Unemployment and self-assessed health: Evidence from panel data*

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ABSTRACT

We analyse the relationship between unemployment and self-assessed health using the European Community Household Panel (ECHP) for Finland over the period 1996-2001. Our results reveal that the event of becoming unemployed does not matter as such for self-assessed health. The health status of those that end up being unemployed is lower than that of the continually employed. Hence, persons who have poor health are being selected for the pool of the unemployed. This explains why, in a cross-section, unemployment is associated with poor self-assessed health. All in all, the cross-sectional negative relationship between unemployment and self-assessed health is not found longitudinally.

JEL Codes: I10, J28

Keywords: health, subjective well-being, unemployment

* An earlier version of this paper was presented at the UNU-WIDER Conference on Advancing Health Equality, Helsinki. We are grateful to the seminar audience for comments. In addition, we are grateful to Eija Savaja for her help with the data. The usual disclaimer applies.

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INTRODUCTION

The welfare effects of unemployment have been considered in many strands of research. Several studies ranging from research papers in medicine to those in the social sciences and economics have shown that unemployment is associated with adverse health outcomes (e.g. Björklund and Eriksson, 1998; Mathers and Schofield, 1998). Both cross-sectional and panel data sets and both objective and subjective measures of health have been used in this literature.[1] Furthermore, the relationship between health and subsequent unemployment has been examined (e.g. Arrow, 1996; Riphahn, 1999). There is evidence that poor health is associated with subsequent unemployment. On the other hand, the growing research on the determinants of happiness in economics reports that the unemployed are very unhappy if they are evaluated by standard subjective measures (e.g. Clark and Oswald, 1994; DiTella *et al.,* 2001). Moreover, there are some recent studies that have looked at unemployment and the subsequent evolution of the subjective measures of well-being, most notably happiness and life satisfaction, in a panel data setting (e.g. Lucas *et al.*, 2004; Clark, 2007), but the available empirical evidence is still sparse in this respect.

In this paper, our purpose is to analyse the evolution of self-assessed health in a panel data setting before and after the event of unemployment occurs and also when unemployed persons become employed again in order to disentangle the causal effect of unemployment on health. In contrast to most of the earlier studies, we apply difference-in-differences models and matching methods. In particular, the use of matching methods allows us to take into account the selection for unemployment and the possibility of reverse causality from poor health to unemployment. Previously, this selectivity issue has mainly been tackled by using plant closings as instruments for unemployment (e.g. Kuhn *et al.*, 2004; Browning *et al.*, 2006).

Our most important empirical finding is that the event of unemployment does not matter as such for self-assessed health in a panel data setting. The health status of those that end up being unemployed is lower than that of the continually employed before their unemployment episodes actually start. Hence, persons who have poor health are being selected for the pool of the unemployed. This explains why in a cross-section unemployment is associated with poor self-assessed health, whereas in longitudinal data this negative relationship is not found.

We take advantage of the European Community Household Panel (ECHP) for Finland, which is a representative household survey. The ECHP has not been much exploited in the literature on the determination of self-assessed health (see, however, Cantanero and Pascual, 2005; Hildebrand and van Kerm, 2005; Etienne *et al.*, 2007).[2] The data span the period 1996-2001. As a result, it covers a period long enough for reliable results about the adverse effects of unemployment on health to be obtained. The effect of unemployment on a subjective measure of health is interesting in the Finnish context, because the national unemployment rate surged very rapidly from 3 to 17 per cent in the early 1990s.[3] Such an increase has been unprecedented among the industrial countries. High unemployment may reduce the negative subjective health effects that are associated with the personal experience of unemployment, because less stress and social stigma may arise from being unemployed in times of high unemployment (e.g. Lindbeck *et al.*, 1999; Clark, 2003). This point is relevant, because a relatively high unemployment rate has persisted in Finland since the great depression of the early

1990s. Importantly, persistent unemployment is helpful when investigating the relationship between health and unemployment, because there are a great number of unemployment episodes that start at any given point of time that allow us to analyse the causal effect of unemployment on health in detail. In addition, long-term unemployment rose a great deal in Finland during the 1990s. This is useful when investigating the habituation effects on unemployment.[4]

Measures of self-assessed health are widely used in empirical research. Despite this, there is still some amount of scepticism regarding the use of self-reported data on health. In particular, subjective measures of health that often originate from household surveys can be criticised on the ground that they provide potentially biased information about persons' health for the very reason that they are self-reported. Accordingly, self-reported information on health cannot be as reliable as that based on the objective measurement of health. However, various subjective measures of health have been proven to have substantial value in predicting objective health outcomes, including morbidity and mortality (e.g. Idler and Benyamini, 1997; Franks *et al.*, 2003; Van Doorslear and Jones, 2003).[5] For that reason alone they are worth analysing.

The paper proceeds as follows. In the next section we provide a description of the data. The last two sections report the results and conclude the paper.

DATA

Our paper takes advantage of the European Community Household Panel (ECHP) for Finland over the period 1996-2001.[6] The ECHP is based on a standardised questionnaire that involves annual interviews of a representative panel of households and individuals in each European Union country (e.g. Peracchi, 2002). The fact that the ECHP is representative over population is an important advantage with respect to some earlier studies on the relationship between unemployment and health that have used panel data sources. The ECHP is composed of a separate personal file and a separate household file that can be linked with each other. In this paper, we use data from the personal file, because it is the file that contains information on self-assessed health.

The ECHP's questions include various topics such as income, health, education, housing, living conditions, demographics and employment characteristics, among other things. The ECHP data allow us to record the health status of individuals before their unemployment episodes actually start. This constitutes an important advantage over cross-section data sources that have been more frequently used in research to compare population averages, because we are in a better position to analyse the causal effect of unemployment on health. In this paper, we focus on transitions between work and unemployment or *vice versa.* Hence, we exclude persons who are out of the labour force, like retirees and students.

One's self-assessed health status is an answer to the question: "How is your health in general?". This question aims to summarise an individual's general state of health at the moment of interview. Self-assessed health is measured on an ordinal 5-point Likert scale with alternatives 5 ('very good'), 4 ('good'), 3 ('fair'), 2 ('bad') or 1 ('very bad'). Hence, a higher value on this scale means that a person feels currently healthier. (We have reversed the scale of the health measure in the ECHP survey to emphasise that higher numbers correspond to better health.) A similar question on self-assessed health appears in many other well-known household surveys such as the British Household Panel Survey (BHPS) and the German Socio-Economic Panel (GSOEP). There are three recent papers that have analysed the determination of self-assessed health by using the ECHP. Hildebrand and Van Kerm (2005) and Etienne *et al.* (2007) focus on the connection between income inequality and self-assessed health by using the ECHP for several countries. Cantarero and Pascual (2005) investigate the relationship between socio-economic status and health by using the ECHP for Spain. To our knowledge, the effect of unemployment on self-assessed health has not been examined previously through the use of the ECHP.

We study persons that are unemployed at least once over the period 1996-2001. The reference group consists of those that are continually at work. This means that unemployed persons are compared with persons with a strong attachment to the labour market. The ECHP does not incorporate direct information about the unemployment duration for the persons interviewed. However, the data record monthly activity statuses (unemployed being one possible alternative) for each person for the whole year before the interview. In addition, the data contain information on the month in which the interview took place in each wave. This piece of information is important, because the main month of interview in the ECHP for Finland has changed from the beginning of the year in the first waves towards the end of the year in the last waves. By combining information on the monthly activity statuses and the month of interview, it is possible to construct a measure for each person's unemployment duration in months at the time of the interview. We define the term 'long-term unemployed' to include those persons that have been unemployed continuously at least for six months. In this way, we avoid problem of “top-coding” in the unemployment duration mentioned, in the context of the ECHP, by Clark (2007).[7]

FINDINGS

Descriptive evidence

Figure 1 documents the evolution of the average level of self-assessed health in the years 1996-2001. The health status of those who are employed has slightly deteriorated over the period. The overall change is not large by any reasonable standards. More interestingly, the population averages reveal that the health status of the unemployed is clearly lower than that of the employed and it also deteriorated somewhat during the 1990s. Furthermore, the health status of the long-term unemployed is lower than that of all unemployed.

=== FIGURE 1 HERE ===

Table I reports a cross-tabulation of the self-assessed health and unemployment status. This simple characterization of the data provides some evidence that the health status of those who are currently unemployed is lower than that of the employed. In particular, long-term unemployment seems to damage self-assessed health. However, this kind of purely descriptive analysis that exploits solely the cross-sectional variation in the data is not able to reveal the causal relationship between unemployment and health.

=== TABLE I HERE ===

To shed light on the causal relationship, we need to take advantage of the panel dimension of the ECHP. Accordingly, it is useful to illustrate the changes in health around the beginning of unemployment episodes. Figure 2 shows a Galton squeeze diagram (see Campbell and Kenny, 1999) of the development of self-assessed health before and after becoming unemployed. The starting points of the lines on the left-hand side of the figure show the initial levels of health while the individuals were still working. The end-points of the lines on the right-hand side of the figure show the average level of health after becoming unemployed. The figure summarizes changes in health using all two-year pairs in the panel. For example, those who had self-assessed health equal to 1 while still working had, on average, level 1.5 after becoming unemployed. The pattern of the lines shows that there is a regression towards the mean: Those with poor or good health tend to converge towards the average. Clearly, we cannot say that on average becoming unemployed leads to a fall in health.[8] Figure 3 shows the same kind of diagram drawn using data from all two-year periods where the individuals were employed in both periods. We can see that even in this case there is a regression towards the mean. The main difference between Figures 2 and 3 is that in Figure 2 those who have low self-assessed health in the first period do not converge as much to the average as those with low health in Figure 3. That is, those with low health while employed tend to stay at a relatively low health level when they become unemployed. However, in general, we cannot say that those becoming unemployed converge to a different mean health level than those in continuous employment.

== FIGURES 2 AND 3 HERE ===

The relationship between health and unemployment can also be evaluated in another way by looking at changes in the subjective perception of health status when a person switches from unemployment to employment. Figure 4 shows the Galton squeeze diagram for those who are unemployed in the first period, but employed in the second period. The pattern in Figure 4 is similar to the previous figures, except for the line starting from 1 which is, however, based on only two observations.[9]

=== FIGURE 4 HERE ===

One problem clearly revealed by Figures 2-4 is that the regression towards the mean is partly driven by the fact that health cannot improve beyond level 5 and hence for those at level 5, the level in the next period is likely to be on average below 5. Similarly, health cannot fall below level 1 and hence for those at level 1, the level in the next period is likely to be on average above 1. It is therefore likely that controlling for the initial level of health is necessary when one is studying changes in the health scores.[10]

Difference-in-differences estimates

To analyse the relationship between unemployment and health more closely, we estimate difference-in-differences models in which an individual's self-assessed health is explained with a dummy variable for the ”unemployment target group” that consists of persons that become unemployed at least once during the period 1996-2001, a dummy variable for those currently unemployed after a period of employment (the dummy is equal to one in all the years of unemployment after an employment spell), a dummy for the “employment target group” that consists of those who become employed at least once, a dummy for those currently employed after a period of unemployment (the dummy is equal to one in all the years of employment after an unemployment spell), a dummy for those who are unemployed for the whole data period, and year dummies to capture the effect of business cycle fluctuations. In some of the models we also include individual-level control variables *X*, age and its square, gender and the level of education in three categories, which capture the 'usual suspects' that should have a bearing on the self-assessed level of health.[11] This analysis of changes in health status assumes that self-assessed health is measured on a cardinal scale, and not on an ordinal scale, and that both experiencing unemployment and becoming employed after a period of unemployment are exogenous events. The estimated model is

$$Health_{it} = \alpha + \beta(Becomes\ unemployed\ at\ least\ once)_i + \gamma(Unemployed\ after\ employment)_{it} + \phi(Becomes\ employed\ at\ least\ once)_i + \mu(Employed\ after\ unemployment)_{it} + \eta(Always\ unemployed)_i + X_{it}\theta + \Sigma_t \tau_t(Year\ t) + \varepsilon_{it} \qquad (1)$$

The average health level for those in continuous employment is α, for those who become unemployed at some stage but are currently employed $\alpha + \beta$, for those who become unemployed $\alpha + \beta + \gamma$, for those who become employed at some stage but are currently unemployed $\alpha + \phi$, for those who become employed $\alpha + \phi + \mu$, and for those who are unemployed for the whole period $\alpha + \eta$. It is possible that some individuals become unemployed in some period and employed in some other period (or *vice versa*), so that they belong to both "target groups". For them, the "basic" level of health is $\alpha + \beta + \phi$ and it is changed by γ (μ) when they become unemployed (employed). The coefficients of the indicator variables from the OLS estimation of model (1) are reported in the first two columns of Table II.

=== TABLE II HERE ===

We first consider the results without controls. The results in Column 1 reveal that the unemployed tend to have lower health than those who are continuously employed (the reference group). Those who are always unemployed in the data period have clearly lower health (the indicator is significant at the 1% level), but also those who become unemployed at some stage have a lower self-assessed health level. However, those who are unemployed but become employed again at some stage have a somewhat higher health level than the reference group. On the other hand, when those working become unemployed, their self-assessed health status does not deteriorate and when those who are unemployed find a job, their health status does not improve. Taken together, our results show that unemployment as such does not seem to worsen the level of self-assessed health. It is more the case that the persons who experience poor health are being selected for the pool of unemployed persons and those who manage to escape unemployment tend to have better health in the first place. When we include the observable characteristics of the individuals, the coefficients of the indicators for the employment status become lower in absolute value, and the indicator for those who at some stage become employed is no longer significant.

Robustness analysis

We have estimated several alternative models to investigate how robust our conclusions are. There may be unobserved attributes of the individuals that affect both the level of health experienced and the probability of being unemployed. This would lead to inconsistency of the OLS estimates of the difference-in-differences model. To account for this, we estimate the model with fixed effects using the within transformation.[12] In this case, the time-invariant group indicators are left out (and the gender dummy is excluded from the controls). The estimated model is

$$Health_{it} = \alpha_i + \gamma(Unemployed\ after\ employment)_{it} + \mu(Employed\ after\ unemployment)_{it}$$
$$+ X_{it}\theta + \Sigma_t \tau_t(Year\ t) + \varepsilon_{it} \qquad (2)$$

The results with and without control variables and without lagged health are shown in Columns 3 and 4 of Table II. Again, the indicators for being unemployed after employment and for being employed after unemployment are not statistically significant. Hence, there is no clear impact of unemployment on health.

The above results are based on treatment of the health scores as cardinal variables.[13] It is likely, however, that the respondents do not treat health level 3, for example, as three times as good as level 1. We therefore estimated the difference-in-differences model (1) also using ordered logit. In this case we assume that *Health* is a continuous latent variable that is observed as a discrete ordinal variable. The results are shown in Columns 5 and 6 of Table II. The results regarding the signs and significance of the coefficients are quite similar to those obtained with the OLS estimation, although the magnitudes of the coefficients are, of course, not comparable.

To include fixed effects in the ordered logit estimation, we follow the suggestion of Ferrer-i-Carbonell and Frijters (2004). They show that an ordered logit model with fixed effects can be estimated as a fixed effect logit (conditional logit) model, where the ordered data are collapsed to binary data with individual-specific thresholds. In our case, the recording of observations to “high” and “low” health is individual-specific, based on the individuals’ average health scores in the panel. In this case, only individuals with changes in health status over time can be included. Columns 7 and 8 of Table II show the estimation results. Again, the labour market status indicators that are time-invariant have been left out. The indicators for becoming unemployed or becoming employed are clearly not significant.[14] As another way of taking fixed effects into account, we used Chamberlain’s random effect estimation in an ordered probit model. The individual means of the control variables were included as additional explanatory variables to proxy the fixed effects and the model was estimated with random effect ordered probit. The estimates are shown in Column 9 of Table II. The results are fairly close to those obtained with ordered logit (column 6). Those who are always unemployed in the data period or become unemployed at some stage have poorer health.

The descriptive analysis of the data suggested that it may be worthwhile to include lagged health status as an explanatory variable. In addition, past health may have an impact on becoming unemployed, which can be controlled by including the lagged health variable in the regression. Table III shows the estimation results for various models with a lagged dependent variable. Columns 1 and 2 show the OLS estimates where it is again assumed that health is a cardinal measure. The lagged health variable has a significant positive coefficient.

=== TABLE III HERE ===

Adding lagged health to the model reduces the significance of the indicators for the groups “*becomes unemployed at least once*” and “*becomes employed at least once*” and "*always unemployed*". This is what one would expect, since if the individuals that experience unemployment have poor health in the first place, it should be picked up by the lagged variable. The lagged health variable has a positive coefficient of 0.588. Therefore, deducting lagged health from both sides, changes in the health scores are negatively related to previous health. This is exactly what regression towards the mean implies: those with a high initial health level are likely to experience a fall in health and those with a low initial level a gain in health.[15] Inclusion of control variables again reduces the absolute values of the coefficients of the indicator variables, but does not change our conclusions.

Including fixed effects in the model with lagged health would lead to inconsistent estimates. We therefore use the Anderson-Hsiao estimator, where the data are first differenced and the lagged difference of the health score is instrumented with the lagged level of health two periods previously. Again the indicators for changing the labour market status are non-significant. The negative sign of the lagged differenced health variable in these fixed effects results is consistent with regression towards the mean.

When the health variable is treated as an ordinal measure, we face an initial condition problem when the lagged health score is included. If the initial health status is fixed, we can simply include lagged health in an ordered logit model. We do this by including separate dummy variables for different health scores in the previous period (denoted *Health (t-1)* $= j$, with $j = 2,\ldots,5$). The lagged values for levels 3 to 5 have significant coefficients. Without observable individual characteristics the results are qualitatively similar to the OLS results. When the personal characteristics are included, only the indicator for being always unemployed is significant. When the initial condition is treated as stochastic we use the Chamberlain type of approach and include individual means of the characteristics (to proxy fixed effects) and initial levels of the lagged health status dummies in a random effect ordered probit estimation (see Wooldridge, 2002). The results are relatively similar to the ordered logit estimates.

Propensity score matching estimates

To evaluate the robustness of the basic results that are based on regression-based models further, we estimate propensity score matching models.[16] Persons with certain observable characteristics are much more likely to be unemployed. For instance, the less educated face disproportionate difficulties in the labour market. The key idea of propensity score matching is to construct a control group from the group of untreated individuals and to ensure that the control group is as similar as possible to the treatment group with respect to available observable characteristics. In our case, the treatment is becoming unemployed (or becoming employed) and we study its effect on self-assessed health. In particular, we need not worry about the endogeneity of becoming unemployed (or becoming employed).

Matching has some important advantages over regression-based methods that were used to produce the basic results. Being a non-parametric method, matching does not impose any specific linearity assumptions on the evaluated effects that are inherent in regression-based modelling. Furthermore, matching explicitly tries to find for each untreated unit a similar treated unit to evaluate the counterfactual, i.e. what would happen to the treatment group without the treatment. As a drawback, it has to be assumed that there are no unobservable factors that affect the individuals' probability of becoming unemployed. Controlling for the observable factors, the outcome (health) is assumed to be independent of the treatment status (conditional independence or unconfoundedness assumption). One need not control for all the observable factors at the same time, but it suffices to condition on the propensity score, i.e. the probability of treatment. In using the propensity score, one has to further rule out the perfect predictability of the treatment (overlap or common support assumption). Corresponding assumptions apply when the treatment is becoming employed.

We first estimate a probit model for the probability of becoming unemployed (i.e. the probability that the person is unemployed, given that he or she was employed in the previous year). The explanatory variables include personal factors such as age, age squared, and the level of education (dummies for medium and high levels). We also include the employer's characteristics, a dummy for small firms (less than 20 employees), and a dummy for the public sector. These variables are lagged by one period. In addition, we include lagged health status and year dummies. For simplicity, we include the lagged health score directly, rather than separate dummies for different health levels. The probit model is estimated using pooled data for the whole period. Since the aim is to model selection on observables, we do not model unobservable individual characteristics in the probit model. The data set in the probit estimation consists of year pairs for those who are employed both in the current year and in the previous year and those who are unemployed in the current year, but were employed in the previous year.

The propensity scores are used with nearest-neighbour matching (one-to-one matching with replacement) and kernel (Epanechenikov kernel) methods when calculating the average treatment effect on the treated (ATT).[17] In particular, the self-assessed health status of those that have become unemployed, i.e. the treatment group, is compared with those employed that have a similar propensity to be in the pool of unemployed persons, but are not currently in that pool, i.e. the control group. Assuming that the health status is a cardinal outcome measure, we then calculate the average treatment effect on the treated.

An alternative measure of health impact, which takes better account of the ordinal nature of self-assessed health, is constructed using the probabilities of different levels of health. Using the same set of individuals as in the probit model, we estimate an ordered probit model for health, using age, age squared, gender, educational levels, and lagged health as the explanatory variables. Using the estimates, we calculate the probabilities of all five health levels for each individual. Using these probabilities we obtain the expected health score $E(Health) = \sum_{j=1}^{5} j \Pr(Health = j)$. This is then used as the outcome in propensity score matching.[18]

The above measures essentially treat the data as separate cross-sections and compare the health status of the treated and controls in each year. As an alternative, we utilize the panel aspect and use changes in health scores or expected health scores as the outcome measures, i.e. we use difference-in-differences matching (e.g. Blundell and Costa Dias, 2000).

The first column of Table IV shows the estimates of the probit model for becoming unemployed. As expected, higher education decreases the probability of becoming unemployed, other things being equal, and employees in small firms are more likely to become unemployed. The age effect is U-shaped with young and old employees more likely to become unemployed; the minimum is at the age of 43. Public sector employees are more likely to face unemployment, which may be related to a large share of temporary employees in this sector. In addition, lagged self-assessed health in the previous year has a negative and significant coefficient when explaining the probability of becoming unemployed in the current year. The propensity score matching is performed using the region of common support for the propensity scores, which included 405 cases of a person becoming unemployed (none off support) and 11006 control cases.[19] Figure 5 plots the distributions of the propensity scores before matching. The figure shows that for the controls the probability of becoming unemployed tends to be smaller. To check the validity of the matching, covariate balancing is tested. The results are shown in Table V. For all the variables the matching succeeds in making the means of the covariates close to each other for the treated and controls.[20]

=== FIGURE 5 HERE ===

=== TABLES IV-V HERE ===

Table VI reports the estimated treatment effects on the treated, with standard errors computed by bootstrapping. When nearest-neighbour matching with replacement is used, the average treatment effect of becoming unemployed on self-assessed health is small and not statistically significant. The same result is obtained when expected health is the outcome. As a robustness check, nearest-neighbour matching was also conducted for each year separately. Although the results slightly varied over time, all of the estimated ATTs were non-significant. (The results are not reported in the table.) To check the robustness of the result further, kernel matching is also used. Now there is a significant effect, when expected health is the outcome. Unemployment causes a somewhat lower expected health level according to kernel matching.

=== TABLE VI HERE ===

However, a closer way to compare the matching results with those obtained with simple parametric regression methods (difference-in-differences) is to use the change in health as the outcome. Now both nearest-neighbour and kernel matching give the same conclusion: changes in health or expected health are not statistically significantly different in the treatment group and control group.[21] Taken together, the results based on matching confirm our earlier conclusions that the experience of unemployment as such does not have an independent influence on the self-assessed level of health, but those persons with a low perception of their health are more likely to become unemployed in the first place. In fact, it is likely that using the health level as the outcome picks up the health difference, even when lagged health is used as a variable in the estimation of the propensity scores.

A corresponding matching analysis is done for the treatment of becoming employed. In this case the data set is restricted in each year pair to those who are unemployed in both the current and past periods and those who become employed in the current period. The second column of Table IV shows the estimates from the probit models for becoming employed. Age has an inverted U-shaped relationship with the probability of becoming employed, with maximum at the age of 35, and higher education increases the employment probability. In addition, self-assessed health in the previous year has a positive and significant coefficient. Note that because all the individuals in this analysis are unemployed in t-1, information about the employer is not available. The propensity score matching is performed using the region of common support for the scores, which includes 542 treated (18 off support) and 793 controls. Figure 6 shows the distribution of the propensity scores for the treated and controls. The treatment group tends to have a higher probability of becoming employed, which is understandable, since the control group also includes those who are unemployed for the whole data period 1996-2001. To check the validity of the matching, covariate balancing is tested. According to Table VII, the matching again succeeds in making the distributions of the covariates similar.[22]

=== FIGURE 6 HERE ===

=== TABLE VII HERE ===

Table VI reports the results. When nearest-neighbour matching is used, the average treatment effect of becoming employed is 0.061, which is statistically significant at the 10 per cent level. The ATTs with expected health as the outcome or the ones from kernel matching are not significant.[23] When we use change in health as the outcome, we again obtain the result that becoming employed does not improve health in a statistically significant way. This is consistent with our findings using difference-in-differences models. As a robustness check, we re-run the nearest-neighbour matching analyses for each year separately. (The results are not reported in the table.) The conclusions were, otherwise, similar to those obtained with the pooled data, but with health as the outcome

ATT was 0.360 and significant at the 1 per cent level (t-value 8.55) in 2000, driving the result for the pooled data.

Effects of long-term unemployment

In the above analysis we have treated all kinds of unemployment in the same way. Now we examine whether the definition of "experiencing unemployment" matters for the robustness of the basic results. In this case, we define long-term unemployment as the relevant measure of unemployment experience.[24] The long-term unemployed are those persons who have been unemployed continuously at least for six months. Because selection by observable characteristics such as education is arguably more important in the case of long-term unemployment than for overall unemployment, we focus on the results that stem by using matching methods.[25]

We drop the short-term unemployed from matching. Hence, we compare the health level of the long-term unemployed with the health of those who are continuously employed. The treatment is in this case being long-term unemployed conditionally on having been employed in the previous period, and the outcome variable is alternatively health, expected health, or changes in them. The probit models for becoming long-term unemployed contain the same explanatory variables as earlier for all unemployed. As expected, persons with a low perception of their health are more likely to become long-term unemployed. A corresponding analysis is done for the treatment of becoming employed after experiencing long-term unemployment.

Table VIII summarizes the results. When nearest-neighbour matching is used with the health level as the outcome variable, the average treatment effect of becoming long-term unemployed is -0.16. The effect is statistically significant at the 10 per cent level. Accordingly, there is some evidence that becoming long-term unemployed leads to a deterioration in self-assessed health. However, the results differ by using different measures of health, i.e. health vs. expected health and health level vs. change, and between different matching methods, nearest-neighbour vs. kernel matching. Additionally, becoming employed after long-term unemployment does improve self-assessed health in a statistically significant way when using nearest-neighbour matching, but not when using kernel matching. The result on health is weaker when the panel dimension of the data is taken into account in difference-in-differences matching.

=== TABLE VIII HERE ===

CONCLUSIONS

We have explored the relationship between unemployment and self-assessed health. Our results show that the event of unemployment does not matter as such for the level of self-assessed health, when evaluated in a panel data setting, since the health status of those who end up being unemployed is already lower than that of the continually employed before their unemployment episodes actually start. Importantly, the matching results are similar to those obtained with simple parametric regression methods (difference-in-differences models) when the change in health is used as the outcome. Hence, persons who have poor self-assessed health, for some reason or another, are being selected for the pool of the unemployed. This explains why, in a cross-section, unemployment is associated with poor self-assessed health. Accordingly, unemployment may merely be a veil that hides the underlying causes of poor self-assessed health. Furthermore, we discover that the definition of "experiencing unemployment" matters somewhat for the findings. In particular, we are more likely to obtain negative effects of unemployment on health when we use long-term unemployment as the relevant measure of unemployment experience.

Our basic finding, according to which unemployment does not appear to have a significant negative effect on self-assessed health, is consistent with the results reported by Browning *et al.* (2006). They discover that being displaced does not cause hospitalization for stress-related disease in Denmark.[26] Our results are also consistent with those by Lucas *et al.* (2004) for unemployment and life satisfaction. They show that individuals tend to shift back towards their baseline levels of life satisfaction after unemployment has lasted for some time. The pattern demonstrates that the unemployed become mentally accustomed to their situation rather quickly. This may arise, because unemployment has fewer stigma effects in the presence of high aggregate unemployment. From the policy perspective, the findings of this paper imply that the allocation of resources to improve the health status of those that are currently unemployed is not enough. It is equally important to put resources into the improvement of health of those persons currently employed, but who are more likely to experience unemployment at some point of time.

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Table I. Distribution of the level of self-assessed health

Health	Employed	Unemployed	Long-term unemployed	Total population
1	32	18	11	50
	(0.18)	(0.66)	(0.90)	(0.24)
2	339	143	83	482
	(1.89)	(5.28)	(6.83)	(2.34)
3	3779	771	439	4550
	(21.12)	(28.46)	(36.10)	(22.09)
4	9807	1296	522	11103
	(54.92)	(47.84)	(42.93)	(53.90)
5	3933	481	162	4414
	(21.98)	(17.76)	(13.24)	(21.43)
Total	17890	2709	1216	20599
	(100)	(100)	(100)	(100)

Note: Percentage shares of column totals in parentheses. Long-term unemployed are those who have been unemployed continuously at least for six months.

Table II. Effect of labour market status on self-assessed health

	OLS	OLS	Fixed effects	Fixed effects	Ordered logit	Ordered logit	FE ordered logit	FE ordered logit	RE ordered probit
Becomes unemployed at least once	-0.207 (0.026)***	-0.097 (0.024)***			-0.541 (0.067)***	-0.273 (0.067)***			-0.228 (0.080)***
Unemployed after employment	-0.028 (0.040)	0.028 (0.037)	0.032 (0.029)	0.032 (0.029)	-0.076 (0.104)	0.067 (0.105)	0.198 (0.139)	0.199 (0.139)	0.073 (0.068)
Becomes employed at least once	0.095 (0.025)***	0.009 (0.029)			0.262 (0.065)***	0.032 (0.066)			0.012 (0.072)
Employed after unemployment	-0.030 (0.031)	-0.018 (0.029)	-0.009 (0.024)	-0.008 (0.024)	-0.098 (0.083)	-0.064 (0.083)	0.010 (0.115)	0.011 (0.116)	-0.031 (0.059)
Always unemployed	-0.457 (0.028)***	-0.263 (0.027)***			-1.150 (0.071)***	-0.694 (0.073)***			-0.558 (0.093)***
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	No	Yes	No	Yes	No	Yes	No	Yes	Yes
N	19206	19206	19206	19206	19206	19206	12891	12891	19206

Note: Robust standard errors in parentheses (except in column 9). Significance: *** 1 %, ** 5 %, * 10 %. Reference group: continuously employed. Unreported control variables include age and its square, gender and the level of education in three categories. In the fixed effects models gender is excluded. In FE ordered logit age is also excluded. In the RE model, individual means of control variables are also included.

Table III. Effect of labour market status and past health on self-assessed health

	OLS	OLS	First difference IV	First difference IV	Ordered logit	Ordered logit	RE ordered probit
Becomes unemployed at least once	-0.084 (0.025)***	-0.047 (0.024)*			-0.264 (0.084)***	-0.134 (0.085)	-0.084 (0.064)
Unemployed after employment	-0.004 (0.036)	0.020 (0.036)	0.056 (0.044)	0.032 (0.027)	-0.021 (0.126)	0.051 (0.127)	0.026 (0.074)
Becomes employed at least once	0.066 (0.026)***	0.033 (0.026)			0.232 (0.093)***	0.118 (0.093)	0.078 (0.065
Employed after unemployment	-0.044 (0.031)	-0.038 (0.030)	0.065 (0.042)	-0.008 (0.024)	-0.164 (0.108)	-0.147 (0.108)	-0.081 (0.068)
Always unemployed	-0.188 (0.028)***	-0.120 (0.028)***			-0.615 (0.092)***	-0.401 (0.095)***	-0.234 (0.070)***
Health(t-1)	0.588 (0.007)***	0.535 (0.008)***	-0.437 (0.011)***	-0.437 (0.011)***			
Health(t-1) = 2					0.406 (0.569)	0.437 (0.588)	0.195 (0.245)
Health(t-1) = 3					2.354 (0.557)***	2.322 (0.576)***	0.661 (0.240)***
Health(t-1) = 4					4.613 (0.558)***	4.400 (0.577)***	1.219 (0.244)***
Health(t-1) = 5					6.536 (0.560)***	6.199 (0.579)***	1.631 (0.250)***
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control variables	No	Yes	No	Yes	No	Yes	Yes
N	14524	14524	6447	6447	14524	14524	14524

Note: Robust standard errors in parentheses (except in columns 3-4 and 7). Significance: *** 1 %, ** 5 %, * 10 %. Reference group: continuously employed. Unreported control variables include age and its square, gender and the level of education in three categories. In the differenced models gender is excluded. In the RE model, individual means of control variables and starting values of lagged health categories are also included.

Table IV. Probit models for change in labour market status

	Becomes unemployed	Becomes employed
Age	-0.074	0.158
	(0.016)***	(0.025)***
Age squared	0.001	-0.002
	(0.0002)***	(0.0003)***
Middle education (t-1)	-0.186	0.196
	(0.057)***	(0.087)***
High education (t-1)	-0.507	0.438
	(0.065)***	(0.109)***
Small firm (t-1)	0.408	
	(0.046)***	
Public sector (t-1)	0.080	
	(0.048)*	
Health (t-1)	-0.062	0.178
	(0.034)**	(0.051)***
Year dummies	Yes	Yes
N	11411	1353

Note: Standard errors in parentheses. Significance: *** 1 %, ** 5 %, * 10 %.

Table V. Test of covariate balancing for becoming unemployed

Variable	Sample	Mean treated	Mean control	% bias	% reduction of bias	t-test
Age	Unmatched	42.541	42.306	2.2		0.47
	Matched	42.541	42.849	-2.9	-31.6	-0.38
Age squared	Unmatched	1938.2	1886.1	5.9		1.24
	Matched	1938.2	1968.9	-3.5	40.8	-0.46
Middle education (t-1)	Unmatched	0.467	0.393	14.8		2.96***
	Matched	0.467	0.481	-3.0	79.8	-0.42
High education (t-1)	Unmatched	0.217	0.434	-47.4		-8.67***
	Matched	0.217	0.207	2.2	95.4	0.34
Small firm (t-1)	Unmatched	0.588	0.351	48.8		9.79***
	Matched	0.588	0.585	0.5	99.0	0.07
Public sector (t-1)	Unmatched	0.427	0.449	-4.3		-0.85
	Matched	0.427	0.373	10.9	-153.7	1.58
Health (t-1)	Unmatched	3.842	3.965	-16.9		-3.40***
	Matched	3.842	3.857	-2.0	88.0	-0.29

Note: Year dummies not reported. Significance: *** 1 %, ** 5 %, * 10 %.

Table VI. Average treatment effect on the treated for overall unemployment

Treatment	Matching method	Outcome			
		Health	E(health)	Change in health	Change in E(health)
Becomes unemployed	Nearest-neighbour	0.006 (0.046)	-0.008 (0.018)	0.020 (0.045)	0.015 (0.030)
	Kernel	-0.061 (0.047)	-0.059 (0.017)**	0.014 (0.044)	-0.001 (0.030)
Becomes employed	Nearest-neighbour	0.061 (0.032)*	0.004 (0.008)	0.037 (0.034)	-0.003 (0.019)
	Kernel	0.030 (0.031)	0.009 (0.007)	0.008 (0.032)	0.009 (0.021)

Note: Bootstrap standard errors (150 replications) in parentheses. Significance: *** 1 %, ** 5 %, * 10 %.

Table VII. Test of covariate balancing for becoming employed

Variable	Sample	Mean treated	Mean control	% bias	% reduction of bias	t-test
Age	Unmatched	37.929	44.889	-60.9		-10.89***
	Matched	38.094	37.738	3.1	94.9	0.56
Age squared	Unmatched	1549.4	2165.2	-66.1		-11.72***
	Matched	1564.5	1529.3	3.8	94.3	0.72
Middle education (t-1)	Unmatched	0.504	0.393	22.3		4.04***
	Matched	0.517	0.526	-1.9	91.6	-0.30
High education (t-1)	Unmatched	0.239	0.137	26.2		4.84***
	Matched	0.218	0.216	0.5	98.2	0.07
Health (t-1)	Unmatched	4.016	3.653	45.8		8.19***
	Matched	3.993	3.969	3.0	93.4	0.52

Note: Year dummies not reported. Significance: *** 1 %, ** 5 %, * 10 %.

Table VIII. Average treatment effect on the treated for long-term unemployment.

Treatment	Matching method	Outcome			
		Health	E(health)	Change in health	Change in E(health)
Becomes long-term unemployed	Nearest neighbour	-0.156 (0.081)*	-0.034 (0.031)	-0.099 (0.080)	0.105 (0.055)*
	Kernel	-0.273 (0.085)**	-0.154 (0.030)**	-0.084 (0.088)	0.028 (0.055)
Becomes employed after long-term unemployment	Nearest neighbour	0.154 (0.054)**	0.025 (0.014)*	0.109 (0.057)*	-0.039 (0.036)
	Kernel	0.057 (0.055)	-0.011 (0.013)	0.074 (0.055)	0.000 (0.032)

Note: Bootstrap standard errors (150 replications) in parentheses. Significance: *** 1 %, ** 5 %, * 10 %.

Figure 1. Average level of self-assessed health

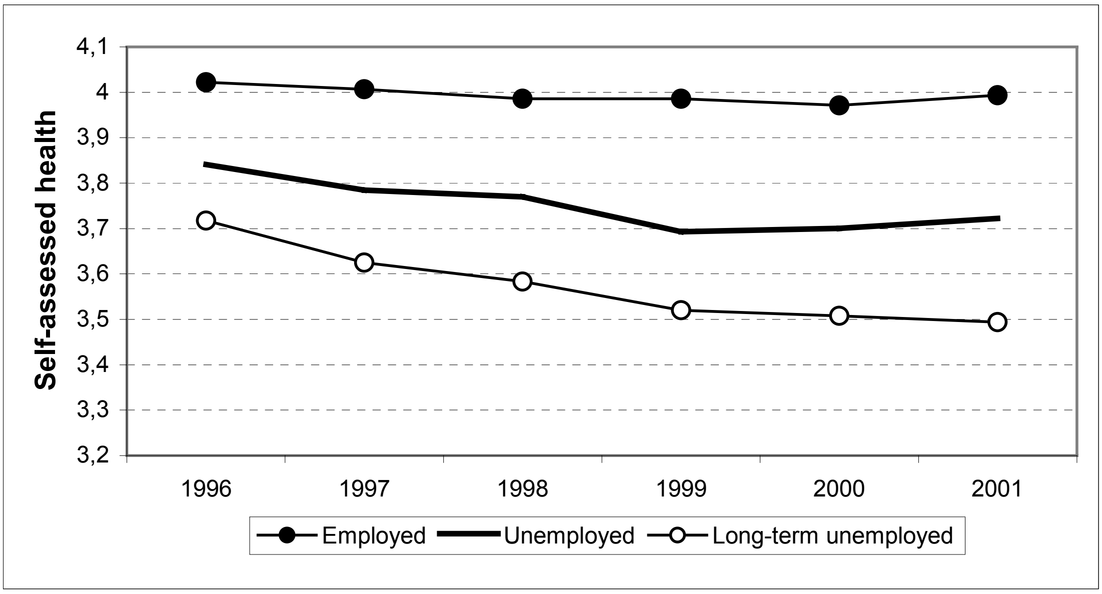


Figure 2. Galton squeeze diagram for those becoming unemployed

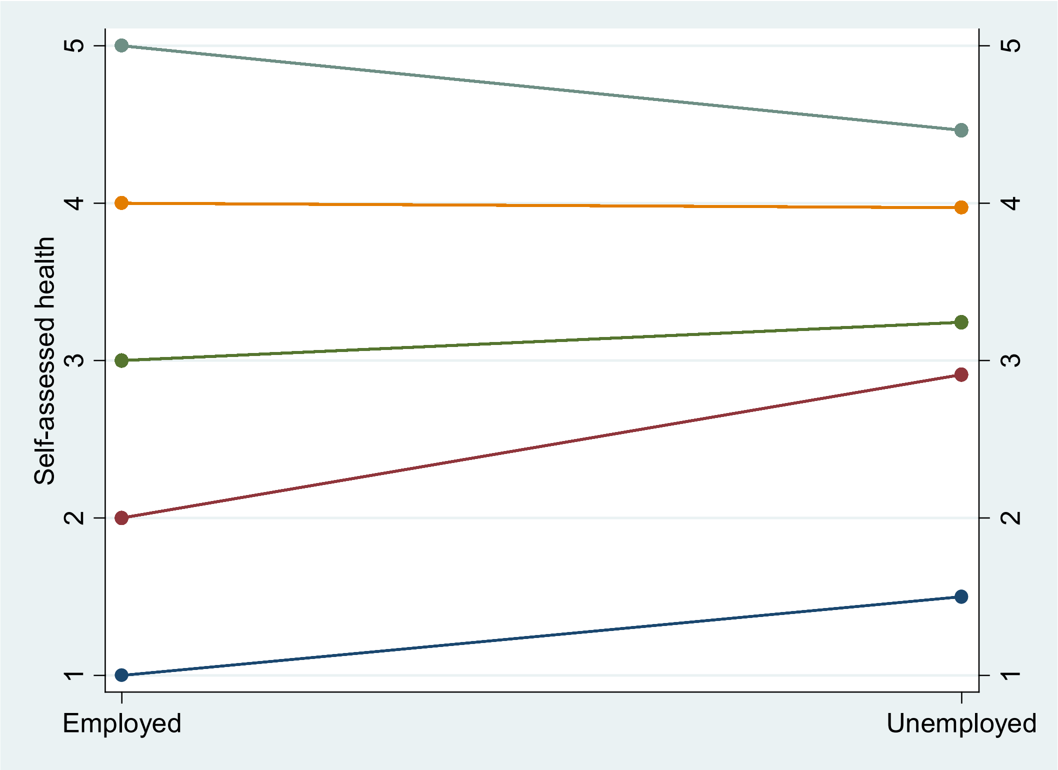


Figure 3. Galton squeeze diagram for those employed in two consecutive periods

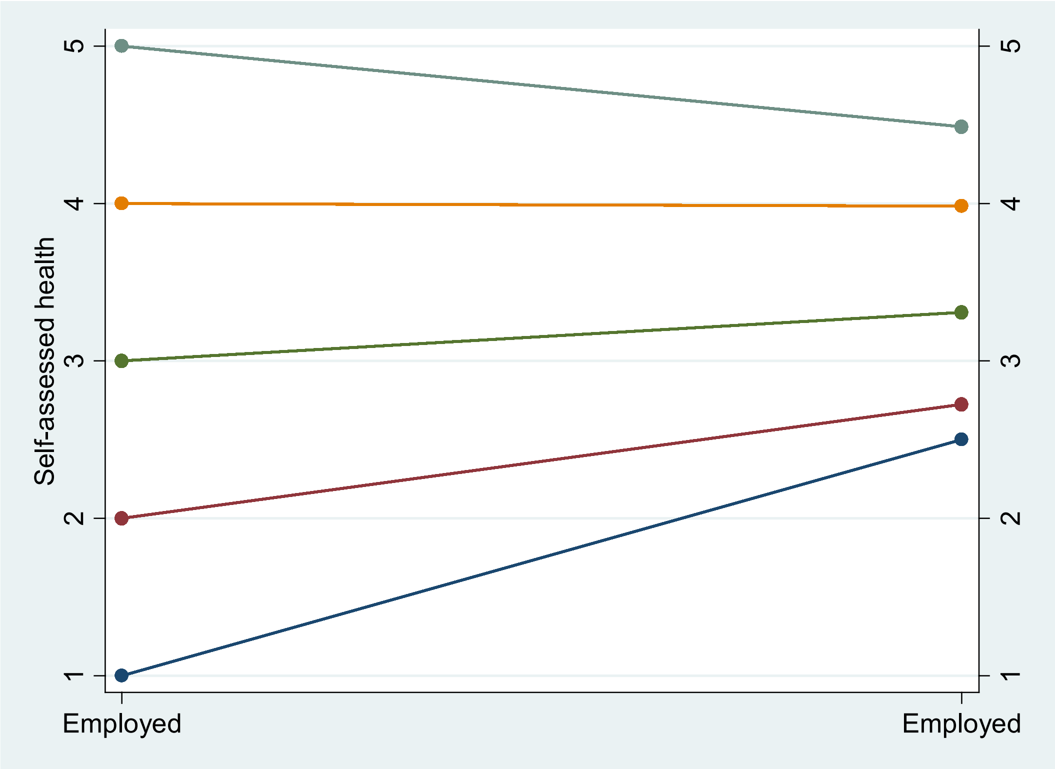


Figure 4. Galton squeeze diagram for those becoming employed

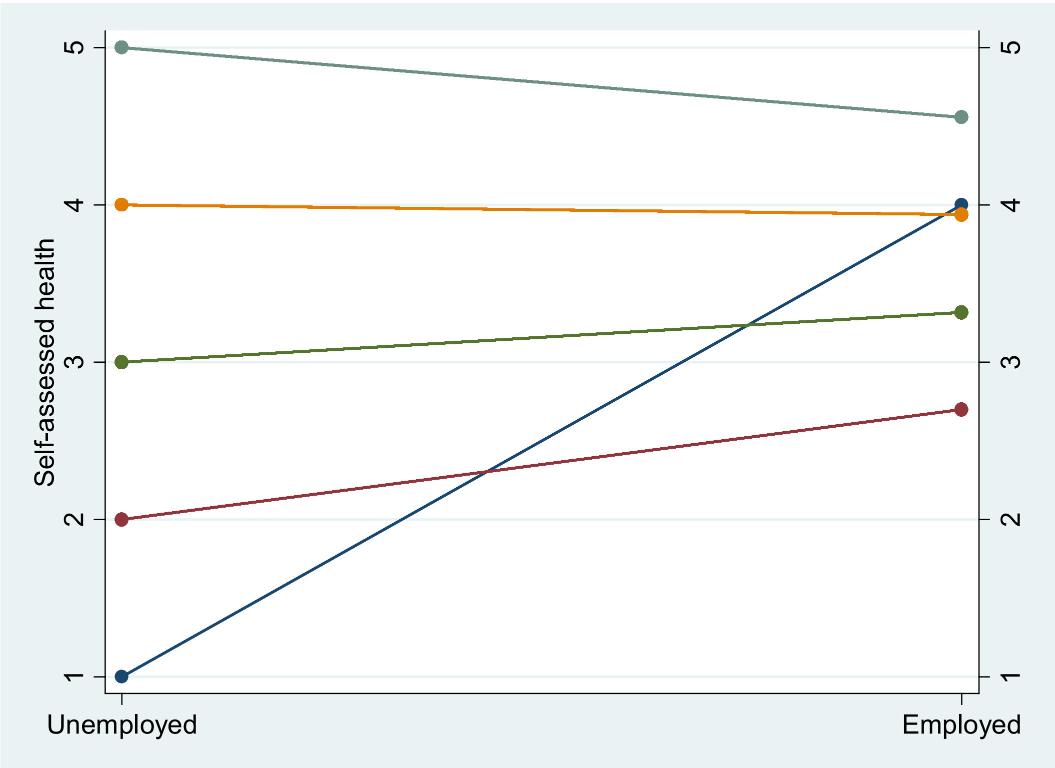


Figure 5. Distribution of propensity scores of becoming unemployed in the region of common support

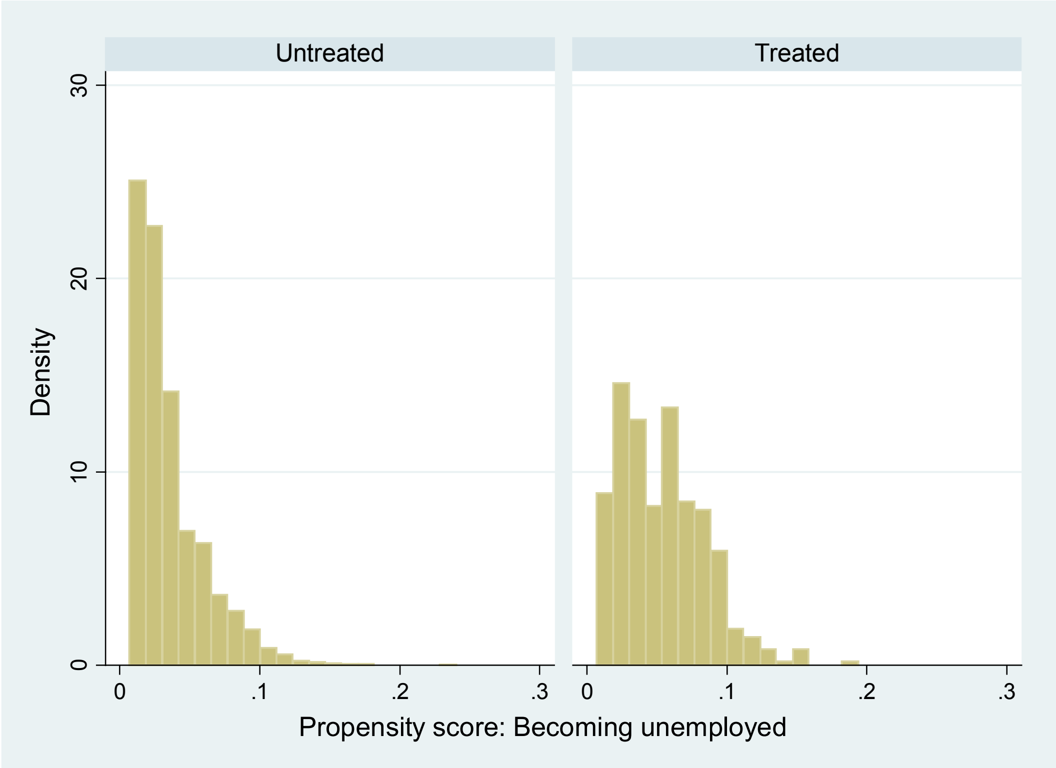
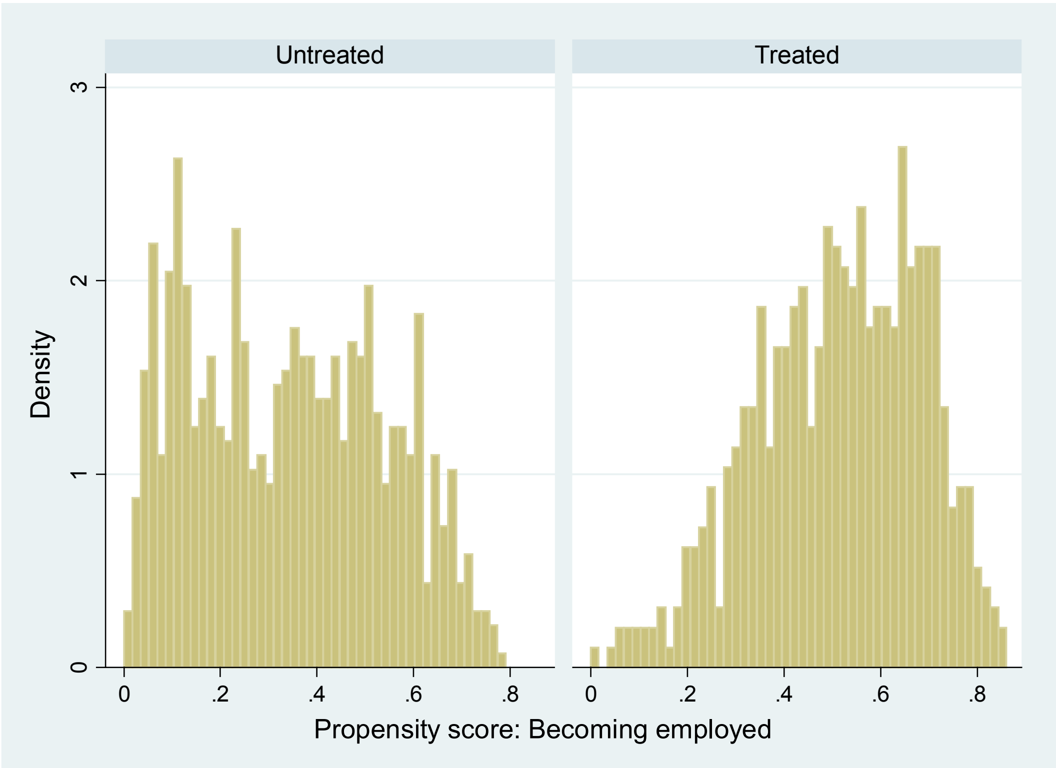


Figure 6. Distribution of propensity scores of becoming employed in the region of common support



Notes

[1] For example, Björklund (1985) provides an early application that uses panel data to study the effect on unemployment on mental health.

[2] McFadden *et al.* (2005) note that one problem with the questions on self-assessed health is that different respondents can interpret the response scales differently. This may be a particular problem in cross-country comparisons of self-assessed health status when using data sources such as the ECHP. In our case, that problem is less severe, because we use information from the ECHP only for one country.

[3] Koskela and Uusitalo (2006) provide an overview of the Finnish unemployment problem. The average rate of unemployment in Finland was around 5% in the 1980s before the recession. After the recession the unemployment rate declined from 14.6% to 9.1% over the period 1996-2001. Therefore, the unemployment problem has been historically exceptional over our whole data period 1996-2001 and it is difficult to segment the period to examine the effects of becoming unemployed separately in periods of low and high aggregate unemployment rate.

[4] There are earlier Finnish studies on the health aspects of unemployment. The results based on the simple comparison of population averages are, for the most part, mixed (e.g. Martikainen and Valkonen, 1996; Lahelma *et al.*, 1997). Jäntti *et al.* (2000) discover, that regional unemployment has not been associated with mortality among Finns. The studies that use panel data are based on restricted samples covering special groups of workers (Lahelma, 1989; Leino-Arjas *et al.,* 1999; Nyman, 2002). This makes it rather difficult to generalize the results obtained. Martikainen *et al.* (2007) report that workplace downsizing and workplace closures increase mortality among the affected workers, but the effects are modest in the context of high unemployment or rapid downsizing. Unemployment does not have a significant negative effect on happiness (conditional on income) based on separate cross-sections in Finland (Böckerman and Ilmakunnas, 2006; Ervasti and Venetoklis, 2006). All in all, the causal relationship between unemployment and health remains largely unsolved.

[5] Crossley and Kennedy (2002) show that the response reliability of self-assessed health is related to age, income and occupation. In total, around 28% of those that respond twice to the same health question changed their response on self-assessed health.

[6] Finland was included in the ECHP for the first time in 1996 after she joined the European Union. The European Union stopped gathering the ECHP in 2001, which means that we have six waves of the data.

[7] For instance, in the first wave of the ECHP from the year 1996, ”top-coding” means that it is not possible to observe unemployment durations of over 12 months.

[8] The same conclusion would be reached from a reverse Galton squeeze diagram where the end-point of each line is the final health level of some unemployed and the starting point is their average health while they were still employed.

[9] There are only two persons who were initially at health level one. After becoming employed both of them were at level 4.

[10] See e.g. Henning *et al.* (2003) for a discussion on this issue in another context.

[11] For instance, it is a well-known fact that better educated persons are usually healthier both by subjective and objective measures (see Martikainen, 1995, for evidence from Finland). The three education categories in the ECHP are third level education (ISCED 5-7), second stage of secondary level education (ISCED 3) and less than second stage of secondary level education (ISCED 0-2).

[12] The problem with this approach is that if self-assessed health contains a lot of measurement error as argued by Crossley and Kennedy (2002), taking the within transformation of the data may worsen the signal-to-noise ratio substantially.

[13] As a robustness check of the results, we estimated the models (1) and (2), but proxied the discrete health scores with a continuous variable that is based on the observed shares of the scores (following Terza, 1987). The estimates using the converted scores (not reported in tables) were quite similar to the ones obtained treating the scores directly as cardinal measures of health.

[14] Due to poor convergence of the estimates, the age variable is left out of the estimation with control variables.

[15] The correlation between change in the health score and initial health can be positive only if the variance of the health scores increases over time (Campbell and Kenny, 1999).

[16] See e.g. Blundell and Costa Dias (2000), Caliendo and Kopeinig (2007), and Lee (2005) for surveys of the matching methods.

[17] In our analysis we use the programs written by Becker and Ichino (2002), and Leuven and Sianesi (2006) for Stata.

[18] We have to treat the results with this outcome measure with some caution, since the basic assumptions of propensity score matching need not carry over to this kind of nonlinear outcomes. For the probability of a binary outcome one could use the adjustment based in inverse probability functions, suggested by Blundell *et al.* (2004). This is not possible in our case, as the expected health involves several probabilities.

[19] We follow the definition of common support in the Leuven-Sienesi program, where this is defined to include all controls and those treated whose propensity score is below the maximum or above the minimum propensity

score of the controls. In the Becker-Ichino program the common support option keeps all treated and those controls with a propensity score below the maximum or above the minimum of that of the treated. In practice, there are small differences in the results, but the significance of the estimated ATTs is not affected.

[20] If the data are divided into 7 blocks (based on an algorithm; see Becker and Ichino, 2002), the balancing property holds in all of the blocks.

[21] When change in expected health is used as the outcome, the number of observations in matching is smaller than in the other cases. In the estimation of the ordered probit models for health we use lagged health as an explanatory variable, so we lose one year and in differencing we lose another year. We have 287 treated and 7773 controls in the region of common support (no treated off support).

[22] When the data are divided to 6 blocks, the balancing holds in all other cases, except for the age variable in the first block (lowest end of the distribution of propensity scores). If the squared age is left out, the balancing property holds. We have kept squared age in the model, but examined the sensitivity of the results to its exclusion. This has practically no effect on the estimated treatment effects.

[23] Again, the number of observations is slightly smaller when we have change in expected health as the outcome. There are 353 treated (17 off support) and 413 controls on common support.

[24] For example, Gordo (2006) discovers, using the GSOEP data with random effects models, that being unemployed for a long period of time has a significant negative effect on health satisfaction while short-term unemployment does not always cause negative effects.

[25] We have made some experiments with parametric models by using long-term unemployment as the relevant measure of unemployment. The results that stem from parametric models are somewhat mixed.

[26] They use a random 10% sample of the male population of Denmark for the years 1981-1999.