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What is the effect of unemployment on all-cause mortality? A cohort study using propensity score matching

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Abstract

Background—There is a strong association between unemployment and mortality but whether this relationship is causal remains debated. This study utilises population level administrative data from Scotland within a propensity score framework to explore whether the association between unemployment and mortality may be causal.

Methods—The study examined a sample of working men and women aged 25 to 54 in 1991. Subsequent employment status in 2001 was observed (in work or unemployed) and the relative all-cause mortality risk of unemployment between 2001 and 2010 was estimated. To account for potential selection into unemployment of those in poor health, a propensity score matching approach was used. Matching variables were observed prior to unemployment and included health status up to the year of unemployment (hospital admissions and self-reported limiting long term illness) as well as measures of socio-economic position.

Results—Unemployment was associated with a significant all-cause mortality risk relative to employment for men (hazard ratio 1.85 95% CI 1.33-2.55). This effect was robust to controlling for prior health and socio-demographic characteristics. Effects for women were smaller and statistically insignificant (HR 1.51 95% CI 0.68-3.37).

Conclusion—For men, the findings support the notion that the often observed association between unemployment and mortality may contain a significant causal component though for women there is less support for this conclusion. However, female employment status, as recorded in the census, is more complex than for men and may have served to under-estimate any mortality effect of unemployment. Future work should examine this issue further.

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Conflict of Interest Statement
None declared.

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Keywords

Unemployment; mortality; Scotland; causality; matching; health-selection; propensity score

Introduction

An extensive literature has documented and described the strong association between labour market disadvantage and health & mortality. (1-5). However, it remains difficult to determine if the link between unemployment and mortality is a causal relationship because poor health (health selection) is a risk factor for both unemployment and mortality. (6) Furthermore, unemployment is more likely to occur among individuals from poorer socio-economic backgrounds and it may be that the deleterious health effects associated with poverty and disadvantage *prior* to unemployment may be responsible for the increase in mortality risk rather than any effects caused by the unemployment itself. (6)

As labour market status cannot be randomised, most studies rely on observational data where adequate analytical control for confounding is difficult. (6) Solutions include using ‘wear-off periods’ during which mortality events are ignored for a period subsequent to baseline observation allowing health selection effects to diminish. (7) However, the effectiveness of this approach in studies of the unemployed has been questioned. (7) Alternatively, use is made of natural experiments such as, for example, instances of mass redundancies following large-scale company downsizing or collapse, (8,9) or comparison of the relationship during periods of recession with periods of economic prosperity when the prevalence of unemployment is lower. (10,11) The rationale underpinning these types of studies is that poor health is less likely (during recession / factory closure) to be the reason for job loss and is more likely to be ‘randomly distributed’ across employment groups. (8-11) In general, this body of evidence casts doubt on or lowers the effect. (12)

The importance of correctly specifying the timing of potentially confounding events relative to the unemployment event appears to have been neglected in previous studies. For example, many studies rely on self-reported health measures that are recorded simultaneously with economic activity rather than more detailed historical health data allowing adjustment for events that occur prior to unemployment. (1,2,13) The timing of health events is particularly important in order to avoid the problem of ‘over-adjustment’ for incidents of poor health that occur after unemployment which may introduce a biasing effect towards the null hypothesis. (14) In the absence of a true randomised design, this study addresses these issues by using propensity score matching to mimic the randomisation of unemployment in an observational dataset (15). In order to do so, we assume that there is minimal unmeasured confounding through the use of a longitudinal linkage study, which links between census, hospital admissions and mortality registry data to provide a large and nationally representative data sample that contains of number of contextual and potentially confounding variables. The aim of the paper is to test for a causal relationship between unemployment and mortality.

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Methods

Data and sample

The Scottish Longitudinal Study (SLS) consists of linked 1991 and 2001 national census records for a 5.3% sample of the Scottish population and contains both socio-demographic and self-reported health information. Importantly, it contains information on current employment status (in both 1991 and 2001) and the length of the current spell (2001 census only). (16) For this study, linkages were made to vital events registry data, including death registrations (from National Records of Scotland vital events) and event based hospital admission records (available for the period 1980 onwards) from the Scottish Morbidity Records (SMR). The SMR records a range of information pertaining to a hospital admission including date of admission and ICD coded diagnoses. Figure one provides details of the initial sample selection which was restricted to individuals aged 25-54 in 1991 in order to capture pre-retirement age individuals in 2001 and to remove economically inactive and unemployed in 1991, those untraced at the 2001 census and cases missing information for any of the baseline variables. The resulting sample size was 29,923 for men and 22,339 for women.

Study Design

We used propensity score matching to mimic the experimental randomisation of unemployment. The ‘treatment group’ was defined as individuals who had moved from employment in 1991 to be unemployed in 2001 with the ‘control group’ defined as individuals who were in work in both 1991 and 2001. The propensity score (i.e. the predicted probability of unemployment in 2001) was estimated separately for men and women as a function of known confounders of the unemployment-mortality relationship including socio-demographic and self-reported health risk factors (from the 1991 census) and from ICD coded psychiatric & hospital admissions and cancer registrations from 1980 onwards (from the SMR). Details of these confounding variables and full sample distributions by treatment status are provided in table one. Given that unemployment was likely to have occurred before census day in 2001, socio-demographic and health variables from the 2001 census were not used to predict the propensity score in order to minimise the potential for over-adjustment for events occurring after unemployment.

Lynch et al. identify ICD coded conditions that are activity limiting or disabling and only these codes were used from the SMR events when predicting the propensity score. (17) Because the SMR data is event based and because the 2001 census contains information about the year of last employment for individuals who are out of work, we were able to differentiate between health events occurring before from those occurring after that year. Thus, for the unemployed, only SMR events occurring before this year were used to predict the propensity score whereas the full available records up to 2001 were used for individuals who were working in 2001. Aggregation of the hospital admission information is detailed in Figure one.

The propensity score was then used to pair unemployed with in work individuals to form the matched sample. A number of matching algorithms exist to do this but simulation studies

have suggested that matching one treatment case with the closest single control case (rather than two or three) optimises the trade-off between bias reduction and sampling variability. (18) Once matched, control cases were removed from the 'pot' to prevent them being matched to more than one treated case. In order to ensure that all matched pairs were adequately similar, an additional restriction, known as caliper matching, was imposed to ensure that the propensity scores of control cases lay within an interval of 0.01 of the propensity score of their matched treated case. All treated cases without an appropriate matched control were excluded from the matched sample along with all of the control cases that were not required to provide a match. To assess whether the matched sample was balanced, distributions of the variables used to predict the propensity score were compared. Balance across variables was assessed using standardised differences which calculate differences in the prevalence of each level of each variable in units of the pooled standard deviation. Standardised differences of roughly 0.1 or less are considered negligible for the purposes of determining balance. (19,20)

To determine the sensitivity of the results when using different propensity score approaches, mortality risks were also calculated using the sub-classification method. This involves calculating quintiles of the entire propensity score distribution and estimating mortality hazard ratios separately within each of these quintiles, which in effect calculates risks separately amongst individuals with similar probability of unemployment. (21) These estimates are reported individually and then combined and weighted appropriately to produce an overall effect. (22) The results from both approaches were compared.

The period of mortality follow-up started from the 2001 census day (29th April 2001) to the end of 2010. Embarkations from the study during the follow-up period due to migration were identified and censored. Cox proportional hazards models were used to estimate the relative mortality risk of unemployment and were conducted separately for men and women.

Results

4% and just under 2.5% of men and women respectively in the initial sample were unemployed in 2001. Death rates in the follow-up period (2001-2010) for unemployed and in work amongst men were 9% and 3.7% and amongst women 3.4% and 2.3% respectively. Means and proportions of the variables used to predict the propensity score and standardised differences between the in-work and those unemployed were examined (Table 1). For both men and women, imbalances (standardised difference > +/− 0.1) were noted for all of the socio-demographic variables and, in terms of health, for hospitalisation for mental and behavioural illness. The degree of imbalance is also illustrated in Figure one of the supplementary material and shows that, in terms of the distribution of the overall propensity scores, the unemployed and the in work were relatively similar for both men and women.

Standardised differences for all variables were less than 0.1 for men in the matched sample indicating balance with respect to these variables (table 2). For women, the degree of balance was broadly similar although mean age was slightly higher in the control group. As the direction of this small difference was likely to result in only a fractional increase in bias

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towards the null hypothesis, it was considered ignorable. Thus, within the matched sample, the transition into unemployment in 2001 was considered independent of these variables.

Table two shows results from the mortality follow-up analyses, estimated from both the full sample sub-classification approach and the restricted case matched sample. A weighted average of the unadjusted sub-class estimates showed effects (2.55 for men and 1.53 for women) which were considerably higher than those acquired from the case matched sample for men though similar for women (1.85 for men and 1.51 for women). The adjusted coefficients in the sub-classification models were more comparable to the matched sample analysis which indicated either potential residual confounding in the unadjusted subclass models (particularly for men) or the fact that stratification on the propensity score (without adjustment) often results in estimates biased away from the null hypothesis in analyses of time-to-event outcomes. (23) All of the coefficients for men were statistically significant ($p < 0.01$) and showed at least an 85% excess mortality risk in the period 2001-2010 for the unemployed relative to those in work in 2001. For women, the findings suggested a 50% increase in the risk of mortality but none of these are significant at $p<0.05$ or $p<0.01$.

Discussion

Main findings

This study examined the effect of unemployment for mortality with an analysis which attempted to mimic a randomised experiment and captured the timing of confounding effects through the use of observational longitudinal data. After matching based on health and other confounding variables the findings showed an 85% and 50% increase in the risk of mortality for men and women respectively who were registered as unemployed ten years from baseline compared to those who remained in employment. Although the effect for men was statistically significant ($p<0.01$) the effect for women was not.

Limitations

There are limitations with the analysis. If there were unmeasured differences between the unemployed and in work that also relate to mortality, our effect estimate will not be free of bias as the matching approach will not be able to take account of the unmeasured confounding. Given that that the study was able to control for a wide range of known confounders it could be argued that considerable residual confounding was less likely but this remains a possibility.

For those who were unemployed in 2001, the year in which the unemployment spell began was used as the censoring variable for hospital admissions with any events occurring after this date considered as possible outcomes of unemployment and ignored. However, for those who were in work in 2001, hospital admissions information for the entire period between 1980 and 2001 were included in the analysis. This may give the appearance of a comparatively higher prevalence of serious health events in some members of the in work group as a result of a lengthier ‘at risk’ period and therefore is a possible source of bias.

The use of census data to capture labour market participation presents two important limitations. Firstly, it represents a snapshot of the population on that particular census day

and will contain both short and long term unemployed but with an oversampling of the latter. (24) Further over-sampling of the long term unemployed would be expected due to the period of economic boom in 2001 when unemployment was identified. In our sample of unemployed, 31% and 36% of men and women respectively had worked within 16 weeks of the census date (29th April 2001) and 64% and 67% within 64weeks. This left 36% and 33% of the samples in a spell of unemployment longer than 64 weeks. Compared to national labour market statistics, these figures appear to confirm under-representation of the short-term unemployed. (25) The inherent difference and the implications for subsequent effect estimates of census based measures of exposure compared to exposure based on length and number of spells has been reflected on in more detail previously elsewhere. (24)

One possible effect of using unemployment measured on one particular day as a measure of exposure might have been to underestimate subsequent hazard ratios. The control group of unexposed might have contained a large number of individuals who experienced previous spells of unemployment and the mixing of exposed and unexposed individuals in the control group in this way is likely to have artificially diluted the resulting effect estimates. It would be of interest in future studies to explore the impact of length of unemployment on mortality.

Secondly, it is widely asserted that female labour market participation differs compared to men in terms of reduced labour market attachment and greater involvement in household responsibilities such as looking after a family. (5,26-29) In census data this may lead to underestimation of the level of unemployment amongst women who may not as readily acknowledge themselves as unemployed in the census compared to men and may instead choose alternative census categorisations such as “looking after home” or “other”. (30) As a result, the lower sample unemployment rate that is shown in table one for women may in fact hide the true level of unemployment in the sample which excludes these alternative categorisations. This introduces two possible problems. Firstly, the significantly lower numbers of unemployed women means that the hazard ratios were estimated with far less precision than those for men making it harder to reject the null hypothesis. Secondly, our hazard ratios could have been biased upwards or downwards because it is difficult to determine whether or not this misclassification was greater amongst women who were more or less vulnerable to the health effects of unemployment. Finally, a general limitation of the study is the relatively small sample size which precluded analysis of cause specific mortality.

Interpretation

Though initial studies on the subject tended to support the theory that unemployment is independently related to mortality they were often lacking adequate control for health. (2) Conversely, studies using both quasi-experimental methods & natural experiments and those with direct control for health have found less evidence for, or have downplayed, the effect size. (8,11,12) A meta-analysis of the unemployment and mortality literature found average ‘age and additional covariate adjusted’ mortality hazard ratios that were similar to our findings at 1.78 and 1.37 for unemployed or out of work men and women respectively. (5) However, a direct comparison with these average values is difficult as it obscures

considerable heterogeneity between studies in terms of research design, availability of adjustment covariates and coding of unemployment status. For example, studies that consider all out of work individuals reported hazard ratios that were around 50% higher compared to studies who restricted their analysis to individuals actively seeking work. Similarly, studies that did not adjust for age were around 16% higher compared to those that did and those that adjust for more than one measure of socio-economic status were reduced by 13% when compared to studies with only one or no measure of socio-economic status. Given, that the present study adjusts for age as well as a range of both individual and area socio-economic status variables and uses a well-defined measure of unemployment which excludes the economically inactive, we might have expected the effects sizes to be considerably smaller than the average sizes observed in this meta-analysis.

There are other features of our study (not considered in the meta-analysis) which might also lead us to expect, a priori, more conservative effect sizes. For example, our baseline sample was observed in 1991 during a period of recession, when health related selection has been suggested to be less likely. (10,11) Furthermore, the analysis was restricted to individuals who were in employment in 1991 which is likely to result, instead, in the selection of individuals who are relatively advantaged in terms of labour market success. This is due to the fact that they have managed to retain employment at a time when the overall unemployment rate and therefore likelihood of unemployment is higher.

One possible explanation for the higher than expected effect sizes observed in this study could be that many previous studies may have miss-specified confounding effects by ignoring the timing of them relative to unemployment. For example, intermediate events that occur after unemployment are unlikely to have caused that unemployment and adjusting for them as if they are confounding rather than mediating effects is likely to result in a bias towards the null hypothesis. (14) Longitudinal data combined with information about when an individual was last in work is therefore an important feature of this study. Another possible explanation is that the effect of unemployment varies between countries, perhaps reflecting differences in the extent of state or welfare support. (31,32) The UK is traditionally less generous in its provision of welfare state support when compared to, for example, many Scandinavian countries and this may also contribute to a worse health effect of unemployment. (33)

The findings for women cannot be interpreted as straightforwardly as for men. On the one hand the null findings for women may indicate that women suffer less from the negative effects of unemployment when compared to men. Support for this explanation can be found in work that argues that women are less tied to work and income generation (34-36) as well as meta-analysis evidence that highlights a consistently higher risk of mortality associated with unemployment for men than for women. However, in contrast, qualitative evidence suggests that women suffer similar feelings of isolation, loneliness and boredom during unemployment casting doubt on the notion that women are less affected. (37) Moreover, other evidence points to the fact that women’s participation in the labour market has and continues to change rapidly to the point that comparisons between men and women show increasingly less marked differences. (38) This evidence, in conjunction with the limitations associated with using census data to capture female labour market participation may cast

doubt on the notion that women are less susceptible to the effects of unemployment than men. In light of these changing patterns, future work should continue to focus on the relatively neglected question of the health effects of labour market position amongst women, perhaps through the use of data, where available, that better captures the details and complexities of women's labour market participation.

Conclusion

This study provides strong evidence that, for men at least, unemployment is independently associated with an elevated all-cause mortality risk. To date, it is the only study of unemployment and mortality in the UK that has utilised information about hospital admissions prior to unemployment to adjust for health selection rather than relying solely on census based self-reported health measures.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Key points

- Unemployment is strongly associated with mortality, however, prior health and other characteristics can confound the association casting doubt on the extent to which the association is evidence of a causal pathway.
- Using a novel research design with a focus on the timing of confounding effects relative to unemployment, this study finds a strong and significant excess risk of mortality associated with unemployment.
- The study extends previous observational evidence and, assuming that there is minimal unobserved confounding, suggests support for a causal explanation for the association between unemployment and mortality.

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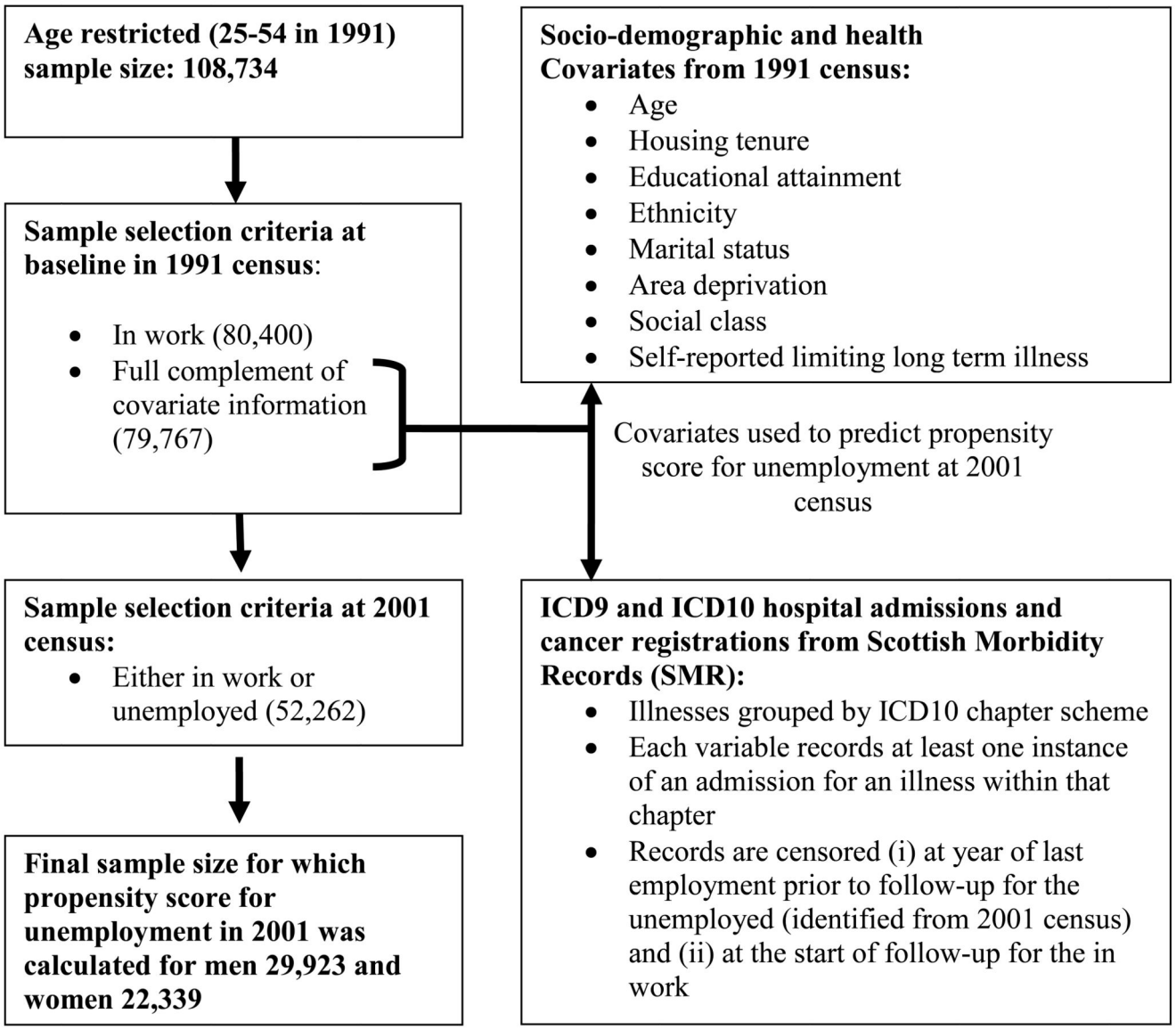

Figure 1. Sample selection criteria (corresponding sample size in brackets) and outline of covariates used to estimate the propensity score for unemployment in 2001

Table 1

Characteristics of full samples of men and women in terms of covariates predicting unemployment in 2001 and standardised differences between 2001 unemployed and in work groups

Covariates	Men (N = 29,923)			Women (N = 22,339)		
	Unemployed	In work	Stan. Diff	Unemployed	In Work	Stan. Diff
Health Variables (from SMR and 1991 census)						
Self-reported limiting long-term illness (in 1991) %						
No	96.8	98.1	−0.083	95.9	98.4	−0.151
Yes	3.2	1.9	−0.083	4.1	1.6	0.151
% of individuals with at least one hospital admission (from SMR) for activity limiting or disabling conditions in following disease categories during period 1980 up to 2001 or the year last worked						
2 Neoplasms	4.4	6.2	−0.080	18.5	16.2	0.061
3 Blood and immune mechanism	<1	<1	<0.1	<1	<1	<0.1
4 Endocrine, nutritional and metabolic	<1	<1	<0.1	<1	<1	<0.1
5 Mental and behavioural	6.1	2.0	0.209	4.8	1.9	0.162
6 Nervous system	1.6	1.3	0.025	1.4	1.7	−0.024
7 Eye and adnexa	<1	<1	<0.1		Not in model	
9 Circulatory system	2.7	3.0	−0.018	1.4	1.3	0.009
10 Respiratory system	<1	<1	<0.1	1.6	<1	<0.1
11 Digestive system	<1	<1	<0.1	<1	<1	<0.1
14 Genitourinary system	<1	<1	<0.1		Not in model	
17 Congenital conditions	<1	<1	<0.1	<1	<1	<0.1
20 External causes of morbidity	15.4	12.8	0.075	8.5	5.9	0.101
Socio-demographic variables (from 1991 census)						
Housing Tenure (in 1991) %						
Owner occupied	53.6	73.8	−0.430	59.5	73.1	−0.291
Privately Rented	6.3	5.9	0.017	6.9	4.7	0.094
Social Housing	39.7	20.0	0.441	33.6	22.2	0.256
Communal Establishment	<1	<1	<0.1		Not in model	
Educational Attainment (in 1991) %						
None	86.3	75.5	0.277	81.9	73.3	0.207
Other higher qualifications (non-degree)	7.6	10.4	−0.098	10.1	15.6	−0.165
First degree and higher degree	4.4	12.3	−0.289	6.0	9.3	−0.124
Not stated	1.7	1.9	−0.015	2.1	1.8	0.022
Mean age (in 1991)	39.1	37.4	0.620	36.8	37.0	−0.086
Marital status (in 1991) %						
Married (first marriage)	60.7	73.1	−0.266	57.2	71.0	−0.291
Single	23.8	16.8	0.175	22.7	15.2	0.192
Remarried	8.0	5.8	0.087	7.3	5.7	0.065
Divorced	6.9	3.9	0.133	11.7	6.9	0.166

Covariates	Men (N = 29,923)			Women (N = 22,339)		
	Unemployed	In work	Stan. Diff	Unemployed	In Work	Stan. Diff
Widowed	<1	<1	<0.1	1.1	1.3	<0.1
Deprivation quintiles (in 1991) %						
Least deprived Quintile	16	23.9	−0.199	15.6	22.0	−0.164
2nd	22.2	29.6	−0.170	30.4	28.5	0.042
3rd	20.6	21.6	−0.025	20.8	22.5	−0.041
4th	22.9	15.5	0.189	19.2	16.4	0.073
Most Deprived Quintile	18.3	9.4	0.260	14.0	10.5	0.107
Social Class (in 1991) %						
Professional Occupations	3.8	7.8	−0.172	1.4	2.5	−0.080
Managerial and Technical Occupations	22.7	30.0	−0.166	24.3	32.8	−0.189
Skilled Non-manual Occupations	9.1	10.6	−0.050	35.2	36.9	−0.035
Skilled Manual Occupations	36.6	33.2	0.071	9.2	6.3	0.109
Partly Skilled Occupations	20.9	14.0	0.183	21.5	13.4	0.215
Unskilled Occupations	6.2	3.4	0.131	8.5	8.1	0.014
Armed Forces	<1	<1	<0.1		Not in model	

To avoid low numbers, cells with very low proportions are rounded up to one.

Source: Scottish Longitudinal Study

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Table 2

Characteristics of case matched samples of men and women in terms of covariates predicting unemployment in 2001 and standardised differences between 2001 unemployed and in work groups

Covariates	Men			Women		
	Unemployed	In work	Stan. Diff	Unemployed	In Work	Stan. Diff
Health Variables (from SMR and 1991 census)						
Self-reported limiting long-term illness (in 1991) %						
No	96.8	97.3	−0.030	95.9	96.8	−0.048
Yes	3.2	2.7	0.030	4.1	3.2	−0.048
% of individuals with at least one hospital admission (from SMR) for activity limiting or disabling conditions in following disease categories during period 1980 up to 2001 or the year last worked						
2 Neoplasms	4.4	3.7	0.036	18.5	19.7	−0.031
3 Blood and immune mechanism	<1	<1	<0.1	<1	<1	<0.1
4 Endocrine, nutritional and metabolic	<1	<1	<0.1	<1	<1	<0.1
5 Mental and behavioural	5.9	4.7	0.054	4.8	5.5	−0.032
6 Nervous system	1.6	1.1	0.043	1.4	<1	<0.1
7 Eye and adnexa	<1	<1	<0.1		Not in model	
9 Circulatory system	2.7	2.7	0.000	1.4	1.2	0.018
10 Respiratory system	<1	<1	<0.1	1.6	1.1	0.043
11 Digestive system	<1	<1	<0.1	<1	<1	<0.1
14 Genitourinary system	<1	<1	<0.1		Not in model	
17 Congenital conditions	<1	<1	<0.1	<1	<1	<0.1
20 External causes of morbidity and mortality	15.4	14.8	0.017	8.5	6.9	0.060
Socio-demographic variables (from 1991 census)						
Housing Tenure (in 1991) %						
Owner occupied	53.7	52.7	0.020	59.5	58.8	0.014
Privately Rented	6.3	4.5	0.080	6.9	6.0	0.037
Social Housing	39.6	42.9	−0.067	33.6	35.2	−0.034
Communal Establishment	<1	<1	<0.1		Not in model	
Educational Attainment (in 1991) %						
None	86.3	87.8	−0.045	81.9	83.3	−0.037
Other higher qualifications (non-degree)	7.6	7.3	0.011	10.1	8.7	0.048
First degree and higher degree	4.4	3.6	0.041	6.0	6.0	0.000
Not stated	1.7	1.3	0.033	2.1	2.1	0.000
Mean age (in 1991)	39.1	39.1	0.02	36.8	37.0	0.102
Marital status (in 1991) %						
Married (first marriage)	60.8	62.5	−0.035	57.2	59.5	−0.047
Single	23.6	22.7	0.021	22.7	22.7	0.000
Remarried	8.0	8.0	0.000	7.3	7.8	−0.019
Divorced	6.9	5.8	0.045	11.7	9.4	0.075

Covariates	Men			Women		
	Unemployed	In work	Stan. Diff	Unemployed	In Work	Stan. Diff
Widowed	<1	1.1	<0.1	1.1	<1	<0.1
Deprivation quintiles (in 1991) %						
Least deprived Quintile	16.0	15.4	0.016	15.6	13.5	0.060
2nd	22.2	22.0	0.005	30.4	31.1	−0.015
3rd	20.7	19.8	0.022	20.8	20.6	0.005
4th	22.9	22.7	0.005	19.2	19.5	−0.008
Most Deprived Quintile	18.2	20.1	−0.048	14.0	15.3	−0.037
Social Class (in 1991) %						
Professional Occupations	3.8	3.2	0.033	1.4	1.8	−0.032
Managerial and Technical Occupations	22.7	23.2	−0.012	24.3	23.3	0.023
Skilled Non-manual Occupations	9.1	8.7	0.014	35.2	36.6	−0.029
Skilled Manual Occupations	36.7	36.7	0.000	9.2	8.5	0.025
Partly Skilled Occupations	20.9	21.7	−0.020	21.5	22.9	−0.034
Unskilled Occupations	6.0	6.1	−0/004	8.5	6.9	0.060
Armed Forces	<1	<1	<0.1		Not in model	

To avoid low numbers, cells with very low proportions are rounded up to one.

Source: Scottish Longitudinal Study

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Table 3

Mortality risks of unemployment relative to employment during follow-up period 2001-2010

Sample	Men		Women	
Sub-classification on the propensity score across full sample	**Cox hazard ratio (C.I. < .05)**			
	Unadjusted models	**Adjusted models**	**Unadjusted models**	**Adjusted models**
Quintile 1	2.91* (0.92-9.14)	2.37ns (0.74-7.60)	1.53ns (0.21-11.02)	1.49ns (0.20-10.88)
Quintile 2	1.95* (0.92-4.16)	1.89ns (0.88-4.06)	0.70ns (0.10-5.03)	0.59ns (0.08-4.27)
Quintile 3	2.64*** (1.70-4.09)	2.48*** (1.58-3.87)	0.56ns (0 .08-4.05)	0.44ns (0.06-3.22)
Quintile 4	1.60** (1.02-2.53)	1.63** (1.03-2.58)	1.36ns (0.43-4.28)	1.30ns (0.41-4.12)
Quintile 5	1 89*** (1.40-2.55)	1 92*** (1.41-2.60)	2.15** (1.09-4.24)	2.14** (1.06-4.32)
Weighted average effect estimate	2.55*** (2.08-3.12)	1 97*** (1.60-2.42)	1.53ns (0.91-2.55)	1.41ns (0.84-2.37)
Case matched restricted sample (no adjusted models)	1.85*** (1.33-2.55)		1.51ns (0.68-3.37)	

ns (not significant)

Hazard ratios show the mortality effect of unemployment relative to being in work. Unadjusted models contain no additional adjustment and adjusted models include adjustment for all of the covariates in tables two and three that were used to predict the propensity score.

Source: Scottish Longitudinal Study

* (p<.10)

** (p<.05)

*** (p<.01)