and II. By restricting the sample to individuals who reported a positive wage in 1981, Sample II is consisting of the prime-aged men and women, i.e. individuals who have a strong labour market attachment. For men the much larger coefficient of the unemployment variable, compared to Table 2, suggests that the adverse effects of unemployment in terms of subsequent wages are larger for this core group. For prime-aged women, the opposite effect is found. That is, unemployment seems to be associated with a wage *premium* rather than a discount. The unemployment coefficient is essentially unchanged when I control for 1981 wages than in the benchmark regression in column (1). Hence, provided that the lagged wage is a good proxy for omitted individual characteristics, the influences of such characteristics seems to be limited.

6.2 Estimates with total number of unemployment spells

The second approach to the heterogeneity problem is inspired by the following observation by Heckman (1981):

> "...it is often noted that individuals who have experienced an event in the past are more likely to experience the event in the future than are individuals who have not experienced the event. The conditional probability that an individual will experience the event in the future is a function of past experience."

In line with this observation, I let the log of wages be estimated conditional on the total number, or the sum, of unemployment spells reported by the respondent.[23] The empirical equation can thus be written as

$$(4) \qquad \ln W_{i91} = b_1 \exp_i + b_2 \exp_i^2 + b_3 \text{educ}_i + b_4 \text{un}_i + b_5 \text{totspell}_i + e_i$$

where totspell is the reported number of unemployment spells. One heuristic rationale for including the totspell-variable is that frequency of spells can bee seen as an indicator of the individual's unobserved heterogeneity that is correlated with unemployment in 1990, but not captured by the other controls in equation (1). That is, many spells of unemployment may, for example, capture an intrinsic (but unobservable) inability to remain with an employer, for example due to shirking or because the individual have a certain preference for temporary work. Including previous

[23] It is assumed that the probability of a positive outcome (unemployment occurs) is independent of *when* the previous outcome took place or $\Pr(u_{it}=1 | u_{it-k}=1)=\Pr(u_{it}=1 | u_{it-s}=1)$, $\forall t$ and where s,k=1,...t. This means that the probability of being unemployed in a particular point in time is independent of whether the individual was last unemployed in the previous period or in a more distant period. What matters is if he or she has been previously unemployed at all. In that case, it is likely that individuals experiencing repeated spells of unemployment differ fundamentally in the proneness to unemployment from those individuals experiencing few or no spells. Thus, I claim that the probability of ending up in unemployment is an increasing function of the number of previous spells or $\Pr(u_{it}=1 | u_{it-s}=1)>\Pr(u_{it}=1 | u_{it-s}=0)$ $\forall k,s,t$. However, nothing can be said about the nature of this proneness. That is, whether repeated spells are caused by genuine state dependence, or so

unemployment experience may, in addition to capturing heterogeneity, also capture genuine long-run effects or state dependence. In this case, past experience has a genuine behavioural effect in the sense that an individual who did not experience the event (e.g. unemployment) would behave differently than an otherwise identical individual who did experience that event. For example, if unemployment involves human capital depreciation or discourages morale, the likelihood of an individual who once have experienced a spell of unemployment to experience one more or perhaps frequent spells increases. [24] If wages are negatively related to previous unemployment, individuals who are frequently unemployed should, in the presence of state dependency, have lower wages. Even though it is impossible to discriminate between those effects, including unemployment history in equation (1) may at least purge the short-run effects from any influences of these kinds.

Table 4 provides the wage regressions where the total number of unemployment spells is included as an additional control variable. Columns (1) and (3) provide the benchmark estimates for this sample for, respectively, men and women. The estimated coefficients of the experience and education variables, respectively, have the expected signs and do not deviate much from the estimates obtained in the other level equations. The coefficient for the dummy indicating Swedish nationality is higher for both men and women than in any of the other two samples and the male marriage premium is also somewhat higher. In columns (2) and (4) we see that the total number of unemployment spells has a negative and significant effect on wages for men, indicating that wages decreases with the number of spells. [25]

When previous unemployment experience is accounted for the coefficient of the dummy indicating unemployment occurrence in 1990 is negative for both men and women, but significant only for men. Thus, the result in column (2) in Table 4 does confirm the previous conclusion, namely that unemployment experience has a negative impact on subsequent wages for men. For

called ”scarring effects”, or by heterogeneity.

[24] Another rationale is based on the assumption that employer may use previous unemployment experience as a signal of individual productivity. Thus, the employers *perceive* previous unemployment spells to be inversely related to productivity they may want to adjust wages accordingly.

[25] A specification with totspell entering as a quadratic as well was also estimated in order to capture possible non-linear effects. In the both the male and the female regression, the coefficient of the quadratic term had the expected (positive) sign but

men, the absolute value of the coefficient falls somewhat compared to the benchmark regression which is consistent with the hypothesis that total spells act as a control for unobserved heterogeneity. However, the difference is not significant.

Table 4. Cross-section Wage Equations based on Sample III

	(1)	(2)	(3)	(4)
	men	men	women	women
const	3.7805 (48.487)	3.8001 (48.412)	4.1131 (18.946)	4.1147 (18.970)
un90	-.1253 (2.894)	-.0982 (2.249)	-.0282 (.581)	-.0252 (.521)
exp	.0203 (7.864)	.0202 (7.821)	.0122 (5.625)	.0121 (5.586)
$exp^2/100$	-.0282 (5.454)	-.0285 (5.600)	-.0714 (3.880)	-.0173 (3.860)
schy	.0297 (8.445)	.0293 (8.351)	.0192 (6.972)	.0191 (6.925)
ed2	.1122 (4.295)	.1087 (4.173)	.0235 (1.024)	.0231 (1.011)
ed3	.1590 (4.719)	.1563 (4.591)	.1083 (5.020)	.1079 (5.008)
ed4	.1614 (4.317)	.1593 (4.264)	.1982 (6.520)	.1978 (6.519)
married	.0651 (4.037)	.0619 (3.852)	.0029 (.225)	.0029 (.222)
sscale	-.0059 (3.469)	-.0059 (3.422)	-.0033 (3.149)	-.0032 (3.060)
swpar	.0936 (4.143)	.0921 (4.063)	.0494 (3.168)	.0493 (3.158)
totspell		-.0271 (2.605)		-.0053 (.725)
R^2 adj.	.3425	.3454	.2718	.2714
n	1,265	1,265	1,285	1,285

Notes: Sample III is described in Appendix B. The standard errors are corrected using the White-estimator (see White (1980)). All regressions include dummies for industry affiliation. t-values in parentheses.

7. Concluding Remarks

I have used a Swedish data set to shed some light on the question whether unemployment experience has the same impact on the careers of men and women measured as the wages they receive if they find a new job. The results suggest that men who experience unemployment will

turned out to be insignificant.

suffer from a reduction of subsequent wages while no such effect could be found for women. In the specifications that control for individual heterogeneity, the negative unemployment effect for men was still negative and the female effect remained insignificant. These findings support the interpretation that women invest more in general human capital which make them less exposed to career interruptions, at least those of short duration. Given the favourable labour market situation, it is likely that unemployment duration where rather short, which may have prevented general capital from depreciating. However, the presence of large negative occurrence effects suggest that even short-time unemployment might be a serious problem for men.

As far as previous Swedish evidence is concerned, these findings are at odds with Joyce (1997) who could not reject the hypothesis that the unemployment effects on wages were equal between men and women, but are consistent with Albrecht et. al. (1997) who found a larger negative effect for men. However, the results in this study be must taken with care. There are (at least) three reasons why caution is called for. First, it is unclear to what extent the possible influences of omitted individual heterogeneity is captured by including the lagged wage or the total number of unemployment spells. Longitudinal data on all variables, not only the wage, would enable us to explore panel data techniques, such as fixed-effects, to address this issue. In addition, longitudinal data would provide more information on job/unemployment history of individuals. Second, since the unemployment information refers to 1990, a year of very modest overall unemployment in Sweden, it is not likely that the results carry over to periods with massive unemployment without qualification as indicated above. In a tight labour market, average unemployment duration is short, and hence the inflow or the occurrence of unemployment, rather than duration, can be expected to be a better predictor of the adverse consequences of unemployment. However, in a less tight labour market, long duration may bring additional losses of human capital and in subsequent wages. Again, longitudinal data would make it possible to include unemployment histories over a longer period of time and thus at different labour market conditions.[26] Third, as mentioned in section III, it is in general not valid to extrapolate the results obtained to the entire population unless sample selection is accounted for. To the extent that women who become unemployed tend to drop out of the labour force, the relative wage-effects of unemployment may very well change if the possibility of selection bias is included in the empirical model.

[26] It might be so that adverse selection mechanisms are stronger during low average unemployment and hence that the concentration of 'lemons' are higher than during high unemployment. However, it is not obvious that such mechanisms should apply to men only as this study might suggest.

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Appendix A

The following variables were used to estimate the earnings functions:

ln W The gross hourly wage expressed in natural logarithms for regular workinghours. The wage, which applies for individuals working full-time or part-time or on leave of absence, is derived from registered earnings and transformed into hourly wages.

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exp Number of years in gainful employment.

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sch Number of years in school.

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ed*k* Dummy for highest level of education =1 if the individual belonged to level *k* at the time of the interview; 0 otherwise.

ed1: Not enrolled in senior-level compulsory school (and may incl. vocational training).
ed2: Completed (nine-year) compulsory school (-" -)
ed3: Completed upper secondary school (- "-)
ed4: Academic degree from university or university-college

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ind*k* Dummy for industry affiliation = 1 if the individual where working in industry *k* at the time of the interview; 0 otherwise. Industry affiliation is classified according to national accounts (SNI).

ind1: Agriculture, forestry, fishing
ind2: Mining and quarrying
ind3: Manufacturing
ind4: Electricity, gas and water (incl. steam and hot water supply and sewage disposal)
ind5: Construction
ind6: Wholesale and retail trade, hotels and restaurants
ind7: Transport, storage and communication
ind8: Financing, insurance, real estate and business services
ind9: Other personal services

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married Dummy for civil status =1 if the individual is either married or cohabiting; 0 otherwise.

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sscale Scale based on answers from the questionnaire on physical and mental health. A high value indicates health problems.

swpar Dummy for parental nationality =1 if the individual's parents are both Swedish; 0 otherwise.

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totspell Total number of unemployment spells experienced by the individual.

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un90 Dummy for unemployment occurrence=1 if the individual had experienced any unemployment during 1990; 0 otherwise.

unw90	Weeks of unemployment during 1990.

Appendix B

The sample has been subject to a number of restrictions, some of which are simply due to missing values (e.g. to coding errors) and some which are imposed with a specific objective.

To improve the understanding of how the sample restrictions work, sample averages for years of schooling, experience and unemployment rate respectively are reported in Table B1. First, the non-interviewed are deleted (1,467 individuals). That is, persons for which there are no information other than age, sex and place of birth. Second, observations are lost due to missing values in the dependent variable (i.e. hourly wages in 1991) (59) as well as in various control variables (270). Next, to avoid outliers, the sample is restricted to those individuals earning a hourly wage more than SEK 20 in 1991 and hence individuals earning a wage greater than zero but less than or equal to SEK 20 are deleted (3).[27] Further, in order to limit the influence from unusual labour market attachment, the sample is confined to individuals between 18 and 65 years of age. The age restriction eliminates additional observations (682) resulting in a slightly more educated but less experienced sample. Note also that the unemployment rate is increased. For the same reason, students working full-time and reporting a positive wage in 1991 (363) are deleted without significantly altering the averages. The most important reduction is the deletion of nonemployed individuals (i.e. zero wages) in 1991 (805). [28] As a result, average experience decreases slightly while the unemployment rate is about one percentage point lower. All in all, I am left with a sample of 3,124 individuals. The gender stratified means and standard deviations are found in table B2.

Table B1. LNU sample construction

Restriction	No. of obs. remaining	Experience(years)	Schooling (years)	Unemployment-rate
No restriction	6,773			
Not interviewed	5,306			
Missing values in the dependent variable (hourly wages 1991)	5,247			

[27] I also set an upper wage limit of SEK 300 but that restriction never turned out to be binding for the final sample.
[29] In LNU, wages are only calculated for individuals employed full- or part-time including leave of absence. Those not employed are - beside unemployed - farmers or individuals assisting in family farms, self employed or individuals assisting in family businesses, spare-time workers, "creative professionals" (free-lance journalists, artists etc.), individuals who manage the household, retired, individuals called up for military service and students.

Missing values in the controls	4,977			
0< Wages≤20	4,974	21.12	10.94	.044
18>Age>65	4,292	18.53	11.41	.050
Students working full-time 1991	3,929	19.55	11.27	.049
Not employed 1991	3,124	18.84	11.55	.038

Table B2. Sample means

	Sample I		Sample II		Sample III	
	men	women	men	women	men	women
lnw91	4.44 (.31)	4.25 (.23)	4.49 (.30)	4.28 (.23)	4.48 (.30)	4.27 (.23)
lnw81			3.64 (.33)	3.47 (.31)		
un90	.037 (.19)	.039 (.19)	.019 (.14)	.013 (.11)	.023 (.15)	.024 (.15)
unw90	.44 (3.06)	.60 (4.30)	.20 (1.83)	.28 (3.24)	.30 (2.72)	.53 (4.45)
exp	20.53 (12.93)	17.16 (10.82)	24.20 (11.37)	20.42 (9.72)	23.12 (11.79)	19.37 (9.96)
schy	11.63 (3.35)	11.46 (2.90)	11.56 (3.56)	11.32 (2.98)	11.71 (3.57)	11.44 (3.06)
ed1	.71 (.45)	.74 (.44)	.71 (.46)	.76 (.43)	.69 (.46)	.74 (.44)
ed2	.11 (.31)	.08 (.27)	.09 (.29)	.05 (.22)	.10 (.29)	.06 (.23)
ed3	.07 (.26)	.11 (.31)	.08 (.27)	.11 (.32)	.08 (.27)	.11 (.32)
ed4	.11 (.32)	.08 (.27)	.12 (.32)	.07 (.26)	.13 (.33)	.09 (.28)
married	.71 (.45)	.74 (.44)	.79 (.41)	.79 (.40)	.78 (.41)	.79 (.41)
sscale	5.00 (4.32)	6.45 (5.24)	5.07 (4.37)	6.51 (5.39)	5.00 (4.30)	6.50 (5.29)
swpar	.87 (.34)	.87 (.33)	.91 (.28)	.90 (.30)	.88 (.33)	.88 (.33)
totspell					.20 (.67)	.17 (.60)
n	1,565	1,559	1,139	1,126	1,265	1,285

Note: Standard deviations in parentheses