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Job Displacement and Health Outcomes*

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Abstract

We investigate whether job loss as the result of displacement causes ill health. In doing this we use much better data than any previous investigators. Our data are a random 10% sample of the adult population of Denmark for the years 1981-1999. For this large representative panel we have very full records on demographics, health and work status for each person throughout the data period. As well as this we can link every person to a firm (if they are working) and can identify all workers who are displaced in any year, using a variety of definitions of displacement. We focus on one very precise health outcome, hospitalisation for stress related disease, since this is a grave condition and is widely thought to be likely to be associated with unemployment. We use the method of ‘matching on observables’ to estimate the counter-factual of what would have happened to the health of a particular group of displaced workers if they had not in fact been displaced. Our results indicate unequivocally that being displaced in Denmark does *not* cause hospitalisation for stress related disease. An analysis of the power of our test suggests that even though we are looking for a relatively rare outcome, our data set is large enough to show even quite small an effect if there were any. Supplementary analyses do not show any causal link from displacement or unemployment to our health outcomes for particular groups that might be thought to be more susceptible.

Keywords: Unemployment, Job Displacement, Health, Matching on observables

JEL-Code: C23, I18, J21

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1. Introduction

It is well established that unemployed people tend to be less healthy than employed workers of similar age (see, for example, the survey paper by Kasl and Jones (2000)). The determinants of this correlation are a matter of considerable debate. Clearly it could be that less healthy people are more likely to become unemployed (Lindblom, Burström and Diderichsen (2001)) or that, having become unemployed, they have longer spells of unemployment (Stewart (2001)). Conversely, it may be that unemployment causes poor physical or mental health. The link from unemployment to health may also be important for other outcomes. For example, findings that job displacement and subsequent unemployment leads to lower future earnings and/or lower future employment (see Kletzer (1998) for references) may be partly attributable to displacement leading to ill health which in turn leads to these deleterious outcomes.

In seeking to establish whether unemployment causes ill health, many investigators have used firm or plant closure as a quasi-experiment (see Morris and Cook (1991) for a review of ten such longitudinal studies⁴ from five different countries). If firm closure is not connected with the health of workers in the firm then we can compare health outcomes for workers who are displaced with health outcomes of workers who are not displaced. A weaker form of this approach is to look at job displacements; that is, permanent separations of workers from firms that are the result of demand conditions and that affect a significant proportion of the workforce in the firm. There are, however, problems with the use of plant closures or displacements in this context. These include:

1. To date, longitudinal studies have been case studies so the workers involved may not be representative. Moreover, the plant studied is often an important local employer so that the experience of the displaced workers is not ‘typical’.
2. In most studies only small numbers of workers are involved. This is particularly problematic

⁴ In this paper we use panel data and we consequently restrict attention to longitudinal studies in our literature review. There is a huge literature using other data sources.

if the health outcome studied is relatively rare. Since most studies do not find a significant impact of displacement on health, the concern is that this is due to the low power of the test.

3. There is sometimes significant attrition in following workers after the closure. This attrition may be associated with health.
4. It is difficult to find a 'control' group.
5. It is usually impossible to adequately control for pre-closure health status and other factors from before the plant started to fail.
6. Health outcomes are different across studies and are sometimes difficult to interpret (for example, a self-reported measure of 'having more ailments'). Moreover, some of the health outcomes used are not very serious in their nature.
7. Displacement does not necessarily lead to unemployment for the workers involved. This may dilute any effect (since we are mixing workers who become unemployed and those who do not). This may also be confounding of the quasi-experimental effect if less healthy workers are more or less likely to be displaced in a firm that displaces but does not close. Additionally, any effects from plant closure to health that are seen may also include the stress of adjusting to a new job for workers who do not experience an unemployment spell.
8. Firm closure is often anticipated well in advance of the actual event. The workers remaining at a firm when it closes may not have the same health status as workers who were in the firm when it first began to experience problems.

In their review of the results from a number of studies, Morris and Cook (1991) find that the "conclusions that can be drawn from the health effects of factory closure are limited"; that is, there is not much conclusive evidence, one way or the other, of a link from plant closure to health outcomes. Thus the issue of whether plant closure or displacement leads to negative health outcomes is still open. The lack of strong evidence may simply reflect the small numbers involved in these studies or the deficiencies of the health measure used or that the particular

workers studied are the segment of the population that is not at risk. Conversely, some of the items above suggest that any ‘significant’ positive findings may not reflect average effects.

In this paper we re-examine the effect of displacement on physical health. In doing this we use much better data than any previous investigators; indeed, we have data that is close to the ‘ideal study design’ that Morris and Cook (1991) specify in their conclusions. Our data are based on a random 10% sample of the adult population of Denmark (giving more than 400,000 people in all) who we can follow from 1981 through until 1999. Thus we have a large and representative sample that does not suffer from attrition (except through death or emigration). The data give information on demographics, income, employment and a wide variety of other personal characteristics. In particular, we have very full health records for each person throughout the data period. As well as this, we can link every person to a firm (if they are working) and we have a great deal of information about these individual firms. Thus we can identify all workers in our sample who are displaced in the data period, using a variety of definitions of displacement. Given our long panel, we can examine post-displacement health outcomes for a number of years, controlling for pre-displacement factors.

In our empirical work below we discuss a number of different definitions of displacement, ranging from separating from a plant that lays off 30% of its workers to being in a firm that closes. We choose to focus on one particular health outcome: hospitalization for diagnoses related to diseases of the circulatory system and diseases of the digestive system (such as high blood pressure, heart diseases, gastric catarrh, gastric ulcers, etc). This is taken because of its seriousness and also because there are well attested links from stress and depression to these diseases (see, for instance, Brunner and Marmot (1999), Brunner (2002), and Stansfeld and Fuhrer (2002)).

We consider two particular outcomes associated with this: being hospitalized in the four years after the displacement and the duration to the first entry into hospital observable in our data, with

due account of left and right censoring. The duration analysis is potentially important in distinguishing incidence from timing. That is, there may be different susceptibilities for stress related diseases across the population and an unpleasant shock such as displacement induces such a disease earlier than it would have happened otherwise. Then the shock has a medium run effect but no long run effect. This would not be apparent from the analysis using the 'four years after' dummy.

When using our data to address the causal link between displacement and health we cannot simply compare displaced workers with workers who are not displaced. This is because the 'selection' into being displaced is very likely to be correlated with health status. To see why, consider two cases. First, displaced workers are typically younger than workers in firms that do not displace. Being younger they are at less risk of hospitalisation for stress related conditions. This would lead to a spurious negative correlation between displacement and ill health. Conversely, more educated workers are less likely to be displaced and they have better health. Ignoring this would lead us to over-state any (positive) correlation between displacement and ill health. To overcome this we adopt an empirical strategy of 'matching on observables'. For this approach, we consider the counter-factual of what would have happened to the health of our displaced workers if they had not in fact been displaced. To construct this we match each displaced worker with a non-displaced worker who has the same age and probability of being displaced. The former is allowed to depend on individual characteristics such as previous observed health, gender, age, education, etc. The identifying assumption is that the conditional expected health outcome is the same for both the displaced workers and their match, given that neither experiences a displacement. Notice that this assumption is much weaker than independence from a variable such as 'separation from a job' or 'being unemployed' since these may be due to (unobserved) health problems. In our empirical analysis we consider men and women separately. We also conduct our analysis for displacements in each year separately and

then we pool the data, taking account of time specific effects.

In section 2 we discuss the econometric methods used in the paper. We also present an analysis of the power of our tests to demonstrate that any finding that there is no effect is unlikely to be due to low power for our tests. Section 3 describes the data set and the identification of displacement and control groups. Section 4 presents the estimation results and section 5 contains conclusions.

The results from our empirical work are unequivocal: we do *not* find any effect of displacement on a serious stress related health outcome. Thus, estimating the average treatment effect on the treated we find that being displaced had no effect on being hospitalized after the displacement. This result is robust for sub-samples such as those who actually experienced a spell of unemployment following displacement or for older men, who are at greater risk of being hospitalized. Given the size of our sample and the quality of our health data we do not think that this negative finding is due to a lack of power; indeed we find negative effects as often as positive (and always completely insignificant). Thus the results for Denmark are clear. In the conclusion we discuss how applicable this result might be for other countries.

2. Econometric methods

2.1 Treatment effects

The aim of this paper is to analyse if there is a causal effect of displacement on health. Investigating this we use methods which have become standard in the econometric evaluation literature; for a recent comprehensive survey of this rapidly growing literature see Heckman, LaLonde and Smith (1999). Displacement status is denoted by the dummy variable D , taking the value 1 if displaced (treated) and 0 otherwise. Let Y_0 and Y_1 denote the potential health outcomes where 0 denotes non-treatment and 1 treatment. The observed outcome for an individual is $Y = DY_1 + (1-D)Y_0$. The evaluation problem is to find the effect of treatment compared to not being

treated on health outcomes.⁵ The parameter of prime interest is the average treatment effect on the treated:

$$E(Y_1 - Y_0 | D=1) = E(Y_1 | D=1) - E(Y_0 | D=1) \quad (1)$$

The problem is that $E(Y_0 * D=1)$ is unobserved, since an individual can not be both treated and non-treated at the same time. So the causal effect of displacement can not be identified without further assumptions. Since in our data treatment is not randomly assigned we can not assume that $E(Y_0 * D=1) = E(Y_0 * D=0)$. The probability of being displaced may be influenced by characteristics (e.g. age and education, see the introduction) which also influence health outcomes. Conditioning on a vector of covariates X the average treatment effect on the treated is given by

$$E(Y_1 - Y_0 | D = 1, X) = E(Y_1 | D = 1, X) - E(Y_0 | D = 1, X) \quad (2)$$

where X is a vector of characteristics not affected by the treatment.

Different methods have been proposed to identify a causal treatment effect on the treated, i.e. to estimate the counter-factual of what would have happened to the (health) outcomes of a particular group of treated (displaced) individuals if they had not in fact been treated. In this paper we consider ‘matching on observables’.

2.2 Matching on observables and the propensity score

The idea of the ‘matching on observables’ approach is to mimic a random experiment by

⁵ To make causal analysis tractable, we impose the stable-unit-treatment-value assumption (SUTVA), see Rubin (1980), which is a standard assumption in the econometric evaluation literature. SUTVA requires that an individual’s potential outcomes do not depend on the treatment status of other individuals in the population. Thus, cross-effects and general equilibrium effects are excluded.

establishing a control group from the group of untreated individuals so that the control group is as similar as possible to the treatment group with respect to observable characteristics. When the set of observable characteristics are informative enough to capture differences between individuals in terms of potential outcomes, the method of matching can produce unbiased estimates of treatment effects.

To be more precise, the average causal treatment effect may be identified by introducing the conditional independence assumption (CIA) (Rubin, 1977):

$$(Y_0, Y_1) \perp\!\!\!\perp D \mid X \quad (3)$$

where " $\perp\!\!\!\perp$ " denotes independence. This assumption ensures that conditional on the observed X 's, potential non-treatment (and treatment) outcomes are independent of treatment status. For the average treatment effect on the treated, a weaker version of the CIA is sufficient:

$$E(Y_0 \mid D=1, X) = E(Y_0 \mid D=0, X) \quad (4)$$

In our case the assumption implies that conditioning on the observables X , the expected potential health outcome in case of non-displacement is the same for the two groups of displaced and not-displaced workers, respectively. So if assumption (4) holds we can use observed health outcomes of non-displaced workers to measure potential health outcomes for displaced workers had they not been displaced, conditional on the characteristics X .

To ensure common support, i.e. that there are both treated and non-treated individuals for each X for which we want to make a comparison, we must assume that

$$0 < P(X) < 1 \tag{5}$$

where $P(X) = \Pr(D = 1 | X)$, which is called the propensity score, denotes the treatment probability given the vector of observed characteristics, X . If the common support assumption is not satisfied for some values of X , i.e. some individuals in the treatment group have characteristics such that $P(X)=1$, one can only estimate the average treatment effect on the treated for the complementary subgroup of the treated, i.e. for the group of treated for which $P(X)<1$.

Given assumption (4) and (5), which are the minimum assumptions needed to be able to interpret a correlation between displacement and the probability of hospitalization as a causal effect of displacement on health, we may estimate treatment effects directly without imposing parametric or functional form assumptions.

Rosenbaum and Rubin (1983) showed that if CIA and (5) are satisfied then the estimation problem simplifies because in that case (4) implies

$$E(Y_0|D=1, P(X)) = E(Y_0|D=0, P(X)) \tag{6}$$

This property is important since it is in practice much easier to condition on the propensity score which is of dimension one when estimating the counterfactual compared to conditioning on a possibly high-dimensional X vector which may also include continuous variables. The problem is of course that the propensity score is not known but has to be estimated, introducing parametric assumptions into the otherwise non-parametric matching method. For matching on an estimated propensity score to be reliable it is essential to check the balancing properties of the estimated score carefully (cf. e.g. Rosenbaum and Rubin, 1985). In this paper we use estimated propensity score functions specified as probit models. We estimate propensity score functions for men and

women separately and for each year in the sample period. Furthermore, some of the estimations are based on matching on both (the linear predictor of) the propensity score and on age (see section 5 for details). The reason why we match exactly on age is that age has a very pronounced (and non-linear) effect on the probability of hospitalization, and it also affects the risk of displacement. If we did not match on age, but only on the propensity score, the age distribution of the treatment group might differ from the age distribution of the matched controls making comparisons of health outcomes problematic.

It is important to note that even though matching is done using estimated parametric propensity scores, the method of matching still has the virtue of not relying on distributional assumptions or functional form restrictions in the outcome equation, and the method does not put any restrictions on heterogeneity of individual treatment effects.

2.3 The choice of matching algorithm

The simplest type of matching is one-to-one matching, where each treated person is matched to that non-treated person who has the closest propensity score. In our case matching is not directly on the propensity score, but on its linear index. The advantage of using the linear index is that it generates better matches in regions where probabilities are very close to zero or one, see Lechner (2000). One-to-one matching may be done with or without replacement. Matching with replacement allows each non-treated person to be matched to more than one person in the treatment group. There is a trade off between matching quality and variance when taking into account the possibility of replacement. Matching without replacement seeks to reduce variance but at the possible cost of increased bias. Since our control groups are very large compared to the treatment groups, matching without replacement is chosen.

Another possibility is to use Kernel regression matching, where every treated person is matched with a weighted average of all non-treated persons. One-to-one matching typically

involves some efficiency loss compared to kernel regression matching since only one non-treated person is used. In contrast Kernel based matching may introduce a larger bias. When we analyse the outcome “duration to hospitalization” we cannot use kernel matching due to the presence of right censoring. For simplicity, we use only one-to-one matching in both the duration analysis and when analysing the other outcome measure (the probability of hospitalization 1-4 years after displacement).

2.4 The power of our test

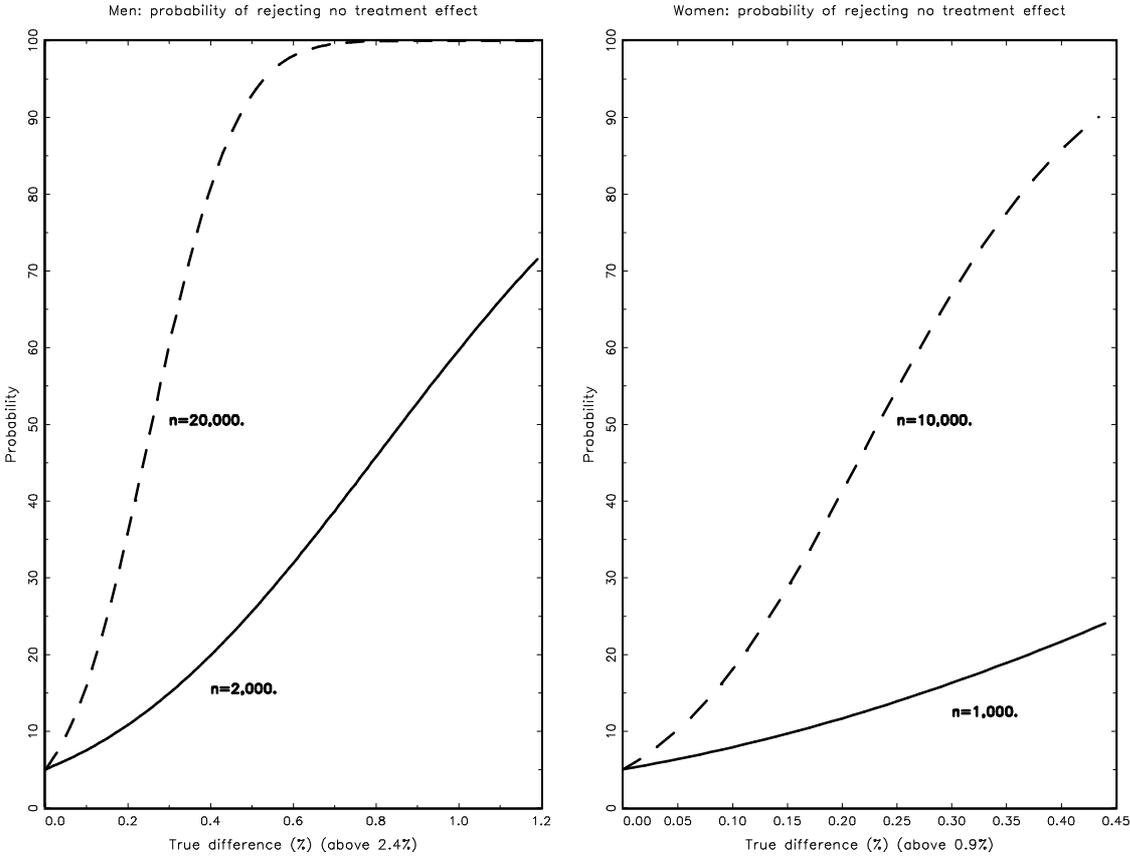
In our analysis below we do not find any ‘significant’ impact of displacement or unemployment on health. Such a negative finding is always open to the possibility that our test lacks power. To consider the power of our test we take a simple example in which each treated person is matched to a control and we compare the mean outcome. Let p be the probability of being hospitalised for a non-displaced person and let $p + d$ be the probability of hospitalisation for a displaced worker. We are interested in testing whether $d > 0$. Denote the sample difference in the two means by Δ . The mean of Δ is d and (using the usual binomial formula and independence between the samples) the variance is given by:

$$\text{var}(\Delta) = v = \frac{p(1-p) + (p+d)(1-p-d)}{n} \quad (7)$$

where n is the number of controls (which equals the number of treated workers). With large sample sizes Δ is approximately normally distributed. Suppose we take a one sided test with a 5% significance level so that we reject the null if and only if Δ divided by \sqrt{v} exceeds 1.64. In this case we have that the power of the test (the probability of rejecting the null) is given by $1 - \Phi(1.64 - d/\sqrt{v})$ where Φ is the standard Normal distribution function.

In our empirical work for men we have values of about 2.4% for p and sample sizes of 2,000 for each year and 20,000 in our pooled sample. For women the corresponding figures are $p = 0.9%$ and $n = 1,000$ for each year. Figure 1 presents the power functions for men and women.

Figure 1. Power functions for men and women



In each case the x-axis gives d (on a scale from zero to $p/2$) and the y-axis presents the probabilities of rejecting the hypothesis of no treatment effect. As can be seen, for men for sample sizes of 2,000 we are quite likely not to reject the null hypothesis ('no effect') even when there is quite a sizable effect. For example, an increase in the hospitalisation propensity from 2.4% to 3% (0.6 on the x-axis) gives a probability of rejecting of only about 30%. On the other hand,

increasing the sample size to 20,000 improves the power considerably and now we would almost always reject the (invalid) null hypothesis of no effect for differences of about one fifth of the control probability (that is, about 0.5%).⁶ We conclude from this that for men our tests on the pooled data do have reasonable power, so long as the effect is not very small. The story is somewhat different for women since the probability of hospitalisation is much lower and we have samples that are only half as big. Consequently the power of our tests for women is rather poor and even for a treatment effect of one fifth of the control probability ($d = 0.18\%$) we are quite likely not to reject, even for the pooled data.

3. Data

3.1 Danish register data

In Denmark all residents have a personal number which is used in a great many transactions such as tax forms, visits to the doctor or hospital, interactions with the welfare system, schooling, work status and registration of residence. This information is collected centrally by Statistics Denmark which then makes these data available for statistical and research purposes. Data are available from 1981 until the present so that we can construct what is effectively a panel census for Denmark for over 20 years. The sample used in this study is based on the Institute of Local Government Studies' longitudinal register which consists of a 10 percent random sample of the Danish adult population and covers the period 1981-1999.

A person who is in the data set in one year will also be in the data set the following year unless he or she died or emigrated. The data set contains information on a large number of demographic,

⁶ An increase in the hospitalization rate of 20% is quite small compared to typical estimates in the medical literature of the effects of life style factors, especially smoking. For instance, Haapanen-Niemi et al. (1999) find that smoking increases the number of hospital days related to cardiovascular disease by 173% for males and 461% for females, and Parish et al. (1995) find that the risk of non-fatal myocardial infarction is almost four times higher for smokers compared to non-smokers at ages 30-59.

educational, income, and labour market variables as well as information about admission to somatic hospitals and frequency of doctor consultations. The data set also contains variables connecting individuals to firms and plants (if they are in work) which we use to identify displacements. The main advantages of these data, as compared to surveys or case studies, are that it is possible to follow a large and representative part of the population over a long period of time; information is registered with very high reliability and that there are no problems of attrition.

3.2 The definition of displacement

We identify displacement and control groups for each base year 1986-1996. By choosing 1986 to be the first base year, we are able to control for previous health status up to five years prior to possible displacement. When defining these groups, the first requirement for a person to be in the displacement or control group of year t is that at the end of year $t-1$ he or she should be employed full-time at a private sector plant with at least 6 employees, and that he or she should be of age 20-63.

In our empirical work we tried using several definitions of displacement. The weakest definition was based on separating from plants that lay off 30% or more of their workers. This criterion resembles criteria used in several papers dealing with effects of displacements on wages and other labour market outcomes, see Jacobson, LaLonde and Sullivan (1993), Kletzer (1998) and Albæk, Audenrode and Browning (2002). The ‘tightest’ definition we used was based on being employed at a plant that closes. We found that choosing different definitions within this span did not change the results in any significant way. Therefore, we only present detailed results based on the weakest definition. We present precise details of the construction of the displaced variable in Appendix A. If a person is displaced more than once during the period 1986-1996, they will only be in the displacement group of the first year they are displaced. Hereafter we shall refer to displaced workers as ‘treated’ and workers who were not displaced as ‘controls’.

Descriptive statistics concerning the two groups are given in subsection 3.4 below.

3.3 Health measures

The health outcome measures used in this paper are based on data on admissions to somatic hospitals due to specific sets of diagnoses. The Danish Public Health Insurance scheme (of which all Danish citizens are members) meets the cost of admission to hospitals implying that economic considerations have no influence on admission decisions. Data on hospital admissions were obtained from the Danish national register of patients which include detailed information on diagnoses, dates of admission and discharge, etc. for all admissions to somatic hospitals in Denmark.⁷

Relevant diagnoses are diseases of the circulatory system and diseases of the digestive system. These diseases include high blood pressure, other heart diseases, gastric catarrh, and gastric ulcer.⁸ This choice of diagnoses is based on what may be likely health outcomes from job loss according to the social epidemiological literature; see, for instance, Kasl and Jones (2000) who point out that these diseases may be caused by stresses associated with job loss. See also Brunner and Marmot (1999), Brunner (2002), and Stansfeld and Fuhrer (2002) for a discussion of the link from stress and depression to these diseases.

If a person is hospitalized more than once in our data period, the date of hospitalization is the date of the first admission after registration in either the treatment or control group (see below). In the statistical analyses we condition on previous health status using two indicators of general health conditions: the number of admissions and the number of days at hospitals for any diagnosis (except birth and a few other diagnoses not related to illness). For persons displaced in year t or in

⁷ An alternative health measure available in the administrative registers is the individual frequency of doctor consultations over a year. However, this health measure is only registered from 1989 and it is not informative about the type of illness associated with a given consultation.

⁸ Precise definitions according to the International Classification of Diseases are given in Appendix D.

the control group of that year, these controls are calculated for the four previous years ($t-5$ to $t-2$). In addition, we also use the number of days of receipt of sickness benefits as a control for past health status.

3.4 Descriptive Statistics for displacement and covariates

Table 1 shows the numbers of displaced and controls for each year 1986 to 1996 for men and women, respectively. The Danish economy experienced a recession from the mid 1980's to 1993 and a boom after 1993 so that the fraction of displaced persons is smaller after 1993 than before. The number of controls is basically determined by the number of full time employed in the private sector. The major part of the marked increase in the number of controls for women is explained by a rise in the overall fraction of full time employed women in the labour market.

Table 1. Numbers of displaced (treated) and controls (non-treated)

Year	Men			Women		
	Displaced	Controls	% displaced	Displaced	Controls	% displaced
1986	1,892	39,276	4.6	827	15,395	5.1
1987	2,357	39,411	5.6	1,032	16,365	5.9
1988	2,473	38,846	6.0	1,129	17,109	6.2
1989	1,762	39,811	4.2	893	17,973	4.7
1990	1,868	39,235	4.5	838	18,037	4.4
1991	2,124	37,381	5.4	1,026	17,263	5.6
1992	1,737	38,834	4.3	925	18,792	4.7
1993	2,364	37,457	5.9	1,072	18,915	5.4
1994	1,375	37,415	3.5	859	19,407	4.2
1995	1,409	40,632	3.4	753	20,020	3.6
1996	1,153	41,730	2.7	686	20,618	3.2
All	20,512	430,028	4.6	10,040	199,894	4.8

As was explained in section 2.2, we estimate probit models for the risk of displacement for men and women separately and for each year 1986-1996. To illustrate our results, we present results for 1986 and 1992 for men and women separately; these results are representative of the results for all years. Summary statistics for 1986 and 1992 for treatment and control groups are shown in appendix B, Tables B.1 and B.2 for men and women, respectively. These tables include the

outcome variable hospitalization (equal to 1 if the person is hospitalized 1-4 years after the base year) and the explanatory variables used in the estimation of the propensity to be displaced in the analysis below. The explanatory variables include indicators of previous health, dummy variables for age, industry of employment (the year before the base year), educational level, unemployment insurance status, tenure, and living in a couple. All explanatory variables are lagged at least one year (i.e., they are measured at least one year prior to the base year) to ensure that the controls are not affected by whether the individuals are treated or not. The variables for the number and duration of previous admissions to hospitals (for any diagnosis except birth and a few other diagnoses not related to illness) are calculated for the period 2-5 years prior to the base year. The variable 'sickness benefits' (i.e., the duration of sickness benefits) and the variable for unemployment insurance status are lagged two years. The variables for the degree of unemployment in previous years ('unemployment ($t-s$)', $s=2, \dots, 5$) are lagged 2-5 years. For the unemployment variable the extra lag is motivated by the fact that plants which are eventually going to close or downsize may, in the year before closing or downsizing, lay off workers temporarily to a larger extent than other plants. This may affect health outcomes of the employees which may also be affected by stresses associated with possible anticipation of plant closure or downsizing. This is the reason why the indicators of previous health are also lagged an extra year. The variable for unemployment insurance status is lagged an extra year since anticipated plant closure or downsizing may induce workers to get insured.

From Tables B.1 and B.2 we see that displaced workers have on average a poorer previous health status in terms of duration of sickness benefits and the number of admissions to hospitals for all diagnoses (for men, the average duration of hospitalization is longer for displaced in 1986, but shorter in 1992, compared to controls), they are younger (specifically, there is a much higher fraction of age 20-29), they are more likely to work in construction and less likely to work in manufacturing, they have a lower level of education (although differences are very small), shorter

labour market experience, and much shorter tenure (at the employer of the year before the base year), they are more likely to be single (not surprising given the age differences), and they have on average a higher degree of unemployment (2-5 years before the base year).

3.5 Descriptive Statistics for health

Before presenting the statistics for our health measure we present some broad facts concerning hospitalisation for the stress related diagnoses discussed in section 3.3. Figure 2 shows how the risk of hospitalisation, for the diagnoses related to the diseases of the circulatory and digestive systems which we analyse, depends on age for males and females, respectively, for our 10% random sample of the Danish population. The figure gives the risk of being hospitalised at least once in a given year and is based on data for the period 1981-99. It will be seen that the risk of hospitalisation is very low (below 0.2%) for persons younger than 25, that it increases non-linearly with age, and that it is much higher for males than for females. The non-linearity in age is important for modelling purposes since it rules out a naïve difference-in-difference estimator for the treatment effect.

Turning to our specific outcome measure, Table 2 displays the numbers hospitalized for diseases of the circulatory system and diseases of the digestive system after the base year and until 1999 for each base year 1986-1996. For instance, for persons in the treatment or control group of 1986 we show the numbers hospitalized in the period 1987 to 1999, and for the base year 1991 the numbers hospitalized from 1992 to 1999. Thus, the main reason why there is a higher frequency of hospitalization for base year 1985 than for later base years is that we have health records for more years for these persons. All in all, we have 1,207 observations on hospitalization for displaced persons and more than 25,000 for controls. The raw data in Table 2 do not indicate significant differences in health outcomes between treatment and control groups, but this could of course be due to the fact that there are systematic differences between the two groups in terms of

covariates.

Figure 2. Hospitalisation rate and age

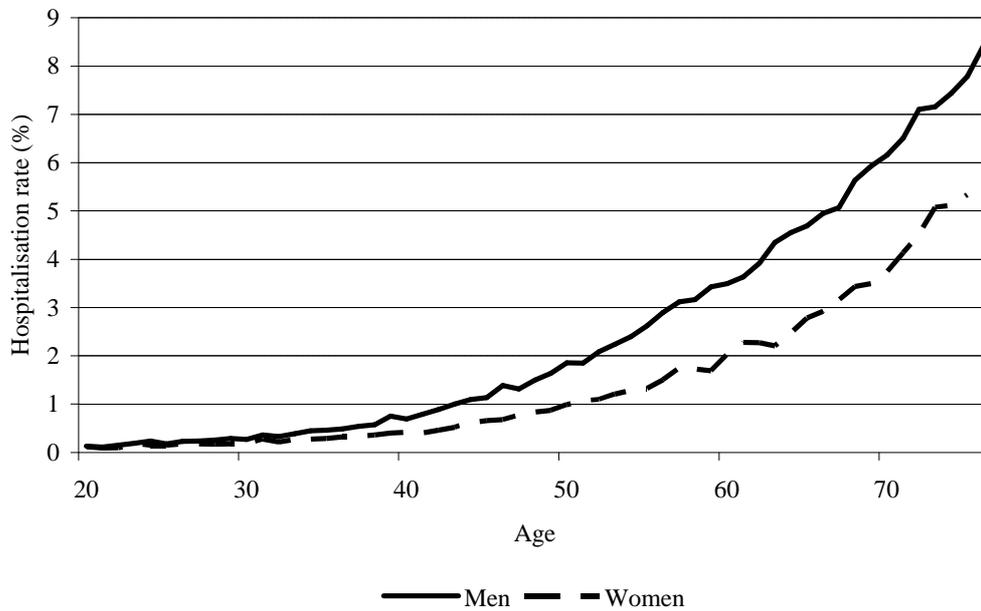


Table 2. Numbers hospitalised due to circulatory and digestive diseases in displacement and control groups for each base year 1986-1996, for men and women, respectively

Year	Men				Women			
	Displaced		Controls		Displaced		Controls	
	#	%	#	%	#	%	#	%
1986	131	6.9	3,725	9.5	31	3.8	590	3.8
1987	180	7.6	3,349	8.5	40	3.9	517	3.2
1988	170	6.9	2,928	7.5	32	2.8	520	3.0
1989	117	6.6	2,590	6.5	26	2.9	456	2.5
1990	72	3.9	2,250	5.7	16	1.9	405	2.3
1991	88	4.1	1,884	5.0	16	1.6	323	1.9
1992	50	2.9	1,626	4.2	11	1.2	285	1.5
1993	88	3.7	1,283	3.4	16	1.5	228	1.2
1994	49	3.6	1,012	2.7	9	1.1	187	1.0
1995	32	2.3	871	2.1	9	1.2	154	0.8
1996	19	1.7	653	1.6	5	0.7	114	0.6
All	996	4.9	22,171	5.2	211	2.1	3,779	1.9

4. Estimation results

4.1 Estimation of the propensity score

As discussed in section 2 we estimate Probit propensity score functions when matching non-

displaced to displaced persons.⁹ For a given definition of displacement, we estimate for each base year 1986-96 (see section 3) two propensity score functions, one for men and one for women. Matching is done separately for each base year and gender. For instance, a male who is displaced in 1986 is matched to the non-displaced male in the control group for those displaced in 1986 with the closest propensity score. In some of the analyses we match exactly on age. In this case, a male of age 40 who is displaced in 1986 is matched to the non-displaced male of age 40 in the control group for those displaced in 1986 who has the closest propensity score, and so on. This set up takes account of the fact that the effects of observable characteristics on the probability of displacement may change over time due to, e.g., the business cycle, changes in industry structure, etc. Likewise, we estimate gender specific propensity score functions in order to take account of gender specific differences in the effects of observable characteristics on the probability of being displaced. The reason for matching exactly on age in some of the analyses is that age has very pronounced (and non-linear) effects on the risk of hospitalization, see section 3.5. By matching on age we make sure that the age distributions of the treatment group and the group of matched controls are exactly the same. After having matched on the propensity score by base year and gender (and age), all male displacement groups are merged into one, and similarly all groups of matched male controls are pooled, and similarly for women.

For both the pooled sample and for each base year separately we estimate average causal effects of being displaced on the probability of being hospitalized. We do that for males and females separately. Our panel data set allows us to follow health outcomes up to 13 years after being displaced. We estimate the average treatment effect on the treated using two different outcome measures: a dummy variable for being hospitalized 1-4 years after displacement and the duration to the first entry into hospital, which is estimated from the Kaplan-Meier survivor

⁹ The estimations are done in Gauss. Parts of the programs we use are originally programmed by Michael Lechner.

function (of survival until hospitalization).

Estimation results for the propensity score functions (i.e., probit models for being displaced) for base years 1986 and 1992 are shown in Tables 3 and 4 for men and women, respectively. The explanatory variables included are described in section 3.4 (and appendix B). Further, a number of interaction terms are included (i.e., interactions between industry dummies and tenure, education, age, and previous unemployment). The parameter estimates of the interaction terms are not shown in Tables 3 and 4 which only include estimates of the ‘main effects’. The parameter estimates of the three indicators of previous health are not significant,¹⁰ although most of them are positive indicating that there might be a weak positive correlation between poor prior health and the risk of being displaced. Most of the coefficients of the age dummies are insignificant and they have no consistent structure. However, the male 1992 base year estimation seems to indicate a higher probability of displacement for older than for younger men. For females, working at a manufacturing plant reduces the risk of displacement compared to working in the service sector. Working in construction increases the risk of displacement (at least for men). The effects of the other industry dummies are not consistently positive or negative across estimations which may reflect the fact that business conditions (and therefore the risk of displacement) in particular industries may be quite different from year to year. A higher educational level reduces the risk of displacement, and the same applies for having no unemployment insurance, having long labour market experience or tenure, being married, and having a low degree of previous unemployment.

Finally, we present specification tests for the propensity score model for normality and homoskedasticity.¹¹ Normality is not rejected. The heteroskedasticity test statistics in columns 3 and 6 of Tables 3 and 4 indicate that heteroskedasticity is not a serious problem, although there

¹⁰ Furthermore, the three parameters are not jointly significant in any of the propensity score estimations according to likelihood ratio tests.

¹¹ Testing for heteroskedasticity and non-normality is done by using conventional specification tests, see White (1982) and Bera, Jarque and Lee (1984). The score test against heteroskedasticity is based on $(\text{expected hessian})^{-1} (\text{outer product of the gradient}) (\text{expected Hessian})^{-1}$, see Lechner (2000).

seems to be heteroskedasticity with respect to a few of the included variables.

Table 3. Estimation of the propensity score (probit model for being displaced), for men in 1986 and 1992

Variable	Men, 1986			Men, 1992		
	Coef.	Std. err.	Heteroskedasticity test $\chi^2(1)$	Coef.	Std. err.	Heteroskedasticity test $\chi^2(1)$
Const	-1.239	0.055		-1.361	0.063	
Age 30-39	0.019	0.033	0.1	-0.005	0.036	0.1
Age 40-49	0.087	0.048	2.9	0.031	0.053	2.9
Age 50-63	-0.040	0.058	0.2	0.203	0.066	0.2
Couple	-0.081	0.026	0.1	-0.038	0.027	0.1
Manufacturing	-0.089	0.068	1.9	0.060	0.073	1.9
Construction	0.166	0.071	2.2	0.501	0.078	2.2
Infrastructure	-0.099	0.067	7.2	-0.141	0.072	7.2
Financial services	0.129	0.070	0.2	0.126	0.076	0.2
Other industries	0.064	0.074	0.1	0.139	0.101	0.1
Vocational education	0.074	0.051	0.5	-0.057	0.058	0.5
College education	-0.022	0.068	0.1	-0.305	0.070	0.1
Bachelor degree	-0.108	0.057	1.2	-0.211	0.055	1.2
Master's degree	-0.212	0.082	3.8	-0.249	0.068	3.8
No unempl. insurance	-0.088	0.033	1.2	-0.031	0.031	1.2
Experience (years/100)	-1.261	0.273	3.0	-0.999	0.252	3.0
Tenure 1 year	-0.095	0.057	6.7	-0.178	0.065	6.7
Tenure 2 years	-0.284	0.057	0.0	-0.261	0.061	0.0
Tenure 3 years	-0.381	0.065	0.9	-0.259	0.067	0.9
Tenure 4 years	-0.513	0.066	0.0	-0.492	0.070	0.0
Tenure 5 years or more	-0.546	0.061	4.5	-0.534	0.069	4.5
Unemployment (t-2)	0.286	0.072	9.1	0.521	0.094	9.1
Unemployment (t-3)	0.148	0.066	0.2	0.240	0.085	0.2
Unemployment (t-4)	0.082	0.067	1.3	0.155	0.096	1.3
Unemployment (t-5)	0.107	0.064	0.0	0.057	0.103	0.0
#of prev. admissions	0.002	0.016	0.0	0.011	0.018	0.0
Dur. prev. admissions	0.002	0.001	2.5	-0.005	0.002	2.5
Sickness benefits(days)	0.001	0.001	0.5	0.001	0.001	0.5
Log likelihood		7,018			6,606	
Normality test $\chi^2(2)$		2.33			0.59	
Observations		41,168			40,571	

Note: All explanatory variables are lagged at least 1 year relative to the base year, and several are lagged at least 2 years, see section 3.3 for details. A number of interaction terms are included, but not shown.

The reference person is less than 30 years of age, without formal educational qualifications (or of unknown educational status), single, insured against unemployment, working in the service sector, and has a tenure less than 1 year.

Table 4. Estimation of propensity score (probit model for being displaced), for women in 1986 and 1992

Variable	Women, 1986			Women, 1992		
	Estimation Coef.	Std. err.	Heteroscedasticity test χ^2 (1)	Estimation Coef.	Std. err.	Heteroscedasticity test χ^2 (1)
Const	-1.022	0.062		-1.099	0.062	
Age 30-39	0.091	0.058	1.4	-0.022	0.057	0.3
Age 40-49	-0.085	0.070	1.1	0.055	0.066	0.1
Age 50-63	-0.019	0.087	0.3	0.004	0.080	5.4
Couple	-0.042	0.039	1.9	-0.083	0.035	0.0
Manufacturing	-0.379	0.074	0.0	-0.404	0.076	5.9
Construction	-0.139	0.117	0.0	0.214	0.088	0.3
Infrastructure	-0.352	0.092	0.2	-0.397	0.071	1.5
Financial services	-0.397	0.051	0.0	-0.097	0.046	5.0
Other industries	-0.265	0.068	8.1	0.023	0.096	0.2
Vocational education	-0.116	0.039	0.0	-0.103	0.037	2.0
College education	-0.297	0.100	0.3	-0.164	0.083	3.3
Bachelor degree	-0.119	0.134	1.2	0.072	0.083	1.1
Master's degree	-0.226	0.126	0.6	-0.346	0.110	4.4
No unempl. insurance	0.027	0.046	0.5	-0.033	0.044	0.5
Experience (years/100)	-2.161	0.451	4.1	-1.173	0.354	0.0
Tenure 1 year	-0.093	0.064	0.3	-0.250	0.058	0.5
Tenure 2 years	-0.111	0.080	0.6	-0.294	0.070	0.3
Tenure 3 years	-0.056	0.082	1.7	-0.440	0.083	1.8
Tenure 4 years	-0.399	0.107	0.0	-0.346	0.082	0.9
Tenure 5 years or more	-0.394	0.064	4.8	-0.545	0.058	0.6
Unemployment	0.219	0.091	1.0	0.431	0.088	6.1
Unemployment (t-3)	-0.048	0.096	0.2	-0.141	0.102	0.5
Unemployment (t-4)	-0.079	0.099	0.2	0.207	0.101	0.0
Unemployment (t-5)	0.162	0.085	0.0	-0.001	0.100	0.0
#of prev. admissions	0.008	0.025	6.9	-0.014	0.018	0.7
Dur. prev. admissions	0.003	0.003	5.1	0.002	0.002	0.8
Sickness benefits(days)	0.000	0.001	0.7	0.001	0.001	3.8
Log likelihood		3,090			3,531	
Normality test χ^2 (2)		4.90			4.35	
Observations		16,222			19,681	

Note: See Table 3.

In appendix C we present kernel density estimates of the distribution of the propensity scores for base years 1986 and 1992 for men and women, respectively. Since estimation of treatment parameters requires common support for the treatment and control groups, a (small) number of observations are excluded from the sample. Only very few treatment and control group observations are deleted, see Tables 5 and 6, implying that there are no serious common support problems with respect to estimating, for instance, the average treatment effect on the treated.

When we match on both age and propensity score we lose a few more observations for the displaced (11 and 10 males, and 7 and 5 females, for base years 1986 and 1992, respectively). This is due to the fact that there may be rather few persons in the control group for a particular age and therefore a higher probability that the propensity score of a displaced person with uncommon characteristics is outside the support of the propensity scores of the controls. When we match exactly on age we often have only very few displaced persons of a particular age. In this case, therefore, we do not exclude controls with propensity scores outside the support of the propensity scores of the displaced since this would reduce matching quality for displaced with maximum or minimum propensity scores for a given age (when there is only one displaced person of a particular age all controls of this age would be excluded).

Table 5. Loss of observations due to common support requirement, men in 1986 and 1992

	1986		1992	
	Treated	Controls	Treated	Controls
Observations before	1892	39276	1737	38834
Observations after	1892	39136	1734	37526
Per cent deleted	0	0.36	0.17	3.37

Table 6. Loss of observations due to common support requirement, women in 1986 and 1992

	1986		1992	
	Treated	Controls	Treated	Controls
Observations before	827	15395	925	18792
Observations after	827	15376	925	18575
Per cent deleted	0	0.12	0	1.15

4.2 Matching

To check the balancing properties of the propensity score for the treated and the matched control group we report in Tables C1-C4 of appendix C two-sample t-statistics and absolute standardised

biases for the propensity score and for each explanatory variable included in the estimation of the propensity score (except interaction terms). Only results from 1986 and 1992 are shown. The standardised bias is the difference between the sample means of the treated and matched controls as a percentage of the square root of the average of the sample variances in the treated and matched control groups (see Rosenbaum and Rubin (1985)). Tables C1 and C2 show matching quality indicators for males and females, respectively, when we match on (the linear predictor of) the propensity score for each base year (and gender) separately. In general, the match quality is satisfactory. As can be seen, the matched sample has very similar means for each of the included explanatory variables, and we can not reject the hypothesis of no difference in mean, indicating that the conditional independence assumption is not rejected. Tables C3 and C4 show matching quality indicators for males and females, respectively, when we are matching on (the linear predictor of) the propensity score *and age*. Comparing Tables C1 and C3 (for males) and C2 and C4 (for females) it will be seen that matching quality is generally better when we match exactly on age, also with respect to other variables than age.

4.3 The treatment effect on duration to hospitalization

We consider first the average treatment effect on the treated with regard to the duration to the first entry to hospital (for the diseases of the circulatory and digestive systems specified in section 3.3 and Appendix D). For each base year, the duration in years until hospitalization is computed for each person. We analyse whether there are any differences in the probability of being hospitalized between displaced workers and the matched controls. This is done by estimating the non-parametric Kaplan-Meier survivor and hazard function for hospitalization for each of the two groups.

Figures 3 and 4 show results from matching on the propensity score alone for men for the sample pooled over all base years 1986-1996. Figure 3 shows the estimated Kaplan-Meier hazard

rates for treated males and their matched controls up to twelve years after the base year. We also graph 95 percent confidence intervals for the control group. The hazard rate at duration (year) t is the probability of hospitalization during year t after the base year, given no hospitalization prior to year t . There does not seem to be significant differences between displaced males and their matched controls in terms of risk of hospitalization with respect to the health outcomes studied. Figure 4 presents the corresponding estimated survivor functions for men. The value of the survivor function at duration (year) t estimates the probability of not being hospitalized up to t years after the base year. As can be seen, the differences between the treatment and control groups are completely insignificant. Finally, for men, we present the pooled hazard figures when we match on age exactly and on the propensity score (Figure 5). The hazard rate for the treatment group is almost identical to that of Figure 3 since the treatment groups are almost identical (the only difference being that a few more persons are excluded when we match on age due to the fact that the common support requirement is imposed for each age). The estimated hazard for the match controls is somewhat different compared to Figure 3, but the conclusion is the same: there are no significant differences in hazard rates between displacement and control groups.

Figures 6-8 show the results for the pooled sample of women. Figures 6 and 7 show the hazard and survivor functions when we match on the propensity score alone (for each base year). The figures indicate that there might be a slightly higher risk of hospitalization for displaced women than for the controls, but the difference is not significant. Figure 8 shows the hazard functions when we match on age exactly and on the propensity score. The indication that there might be a higher risk of hospitalization for displaced women is more pronounced compared to Figure 6. The hazard rate for displaced women is above the upper 95% confidence bound of the matched controls for durations 2-5 (and 8-9) years from the base year, but taking into account the uncertainty of hazard estimates for the treatment group these differences are not significant.

Figure 3. Pooled Kaplan-Meier estimates of hospitalization hazard rate for men

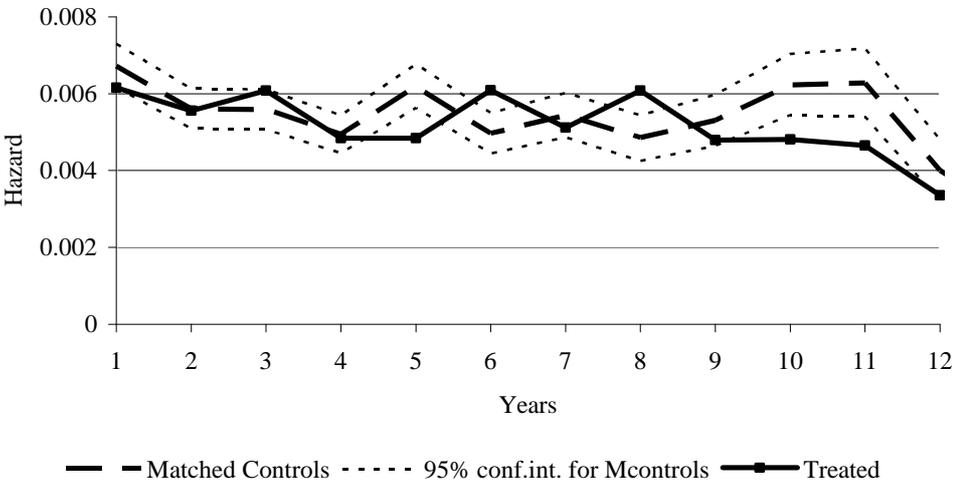


Figure 4. Pooled survivor function for men

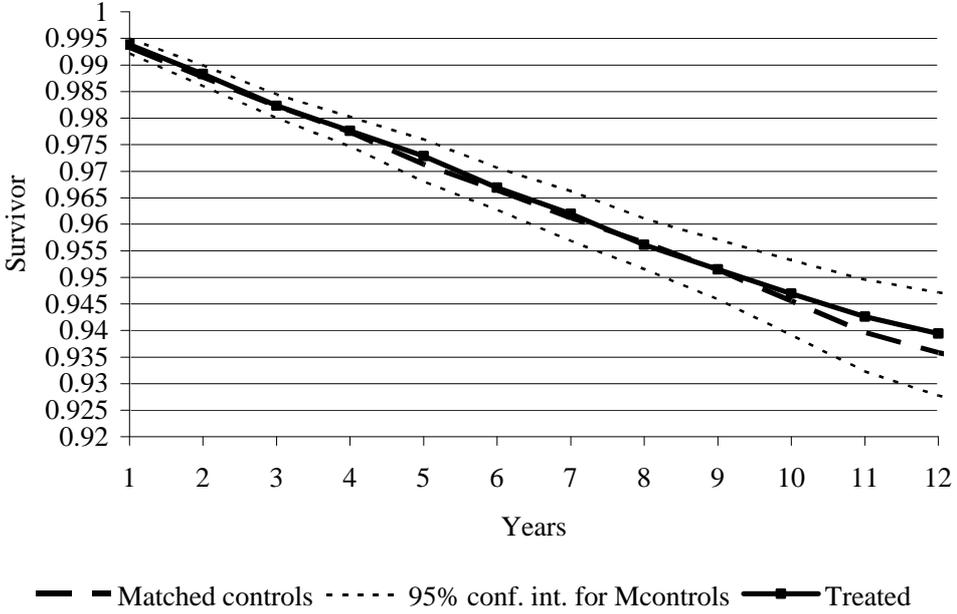


Figure 5. Pooled Kaplan-Meier estimates of hospitalization hazard rate for men, where matching is conditioned on the propensity score and age

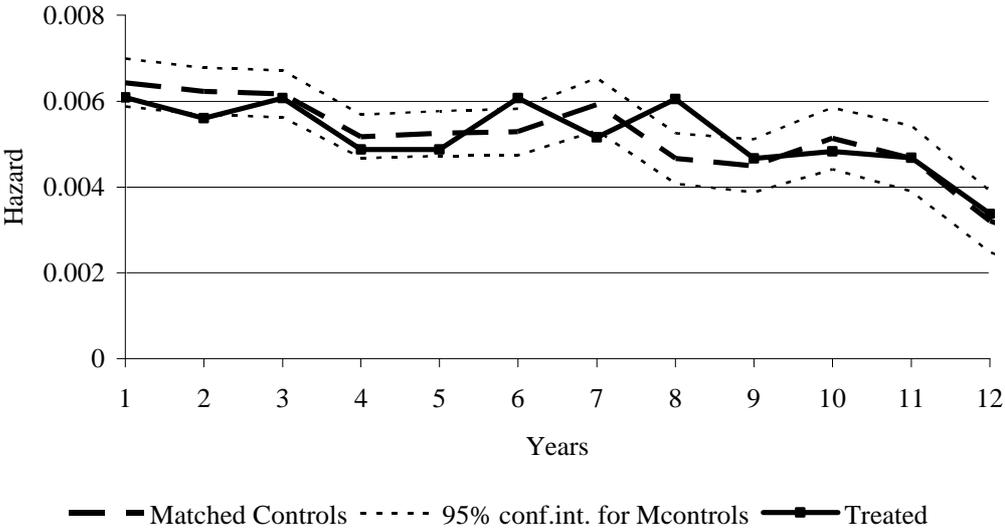


Figure 6. Pooled Kaplan-Meier estimates of hospitalization hazard rate for women

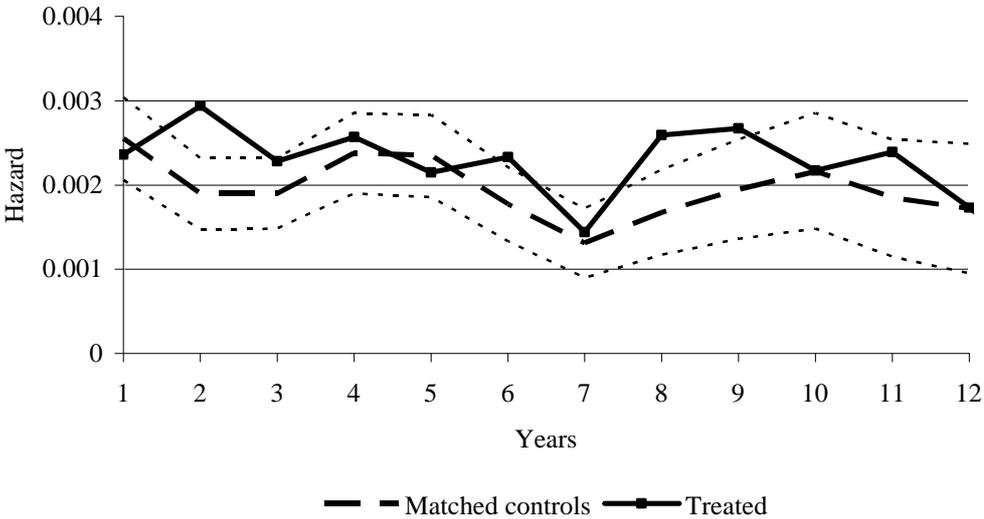


Figure 7. Pooled survivor function for women

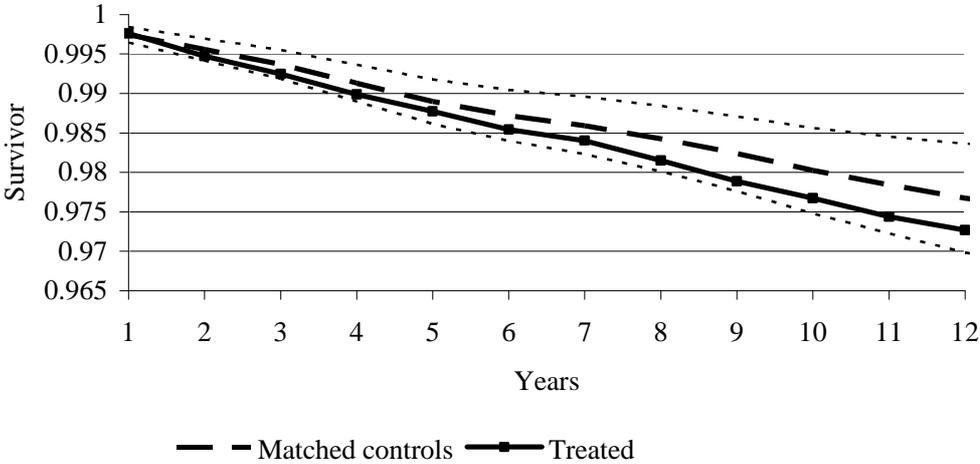
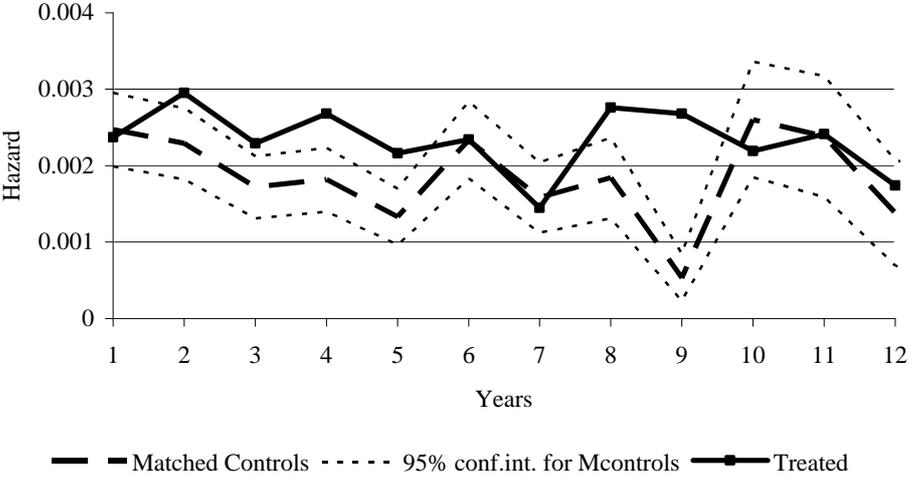


Figure 8. Pooled Kaplan-Meier estimates of hospitalization hazard rate for women, where matching is conditioned on the propensity score and age



4.4 The treatment effect on hospitalisation rates

The alternative outcome measure we use is a dummy variable for being hospitalized 1-4 years after the base year. Tables 7 and 8 report the results for males and females, respectively, for the

average treatment effect on the treated, that is the difference in the risk of hospitalization 1-4 years after the base year between the displacement and matched control groups. The first row in the tables shows the estimates for the pooled sample for all base years 1986-96, whereas the two last rows show the estimates for the illustrative years 1986 and 1992, respectively. We find no significant effects, neither for males nor females, again indicating that being displaced does not have an effect on physical health.

Table 7. Proportions hospitalised (%) 1-4 years after base year, and average treatment effects on the treated (ATET), males

Base year	Matching on the propensity score				Matching on the propensity score and age			
	Treated	Controls	ATET	Obs.	Treated	Controls	ATET	Obs.
1986-96	2.24	2.34	-0.11 (0.38)	20,816	2.24	2.37	-0.14 (0.38)	20,678
1986	1.96	2.43	-0.48 (0.48)	1,892	1.97	2.50	-0.53 (0.48)	1,881
1992	1.61	2.42	-0.81 (0.48)	1,734	1.62	2.32	-0.70 (0.47)	1,724

Note: ATET is the difference in hospitalisation between the displacement group and matched control group 1-4 years after base year. Standard errors are in brackets.

Table 8. Proportions hospitalised (%) 1-4 years after base year, and average treatment effects of the treated (ATET), females

Base year	Matching on the propensity score				Matching on the propensity score and age			
	Treated	Controls	ATET	Obs.	Treated	Controls	ATET	Obs.
1986-96	1.01	0.87	0.14 (0.42)	10,576	1.03	0.83	0.20 (0.43)	10,527
1986	1.33	1.09	0.24 (0.54)	827	1.34	0.49	0.85 (0.47)	820
1992	0.65	0.65	0.00 (0.37)	925	0.65	0.43	0.22 (0.34)	920

Note: ATET is the difference in hospitalisation between the displacement group and matched control group 1-4 years after base year. Standard errors are in brackets.

4.5 Average treatment effects for subgroups of displaced workers

The conclusion from the analysis above is unequivocal: there is no significant average treatment

effect in terms of the risk of hospitalisation for the diagnoses analysed. However, there might still be significant effects of displacement for subgroups of displaced workers. Merging these subgroups with the other groups of displaced persons for whom there are no significant effects might explain our finding of no significant average treatment effect. In this subsection we present results for two subgroups of the displaced: workers aged 40 or more and those who experience a significant amount of unemployment following the displacement.

As will be seen from Tables B1 and B2 in Appendix B, 60-70% of the displaced males and 70-80% of the displaced females are younger than 40, and persons below 40 have a very low risk of hospitalization. To check whether there might be health effects of displacement for ‘older’ workers, we repeated the analysis for workers of age 40 or above. That is, we matched the displaced males of age 40 or above to males in the control group aged 40 or more for each base year separately, using the estimated propensity scores for the different base years, and pooled the samples of displaced males and matched controls – and similarly for females. The estimated hazard rates for males are shown in Figure 9. As can be seen, there is no systematic difference in hazard rates of hospitalization between displaced persons older than 40 and matched controls. Similarly our other outcome variable (hospitalization 1-4 years after the base year) shows no evidence of an effect; the coefficient is negative and insignificant. Figure 10 shows the estimated hazard rates for females of age 40 or above for the treatment and control groups, respectively. There is an indication that displacement may increase the risk of hospitalization for women above 40, but it is only the hazard rates at durations 2 and 4 years which are “significantly” higher for the displacement group, and the alternative outcome measure (hospitalization 1-4 years after the base year) is not significantly higher for displaced women.

Figure 9. Pooled Kaplan-Meier estimates of hospitalization hazard rates for men of more than 40 years of age

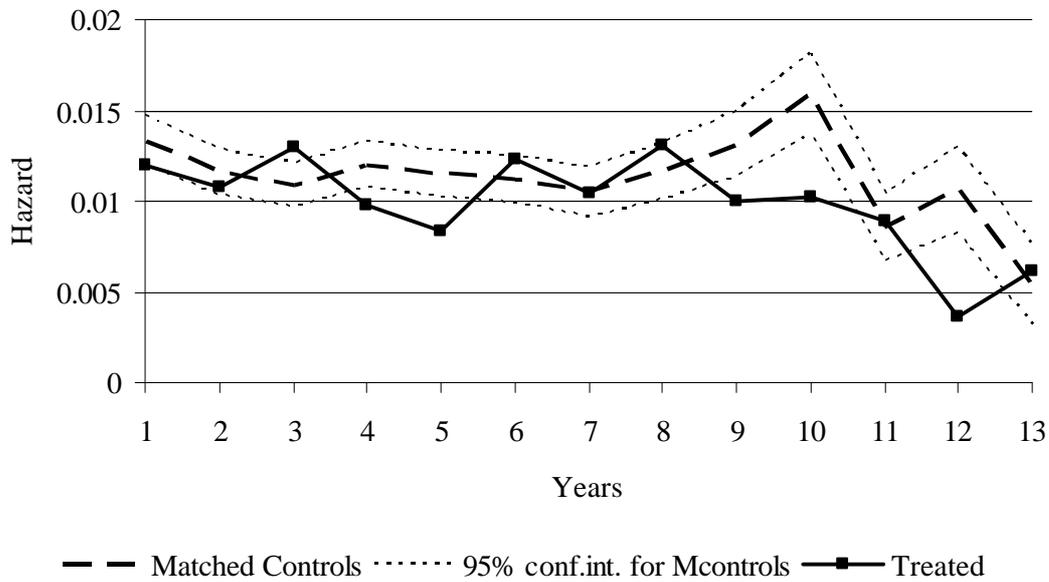
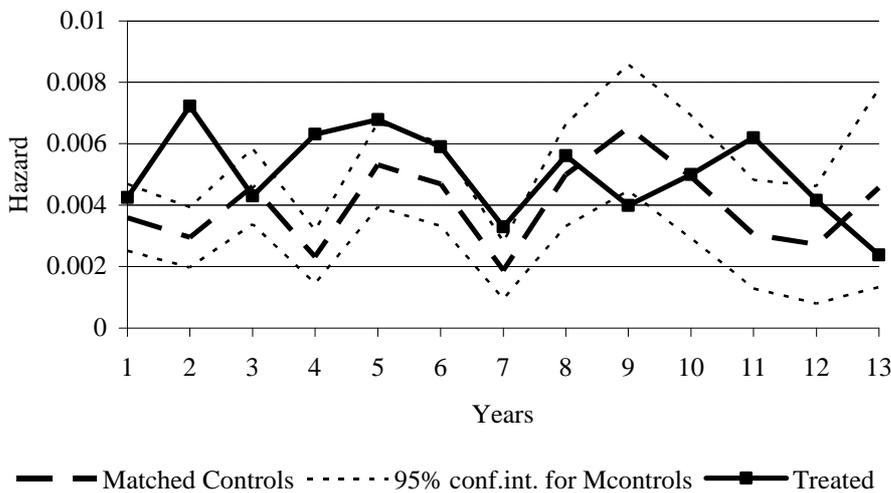


Figure 10. Pooled Kaplan-Meier estimates of hospitalization hazard rates for women of more than 40 years of age



As we discussed in the introduction, only some of the workers in the displacement group experience unemployment of a significant duration after displacement. For our sample, 37% of the displaced persons are unemployed during more than 10% of the year in which they are

displaced, compared to 7% for the control group. And 51% of the displacement group have no unemployment at all in the displacement year, compared to 86% of the control group. This means that the majority of displaced workers find a new job very quickly. If displacement only has negative health effects for the subgroup of workers who experience unemployment in connection with the displacement, these effects might be blurred in the analyses above covering both displaced workers who get unemployed and displaced workers who find a job immediately. Using the same matched control group as in Figure 3 (for males) and Figure 6 (for females) for comparison, we selected the subgroups of displaced males and females, respectively, who were unemployed for more than 10% of the year in which they were displaced.

Figure 11. Pooled Kaplan-Meier estimates of hospitalization hazard rates for men where the treatment group consists of displaced workers who were unemployed for more than 10% of the year of displacement

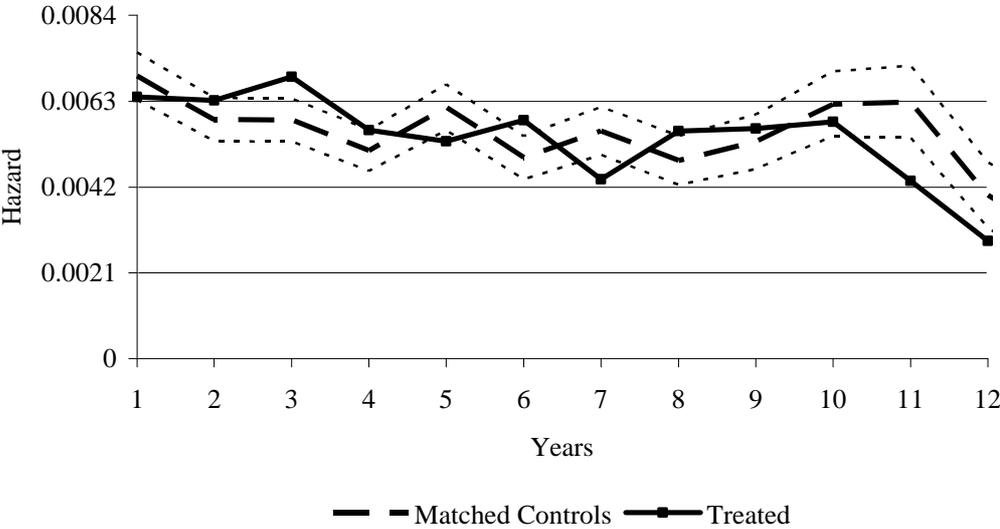
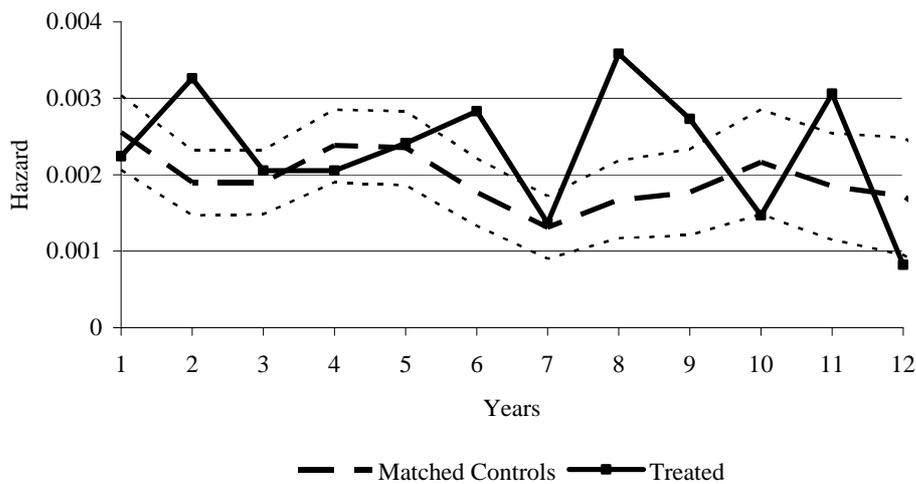


Figure 12. Pooled Kaplan-Meier estimates of hospitalization hazard rates for women where the treatment group consists of displaced workers who were unemployed for more than 10% of the year of displacement



Comparing Figures 11 and 3, this shifts the hazard rates of the male treatment group upwards for the first five years following displacement, but the shift is very small and it is only the hazard rate three years after the base year which is above the upper 95% confidence bound of the hazard rate of the controls. Taking account of the uncertainty of the estimate of the hazard of the treatment group, the difference in hazards after three years is not significant. Furthermore, the hazard of the treatment group is still below the lower 95% confidence bound of the hazard of the controls 5 years after the base year, and it shifts downwards compared to Figure 3 for years 6-12 after displacement. The risk of hospitalization 1-4 years after the base year (the other outcome) is still very similar for the male treatment and control groups, and the difference is clearly insignificant. Thus, even when we pick such a very selective subset of the treatment group, which should bias our results towards finding negative health effects of displacement, we do not find any significant effects for males.

The conclusions for females are the same. Comparing Figures 12 and 6 it will be seen that restricting the female treatment group to those with at least 10% of unemployment in the

displacement year does not systematically increase differences in hospitalization hazard rates between treatment and control groups. For the other outcome (risk of hospitalization 1-4 years after the base year) the ATET is also clearly insignificant.

5. Conclusion

As discussed in the introduction, the evidence for a causal link from unemployment to ill health is very mixed. In the introduction we listed eight conceptual problems with previous studies. In this paper we use a data source that allows us to address most of these issues. Specifically for the listed items: our sample is representative of the Danish population; we have very large sample sizes; there is no attrition in following workers after the closure; we can construct good ‘control’ groups for the displaced workers; we can control for pre-closure health status and other factors from before the plant started to fail; we have a very specific health outcome, hospitalization for stress related diseases, which is grave, precise and observed with only minimal measurement error; in our robustness checks in section 4.5 we considered only displaced workers who had a significant unemployment following the displacement. In this analysis we have not made any attempt to consider ‘anticipation’ effects nor the stress that non-displaced workers in a displacing firm may experience.

In this study we have chosen to use matching methods that rely on having an adequate set of variables to ‘control’ for differences in counter-factual health status between those who are displaced and those who are not. Although our set of variables is very comprehensive, it would be useful to supplement this study with alternative treatment estimators that rely on different identifying assumptions.

We find that there does not appear to be any impact of displacement on hospitalisation for stress related diseases for men or women. Our large sample size suggests this negative finding is

not the result of a lack of power for the test (at least for males), despite the relative rareness of the health outcome. This is in line with our reading of previous studies which never find significant health effects but sometimes find positive but insignificant effects. Moreover, we do not find any effect when we consider subgroups consisting of older workers or those who experience some unemployment following their displacement. An obvious extension of our results would be to consider less serious stress related health outcomes. Although the latter would not weigh so heavily in the costs of displacement they may still be important in any welfare analysis.

We believe that the results for Denmark are clear and robust. Do they have any relevance for other countries? After all, Denmark is an advanced social democracy that provides a wide range of safety nets for its citizens. Consequently, displacement in Denmark may be a less stressful event than in other countries and our results may not generalise. There are two aspects to this. First, how likely is it that a displaced worker will find a new job? Second, if there is a spell of unemployment, how adequate is Unemployment Insurance and how well does it insulate an unemployed worker from a short run fall in living standards? As regards the first aspect, we note that hiring and firing rates in Denmark are high by international standards and are comparable to those observed in the USA (see Albæk and Sørensen (1998)).¹² Thus our results would be applicable to countries where unemployed workers have a high rate of job offers. As regards the second consideration, during our data period the Danish UI system did provide a very high replacement rate for earnings for low wage workers, but the benefit is capped so that median wage workers face a replacement rate of about 60% which is comparable to countries such as Canada and France and not dramatically higher than the USA. Thus we believe that whilst displacement may be less stressful in Denmark than in other countries, the differences are not so large as to make our wholly negative result invalid for other countries.

¹² This is usually attributed to Denmark having very limited employment protection laws so that Danish employers are very ready to fire and consequently to hire.

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Appendix A. The definition of displacement

According to the basic definition of displacement (Kletzer (1998)), a worker is identified as *displaced* in year t if they separated from the plant at which they worked at the end of year $t-1$ and if that plant reduced the number of employees by at least 30% during year t .¹³ However, even if a person meets the criteria for being displaced stated above, they are not considered displaced if they continue working at the same firm (but at a different plant). The following gives the specific details of the displacement variable for the Danish data.

If a plant is closed during year t , employment is zero at the end of year t . The criteria for considering a firm as closed or continuing are therefore important for the loose definition of displacement stated above. As has become standard in analyses on Danish register data, we consider a plant as continuing if at least one of the following criteria is satisfied: (1) The same owner and the same industry; (2) the same owner and the same employees; (3) the same employees and the same address; (4) the same employees and the same industry. The “same industry” means the same ISIC code at the 5 digit level. In case (2) “same employees” means that those who remain employed at the plant at the end of the current year constitute either at least 30% of the employees at the end of the preceding year *or* they make up at least 30% of the employees at the end of the current year. In cases (3) and (4) the definition of “same employees” is more restrictive since here it means that those who remain employed at the plant at the end of the current year constitute at least 30% of the employees at the end of the preceding year *and* they make up at least 30% of the employees at the end of the current year. If none of the four criteria are satisfied the plant is defined as closed.

¹³ A worker is said to have experienced a job separation in year t if, at the end of year t , they did not work at the plant at which they worked at the end of year $t-1$. In the administrative registers the employment status of a person in a given year is recorded at the end of November that year. Similarly, the number of employees at each plant is recorded at the end of November. In the following, we will use the term “end of year t ” instead of “end of November of year t ”. The year of admission to a hospital is recorded similarly, i.e. hospitalization in year t is defined as admission between 1 December year $t-1$ and 30 November year t .

There are basically two problems with this – and any other – definition of continuing or closing plants in relation to identification of displacements. First, a plant may be closed via absorption into (or merging with) another plant. In the registers we can identify “closure via absorption” (defined as at least 30% of the employees of the closing plant obtaining employment at the absorbing plant). We therefore modify the definition of displacement to be more restrictive: In case of closure via absorption, workers initially employed in the closing firm are not considered as displaced.

Secondly, even if a plant continues, the number of employees may fall because part of the plant and its employees are separated out from that plant to another plant. In the registers this “not-identical continuation” of a plant can be identified. (It is defined on the basis that at least two workers are separated out to another plant). We therefore also in this case modify the definition of displacement to be more restrictive: In case of “not-identical continuation” of a plant, workers initially employed at the plant are not considered as displaced even if they have separated from the plant and employment is reduced by at least 30%.¹⁴

To avoid that some persons would be included in the control group in one year and in the treatment group in a later year, we restrict the control group to only include persons who are not displaced in any year in the sample period. Thus, for each year t in the sample 1986-1997, the control group of those displaced in year t consists of workers who were in period $t-1$ employed full-time at private sector plants with at least 6 employees and who were not displaced in any year in the sample period.

¹⁴ Workers excluded from the treatment group of a given year due to these two modifications are not absorbed in the control group of that year, but are excluded from both groups.

Appendix B. Summary statistics

Table B.1 Summary statistics for men for base years 1986 and 1992

Variable	Men 1986				Men 1992			
	Treated		Controls		Treated		Controls	
	Mean	Std Dev						
Hospitalization (1)	0.02	0.14	0.03	0.17	0.02	0.13	0.02	0.15
Number prev. admissions (2)	0.34	0.89	0.29	0.85	0.31	0.77	0.29	0.86
Duration prev. admissions (3)	2.26	7.60	1.88	7.69	1.46	4.56	1.51	6.43
Age 20-29	0.42	0.49	0.27	0.44	0.40	0.49	0.28	0.45
Age 30-39	0.29	0.45	0.29	0.46	0.23	0.42	0.26	0.44
Age 40-49	0.18	0.38	0.23	0.42	0.22	0.41	0.27	0.45
Age 50-63	0.12	0.32	0.20	0.40	0.16	0.37	0.18	0.38
Manufacturing	0.28	0.45	0.37	0.48	0.36	0.48	0.40	0.49
Services	0.23	0.42	0.20	0.40	0.17	0.37	0.20	0.40
Construction	0.21	0.41	0.12	0.32	0.23	0.42	0.10	0.30
Infrastructure	0.08	0.27	0.10	0.30	0.08	0.28	0.13	0.34
Financial services	0.10	0.30	0.12	0.32	0.12	0.33	0.13	0.34
Other industries	0.10	0.30	0.10	0.30	0.04	0.18	0.04	0.21
No further education	0.36	0.48	0.36	0.48	0.34	0.47	0.31	0.46
Vocational education	0.52	0.50	0.50	0.50	0.52	0.50	0.52	0.50
College education	0.03	0.17	0.03	0.18	0.03	0.16	0.04	0.20
Bachelor degree	0.04	0.20	0.06	0.23	0.05	0.21	0.07	0.26
Master's degree	0.02	0.14	0.03	0.17	0.03	0.17	0.04	0.20
Full time insured (t-2)	0.89	0.32	0.82	0.39	0.88	0.32	0.84	0.37
No unempl. Insurance (t-2)	0.14	0.35	0.20	0.40	0.19	0.39	0.20	0.40
Experience (years/100)	0.11	0.06	0.14	0.07	0.14	0.08	0.16	0.08
Tenure <1 year	0.48	0.50	0.21	0.41	0.40	0.49	0.18	0.38
Tenure 1 year	0.17	0.37	0.13	0.34	0.16	0.36	0.12	0.33
Tenure 2 years	0.09	0.29	0.08	0.28	0.10	0.30	0.10	0.30
Tenure 3 years	0.06	0.23	0.07	0.26	0.08	0.27	0.09	0.28
Tenure 4 years	0.04	0.20	0.07	0.25	0.05	0.22	0.07	0.26
Tenure 5 years or more	0.17	0.37	0.43	0.50	0.21	0.41	0.44	0.50
Couple	0.62	0.49	0.75	0.43	0.64	0.48	0.72	0.45
Unemployment (t-2) (4)	0.15	0.26	0.06	0.17	0.10	0.22	0.03	0.12
Unemployment (t-3) (4)	0.17	0.28	0.07	0.19	0.10	0.22	0.03	0.13
Unemployment (t-4) (4)	0.17	0.28	0.07	0.20	0.08	0.19	0.03	0.12
Unemployment (t-5) (4)	0.15	0.27	0.07	0.19	0.06	0.16	0.03	0.11
Sickness benefits (days) (t-2)	4.20	21.27	2.04	15.20	4.60	20.94	2.62	16.12
#observations	1,892		39,276		1,737		38,834	

Note: All variables except 'hospitalization' are lagged at least 1 year relative to the base year. The notation '(t-j)' after a variable signifies that it is lagged j years relative to the base year. (1) Dummy for admission to a hospital for stress-related diagnoses 1-4 years after the base year (outcome variable). (2) Admissions to a hospital for all diagnoses (except birth and a few other diagnoses not related to illness), 2-5 years prior to the base year. (3) Duration (in days) of previous admissions to hospital, 2-5 years prior to the base year. (4) Fraction of the year the person is unemployed.

Table B.2 Summary statistics for women for base years 1986 and 1992

Variable	Women 1986				Women 1992			
	Treated		Controls		Treated		Controls	
	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Hospitalization (1)	0.01	0.09	0.01	0.12	0.01	0.10	0.01	0.13
Number of prev. admissions (2)	0.56	0.96	0.47	0.98	0.47	1.00	0.44	0.99
Duration prev. admis. (days) (3)	3.33	7.79	2.78	7.74	2.31	6.50	2.14	6.80
Age 20-29	0.48	0.50	0.39	0.49	0.45	0.50	0.37	0.48
Age 30-39	0.28	0.45	0.29	0.45	0.25	0.43	0.26	0.44
Age 40-49	0.15	0.35	0.19	0.40	0.21	0.41	0.25	0.43
Age 60-63	0.10	0.30	0.12	0.33	0.09	0.29	0.12	0.32
Manufacturing	0.34	0.48	0.37	0.48	0.33	0.47	0.36	0.48
Services	0.38	0.49	0.25	0.43	0.32	0.47	0.25	0.43
Construction	0.02	0.15	0.02	0.14	0.05	0.21	0.03	0.16
Infrastructure	0.04	0.19	0.04	0.20	0.05	0.22	0.10	0.30
Financial services	0.14	0.35	0.24	0.43	0.22	0.41	0.24	0.43
Other industries	0.08	0.27	0.08	0.27	0.04	0.19	0.02	0.15
No further education	0.55	0.50	0.46	0.50	0.43	0.49	0.38	0.49
Vocational education	0.37	0.48	0.44	0.50	0.44	0.50	0.49	0.50
College education	0.03	0.16	0.04	0.20	0.04	0.19	0.05	0.21
Bachelor degree	0.02	0.13	0.02	0.13	0.05	0.21	0.03	0.17
Master's degree	0.02	0.13	0.02	0.15	0.02	0.14	0.03	0.16
Full time insured (t-2)	0.86	0.34	0.83	0.38	0.90	0.31	0.85	0.36
No unempl. Insurance (t-2)	0.19	0.39	0.19	0.39	0.18	0.38	0.17	0.38
Experience (years/100)	0.08	0.05	0.10	0.06	0.10	0.07	0.12	0.07
Tenure <1 year	0.40	0.49	0.24	0.43	0.40	0.49	0.21	0.41
Tenure 1 year	0.19	0.39	0.14	0.35	0.19	0.39	0.14	0.35
Tenure 2 years	0.08	0.28	0.09	0.28	0.11	0.31	0.11	0.31
Tenure 3 years	0.08	0.27	0.07	0.26	0.07	0.25	0.10	0.29
Tenure 4 years	0.04	0.21	0.07	0.25	0.06	0.23	0.08	0.27
Tenure 5 years or more	0.20	0.40	0.39	0.49	0.19	0.39	0.37	0.48
Couple	0.68	0.47	0.72	0.45	0.66	0.47	0.73	0.45
Unemployment (t-2) (4)	0.18	0.30	0.09	0.22	0.14	0.26	0.06	0.17
Unemployment (t-3) (4)	0.16	0.27	0.10	0.23	0.12	0.24	0.06	0.18
Unemployment (t-4) (4)	0.14	0.26	0.09	0.21	0.12	0.25	0.06	0.18
Unemployment (t-5) (4)	0.14	0.27	0.08	0.21	0.10	0.21	0.06	0.17
Sickness benefits (days) (t-2)	3.73	22.68	2.42	17.90	4.12	19.00	2.97	17.41
# observations	827		15,395		925		18,792	

Note: See Table B.1

Appendix C. Estimated densities of the linear predictors of displacement risk

Figure C.1 Density of the linear predictor of displacement risk for men, 1986

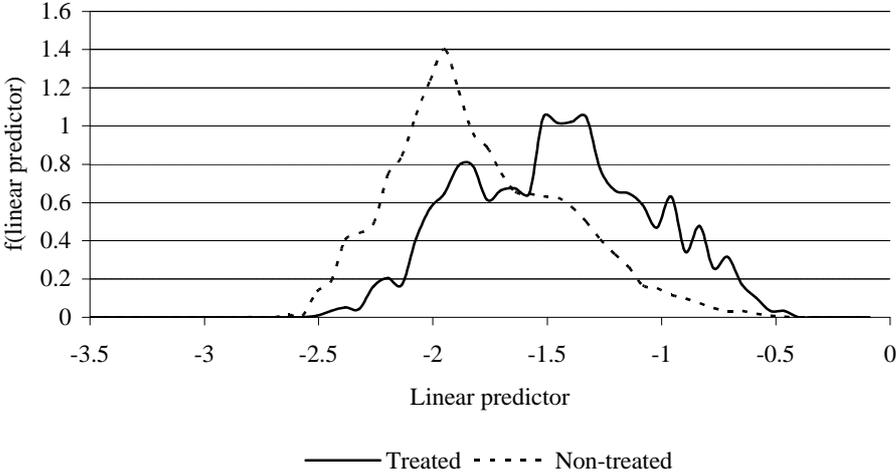


Figure C.2 Density of the linear predictor of displacement risk for men, 1992

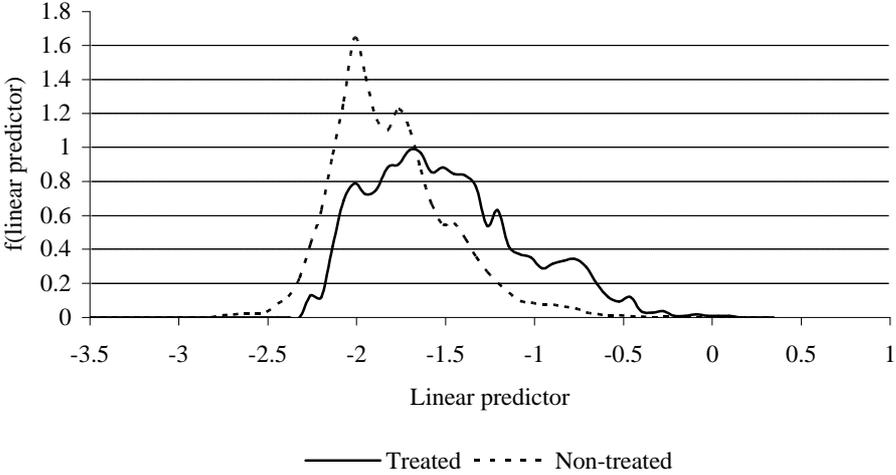


Figure C.3 Density of the linear predictor of displacement risk for women, 1986

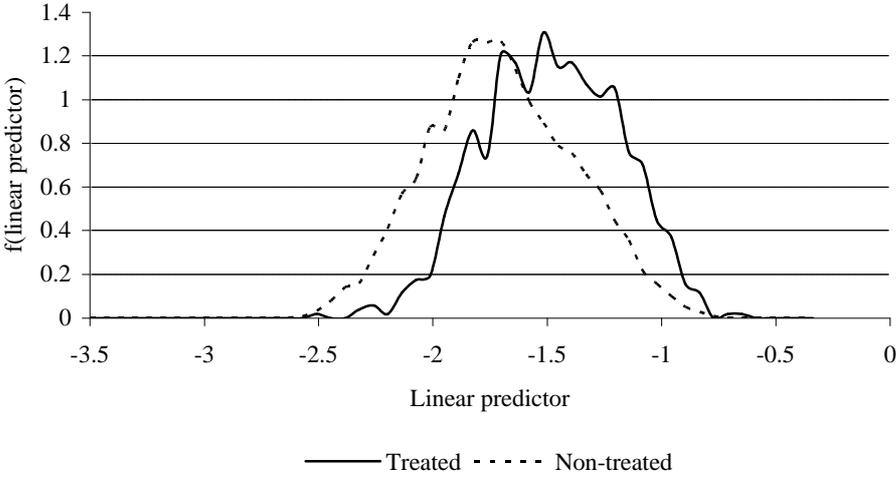


Figure C.4 Density of the linear predictor of displacement risk for women, 1992

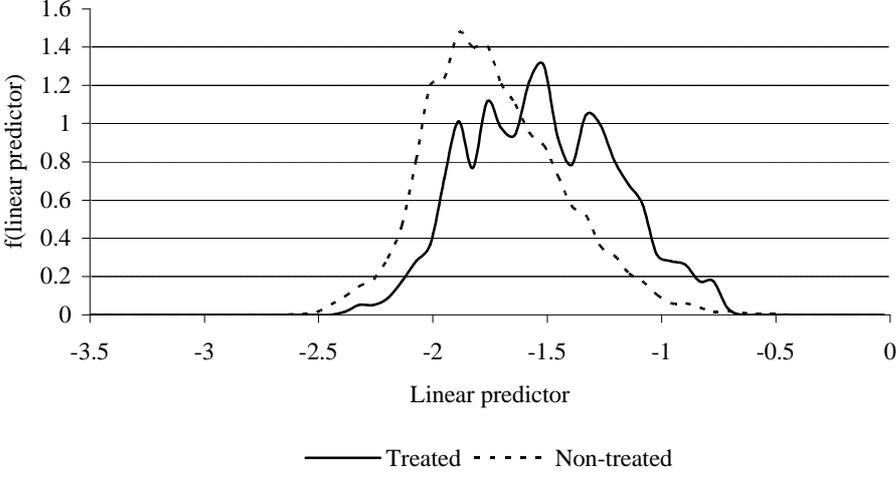


Table C.1 Matching quality in terms of imbalance of individual characteristics. Two-sample t-tests and standardised differences (% bias), men, base years 1986 and 1992

	Men							
	1986				1992			
	Matched controls	Displaced		% bias	Matched controls	Displaced		% bias
Mean	mean	Two-sample t-test		mean	mean	Two-sample t-test		
Age 30-39	0.28	0.29	-0.14	0.47	0.23	0.23	0.12	-0.41
Age 40-49	0.17	0.18	-0.39	1.26	0.21	0.22	-0.75	2.54
Age 50-63	0.13	0.12	0.79	-2.58	0.15	0.16	-0.56	1.91
Couple	0.61	0.62	-0.40	1.30	0.62	0.64	-0.84	2.86
Manufacturing	0.28	0.28	-0.11	0.35	0.36	0.36	-0.25	0.84
Construction	0.19	0.21	-1.67	5.42	0.23	0.23	-0.20	0.69
Infrastructure	0.08	0.08	-0.06	0.19	0.09	0.08	0.61	-2.04
Financial services	0.11	0.10	0.70	-2.26	0.11	0.12	-1.17	3.97
Other industries	0.10	0.10	0.00	0.00	0.03	0.04	-0.57	1.92
Voc. Education	0.53	0.52	0.42	-1.38	0.52	0.52	0.48	-1.62
College education	0.03	0.03	-0.39	1.29	0.02	0.03	-0.80	2.67
Bachelor degree	0.04	0.04	0.00	0.00	0.06	0.05	1.22	-4.12
Master's degree	0.02	0.02	0.78	-2.53	0.02	0.03	-1.05	3.55
No unempl. Insurance	0.15	0.14	0.78	-2.52	0.17	0.19	-1.01	3.46
Experience	0.11	0.11	-0.10	0.39	0.13	0.14	-1.31	4.47
Tenure 1 year	0.18	0.17	1.20	-3.90	0.16	0.16	0.14	-0.48
Tenure 2 years	0.09	0.09	0.39	-1.28	0.09	0.10	-1.16	3.94
Tenure 3 years	0.06	0.06	-0.07	0.23	0.09	0.08	0.67	-2.29
Tenure 4 years	0.05	0.04	0.86	-2.77	0.05	0.05	-0.16	0.53
Tenure 5 years or more	0.15	0.17	-1.39	4.50	0.22	0.21	0.49	-1.68
Unemployment (t-2)	0.16	0.15	0.32	-1.01	0.10	0.10	0.27	-0.94
Unemployment (t-3)	0.18	0.17	1.19	-3.89	0.09	0.10	-0.38	1.27
Unemployment (t-4)	0.17	0.17	-0.22	0.70	0.08	0.08	0.69	-2.31
Unemployment (t-5)	0.15	0.15	-0.57	1.83	0.06	0.06	0.50	-1.72
Number of admissions	0.31	0.34	-1.10	3.59	0.31	0.31	0.07	-0.21
Duration of admissions	2.46	2.26	0.55	-1.79	1.43	1.46	-0.19	0.65
Sickness benefit (days)	4.22	4.20	0.03	-0.09	5.85	4.61	1.39	-4.71
Propensity score	-1.49	-1.49	0.00	0.00	-1.53	-1.53	-0.00	0.00
# of observations	1,892	1,892			1,734	1,734		

Note: “% bias” is the standardised difference in per cent:

$$100(\bar{x}_{treated} - \bar{x}_{mcontrols}) / [(s^2_{treated} + s^2_{mcontrols}) / 2]^{1/2}$$

Table C.2 Matching quality in terms of imbalance of individual characteristics. Two-sample t-tests and standardised differences (% bias), women, base years 1986 and 1992

	Women							
	1986				1992			
	Matched controls	Displaced	Two-sample t-test	% bias	Matched controls	Displaced	Two-sample t-test	% bias
mean	mean			mean	mean			
Age 30-39	0.27	0.28	-0.50	2.44	0.26	0.25	0.69	-3.22
Age 40-49	0.16	0.15	0.62	-3.03	0.19	0.21	-0.76	3.51
Age 50-63	0.09	0.10	-0.33	1.64	0.10	0.09	0.32	-1.48
Couple	0.65	0.68	-1.41	6.91	0.65	0.66	-0.49	2.28
Manufacturing	0.36	0.34	0.52	-2.53	0.34	0.33	0.44	-2.05
Construction	0.02	0.02	0.00	0.00	0.05	0.05	0.32	-1.53
Infrastructure	0.03	0.04	-0.41	1.99	0.05	0.05	-0.11	0.49
Financial services	0.14	0.14	-0.14	0.70	0.21	0.22	-0.17	0.79
Other industries	0.08	0.08	-0.09	0.45	0.03	0.04	-0.51	2.40
Voc. Education	0.39	0.37	0.61	-2.99	0.48	0.44	1.40	-6.51
College education	0.02	0.03	-0.46	2.28	0.03	0.04	-0.78	3.60
Bachelor degree	0.01	0.02	-0.60	2.97	0.04	0.05	-1.16	5.38
Master's degree	0.02	0.02	0.54	-2.59	0.01	0.02	-0.90	4.21
No unempl. Insurance	0.19	0.19	0.06	-0.31	0.17	0.18	-0.24	1.14
Experience	0.08	0.08	0.04	-0.04	0.10	0.10	0.20	-0.87
Tenure 1 year	0.17	0.19	-0.64	3.14	0.20	0.19	0.77	-3.55
Tenure 2 years	0.08	0.08	-0.36	1.76	0.10	0.11	-0.45	2.14
Tenure 3 years	0.08	0.08	-0.54	2.69	0.06	0.07	-0.38	1.77
Tenure 4 years	0.04	0.04	-0.48	2.40	0.05	0.06	-0.73	3.36
Tenure 5 years or more	0.20	0.20	0.00	0.00	0.19	0.19	0.30	-1.37
Unemployment (t-2)	0.19	0.18	0.67	-3.28	0.14	0.14	-0.18	0.85
Unemployment (t-3)	0.17	0.16	0.23	-1.14	0.11	0.12	-0.30	1.36
Unemployment (t-4)	0.14	0.14	0.17	-0.86	0.12	0.12	-0.06	0.29
Unemployment (t-5)	0.14	0.14	0.03	-0.16	0.09	0.10	-0.38	1.80
Number of admissions	0.50	0.56	-1.02	5.00	0.42	0.47	-1.02	4.75
Duration of admissions	2.74	3.33	-1.66	8.19	1.90	2.31	-1.37	6.37
Sickness benefit (days)	3.83	3.73	0.09	-0.46	5.29	4.12	1.03	-4.78
Propensity score	-1.52	-1.52	0.01	0.02	-1.55	-1.55	0.00	0.01
# of observations	827	827			925	925		

Note: “%bias” is the standardised difference in per cent:

$$100(\bar{x}_{treated} - \bar{x}_{mcontrols}) / [(s^2_{treated} + s^2_{mcontrols}) / 2]^{1/2}$$

Table C.3 Matching quality in terms of imbalance of individual characteristics. (Matching is conditioned on the propensity score and age). Two-sample t-tests and standardised differences (% bias), men, base years 1986 and 1992

	Men							
	1986				1992			
	Matched controls	Displaced	Two-sample t-test	%bias	Matched controls	Displaced	Two-sample t-test	%bias
Age 30-39	0.29	0.29	0.00	0.00	0.23	0.23	0.00	0.00
Age 40-49	0.18	0.18	0.00	0.00	0.22	0.22	0.00	0.00
Age 50-63	0.12	0.12	0.00	0.00	0.16	0.16	0.00	0.00
Couple	0.62	0.62	-0.17	0.55	0.64	0.64	0.04	-0.12
Manufacturing	0.28	0.29	-0.40	1.30	0.36	0.36	-0.18	0.60
Construction	0.21	0.21	0.00	0.00	0.24	0.23	0.76	-2.60
Infrastructure	0.08	0.08	0.18	-0.58	0.08	0.08	-0.82	2.76
Financial services	0.10	0.10	-0.06	0.18	0.13	0.12	0.52	-1.76
Other industries	0.10	0.10	0.17	-0.54	0.03	0.03	-0.68	2.28
Voc. Education	0.52	0.52	-0.23	0.74	0.51	0.52	-0.48	1.62
College education	0.03	0.03	1.03	-3.34	0.03	0.03	0.53	-1.79
Bachelor degree	0.04	0.04	-0.67	2.20	0.06	0.05	0.85	-2.88
Master's degree	0.02	0.02	0.57	-1.83	0.03	0.03	0.57	-1.98
No unempl. Insurance	0.15	0.15	0.05	-0.15	0.18	0.19	-0.44	1.50
Experience	0.11	0.11	0.05	-0.16	0.14	0.14	0.22	-0.84
Tenure 1 year	0.16	0.17	-0.80	2.59	0.16	0.16	0.28	-0.95
Tenure 2 years	0.10	0.09	0.84	-2.72	0.11	0.10	0.40	-1.34
Tenure 3 years	0.06	0.06	0.15	-0.46	0.07	0.08	-0.83	2.83
Tenure 4 years	0.05	0.04	0.24	-0.77	0.05	0.05	0.08	-0.27
Tenure 5 years or more	0.16	0.17	-0.31	1.00	0.22	0.21	0.34	-1.13
Unemployment (t-2)	0.16	0.17	-0.57	1.85	0.08	0.07	0.27	-0.88
Unemployment (t-3)	0.14	0.15	-0.63	2.04	0.05	0.06	-0.20	0.67
Unemployment (t-4)	0.32	0.33	-0.42	1.37	0.32	0.30	0.64	-2.18
Unemployment (t-5)	2.51	2.23	0.71	-2.32	1.63	1.45	0.92	-3.13
Number of admissions	0.14	0.15	-0.71	2.32	0.09	0.10	-0.57	1.95
Duration of admissions	0.17	0.17	-0.34	1.08	0.09	0.09	-0.74	2.54
Sickness benefit (days)	4.54	3.96	0.77	-2.52	5.32	4.50	1.01	-3.45
Propensity score	-1.49	-1.49	0.02	0.00	-1.54	-1.54	-0.08	0.00
# of observations	1881	1881			1724	1724		

Note: “%bias” is the standardised difference in per cent:

$$100(\bar{x}_{treated} - \bar{x}_{mcontrols}) / [(s^2_{treated} + s^2_{mcontrols}) / 2]^{1/2}$$

Table C.4 Matching quality in terms of imbalance of individual characteristics. (Matching is conditioned on the propensity score and age). Two-sample t-tests and standardised differences (% bias), women, base years 1986 and 1992

	Women							
	1986				1992			
	Matched controls	Displaced	Two-sample t-test	%bias	Matched controls	Displaced	Two-sample t-test	%bias
Age 30-39	0.28	0.28	0.00	0.00	0.25	0.25	0.00	0.00
Age 40-49	0.15	0.15	0.00	0.00	0.21	0.21	0.00	0.00
Age 50-63	0.10	0.10	0.00	0.00	0.09	0.09	0.00	0.00
Couple	0.71	0.68	1.13	-5.56	0.66	0.66	-0.18	0.92
Manufacturing	0.37	0.34	1.29	-6.35	0.32	0.34	-0.90	4.41
Construction	0.02	0.02	-0.86	4.22	0.04	0.04	-0.44	2.16
Infrastructure	0.04	0.04	0.13	-0.64	0.05	0.05	0.00	0.00
Financial services	0.13	0.14	-1.02	5.01	0.25	0.22	1.57	-7.72
Other industries	0.09	0.08	1.15	-5.65	0.04	0.03	0.46	-2.30
Voc. Education	0.40	0.38	0.92	-4.51	0.45	0.44	0.31	-1.53
College education	0.02	0.03	-1.13	5.60	0.04	0.04	0.45	-2.24
Bachelor degree	0.02	0.02	0.36	-1.82	0.05	0.05	0.60	-2.96
Master's degree	0.02	0.02	-0.19	0.92	0.01	0.02	-1.24	6.11
No unempl. Insurance	0.20	0.19	0.31	-1.55	0.18	0.18	0.17	-0.85
Experience	0.08	0.08	-0.44	2.05	0.10	0.10	-0.71	3.44
Tenure 1 year	0.21	0.19	1.00	-4.90	0.19	0.19	0.34	-1.66
Tenure 2 years	0.11	0.09	1.51	-7.44	0.10	0.11	-0.73	3.60
Tenure 3 years	0.09	0.08	0.70	-3.45	0.08	0.07	1.02	-5.02
Tenure 4 years	0.04	0.05	-0.87	4.31	0.06	0.06	0.00	0.00
Tenure 5 years or more	0.20	0.21	-0.31	1.51	0.19	0.19	-0.11	0.56
Unemployment (t-2)	0.16	0.17	-1.04	5.12	0.14	0.13	0.17	-0.82
Unemployment (t-3)	0.16	0.16	0.07	-0.35	0.12	0.11	0.54	-2.67
Unemployment (t-4)	0.13	0.14	-0.06	0.29	0.12	0.12	-0.07	0.31
Unemployment (t-5)	0.14	0.13	0.45	-2.18	0.09	0.09	-0.23	1.10
Number of admissions	0.59	0.55	0.84	-4.11	0.48	0.47	0.21	-1.06
Duration of admissions	3.41	3.26	0.37	-1.82	2.27	2.33	-0.17	0.83
Sickness benefit (days)	3.19	3.33	-0.15	0.74	5.37	4.14	1.10	-5.40
Propensity score	-1.52	-1.52	0.06	0.28	-1.56	-1.56	0.03	0.14
# of observations	820	820			920	920		

Note: “%bias” is the standardised difference in per cent:

$$100(\bar{x}_{treated} - \bar{x}_{mcontrols}) / [(s^2_{treated} + s^2_{mcontrols}) / 2]^{1/2}$$

Appendix D. Diagnoses used as health outcomes

Table D.1. Diseases included in the analyses: Description and definition according to the International Classification of Diseases, 8th revision (used until 1993 in Danish hospitals) and 10th revision (used from 1994 onwards).

Description	ICD 8 codes	ICD10 codes
Diseases of the digestive system: gastric catarrh, gastric ulcer, etc.	530.90-92, 530.95, 530.98, 531-537	K209, K222-K223, K250-K257, K259-K267, K269-K277, K279-K287, K289-K299, K309-K312, K314-K316, K318-K319, K902
High blood pressure	400-404	I109-I110, I119-I120, I129-I132, I139, I150-I151, I158-I159
Other diseases of the circulatory system	410-414, 427	I200, I201, I208-I214, I219-I221, I228-I236, I238, I240, I241, I248-I256, I258-I259, I440-I447, I450-I456, I458-I461, I469-I472, I479, I489-I495, I498-I499, I500, I501, I509, I512-I513, R001