AKF Working paper

Martin Browning and Eskil Heinesen

Effect of Job Loss Due to Plant Closure on Mortality and Hospitalisation

The publication *Effect of Job Loss Due to Plant Closure on Mortality and Hospitalisation* is available at www.akf.dk

AKF, Danish Institute of Governmental Research
Købmagergade 22, DK-1150 Copenhagen K
Phone: +45 43 33 34 00
Fax: +45 43 33 34 01
E-mail: akf@akf.dk
Internet http://www.akf.dk

Publisher: AKF
ISBN: 978-87-7509-976-4
I:\08 SEKRETARIAT\FORLAGET\ESH\2640\2640_EFFEKCT_JOB_LOSS_OMS.DOCX
April 2011(1)

AKF, Danish Institute of Governmental Research
Carries out and reports social science research of interest to the public sector and in particular to regions and local governments.

Effect of Job Loss Due to Plant Closure on Mortality and Hospitalisation

Martin Browning

Department of Economics, University of Oxford, Manor Road, Oxford OX1 3UQ, England; Martin.Browning@economics.ox.ac.uk

Eskil Heinesen

AKF, Danish Institute of Governmental Research, Købmagergade 22, DK-1150 Copenhagen K, Denmark; esh@akf.dk

April 2011

Abstract

We investigate whether job loss due to plant closure causes an increased risk of (cause-specific) mortality and hospitalisation for full-time male workers having strong labour-market attachment. We use unique administrative data: A panel of *all* persons in Denmark in the period 1980-2006, containing full records on demographics, health and work status, and a link from workers to plants. We use propensity score weighting combined with non-parametric duration analysis. We find that job loss increases the risk of overall mortality and mortality caused by circulatory disease; of suicide and suicide attempts; and of death and hospitalisation due to traffic accidents, alcohol-related disease and mental illness.

Keywords: Unemployment, Job displacement, Plant closure, Health, Mortality, Hospitalisation, Propensity score weighting, Duration analysis

JEL-Code: C23, I18, J21

We thank Jeffrey Smith, Søren Leth-Petersen, Bo Honoré and Nabanita Datta Gupta, seminar participants at CAM, University of Copenhagen, and University of Aarhus, and participants at the SOLE-EALE Conference in London 2010 and Econometric Society World Congress in Shanghai 2010, for their helpful comments. The Danish Council for Independent Research – Social Sciences is acknowledged with gratitude for its financial support.

1. Introduction

It is well documented that unemployment is associated with poor health; see, for instance, the survey of Kasl & Jones (2000). The determinants of this correlation are far from fully understood, however. Many studies have considered whether there is a causal effect of unemployment on health by using the downsizing or closure of firms or plants as a quasi-experiment; see e.g. Morris & Cook (1991) for a review. In recent years this approach has been applied using large administrative datasets; see Browning, Danø & Heinesen (2006), Sullivan & Wachter (2009), and Eliason & Storrie (2009a, 2009b, 2010). The present paper also follows this approach.

We focus on the effect of displacement due to plant closure on the risk of overall mortality and cause-specific mortality and hospitalisation, and we investigate its effects on a wide range of diseases. The population studied is full-time male workers with a strong labour-market attachment. We use unique administrative data: A sample of *all* persons in Denmark in the period 1980-2006. We identify workers who lost their job due to closure of plants in the private sector in the period 1986-2002, and we identify a control group of workers. The data contain very full records on demographics, health and work status for each person, and include a link from every working person to a plant. Health outcomes are based on causes of death and diagnoses from somatic and psychiatric hospital departments. We use propensity score weighting, combined with non-parametric duration analysis, to estimate the effects of plant closure on death and hospitalisation.

Five new contributions emerge from this study. First, we use a much larger dataset than any previous study, which is essential when considering rare outcomes such as death or hospitalisation due to specific causes. For instance, point estimates in Eliason & Storrie (2009a) of the effect of job loss on the risk of death of male workers from circulatory disease and cancer in the first four years after plant closure are large (about 20% and 40%, respectively), but they are imprecise and not significantly different from zero, even though circulatory disease and cancer are the two most important causes of death. As we have a much larger dataset, our estimates are more precise. Second, we do not impose parametric restrictions when analysing the duration to death (or hospitalisation), and we are able to analyse the time pattern of effects more precisely. Third, we investigate the effects of job loss on mental disorders. Hospitalisation for mental disorders, and mental disorders as a secondary cause of death, are rare events, but our analysis finds strong, statistically significant effects. Fourth, this study is the first to analyse the effects of job displacement due to plant closure on hospitalisation for a wide range of diagnoses, including both fatal and non-fatal events. We also study its effects on 'combined death and hospitalisation outcomes', such as duration to

suicide or a suicide attempt. Fifth, we investigate how the effects of job loss depend on local labour-market conditions.

An earlier paper (Browning, Danø & Heinesen 2006), which also used Danish administrative data, estimated the effect of displacement on hospitalisation due to stress-related diseases of the circulatory and digestive systems, but found no significant effect. In the present paper we study its effects on cause-specific mortality and hospitalisation for many different categories of diagnosis. Other important differences from Browning, Danø & Heinesen (2006) are that the present paper focuses exclusively on workers with a strong labour-market attachment and on displacements due to plant closure (not just downsizing), and we use a much larger dataset.

Using Swedish data, Eliason & Storrie (2009a) find that in the first four years after plant closure males have an increased risk of overall mortality and mortality from external causes, including suicide, and from alcohol-related diseases. They find no effects after four years after displacement. Point estimates indicate rather large increases in risk of death from circulatory disease, and especially from cancer, in the first four years after plant closure, but these effects are not statistically significant. Eliason & Storrie (2009b) analyse effects on hospitalisation, but only for non-fatal events, i.e. their analysis does not include serious events such that patients die in hospital. Upon restricting the effect to be constant within their follow-up period of 12 years after plant closure, they find no effect on circulatory disease (myocardial infarction and stroke), but significantly increased risk of hospitalisation of displaced workers due to alcohol-related diseases, traffic accidents and self-harm. Eliason & Storrie (2010) use the same dataset and methods to study the effects of job loss on hospitalisation for mental illness; they find no effects for males. Analysing the effects of job loss due to downsizing on overall mortality in Pennsylvania, Sullivan & Wachter (2009) find significantly increased risk of death for displaced workers; the effects are largest in the first years after displacement, but even 20 years after displacement the estimated increase in annual death hazards is 10%-15%.

In the present analysis we find that job loss increases the risk of overall mortality and death from circulatory disease; suicide and suicide attempts; and death and hospitalisation due to traffic accidents, alcohol-related disease and mental illness. We find no effect on mortality or hospitalisation due to cancer, and no effect on hospitalisation due to circulatory diseases. The risk of overall mortality is 84% higher in the year of displacement, 36% higher 1-4 years after displacement, 17% higher 1-10 years after displacement and 10% higher 1-20 years after displacement. Thus, the effects are largest just after plant closure, but they remain statistically significant even after 20 years, indicating that short-term effects do not 'just' represent a speeding-up of deaths that would have happened a few years later anyway. The same pattern

applies for deaths from circulatory diseases, but the effects are larger. Job displacement significantly increases the risk of hospitalisation and death due to alcohol-related diseases in both the short and long term. The effect on suicide and suicide attempts is very strong in the first three years after displacement, but is insignificant in the long term, whereas the effects on hospitalisation or death due to traffic accidents are smaller in the short term, but significant in the long term. The effect on hospitalisation for mental disorders is large in the short term, and remains clearly significant in the long term. There is a very large short-term effect for mental diseases as a (secondary) cause of death, but no significant long-term effect. Effects on mortality are larger when the local unemployment rate is high. In the concluding section we compare our findings with earlier studies.

In Section 2 we discuss the choice of health outcomes and hypotheses on the effect of job loss on these outcomes. Section 3 considers the econometric methods used. Section 4 describes the data and the identification of treatment and control groups. Sections 5 and 6 present the results of estimation for the propensity score and health outcomes, respectively. Conclusions are stated in Section 7.

2. Reasons why job displacement may affect health

Involuntary job loss may have effects on health outcomes, both directly and indirectly through negative economic and social consequences of job loss. Job losers may face large declines in earnings in both the short and long run (see, e.g., Ruhm 1991; Jacobson, LaLonde & Sullivan 1993; Kuhn 2002; Eliason & Storrie 2006; Hijzen, Upward & Wright 2010), and this may give rise to stress. Social and psychological consequences of job loss, such as loss of work relationships, of self-esteem, sense of control, meaning in life, and time structure may also have serious negative health effects (see e.g., Pearlin et al. 1981; Jahoda 1982; Warr 1987). These consequences are presumably most serious for those who become unemployed for a longer duration after displacement, but loss of work relationships and self-esteem may also be important for workers who find reemployment in an inferior job. Negative economic, social and psychological consequences of job loss may increase vulnerability to other negative life events, and reduce constructive coping capabilities and self-control (e.g. Kessler, Turner & House 1987). Involuntary job loss is therefore likely to have negative effects on health through acute stress and potential chronic stress, and through negative long-term economic and social consequences.

Involuntary job loss may therefore be expected to have effects on stress-related diseases, in particular circulatory diseases; on mental diseases such as depression, psychoses and neuroses; on alcohol abuse, which may result in mental disorders (including diseases such as

alcohol poisoning, addiction syndrome, delirious abstinence, alcohol psychosis) and somatic diseases (including alcoholic disease of the liver and pancreas); on suicide and suicide attempts; and on traffic accidents.

The 'alcohol-related' diagnoses represent long-term consequences of alcohol abuse. Most of these diagnoses are related to very serious abuse or abuse over several years. Suicide and suicide attempts may be a consequence of displacement and unemployment through, for instance, depression. Traffic accidents may to some extent represent suicide attempts, or they may be a consequence of loss of self-control, e.g. in connection with alcohol abuse. Displacement is not expected to have significant effects on hospitalisation or death due to cancer, at least in the short run. We include cancer outcomes in our analysis because these are serious conditions, and one mechanism through which job loss might lead to cancer is through increased smoking. We investigate a wide range of mortality and hospitalisation outcomes, described in detail in Section 4.4.

3. Empirical methods

This paper investigates whether there is a causal effect of undergoing displacement as a result of plant closure on all-cause mortality and on cause-specific mortality, and on hospitalisation for different categories of diagnosis. We use propensity score weighting; see Hirano & Imbens (2001) and Wooldridge (2002, ch. 18.3). A great advantage of this method compared to, e.g., nearest neighbour matching based on the propensity score, is that it produces more stable results, especially when rare outcomes such as cause-specific mortality or hospitalisation are analysed; since all control group observations are used, the estimated effects are less sensitive to changes in specification of the propensity score function.

Denote displacement status by the dummy variable *D*, where *D*=1 if displaced (treated) and 0 otherwise, and let $Y(0)$ and $Y(1)$ denote potential health outcomes, where 0 denotes non-treatment and 1 denotes treatment. The observed outcome for an individual is $Y = DY(1) + (1 - D)Y(0)$. We wish to estimate the average treatment effect on the treated (ATT), i.e. the parameter α where

$$\alpha = E(Y(1) - Y(0)|D = 1) = E(Y(1)|D = 1) - E(Y(0)|D = 1) \quad (1)$$

The last term on the right-hand side is not observed. To estimate it we use propensity score weighting. We first estimate a propensity score function, using a logit model for the probability of displacement, where we include as explanatory variables the baseline characteristics which may affect both the probability of displacement in year *t*+1 (i.e. the

probability of working at the end of year *t* at a plant which will close within the following year), and potential outcomes. We then calculate weights for each observation in the control group:

$$v_i = \hat{p}_i / (1 - \hat{p}_i) \tag{2}$$

where $\hat{p}_i$ is the estimated value of the propensity score function for control group observation *i*. These weights for the control group sum in expectation to the number of observations in the treatment group, N_T, but they do not do so exactly in practice. They are therefore normalised:

$$w_i = v_i N_T / \sum_{i \in C} v_i \tag{3}$$

where *C* denotes the set of control group observations. The propensity score weighting (PSW) estimator of the ATT is then

$$\hat{\alpha} = \frac{1}{N_T} \sum_{i \in T} Y_i - \frac{1}{N_T} \sum_{i \in C} w_i Y_i \tag{4}$$

i.e. the difference between the observed average of Y among the observations in the displacement group less the weighted average of Y among the observations in the control group. We also estimate the ATT in the form of a relative risk, i.e. $r = E(Y(1)|D = 1) / E(Y(0)|D = 1)$, by

$$\hat{r} = \frac{1}{N_T} \sum_{i \in T} Y_i \; / \; \frac{1}{N_T} \sum_{i \in C} w_i Y_i \tag{5}$$

Instead of using the simple weighting estimator (4) (or (5)), we may correct for possible remaining covariate bias between the treatment and control groups using weighted regression, with weights equal to 1 for observations in the treatment group and equal to w_i for control group observations:

$$Y_i = \beta_0 + \alpha D_i + \beta_1 X_i + \beta_2 (X_i - \bar{X}_1) D_i + \varepsilon_i \tag{6}$$

where α is the parameter of interest (the ATT), X_i is a set of covariates which may be a subset of the covariates entering the propensity score function, $\bar{X}_1$ is the average of X_i for the displacement group, and ε_i is an error term.

If we do not adjust for possible remaining covariate bias in the second step (i.e. *Y* is regressed on *D* and a constant), the estimate of ATT in (6) is equivalent to that in (4).

We consider a wide range of different health outcomes in terms of cause-specific mortality and hospitalisation, and for each of these we conduct a non-parametric duration analysis so as to compare the cumulative hazard rates for the displacement and weighted control groups at different durations after displacement.[1]

4. Data

4.1 Register data

We use Danish administrative register data. In Denmark all residents have a personal number which is used for administrative purposes to record activities such as education, hospitalisation, employment status, interactions with the welfare system, income, and residence. This information is collected centrally by Statistics Denmark and the Danish National Board of Health which makes these data available for statistical and research purposes. Our sample comprises *all* persons in Denmark in the period 1980-2006. We have data on diagnoses from all hospital departments (both somatic and psychiatric) and data on mortality and cause of death.

The dataset contains variables connecting individuals to plants (if they are at work), and data for plants, such as the number of employees (recorded at the end of November each year), the status of the plant one year ahead (e.g. continuing or closed), and the firm to which the plant belongs.

4.2 Definition of plant closure

We identify plant closures in the private sector for the period 1986-2002. We restrict the analysis to single-plant firms in order to avoid complications due to the fact that some workers of closing plants may be transferred to other plants within the same firm.[2] It is not trivial to specify whether a plant is closed or continuing from one year to the next. For instance, it is important to avoid defining a plant as closed merely because its administrative registration number has changed due to a new owner; see e.g. Kuhn (2002). Fortunately, our administrative data allow us to avoid such problems. Details of how we define plant closure are set out in the Appendix.

---

[1] We use simple estimates of the standard errors (and confidence intervals) of the ATT in which the estimation of weights based on the propensity score estimation is ignored. We have checked that these estimates of standard errors are very similar to estimates based on the formulas in Hirano & Imbens (2001), which do take account of the first-step estimation of the propensity score.

[2] Huttunen, Møen & Salvanes (2010) find that workers at downsizing plants who transfer to another plant within the same firm do not face any loss of earnings.

The year a plant finally closes is not necessarily the most appropriate year to consider as the year of plant closure for our purposes. For instance, a plant may reduce its number of employees from 100 to 10 in year *t*, from 10 to 5 in year *t*+1, and from 5 to 0 in year *t*+2 (when it closes). In this case it is obvious that year *t* (and not *t*+2) should be defined as the year of plant closure. Accordingly, in this paper we define the year *t* of plant closure as *the year with the largest absolute reduction in the number of employees* (details are given in the Appendix).

Most plant closures happen rather quickly. For 62% of displaced workers in our data the year of final closure is identical to the year of plant closure as defined above; for 70% the closing plants downsize by at least 90% in the year of plant closure; for 92% downsizing is at least 50%. Furthermore, 85% of displaced workers leave the plant in the year of plant closure as defined above, and 92% within two years. In Section 6.4 we consider robustness checks regarding the definition of plant closure and displacement.

The data on plants – including the number of employees, the link between plants and employees and status variables concerning closure/continuation – are recorded at the end of November each year. We define the *base year* as the year prior to the year of plant closure defined above. For instance, those who are employed in November 1989 at plants which close in 1990 (strictly speaking between the end of November 1989 and the end of November 1990) are in the treatment group for base year 1989. Our analysis focuses on plants which close in the years 1986-2002 and employees at these plants in base years 1985-2001. We do not consider plant closures in the first five years of our data period, since we wish to condition on initial health status and labour-market attachment (see below).

4.3 Identification of treatment and control groups

The treatment group in our analysis covers employees in base years 1985-2001 at plants which close in the following year (according to the definition above). We identify treatment and control groups for each base year 1985-2001. Both groups of workers are defined by the following characteristics in the base year: They are males of age 20-60, they have at least one year of tenure, they are employed in a private sector single-plant firm with at least five employees, they are full-time employed, they had no unemployment for three or four years prior to the base year, and they are not self-employed either in the base year or in the three previous years. Thus, we focus on workers with a strong labour-market attachment. We allow workers to be unemployed for part of the base year and part of the two years prior to the base year. This is because plants which will eventually close down may be more inclined to lay off workers temporarily in the years prior to closure, so that part of the possible health effect of plant closure on employees may act through some degree of unemployment in the period prior

to the year of closure. A person can only be in the treatment group in one base year. Persons meeting the requirements for inclusion in the treatment group in more than one base year are defined to be in the treatment group in the first year only.

In order that employees be in the control group of base year *t*, we furthermore require that the plant is not downsizing (neither in the base year nor in the year after); that if they are employed in year *t* at a plant which will eventually close down, this will happen more than five years later; and that they are not in the treatment group in any year.

We end up with 33,070 persons in the treatment group and 629,902 observations in the control group. Table 4.1 shows the numbers in the two groups for each base year. There are relatively large numbers in the treatment group in the period 1986-92, reflecting the recession in the Danish economy in this period. The control group is very large compared to the treatment group; on average the number treated is only 5% of the total number of treated and controls.

Table 4.1. Numbers in the displacement and control groups by base year

Base year	# displaced	%	# controls	%	% displaced
1985	1,526	4.6	38,270	6.1	3.8
1986	2,489	7.5	30,917	4.9	7.5
1987	2,738	8.3	25,676	4.1	9.6
1988	2,214	6.7	27,973	4.4	7.3
1989	2,332	7.1	31,776	5.0	6.8
1990	2,509	7.6	29,554	4.7	7.8
1991	2,596	7.9	28,251	4.5	8.4
1992	2,707	8.2	27,163	4.3	9.1
1993	1,815	5.5	36,944	5.9	4.7
1994	1,538	4.7	49,237	7.8	3.0
1995	1,282	3.9	43,476	6.9	2.9
1996	1,295	3.9	44,814	7.1	2.8
1997	1,379	4.2	46,858	7.4	2.9
1998	2,114	6.4	43,824	7.0	4.6
1999	1,572	4.8	42,802	6.8	3.5
2000	1,747	5.3	43,425	6.9	3.9
2001	1,217	3.7	38,942	6.2	3.0
All	33,070	100.0	629,902	100.0	5.0

All workers who at the end of year *t* are working at a plant which closes in year *t*+1 and who meet the above criteria are considered displaced in year *t*+1, even if some of them may not actually separate from the plant in year *t*+1 (because some of the plants which close in year *t*+1 according to our definition may not close finally down until a few years later). In Section

6.4 we consider the sensitivity of our results to alternative definitions of the displacement group.

4.4 Mortality and hospitalisation outcomes

Since our base years are 1985-2001 and we have hospitalisation and mortality data up to 2006, we can follow health outcomes for at least five years after the base year for all persons in the displacement and control groups. We investigate a wide range of mortality and hospitalisation outcomes (precise definitions via the International Classification of Diseases are given in the Appendix, Table A1).[3] We first study all-cause mortality and four major causes of death: cancer, circulatory disease, external causes (including suicide and accidents), and all other causes. We also report mortality due to more specific causes, such as myocardial infarction or stroke, alcohol-related diseases, mental diseases, suicides and traffic accidents. When we analyse mortality by cause we consider both primary and secondary causes of death, i.e. some deaths may count both as, e.g., 'suicide' and 'mental disease'. The results are not changed in any significant way by restricting the analysis to primary causes of death, except for mental diseases, which may be a secondary cause of death, but almost never a primary cause. The category 'all other causes' covers deaths for which neither primary nor secondary causes are in the first three categories (cancer, circulatory or external).

We have data for all hospitalisations requiring in-patient care, at both somatic and psychiatric hospital departments. We investigate hospitalisation due to two broad categories of diagnoses, cancer and circulatory diseases, and also more specific categories corresponding to the specific causes of death discussed above. We do not report results for overall hospitalisation for any diagnosis, since this is a very broad measure which is dominated by many diagnoses which are not very serious and have negligible relation to labour-market careers.

Finally, we investigate three combinations of hospitalisation diagnoses and causes of death: Death or hospitalisation due to myocardial infarction or stroke; suicide or suicide attempt; and death or hospitalisation due to traffic accidents. These combinations are interesting because quite a few people who die from myocardial infarction, stroke, suicide or traffic accidents are not hospitalised (since they die before reaching the hospital).

[3] 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 and include detailed information on diagnoses, dates of admission and discharge, etc. for all admissions to somatic and psychiatric hospital departments in Denmark.

Table 4.2. Unadjusted incidence of (cause-specific) mortality and hospitalisation 1-4 years after base year for the displacement and control groups (cumulative hazard rates), and cumulative hazard ratio with lower and upper bounds of 95% confidence interval

	Cumulative	95% CI bounds		Cumulative hazards (%)		No. failures
	hazard ratio	Lower	Upper	Displaced	Controls	Displaced
Mortality						
All-cause mortality	1.54	1.37	1.73	1.368	0.888	449
Cancer	1.25	1.04	1.50	0.451	0.362	148
Circulatory diseases	1.82	1.51	2.20	0.600	0.329	197
External causes	1.68	1.24	2.27	0.219	0.131	72
All other causes	1.47	1.06	2.03	0.168	0.114	55
Myocardial infarction or stroke	1.90	1.46	2.46	0.329	0.173	108
Alcohol-related diseases	2.08	1.34	3.24	0.122	0.059	40
Mental diseases	1.48	0.90	2.45	0.070	0.047	23
Suicide	2.11	1.31	3.39	0.106	0.051	35
Traffic accidents	1.50	0.83	2.70	0.052	0.035	17
Hospitalisation						
Cancer	1.09	1.00	1.18	1.877	1.725	613
Circulatory diseases	1.06	1.01	1.13	4.159	3.908	1346
Myocardial infarction or stroke	1.16	1.05	1.29	1.325	1.142	433
Alcohol-related diseases	1.54	1.30	1.84	0.616	0.399	202
Mental diseases	1.48	1.30	1.68	1.076	0.727	352
Suicide attempts	1.78	1.26	2.53	0.171	0.096	56
Traffic accidents	1.15	0.97	1.37	0.494	0.428	162
Death or hospitalisation						
Myocardial infarction or stroke	1.20	1.09	1.33	1.462	1.218	478
Suicide or suicide attempts	1.79	1.34	2.39	0.253	0.141	83
Traffic accidents	1.18	1.01	1.39	0.540	0.456	177

The fourth and fifth columns of Table 4.2 show, for the different outcomes considered, cumulative hazard rates for mortality and hospitalisation 1-4 years after the base year for the treatment and control groups, respectively. Thus, if a person is hospitalised more than once for a given category of diagnoses, he counts as a single admission. That is, these two columns show the probability of mortality or hospitalisation for the different causes (diagnoses). The first column shows the cumulative hazard ratios (or relative risks), i.e. the cumulative hazard rate for the displacement group divided by the rate for the control group; columns 2 and 3 show lower and upper bounds of the 95% confidence intervals of the cumulative hazard ratios. These bounds are calculated as $r^{1\pm 1.96/t}$, where r is the cumulative hazard ratio

(relative risk) and *t* is the t statistic of the test of equal means in the two groups; see e.g. Daly & Bourke (2000).

The last column shows the number of displaced workers who died or were hospitalised due to the respective causes. Since the control group is about 20 times larger than the displacement group, the width of the confidence intervals is determined largely by the number of 'positive' outcomes for the displacement group. Table 4.2 is merely descriptive, since we have not adjusted for differences in baseline characteristics between the displacement and control groups. For all outcomes the cumulative hazard ratios indicate higher probabilities of death or hospitalisation for the displacement group (although not all ratios are significantly different from 1). In particular, Table 4.2 indicates large and significant differences between the displacement and control groups regarding all-cause mortality, death due to circulatory diseases (including myocardial infarction and stroke), external causes (including suicide) and alcohol-related diseases, and hospitalisation due to alcohol-related and mental diseases and suicide attempts. For brevity, the descriptive figures in Table 4.2 relate only to the incidence of the different health outcomes within four years of the base year, whereas the estimates of causal effects in Section 6 show that it is important to distinguish between short, medium and long-term effects.

4.5 Baseline health indicators

In the propensity score function we control for health 3-5 years prior to the base year. We use the incidence of hospitalisation due to nine different broad categories of diagnoses (from a standard categorisation called the 'S-list'), including the broad cancer and circulatory disease categories which we also investigate as outcomes;[4] in addition we control for two specific categories of diagnoses (also used as outcomes), namely myocardial infarction or stroke, and alcohol-related diseases. Descriptive statistics for baseline health indicators and other control variables are shown below. Although we do not control for lagged suicide attempts and traffic accidents in the propensity score, we check the 'balancing properties' with respect to these variables also (see below). We do not control for health in the base year or in the two preceding years, since some closing plants may be characterised by gradual downsizing or other problems that affect the work environment and health conditions of employees. Similarly, many other baseline variables are lagged (see Section 5.2). This appears to be important, since several studies find that displaced workers experience loss of earnings before displacement (see e.g. Jacobson, Lalonde & Sullivan 1993).

[4] Some categories of diagnoses from the S-list are ignored because they are clearly irrelevant, and a few categories are merged since they are very rare in our sample.

5. Estimation procedure and the propensity score

In this section we first discuss the estimation procedure, and then the estimation of the propensity score and balancing properties of the non-weighted and weighted samples.

5.1 Estimation procedure and empirical strategy

We use propensity score weighting. We estimate a logit propensity score function for each base year 1985-2001 and weight observations in the control group by their odds (calculated from their predicted propensity scores) so that the weighted number of control observations is equal to the number of displaced persons in each base year. We therefore control exactly for the distribution of treatment and control groups on base years, which is important since we measure health outcomes for more than five years after the base year, and when we do this the observations for some of the base years are right censored. For instance, when measuring outcomes six years after the base year, all observations for base year 2001 are right censored, and upon measuring outcomes 10 years after the base year, observations for base years 1997-2001 are right censored.

The displacement groups of the different base years are pooled into one, and similarly the weighted control groups are pooled. For the pooled sample we estimate the average treatment effect on the treated. We estimate non-parametric cumulative hazard functions (at different durations from the base year) for the treatment and control groups, respectively, and calculate their ratio, i.e. the cumulative hazard ratio or the relative risk, and its 95% confidence interval.

We do not control for any covariates when calculating the cumulative hazards (except through weighting of the control observations). Hirano & Imbens (2001) suggest a weighted regression of the outcome on the treatment dummy and the controls (and interactions between controls and treatment dummy), to adjust for remaining bias due to possible lack of covariance balance. In our case, however, the balancing properties using propensity score weighting are very good, and robustness checks in Section 6 show that a second-step regression adjustment gives almost exactly the same results as a 'simple' comparison of outcomes for the treatment and weighted control groups.

5.2 The propensity score functions

We estimate 17 propensity score logit functions, one for each base year. There are 56 explanatory variables for each year, including: age, education, marital/co-habitation status and children (1 year prior to base year), membership of unemployment insurance fund (3 years prior to base year), working experience, log hourly wage rate and log yearly earnings (3-4 years prior to base year), industry dummies, plant size, age of plant, tenure at plant, region of residence, local unemployment rate (for the commuting area of the municipality of residence

1 year after the base year), hospitalisation (3-5 years prior to base year), and sickness benefits (3-4 years prior to base year).

The estimated parameters of the propensity score function are rather different for the different base years, especially the estimated coefficients of the industry dummies vary considerably. This is because business cycles vary over industries, and closure of a few large plants within a specific industry significantly affects the sample of a particular base year. We do not show the estimates of the 17 propensity scores here, but to give an impression of the average effects of the conditioning variables we show the propensity score function estimated on the pooled sample of all base years. The specification of this logit model for the pooled sample is identical to that of the propensity score functions estimated for each base year, except that we include base year dummies. The result is shown in the first columns of Table 5.1. The last four columns of this table show the means of the explanatory variables for the treatment and control groups, the two-sample t-statistics of the test for equal means, and the standardised difference in means. The means of the two groups differ significantly for most variables, and most parameter estimates are significant. We do not show estimated parameters for base year dummies, but their coefficients are highly significant, which is consistent with Table 4.1. The estimation results show that, given the other covariates, the probability of working at a closing plant is higher for workers who are older; who have only compulsory education (the reference category); who have low working experience; who have no unemployment insurance; who have a high hourly wage rate, but small yearly earnings; who are singles without children (the reference group); who are working in manufacturing industries (except chemicals) or construction compared to working in financial services or other services (the reference group) or other industries; who are working at young or small plants (the reference category for plant size is plants with less than 10 employees); who have short tenure (the reference is more than four years); and who live in the metropolitan area of Copenhagen or in an area with a high rate of unemployment. The baseline health variables are not significant in the propensity score, even though some of them are significant according to the two-sample t-statistics.

Table 5.1. Propensity score estimation result for the pooled sample of all base years 1985-2001 (logit model for working at a plant closing in the base year), and means for the treatment and control groups, and two-sample t-statistic and standardised difference in means

	Coef.	Std.Err.		Mean T	Mean C	t-stat	SDM(%)
Age 20-24	-0.402	0.049	***	0.0811	0.0722	5.77	3.33
Age 25-29	-0.334	0.036	***	0.1090	0.1122	-1.86	-1.04
Age 30-34	-0.191	0.028	***	0.1393	0.1527	-6.83	-3.79
Age 35-39	-0.087	0.023	***	0.1470	0.1578	-5.38	-3.00
Age 45-49	0.102	0.023	***	0.1440	0.1399	2.08	1.18
Age 50-54	0.184	0.028	***	0.1221	0.1175	2.49	1.41
Age 55-60	0.242	0.031	***	0.1055	0.0942	6.53	3.76
Vocational education	-0.032	0.019	*	0.5786	0.5794	-0.31	-0.17
Short further education	-0.232	0.043	***	0.0309	0.0412	-10.43	-5.51
Long further education	-0.064	0.044		0.0637	0.0665	-2.07	-1.16
Higher education	-0.320	0.075	***	0.0254	0.0338	-9.43	-4.98
Working experience (years/100)	-0.954	0.468	**	0.1745	0.1782	-7.98	-4.42
Working experience squared	-0.625	1.341		0.0372	0.0391	-11.63	-6.40
No unemployment insurance (t-3)	-0.083	0.026	***	0.1704	0.1648	2.65	1.50
Log hourly wage rate (average t-4 – t-3)	0.160	0.063	***	5.1827	5.2048	-6.57	-3.92
Hourly wage rate not available	1.009	0.288	***	0.0075	0.0044	6.43	4.03
Log yearly earnings (average t-4 – t-3)	-0.247	0.051	***	5.7078	5.7179	-3.91	-2.23
Immigrant	0.167	0.073	**	0.0132	0.0101	4.75	2.83
Single with children (t-1)	-0.171	0.062	***	0.0090	0.0095	-0.79	-0.44
Married without children (t-1)	-0.114	0.021	***	0.2270	0.2177	3.91	2.22
Married with children (t-1)	-0.117	0.025	***	0.3649	0.3844	-7.19	-4.04
Co-habiting without children (t-1)	-0.106	0.023	***	0.0898	0.0930	-1.99	-1.12
Co-habiting with children (t-1)	-0.106	0.030	***	0.0704	0.0799	-6.58	-3.62
Number of children (t-1)	-0.028	0.011	***	0.7337	0.8072	-13.61	-7.56
Manufacturing, food, beverages, etc.	0.627	0.187	***	0.0468	0.0295	14.66	9.05
Manufacturing, wood, paper, printing, etc.	0.303	0.080	***	0.0659	0.0536	8.79	5.17
Manufacturing, chemicals	-0.276	0.264		0.0224	0.0381	-18.39	-9.13
Manufacturing, metals and machinery	0.052	0.084		0.1695	0.1992	-13.94	-7.64
Manufacturing, other	0.449	0.095	***	0.0583	0.0466	8.87	5.24
Construction	0.126	0.055	**	0.1359	0.1289	3.61	2.06
Infrastructure	0.298	0.065	***	0.1020	0.0778	14.25	8.47
Financial services	0.054	0.080		0.1069	0.1097	-1.63	-0.91
Other industries (except services)	-0.403	0.084	***	0.0238	0.0389	-17.34	-8.69
11-20 employees at plant	-0.130	0.035	***	0.2278	0.2091	7.90	4.52
21-50 employees at plant	-0.292	0.045	***	0.2308	0.2526	-9.16	-5.10
51-100 employees at plant	-0.489	0.086	***	0.0893	0.1183	-17.93	-9.53
101 or more employees at plant	-0.517	0.171	***	0.1154	0.1541	-21.33	-11.36
Tenure at plant 1 year	0.402	0.034	***	0.1515	0.1079	21.71	13.02
Tenure at plant 2 years	0.225	0.035	***	0.0969	0.0803	10.04	5.88
Tenure at plant 3 years	0.063	0.034	*	0.0922	0.0812	6.73	3.89
Tenure at plant 4 years	0.099	0.034	***	0.0870	0.0790	5.01	2.88
Plant age 3 years	0.559	0.066	***	0.0523	0.0260	21.21	13.59
Plant age 4 years	0.507	0.067	***	0.0496	0.0264	19.20	12.18
Copenhagen area	0.359	0.047	***	0.3667	0.3035	23.31	13.43
Local unemployment rate (t+1)	0.067	0.017	***	8.8284	8.1687	42.57	24.21
Infective and parasitic diseases (t-5 – t-3)	0.058	0.083		0.0050	0.0044	1.33	0.77
Cancer (malignant neoplasm) (t-5 – t-3)	0.024	0.070		0.0065	0.0062	0.63	0.36
Mental disorders (t-5 – t-3)	0.148	0.097		0.0049	0.0037	3.03	1.81
Diseases of the nervous system and sensory organs (t-5 – t-3)	-0.017	0.066		0.0072	0.0071	0.35	0.20
Circulatory diseases (t-5 – t-3)	-0.031	0.055		0.0156	0.0147	1.22	0.70
Diseases of respiratory organs (t-5 – t-3)	0.080	0.053		0.0120	0.0109	1.80	1.04
Symptoms and other ill-defined conditions (t-5 – t-3)	-0.048	0.048		0.0141	0.0147	-0.98	-0.55
Trauma, poisonings and other violent bodily harm (t-5 – t-3)	0.011	0.032		0.0400	0.0384	1.50	0.85
Other diseases (t-5 – t-3)	-0.004	0.024		0.0598	0.0589	0.66	0.37
Myocardial infarction or stroke (t-5 – t-3)	0.136	0.105		0.0040	0.0032	2.18	1.29
Alcohol-related diseases (t-5 – t-3)	-0.010	0.133		0.0025	0.0021	1.64	0.97
Sickness benefits (average t-4 – t-3), (2000 DKK/1000)	0.002	0.002		0.7503	0.5457	6.41	4.11
Constant	-2.897	0.244	***				
Observations	662,972			33,070	629,902		
Log pseudo-likelihood	-125,305						
LR test of model, $chi^2(73)$	1738.1						

Note: *, ** and *** indicate significance at 10, 5 and 1% levels, respectively; t-j denotes a lag of j years compared to the base year. The estimation includes dummy variables for base years. The reference categories are: Age 40-44, no education beyond compulsory, single without children, service industry, less than 10 employees at plant, tenure at least 5 years, plant age at least 5 years, and living outside the metropolitan area of Copenhagen. SDM(%) is the standardised difference in means (or the standardised bias), i.e. the difference in means as a percentage of the square root of the average sample variances of the treatment and control groups.

5.3 Distribution of the estimated propensity score

Table 5.2 shows the distributions of the estimated propensity score for the treatment and control groups, respectively. There are *many* controls for each treated individual for estimated propensity scores below 0.2, i.e. for 95% of the treatment group observations. For propensity scores above 0.2 the number of controls is also larger than the number of treated. The maximum propensity score is 0.685 in the treatment group and 0.730 in the control group. Consequently, there are no common support problems. There are no persons in the control group with large weights; all weights are below 2.8, and for 99.9% of the control persons the weights are below 1.

Table 5.2. Distribution of estimated propensity scores for treated individuals and controls by intervals of 0.005

Lower bound of PS interval	No. of observations		Lower bound of PS interval	No. of observations		Lower bound of PS interval	No. of observations	
	Treated	Controls		Treated	Controls		Treated	Controls
0.000	13	18996	0.200	131	536	0.400	4	9
0.005	166	24508	0.205	126	489	0.405	6	7
0.010	524	43027	0.210	104	428	0.410	6	8
0.015	997	55263	0.215	88	386	0.415	3	5
0.020	1277	54330	0.220	98	332	0.420	1	5
0.025	1445	50785	0.225	88	341	0.425	3	8
0.030	1622	48286	0.230	76	258	0.430	3	3
0.035	1710	44048	0.235	69	245	0.435	4	5
0.040	1800	38658	0.240	80	229	0.440	1	3
0.045	1697	33129	0.245	80	187	0.445	4	3
0.050	1618	27730	0.250	64	190	0.450	1	2
0.055	1632	24115	0.255	61	168	0.455	5	5
0.060	1435	20352	0.260	51	143	0.460	0	5
0.065	1253	17947	0.265	45	136	0.465	4	1
0.070	1177	15487	0.270	55	103	0.470	1	1
0.075	1120	13547	0.275	61	120	0.475	2	2
0.080	1062	12106	0.280	60	89	0.480	0	1
0.085	1017	10738	0.285	52	73	0.485	2	1
0.090	886	9234	0.290	31	78	0.490	2	4
0.095	820	8091	0.295	36	68	0.495	1	2
0.100	774	7276	0.300	33	63	0.500	2	2
0.105	729	6336	0.305	39	52	0.505	1	2
0.110	693	5568	0.310	28	43	0.510	2	1
0.115	591	4994	0.315	26	49	0.515	0	1
0.120	575	4140	0.320	18	54	0.520	0	1
0.125	579	3697	0.325	24	36	0.525	0	2
0.130	505	3301	0.330	23	43	0.530	1	1
0.135	443	2742	0.335	21	31	0.535	0	0
0.140	394	2395	0.340	20	31	0.540	0	1
0.145	349	2165	0.345	14	23	0.555	1	1
0.150	297	1951	0.350	16	31	0.560	0	0
0.155	306	1661	0.355	25	23	0.565	0	1
0.160	280	1445	0.360	7	13	0.580	0	1
0.165	280	1308	0.365	13	23	0.585	0	1
0.170	240	1172	0.370	10	15	0.590	0	1
0.175	228	1022	0.375	5	20	0.685	1	1
0.180	209	913	0.380	3	16	0.730	0	1
0.185	181	789	0.385	8	7			
0.190	147	725	0.390	2	10			
0.195	145	637	0.395	2	8			

5.4 Balancing properties after propensity score weighting

To check the balancing properties, we calculated two-sample t test statistics and standardised differences in means for the explanatory variables of the propensity score function, and also for other baseline health indicators. The balancing properties are very good in each base year, and also for all base years merged. Table 5.3 shows the statistics for the whole sample. The weighted means for the control groups are very close to the means for the displacement group. The t statistics for the test of equality of means are clearly insignificant for all of the variables included in the propensity score function (and the reference categories), and for the additional baseline health indicators (hospitalisation for suicide attempts and traffic accidents) shown at the end of the table. The t values are very small for almost all variables even though the large sample size implies very small standard errors for the means. Table 5.3 also shows statistics for the continuous variables ‘age’ and ‘number of employees’ corresponding to the categorised variables included in the propensity score model in order to check whether the overall averages of these variables are balanced. Both are accurately balanced.

Table 5.3. Balancing properties with respect to explanatory variables of the propensity score. Treatment and weighted control groups

Variable	Mean of treated	Weighted mean of controls	Diff. in means	SE of diff.	t-stat.	SDM (%)
Age	40.2108	40.1444	0.0664	0.0610	1.09	0.63
Age 20-24	0.0811	0.0817	-0.0007	0.0016	-0.41	-0.24
Age 25-29	0.1090	0.1093	-0.0003	0.0018	-0.17	-0.10
Age 30-34	0.1393	0.1394	-0.0001	0.0020	-0.06	-0.04
Age 35-39	0.1470	0.1467	0.0003	0.0020	0.13	0.08
Age 40-44	0.1521	0.1519	0.0002	0.0021	0.11	0.06
Age 45-49	0.1440	0.1437	0.0003	0.0020	0.13	0.08
Age 50-54	0.1221	0.1219	0.0002	0.0019	0.11	0.06
Age 55-60	0.1055	0.1054	0.0001	0.0018	0.06	0.04
No education beyond compulsory	0.2883	0.2899	-0.0016	0.0026	-0.63	-0.36
Vocational education	0.5786	0.5790	-0.0004	0.0028	-0.15	-0.09
Short further education	0.0309	0.0310	-0.0001	0.0010	-0.11	-0.06
Long further education	0.0637	0.0633	0.0004	0.0014	0.31	0.18
Higher education	0.0254	0.0254	0.0000	0.0009	0.03	0.02
Working experience (years/100)	0.1745	0.1743	0.0002	0.0005	0.47	0.27
Working experience (years/100) squared	0.0372	0.0372	0.0001	0.0002	0.41	0.24
No unemployment insurance (t-3)	0.1704	0.1709	-0.0005	0.0022	-0.24	-0.14
Log hourly wage rate (average t-4 – t-3)	5.1827	5.1804	0.0023	0.0036	0.64	0.38
Hourly wage rate not available	0.0075	0.0077	-0.0002	0.0005	-0.46	-0.28
Log yearly earnings (average t-4 – t-3)	5.7078	5.7060	0.0018	0.0027	0.68	0.39
Immigrant	0.0132	0.0133	-0.0001	0.0007	-0.22	-0.13
Single without children (t-1)	0.2389	0.2397	-0.0007	0.0025	-0.30	-0.17
Single with children (t-1)	0.0090	0.0090	0.0000	0.0005	0.04	0.02
Married without children (t-1)	0.2270	0.2265	0.0004	0.0024	0.18	0.11
Married with children (t-1)	0.3649	0.3644	0.0005	0.0028	0.17	0.10
Co-habiting without children (t-1)	0.0898	0.0901	-0.0003	0.0016	-0.18	-0.10
Co-habiting with children (t-1)	0.0704	0.0703	0.0001	0.0015	0.08	0.04
Number of children (t-1)	0.7337	0.7329	0.0008	0.0055	0.15	0.09
Service industries (except financial services)	0.2685	0.2689	-0.0004	0.0025	-0.16	-0.09
Manufacturing, food, beverages, etc.	0.0468	0.0472	-0.0004	0.0013	-0.29	-0.17
Manufacturing, wood, paper, printing, etc.	0.0659	0.0660	-0.0001	0.0014	-0.07	-0.04
Manufacturing, chemicals	0.0224	0.0218	0.0006	0.0009	0.72	0.42
Manufacturing, metals and machinery	0.1695	0.1690	0.0006	0.0022	0.26	0.15
Manufacturing, other	0.0583	0.0586	-0.0003	0.0014	-0.22	-0.13
Construction	0.1359	0.1361	-0.0002	0.0020	-0.11	-0.06
Infrastructure	0.1020	0.1016	0.0004	0.0017	0.21	0.12
Financial services	0.1069	0.1071	-0.0002	0.0018	-0.12	-0.07
Other industries (except services)	0.0238	0.0237	0.0001	0.0009	0.09	0.05
5-10 employees	0.3367	0.3391	-0.0024	0.0027	-0.88	-0.51
11-20 employees at plant	0.2278	0.2292	-0.0014	0.0024	-0.59	-0.34
21-50 employees at plant	0.2308	0.2309	-0.0001	0.0024	-0.05	-0.03
51-100 employees at plant	0.0893	0.0886	0.0007	0.0016	0.41	0.23
101 or more employees at plant	0.1154	0.1121	0.0033	0.0018	1.78	1.03
No. of employees	55.8520	55.5760	0.2760	0.7153	0.39	0.19
Tenure at plant 1 year	0.1515	0.1524	-0.0009	0.0021	-0.42	-0.25
Tenure at plant 2 years	0.0969	0.0974	-0.0004	0.0017	-0.25	-0.14
Tenure at plant 3 years	0.0922	0.0926	-0.0004	0.0017	-0.24	-0.14
Tenure at plant 4 years	0.0870	0.0874	-0.0004	0.0016	-0.25	-0.14
Plant age 3 years	0.0523	0.0529	-0.0005	0.0013	-0.41	-0.24
Plant age 4 years	0.0496	0.0500	-0.0004	0.0013	-0.31	-0.19
Copenhagen area	0.3667	0.3681	-0.0014	0.0028	-0.48	-0.28
Local unemployment rate (t+1)	8.8284	8.8274	0.0010	0.0158	0.06	0.04
Infective and parasitic diseases (t-5 – t-3)	0.0050	0.0050	-0.0001	0.0004	-0.14	-0.08
Cancer (malignant neoplasm) (t-5 – t-3)	0.0065	0.0064	0.0000	0.0005	0.10	0.06
Mental disorders (t-5 – t-3)	0.0049	0.0050	-0.0001	0.0004	-0.23	-0.14

Variable	Mean of treated	Weighted mean of controls	Diff. in means	SE of diff.	t-stat.	SDM (%)
Diseases of the nervous system and sensory organs (t-5 – t-3)	0.0072	0.0072	0.0000	0.0005	-0.03	-0.02
Circulatory diseases (t-5 – t-3)	0.0156	0.0156	0.0000	0.0007	-0.02	-0.01
Diseases of respiratory organs (t-5 – t-3)	0.0120	0.0120	0.0000	0.0006	0.02	0.01
Symptoms and other ill-defined conditions (t-5 – t-3)	0.0141	0.0140	0.0001	0.0007	0.14	0.08
Trauma, poisonings and other violent bodily harm (t-5 – t-3)	0.0400	0.0400	0.0000	0.0011	0.00	0.00
Other diseases (t-5 – t-3)	0.0598	0.0598	0.0000	0.0014	-0.01	-0.01
Myocardial infarction or stroke (t-5 – t-3)	0.0040	0.0041	-0.0001	0.0004	-0.16	-0.10
Alcohol-related diseases (t-5 – t-3)	0.0025	0.0025	0.0000	0.0003	0.01	0.00
Sickness benefits (average t-4 – t-3), (2000 DKK/1000)	0.7503	0.7609	-0.0106	0.0396	-0.27	-0.18
Baseline health variables not in the propensity score function						
Suicide attempts (t-5 – t-3)	0.0009	0.0008	0.0001	0.0002	0.42	0.25
Traffic accidents (t-5 – t-3)	0.0047	0.0052	-0.0005	0.0004	-1.16	-0.66

6. Estimation results

In this section we present estimation results based on non-parametric duration analysis for all-cause mortality, cause-specific mortality and hospitalisation outcomes corresponding to the variables in Table 4.2. Thus, we consider the effect of job displacement on the duration to death or hospitalisation; the estimated (cumulative) hazard rates for the control group are based on weighting of each observation by the odds of its estimated propensity score (see Section 3). For each outcome we show the cumulative hazard ratio with 95% confidence interval (and other statistics) for durations between 1 and 20 years from the base year.

6.1 Effect of job displacement on duration until death

Table 6.1 shows results for all-cause mortality and for death from major causes, for durations 1, 2, 3, 4, 10, 15 and 20 years after the base year. The first column shows the cumulative hazard ratio, i.e. the ratio of the (weighted) cumulative hazards for the displacement and control groups (reported in columns 5 and 6). Lower and upper bounds of the 95% confidence interval of the cumulative hazard ratio are shown in columns 2 and 3. The hazard ratio is shown in the next column; this is the ratio of the (yearly) hazard rates for the displacement and control groups. At durations 10, 15 and 20 years after the base year it is calculated as the ratio of the average hazard rates for durations 5-10, 11-15 and 16-20, respectively. The last column shows the cumulative number of failures (deaths) among the displaced, which is the most important determinant of the width of the confidence interval, since the control group is very large (as discussed above). The structure of Tables 6.2-6.4 is similar.

Table 6.1. Propensity score weighting estimates of the effect of job loss on all-cause mortality and death from major causes: Cumulative hazard ratios 1-20 years after base year

Cause of death	Duration from base year	Cumulative hazard ratio	95% CI bounds Lower	Upper	Hazard ratio	Cumulative hazards (%) Displaced	Controls	No. of deaths among displaced
All-cause mortality	1	1.84	1.44	2.34	1.84	0.363	0.197	120
	2	1.46	1.24	1.72	1.15	0.646	0.443	213
	3	1.38	1.21	1.57	1.24	0.976	0.709	321
	4	1.36	1.22	1.51	1.32	1.368	1.008	449
	10	1.17	1.10	1.25	1.10	4.016	3.421	1218
	15	1.13	1.07	1.19	1.08	7.387	6.549	1911
	20	1.10	1.05	1.16	1.07	11.666	10.558	2341
Cancer	1	1.38	0.92	2.06	1.38	0.103	0.075	34
	2	1.15	0.88	1.50	0.97	0.194	0.169	64
	3	1.13	0.91	1.39	1.09	0.307	0.273	101
	4	1.15	0.97	1.38	1.22	0.451	0.391	148
	10	1.09	0.99	1.20	1.07	1.576	1.441	476
	15	1.06	0.98	1.14	1.02	3.036	2.870	777
	20	1.03	0.96	1.11	0.99	4.883	4.737	960
Circulatory diseases	1	2.39	1.62	3.51	2.39	0.178	0.075	59
	2	1.77	1.36	2.30	1.29	0.300	0.169	99
	3	1.58	1.28	1.95	1.27	0.428	0.271	141
	4	1.54	1.29	1.83	1.44	0.600	0.390	197
	10	1.21	1.10	1.34	1.08	1.628	1.345	495
	15	1.14	1.06	1.23	1.07	3.043	2.666	787
	20	1.16	1.07	1.25	1.18	5.132	4.430	994
External causes	1	2.30	1.23	4.28	2.30	0.067	0.029	22
	2	1.61	1.06	2.44	1.10	0.109	0.068	36
	3	1.56	1.11	2.18	1.47	0.164	0.105	54
	4	1.53	1.15	2.04	1.45	0.219	0.143	72
	10	1.22	1.01	1.48	1.02	0.439	0.359	137
	15	1.23	1.04	1.45	1.23	0.681	0.555	186
	20	1.20	1.01	1.42	1.12	0.929	0.776	212
Other causes	1	1.33	0.69	2.59	1.33	0.036	0.027	12
	2	1.25	0.79	1.96	1.17	0.073	0.058	24
	3	1.24	0.87	1.77	1.23	0.119	0.096	39
	4	1.22	0.91	1.65	1.19	0.168	0.137	55
	10	1.18	1.00	1.40	1.17	0.593	0.501	177
	15	1.18	1.04	1.35	1.18	1.126	0.951	285
	20	1.14	1.00	1.30	1.07	1.788	1.568	352

The first rows in Table 6.1 show that the risk of overall mortality is 84% higher for the displacement group 1 year after the base year (i.e. in the year of displacement), 36% higher 1-4 years after the base year, 17% higher 1-10 years after the base year, and 10% higher 1-20 years after the base year. Thus, the effects are largest just after plant closure, but they remain statistically significant even after 20 years, indicating that the short-term effects do not merely represent a speeding-up of deaths that would have happened anyway a few years later. This is also reflected in the ratio of the yearly hazard rates, which exceeds 1 in almost all years.

The risk of death from cancer is not statistically different between the two groups, although the point estimate of the hazard ratio is 1.38 in the year of plant closure. Estimates for different subgroups of cancer diseases (not shown), including smoking-related cancer, are similar and clearly insignificant.

Displacement has a large and significant effect on death from circulatory disease. The risk of death is 139% higher for the displacement group 1 year after the base year, 54% higher 1-4 years after the base year, and 16% higher 20 years after the base year. Thus, the time pattern of relative risks of death from circulatory disease resembles the pattern for overall mortality. The excess mortality for the displacement group is much larger in the year of plant closure than in the following years. Thus, the hazard ratio is 2.39 in the year of plant closure, which is larger than the upper limit of the confidence interval of the cumulative hazard ratio 1-2 years after the base year (and at longer durations).

Displacement has a similar large effect on death from external causes, and the time pattern of the effect is also very much the same. Deaths from ‘other causes’ are deaths where neither primary nor secondary causes of death are cancer, circulatory or external. The results indicate that displacement might increase the risk of deaths from ‘other causes’, but the estimates are scarcely significant.

Table 6.2 shows results for five more specific causes of death. Myocardial infarction and stroke are important causes of death which account for about half of all deaths from circulatory diseases. The cumulative hazard ratios for this specific outcome are very similar to the corresponding estimates for death from circulatory disease in Table 6.1.

Table 6.2. Propensity score weighting estimates of the effect of job loss on death from some specific causes: Cumulative hazard ratios 1-20 years after base year

Cause of death	Duration from base year	Cumulative hazard ratio	95% CI bounds Lower	Upper	Hazard ratio	Cumulative hazards (%) Displaced	Controls	No. of deaths among displaced
Myocardial infarction or stroke	1	2.23	1.37	3.63	2.23	0.106	0.048	35
	2	1.75	1.25	2.47	1.33	0.176	0.100	58
	3	1.53	1.16	2.02	1.10	0.234	0.153	77
	4	1.58	1.24	2.00	1.73	0.329	0.208	108
	10	1.13	0.98	1.31	0.92	0.730	0.643	224
	15	1.09	0.97	1.22	1.04	1.329	1.218	349
	20	1.09	0.97	1.23	1.09	2.178	1.996	432
Alcohol-related diseases	1	2.82	1.08	7.34	2.82	0.033	0.012	11
	2	1.90	1.02	3.52	1.31	0.058	0.030	19
	3	1.90	1.17	3.08	1.91	0.094	0.050	31
	4	1.62	1.09	2.41	1.08	0.122	0.075	40
	10	1.08	0.87	1.35	0.88	0.295	0.272	91
	15	1.30	1.08	1.56	1.60	0.602	0.464	155
	20	1.15	0.96	1.38	0.90	0.854	0.743	180
Mental diseases	1	5.28	1.64	16.95	5.28	0.036	0.007	12
	2	2.87	1.29	6.39	1.22	0.048	0.017	16
	3	2.00	1.10	3.64	1.02	0.064	0.032	21
	4	1.47	0.89	2.44	0.40	0.070	0.047	23
	10	1.23	0.92	1.64	1.13	0.198	0.161	60
	15	1.21	0.97	1.50	1.18	0.415	0.344	104
	20	1.18	0.96	1.46	1.15	0.737	0.624	138
Suicide	1	4.31	1.64	11.37	4.31	0.045	0.011	15
	2	2.52	1.30	4.89	1.24	0.064	0.025	21
	3	2.32	1.37	3.92	1.98	0.094	0.041	31
	4	1.86	1.19	2.92	0.74	0.106	0.057	35
	10	1.28	0.95	1.72	0.91	0.188	0.147	59
	15	1.17	0.90	1.51	0.94	0.254	0.217	73
	20	1.13	0.87	1.47	0.98	0.311	0.275	79
Traffic accidents	1	0.40	0.11	1.42	0.40	0.003	0.008	1
	2	1.20	0.52	2.74	1.80	0.021	0.018	7
	3	1.31	0.66	2.60	1.56	0.033	0.026	11
	4	1.53	0.84	2.76	2.18	0.052	0.034	17
	10	1.56	1.03	2.37	1.59	0.112	0.072	35
	15	1.28	0.89	1.84	0.77	0.142	0.111	41
	20	1.27	0.88	1.83	1.22	0.168	0.133	44
Suicide or traffic accidents	1	2.67	1.23	5.82	2.67	0.048	0.018	16
	2	1.98	1.18	3.32	1.47	0.085	0.043	28
	3	1.93	1.27	2.93	1.84	0.128	0.066	42
	4	1.74	1.21	2.48	1.23	0.158	0.091	52
	10	1.37	1.08	1.74	1.11	0.300	0.219	94
	15	1.21	0.98	1.49	0.88	0.396	0.328	114
	20	1.17	0.95	1.46	1.04	0.479	0.408	123

Alcohol-related diseases are defined here as including both psychiatric and somatic diseases: alcohol poisoning, addiction syndrome, delirious abstinence, alcohol psychosis, varicose vein on gullet, alcoholic disease of the liver, and alcoholic disease of the pancreas. These are serious long-term consequences of alcohol abuse, but there may be short-term effects of job displacement on death (or hospitalisation) from these diseases if displacement leads to increased alcohol consumption of workers who already have a drink problem. The estimates

in Table 6.2 indicate that job displacement increases the risk of death from alcohol-related diseases in the first years after displacement: The yearly hazard ratios for the first three years are between 1.31 and 2.82, and the cumulative hazard ratio is significant for the first four years after the base year. From years 5 to 10 after the base year the yearly hazard ratios are below 1 on average, and the cumulative hazard ratio is 1.08 after 10 years. This finding indicates that displacement primarily speeds up deaths from these alcohol-related diseases. However, the yearly hazard rates of the displaced are on average 60% higher than for controls 11-15 years after the base year, and the cumulative hazard ratio is about 1.3 after 15 years and is significant, indicating that there may be long-term effects of displacement on death from alcohol-related diseases. These long-term effects may include effects for workers who did not have a drink problem before job displacement.

Displacement appears to have very strong short-term effects on death related to mental diseases (as primary or secondary causes of death). The hazard rate is five times higher in the year of plant closure, and the risk of death related to mental diseases 1-3 years after the base year is doubled for displaced persons compared to controls. There are no significant long-term effects, however: The cumulative hazard ratio does not differ significantly from 1 after 4 years. This pattern may be consistent with acute stress just after displacement which provokes mental disorders (e.g., depression or psychoses) in vulnerable individuals, possibly leading to loss of self-control and self-destructive behaviour.

The estimates in Table 6.2 indicate that displacement increases the risk of suicide in the short term: the hazard ratio exceeds four in the displacement year and the cumulative hazard ratio is 1.86 after four years, and is statistically different from 1. There are no significant long-term effects (although the cumulative hazard ratio estimate is 1.28 10 years after the base year).

Death from traffic accidents is rare, and the estimated effects of displacement are not significant, although the point estimates of the cumulative hazard rates are above 1 from two years after the base year. At the bottom of Table 6.2 we show estimates for the combined outcome comprising death from traffic accidents or suicide. Both are external causes of death, and some traffic accidents may be suicides. For this combined outcome, the cumulative hazard ratio is significantly above 1 in the year of plant closure and for up to 10 years from the base year.

6.2 Effect of job displacement on duration until hospitalisation

Estimates for hospitalisation outcomes are shown in Table 6.3. There is no significant effect of job displacement on the risk of cancer in the short term, but the cumulative hazard ratio is between 1.04 and 1.08 and is marginally significant from 10 years after the base year. The

cumulative hazard ratios for circulatory diseases are also only slightly above 1 (falling from 1.08 in the displacement year to 1.02 20 years after); even if they are more precisely estimated they are only significantly different from 1 15 years after the base year, and only marginally so. The estimates for hospitalisation for myocardial infarction or stroke do not differ significantly from the estimates for circulatory disease in general. It is surprising that the point estimate indicates that displacement reduces the risk of hospitalisation for myocardial infarction or stroke 1-2 years after the base year by 2%, but this is explained mainly by the fact that a large fraction of people with these serious diseases die without being hospitalised (see below). It is surprising that displacement has no (or only a small long-term) effect on hospitalisation for circulatory diseases, since the estimated effects on death from these diseases are rather large. For circulatory disease in general the number of hospitalisations is much larger than the number who died, and data on hospitalisation may be dominated by less serious circulatory diseases. For the more specific diagnoses of myocardial infarction and stroke, the difference in results between the death and hospitalisation outcomes may be consistent with stress from job displacement affecting mainly vulnerable workers who would have been hospitalised for these diseases anyway, but affecting them more seriously. The fact that we do not find any significant effects of job displacement on hospitalisation for these circulatory diseases is consistent with the findings of Browning, Danø & Heinesen (2006) and Eliason & Storrie (2009b).

Table 6.3. Propensity score weighting estimates of the effect of job loss on hospitalisation due to some major and specific diagnoses: Cumulative hazard ratios 1-20 years after base year

Diagnosis	Duration from base year	Cumulative hazard ratio	95% CI bounds Lower	Upper	Hazard ratio	Cumulative hazards (%) Displaced	Controls	No. of hospitalisations among displaced
Cancer	1	1.06	0.91	1.23	1.06	0.556	0.525	184
	2	1.04	0.92	1.16	1.01	0.969	0.935	319
	3	1.07	0.97	1.18	1.15	1.446	1.351	474
	4	1.06	0.98	1.15	1.03	1.877	1.768	613
	10	1.06	1.01	1.12	1.06	5.172	4.875	1556
	15	1.08	1.03	1.13	1.10	8.921	8.289	2296
	20	1.04	0.99	1.08	0.94	12.323	11.902	2623
Circulatory diseases	1	1.08	0.97	1.21	1.08	1.158	1.068	383
	2	1.04	0.96	1.12	0.98	2.114	2.040	694
	3	1.03	0.97	1.10	1.03	3.133	3.028	1021
	4	1.03	0.98	1.09	1.02	4.159	4.034	1346
	10	1.03	0.99	1.07	1.03	11.423	11.099	3344
	15	1.03	1.00	1.06	1.02	19.167	18.684	4755
	20	1.02	0.99	1.05	1.00	26.792	26.314	5426
Myocardial infarction or stroke	1	1.05	0.87	1.28	1.05	0.336	0.319	111
	2	0.98	0.85	1.13	0.90	0.613	0.626	202
	3	1.03	0.92	1.15	1.12	0.988	0.960	324
	4	1.03	0.93	1.13	1.02	1.325	1.293	433
	10	1.00	0.94	1.06	0.98	3.605	3.608	1090
	15	0.99	0.94	1.04	0.97	6.274	6.349	1625
	20	1.00	0.95	1.06	1.04	9.133	9.110	1909
Alcohol-related diseases	1	1.70	1.26	2.31	1.70	0.215	0.126	71
	2	1.36	1.09	1.70	1.01	0.337	0.247	111
	3	1.29	1.08	1.55	1.14	0.468	0.363	154
	4	1.30	1.11	1.53	1.33	0.616	0.474	202
	10	1.28	1.15	1.42	1.26	1.483	1.163	453
	15	1.31	1.19	1.43	1.36	2.407	1.842	640
	20	1.23	1.12	1.35	1.06	3.239	2.625	725
Mental diseases	1	1.61	1.28	2.02	1.61	0.357	0.222	118
	2	1.43	1.21	1.69	1.25	0.619	0.432	204
	3	1.34	1.16	1.53	1.12	0.837	0.627	275
	4	1.31	1.16	1.48	1.24	1.076	0.819	352
	10	1.25	1.15	1.35	1.21	2.549	2.039	779
	15	1.20	1.12	1.29	1.12	4.014	3.344	1074
	20	1.20	1.12	1.29	1.20	5.898	4.909	1265
Suicide attempts	1	1.53	0.83	2.83	1.53	0.048	0.032	16
	2	1.45	0.95	2.21	1.37	0.097	0.067	32
	3	1.48	1.04	2.09	1.55	0.146	0.099	48
	4	1.36	1.00	1.85	0.91	0.171	0.126	56
	10	1.19	0.95	1.49	1.01	0.292	0.246	92
	15	1.21	0.98	1.49	1.28	0.384	0.318	111
	20	1.12	0.91	1.38	0.79	0.448	0.398	119

Diagnosis	Duration from base year	Cumulative hazard ratio	95% CI bounds Lower	Upper	Hazard ratio	Cumulative hazards (%) Displaced	Controls	No. of hospitalisations among displaced
Traffic	1	0.98	0.72	1.34	0.98	0.124	0.126	41
accidents	2	1.24	0.98	1.56	1.55	0.282	0.228	93
	3	1.27	1.05	1.54	1.35	0.411	0.324	135
	4	1.16	0.98	1.38	0.82	0.494	0.425	162
	10	1.21	1.08	1.36	1.25	1.191	0.983	368
	15	1.16	1.04	1.28	1.03	1.625	1.405	458
	20	1.10	0.99	1.22	0.88	1.935	1.758	490
Suicide	1	1.10	0.83	1.45	1.10	0.172	0.157	57
attempts	2	1.28	1.04	1.56	1.49	0.376	0.295	124
or traffic	3	1.30	1.10	1.54	1.36	0.548	0.421	180
accidents	4	1.20	1.03	1.39	0.84	0.656	0.549	215
	10	1.21	1.09	1.34	1.22	1.477	1.220	457
	15	1.15	1.05	1.27	1.02	1.974	1.709	560
	20	1.10	1.00	1.21	0.86	2.343	2.136	599

Job displacement significantly increases the risk of hospitalisation for alcohol-related diseases at all durations, and in the year of plant closure by 70%; the cumulative hazard ratio is about 1.30 from durations 3 to 15, and falls to 1.23 20 years after the base year. The effect on hospitalisation for mental disorders is also significant at all durations; the cumulative hazard ratio is 1.61 in the year of plant closure, and 1.20 20 years after the base year.

Job displacement appears to increase the risk of suicide attempts in the short term. The cumulative hazard ratios are about 1.5 in the first 3 years after the base year, and are significant 3 and 4 years after the base year. Long-term effects are smaller and not significant. The effect on hospitalisation due to traffic accidents is smaller and less significant, although point estimates of the cumulative hazard ratio exceed 1 in all years except the first, and are significant at durations 3, 10 and 15 years. At the bottom of table 6.3 we report results for hospitalisation due to suicide attempt or traffic accident; the cumulative hazard ratio exceeds 1 at all durations, and significantly so except in the year of plant closure.

For some of the very serious health events, such as myocardial infarction, stroke, suicide and traffic accidents, it is common for people to die without being hospitalised. We therefore show, in Table 6.4, the results when the outcome is death or hospitalisation due to these causes. For myocardial infarction and stroke the point estimates indicate increased risk in the first years after displacement, although they are not significant. The risk of suicide or suicide attempt is significantly higher for displaced workers in the first 4 years after the base year (twice as high in the displacement year, and 59% higher 1-3 years after the base year), whereas the long-term effect is smaller and not significant. The risk of death or hospitalisation from traffic accidents is significantly larger for displaced workers at all durations, except in

the displacement year. The same is true for the combined outcome of suicide/suicide attempts or traffic accidents.

Table 6.4. Propensity score weighting estimates of the effect of job loss on death or hospitalisation due to some specific causes: Cumulative hazard ratios 1-20 years after base year

Cause	Duration from base year	Cumulative hazard ratio	95% CI bounds Lower	Upper	Hazard ratio	Cumulative hazards (%) Displaced	Controls	No. of failures among displaced
Myocardial infarction or stroke	1	1.13	0.94	1.37	1.13	0.387	0.341	128
	2	1.04	0.91	1.19	0.94	0.695	0.669	229
	3	1.07	0.96	1.19	1.12	1.091	1.022	358
	4	1.06	0.97	1.17	1.05	1.462	1.374	478
	10	1.00	0.95	1.07	0.97	3.882	3.864	1175
	15	0.99	0.94	1.04	0.96	6.725	6.823	1745
	20	1.00	0.95	1.05	1.04	9.830	9.810	2052
Suicide or suicide attempts	1	1.95	1.16	3.30	1.95	0.082	0.042	27
	2	1.57	1.09	2.25	1.24	0.143	0.091	47
	3	1.59	1.19	2.14	1.65	0.216	0.135	71
	4	1.43	1.10	1.86	0.89	0.253	0.177	83
	10	1.18	0.99	1.43	0.97	0.445	0.376	140
	15	1.16	0.98	1.38	1.11	0.593	0.510	171
	20	1.06	0.90	1.24	0.64	0.679	0.643	182
Traffic accidents	1	0.97	0.72	1.32	0.97	0.127	0.131	42
	2	1.26	1.01	1.58	1.61	0.304	0.240	100
	3	1.30	1.08	1.57	1.38	0.445	0.342	146
	4	1.20	1.02	1.41	0.88	0.540	0.450	177
	10	1.24	1.11	1.39	1.28	1.298	1.043	401
	15	1.17	1.06	1.29	1.00	1.753	1.498	495
	20	1.12	1.01	1.24	0.89	2.081	1.865	529
Suicide, suicide attempts or traffic accidents	1	1.21	0.93	1.58	1.21	0.209	0.172	69
	2	1.34	1.11	1.62	1.48	0.443	0.330	146
	3	1.37	1.17	1.61	1.43	0.652	0.476	214
	4	1.25	1.09	1.44	0.89	0.784	0.625	257
	10	1.23	1.11	1.35	1.20	1.726	1.408	535
	15	1.16	1.06	1.26	0.99	2.307	1.993	655
	20	1.09	1.00	1.19	0.83	2.717	2.485	699

6.3 Effects for subgroups of displaced workers

Table 6.5 shows the effects of job loss on all-cause mortality for six different subgroups of the population: three age groups and workers in high and low-unemployment areas in the displacement year (i.e. areas where the local unemployment rate was above or below 9%), and workers with at least three years of tenure at the plant.[5] The results indicate that the long-term relative increase in mortality due to job loss is greater for younger than for older

[5] The weights are obtained by re-estimating the propensity score functions for each subsample using the same specification as for the full sample (except for the exclusion of some age-category variables). Balancing properties (not shown) are fine for each subsample.

workers. The short-term effects for older workers are similar to the average effects for the full sample (see Table 6.1). The short-term effects are largest for the 40-49-year-olds, whereas the estimated short-term effects for younger workers are imprecise due to the low number of deaths. We do not show effects for the more specific health outcomes for subgroups since these are imprecisely estimated; in most cases differences between subgroups are small. There is some indication, however, of larger effects on death from circulatory and alcohol-related diseases for 40-49-year-olds.

Table 6.5. Propensity score weighting estimates of the effect of job loss on all-cause mortality by age and local unemployment rate: Cumulative hazard ratios 1-20 years after base year

Subgroup	Duration from base year	Cumulative hazard ratio	95% CI bounds Lower	Upper	Hazard ratio	Cumulative hazards (%) Displaced	Controls	No. of deaths among displaced
Age 20-39	1	1.53	0.83	2.81	1.53	0.102	0.067	16
	2	1.21	0.79	1.85	0.94	0.172	0.141	27
	3	1.30	0.93	1.82	1.45	0.286	0.221	45
	4	1.26	0.94	1.68	1.16	0.382	0.303	60
	10	1.28	1.08	1.51	1.28	1.224	0.960	178
	15	1.21	1.06	1.38	1.14	2.190	1.811	276
	20	1.17	1.03	1.33	1.11	3.450	2.948	344
Age 40-49	1	2.13	1.33	3.42	2.13	0.368	0.172	36
	2	1.62	1.19	2.21	1.25	0.666	0.410	65
	3	1.47	1.15	1.88	1.22	0.975	0.665	95
	4	1.35	1.10	1.66	1.09	1.297	0.962	126
	10	1.28	1.14	1.44	1.25	4.051	3.171	367
	15	1.18	1.08	1.29	1.08	7.346	6.228	575
	20	1.04	0.95	1.14	0.87	11.506	11.016	700
Age 50-60	1	1.75	1.28	2.40	1.75	0.904	0.515	68
	2	1.42	1.15	1.77	1.15	1.616	1.135	121
	3	1.34	1.13	1.58	1.19	2.429	1.819	181
	4	1.40	1.21	1.62	1.57	3.550	2.533	263
	10	1.11	1.02	1.21	1.00	10.126	9.115	673
	15	1.09	1.02	1.17	1.08	19.878	18.173	1060
	20	1.10	1.02	1.18	1.10	33.935	30.905	1297
High local unemploy-ment rate	1	2.52	1.73	3.67	2.52	0.418	0.166	65
	2	1.79	1.39	2.30	1.25	0.703	0.393	109
	3	1.63	1.33	1.98	1.38	1.060	0.652	164
	4	1.58	1.34	1.86	1.46	1.483	0.942	229
	10	1.24	1.13	1.35	1.11	4.234	3.416	643
	15	1.15	1.08	1.23	1.05	7.378	6.416	1061
	20	1.12	1.04	1.21	1.07	11.051	9.846	1222
Low local unemploy-ment rate	1	1.36	0.99	1.85	1.36	0.314	0.231	55
	2	1.21	0.98	1.50	1.08	0.595	0.491	104
	3	1.18	0.99	1.40	1.11	0.901	0.766	157
	4	1.19	1.03	1.37	1.21	1.267	1.067	220
	10	1.12	1.02	1.22	1.09	3.812	3.412	575
	15	1.12	1.04	1.21	1.13	7.576	6.751	850
	20	1.10	1.03	1.18	1.08	12.406	11.232	1119
Tenure at least 3 years	1	1.92	1.47	2.49	1.92	0.427	0.222	106
	2	1.43	1.20	1.71	1.05	0.722	0.505	179
	3	1.36	1.18	1.57	1.25	1.092	0.800	270
	4	1.35	1.20	1.53	1.33	1.537	1.135	379
	10	1.19	1.11	1.27	1.11	4.526	3.816	1031
	15	1.15	1.09	1.21	1.11	8.393	7.303	1624
	20	1.11	1.05	1.18	1.06	13.149	11.801	1975

Job loss in high-unemployment areas has a larger effect on mortality than job loss in low-unemployment areas, in both the short term and long term. This is due primarily to its larger effect on death from circulatory diseases, including myocardial infarction and stroke. One

might expect adverse health effects of displacement to be larger for high-tenure workers, since they may have accumulated more plant or firm specific human capital, but the effect on all-cause mortality is not larger for this group than for workers with at least one year of tenure (see the last panel in Table 6.5 and the first panel in Table 6.1). The same is true for the other health outcomes reported in Tables 6.1-6.4, except for suicide attempts where the effect for high-tenure workers is larger and more significant (the cumulative hazard rates are 1.7 in the first two years and 1.4 in the long term, and significant from year 2 after the base year).

6.4 Robustness checks

Table 6.6 shows the sensitivity of estimation results with respect to adjustment for remaining covariate bias between the treatment and control groups after propensity score weighting. The last two columns of the table show estimated coefficients of the displacement dummy in weighted least squares regressions for linear probability models of mortality dummies on the displacement dummy, a constant term, and (in the last column) additional control variables; these are all of the explanatory variables of the propensity score estimation, including year dummies and cross-terms between the treatment dummy and all other variables corrected for the mean for the treatment group (see eq. (6)). The weights for the control group observations in these regressions are the odds of the predicted propensity scores (whereas displaced workers all have weight equal to 1); see Section 3. The mortality dummies used in the estimations in the first five rows are 1 in case of death during the year of plant closure; in the last five regressions they are 1 if death occurs 1-4 years after the base year. It is clear that controlling for covariates in this second-step weighted regression has almost no effect on the estimated ATT. For comparison, the first column shows the absolute difference in cumulative hazards between the displacement and control groups one and four years after the base year, respectively; these hazards are reported in Table 6.1. The small difference between these estimates and the regression coefficients (without control variables) in the last five rows is because estimation of the cumulative hazard rate takes account of right censoring each year. For years 1-4 after the base year, right censoring is very limited, since it is due only to death and emigration.

Table 6.6. Robustness checks: Estimated effect of job loss on death 1 year after the base year and 1-4 years after. Difference in propensity score weighted cumulative hazard rates, and propensity score weighted regression

Mortality outcome	Difference in cumulative hazards	Regression	
		No controls	Many controls
1 year after the base year			
All-cause mortality	0.001655	0.001655	0.001660
Death from cancer	0.000283	0.000283	0.000287
Death from circulatory diseases	0.001037	0.001037	0.001038
Death from external causes	0.000376	0.000376	0.000376
Death from other causes	0.000091	0.000091	0.000092
1-4 years after the base year			
All-cause mortality	0.003604	0.003547	0.003538
Death from cancer	0.000604	0.000586	0.000584
Death from circulatory diseases	0.002104	0.002078	0.002074
Death from external causes	0.000759	0.000750	0.000749
Death from other causes	0.000307	0.000299	0.000298

Note: The first column shows the absolute difference in cumulative hazards between the treatment and weighted control groups one and four years after the base year, respectively. These cumulative hazards are reported in Table 6.1. The regression estimates are the coefficient of the displacement dummy in a weighted regression of the outcome on this dummy, a constant term and (in the last column) control variables. The last column shows the result when all of the explanatory variables of the propensity score estimation are used as controls (including year dummies and cross-terms between the treatment dummy and all other variables; see eq. (6)).

We have investigated how sensitive the results are to the precise definition of the displacement group. We consider in our analysis all workers who at the end of year t are working at a plant which closes in year t+1 (and who meet the criteria mentioned in Section 4) displaced in year t+1 even if some of them may not actually leave the plant in year t+1 (because some of the plants which we log as closing in year t+1 may not totally close down until some years later). The year of separation is equal to the year of plant closure (displacement) for 85% of the displaced workers, and the final year of plant closure is at most one year after the displacement year for 80% of displaced workers. Thus, restriction of the displacement group of base year t to workers at plants which finally close down in year t+1 or t+2 reduces the number of displaced workers by 20%, but does not change the results in any significant way. Alternatively, upon restricting the displacement group to workers who separate from their plant in the displacement year (the year with the largest absolute reduction in number of employees), the estimates indicate health effects slightly larger than those reported above (although mainly not significantly different). This may be due to selection, however, since plants which do not finally close down in year t+1 may choose to primarily lay off workers with inferior (unobserved) health conditions.

7. Conclusion

Based on a unique administrative dataset of all persons in Denmark in the period 1980-2006, we have estimated the causal effects of job displacement due to plant closure on mortality and hospitalisation outcomes for male workers in private sector firms. The very large dataset makes it possible to estimate effects on rare health outcomes with much more precision than in previous studies.

We find that job loss increases the risk of overall mortality and mortality caused by circulatory disease; of suicide and suicide attempts; and of death and hospitalisation due to traffic accidents, alcohol-related disease and mental illness.

The risk of overall mortality is 84% higher in the year of displacement, 36% higher 1-4 years after the base year, 17% higher 1-10 years after the base year and 10% higher 1-20 years after the base year. These estimated effects on overall mortality have a time pattern similar to those reported by Sullivan & Wachter (2009) for the US, but the Sullivan-Wachter estimates are about twice as large. Our estimates of short-term effects are similar to the estimates based on Swedish data in Eliason & Storrie (2009a), but our long-term estimates are larger. It is not surprising that estimated mortality effects are smaller in Denmark and Sweden than in the US, since losses of earnings are much smaller (see below), and economic stress may also be smaller due to a more comprehensive welfare state and public health insurance scheme.

Furthermore, we find large and clearly significant effects on death from circulatory disease (with a time pattern similar to that for overall mortality), and no significant effect on death from cancer. Within the first four years after the base year we estimate an increased risk of death from circulatory disease of 54%, whereas the insignificant point estimate for death from cancer is 15%. The corresponding estimates in Eliason & Storrie (2009a) are 24% and 39%, respectively. Their dataset is smaller and their estimates are statistically insignificant, however. We find no effect on hospitalisation for circulatory diseases (except a small, marginally significant long-term effect), consistent with the findings of Browning, Danø & Heinesen (2006) and Eliason & Storrie (2009b).

Job displacement significantly increases the risk of hospitalisation due to alcohol-related diseases in both the short and long term; the effect is 28% 1-10 years after the base year. This estimate is significantly smaller than the increased risk of 105% 1-12 years after displacement estimated by Eliason & Storrie (2009b). We find that the risk of death from alcohol-related diseases is increased by 182% in the year of plant closure, and by 62% 1-4 years after the base year, whereas long-term effects are imprecisely estimated. Eliason & Storrie (2009a) find a stronger effect 1-4 years after displacement; their point estimate indicates an increased

risk of 121%, but it is rather imprecise and not significantly different from our estimate of 62%.

The effect on suicide is very strong just after displacement, and weaker afterwards. The increased risk of 86% 1-4 years after the base year is close to the estimate in Eliason & Storrie (2009a), whereas our estimates of the effect on suicide attempts and hospitalisation due to traffic accidents are smaller than the corresponding estimates in Eliason & Storrie (2009b).

The increased risk of hospitalisation for mental disorders is large in the short term (61% in the year of plant closure), and remains significant in the long run (20% 1-20 years after the base year); Eliason & Storrie (2010) find no significant effect within a 12-year follow-up period for males, but they do find significant effects for females. We find a very large short-term effect for mental diseases as a secondary cause of death (the risk of death is five times higher for the displaced in the year of plant closure), but no significant long-term effect.

To illustrate the implications of the results, suppose that 10,000 male employees with characteristics corresponding to our displacement group lose their job because of plant closures. Within a period of four years 137 would die, whereas 101 would have died if the 10,000 employees had been working at plants which did not close; over a period of 20 years these numbers are 1167 and 1056, respectively. Thus, within the first four years 36 would die due to the job loss, and over 20 years the number is 111. Among the 36 deaths within the first four years 12 would be due to myocardial infarction or stroke, 5 to alcohol-related disease, and 5 to suicide. Furthermore, within the first four years the plant closures have the effect of increasing the numbers hospitalised due to alcohol-related disease and mental illness by 14 and 26, respectively; over 20 years these numbers are 61 and 99, respectively. Within four years 9 would be hospitalised or die due to traffic accidents; within 20 years this number is 22.

Restriction of the analysis to workers with three years of tenure (instead of one) does not change the results significantly. Job loss in a local labour market characterised by a high unemployment rate has a larger effect on all-cause mortality, due mainly to increased risk of death from circulatory diseases. This is not surprising, since economic stress may be an important intervening variable between job loss and negative health outcomes (see Section 2): a high local unemployment rate may increase the risk of becoming unemployed after job loss, as well as the risk of having to accept lower wages in order to get a new job.

There are indeed significant negative economic consequences of job loss in our sample. Although the majority of displaced workers finds a new job immediately, the effect of displacement on unemployment is very significant: excess risk in the displacement group of experiencing some unemployment during the year is 19 percentage points in the year of displacement (compared to the weighted control group), and even after 10 years it is 4

percentage points. Displacement also increases the probability of leaving the labour force: the effect is about 5 percentage points 4-10 years after the base year. This effect is largest for older workers, and is probably due mainly to early retirement and disability pension (see also Rege, Votruba & Telle 2009). The earnings losses of displaced workers are 13% in the year of plant closure. After that the losses are smaller, but 10 years after the base year they are still 8.5%. The estimated effects on both unemployment and earnings are similar to those for Sweden found by Eliason & Storrie (2006), but estimated earnings losses are much smaller than similar estimates for the UK (Hijzen, Upward & Wright 2010) and the US (Jacobson, LaLonde & Sullivan 1993; Sullivan & Wachter 2009).[6]

[6] Straightforward comparison with these studies for the UK and US is not possible because of important differences in study design. For instance, Jacobson, LaLonde & Sullivan (1993) study the effect of job loss due to downsizing, not plant closure, and the control group is restricted to workers who remain employed in the same firm at least seven years after the beginning of a displacement period. Both features may be expected to result in larger estimates of losses of earnings.

Appendix A. Definition of plant closure

As is now standard in analyses using Danish administrative register data, we consider a plant as closed if none of the following four criteria are satisfied: (1) same owner and same industry; (2) same owner and same employees; (3) same employees and same address; (4) same employees and 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 it means here 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. Even if condition (1) above is fulfilled, however, the plant is considered closed if no employees remain at the end of the year.

There are two problems with this and with any other definition of plant closure in relation to identification of displacements. First, a plant may be closed via absorption into (or merger 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 plant closure to be more restrictive; closure via absorption is not considered as closure.

Second, even if a plant closes, a large proportion of employees may at the end of the year be employed at other newly established plants. If this is the case for at least 40% of the employees we do not consider the plant closed.

Having identified plants which close according to the definition above, we define the year *t* of plant closure as *the year with the largest absolute reduction in the number of employees* given that the following conditions are satisfied for this year:

1. There are at least five employees at the end of year *t*-1
2. The number of employees is reduced by at least 10% and by at least three persons in year *t*
3. The plant is not, in year *t* or in following years, characterised by 'non-identical continuation' in the sense that the number of employees falls because part of the plant and its employees are separated out to another plant.

If a plant is closed according to the definition above, but these three conditions are not satisfied in any year in the sample period then this plant closure is ignored in the analysis. We also ignore closure of any plants which have existed for less than three years prior to closure.

Table A1. International Classification of Diseases (ICD) codes for categories of hospitalisation diagnoses and causes of death

Disease/cause of death	ICD8 codes (1980-1993)	ICD10 codes (1994-2006)
Cancer	140-239	C00-C97
Circulatory diseases	390-459	I00-I99
External causes of death	800-999	S00-Y91
Myocardial infarction	410	I21-I22
Stroke (cerebrovascular disease)	430-438	I60-I69
Alcohol-related diseases (including alcohol poisoning, addiction syndrome, delirious abstinence, alcohol psychosis, varicose vein on gullet, alcoholic disease of the liver, alcoholic disease of the pancreas)	291, 303, 4560, 57109, 57110, 57710, 78019, 97959, 980, E860 (up to 1986)	F100, F102, F104, F105, I85, K70, K860, T500, T510
Mental diseases	290-315	F00-F99
Suicide attempts	E950-E959 (1980-86), special variable kontaars=4 (1987-93)	Special variable kontaars=4
Traffic accidents (hospitalisation)	E810-E823 (1980-86), Special variables kontaars=2 and etraf=1 (1987-93)	Special variables: kontaars=2 and etraf=1
Suicide	950-959	X600-X849
Traffic accidents (death)	810-823	V010-V899

References

Browning, M.; A.M. Danø & E. Heinesen (2006): Job displacement and stress related health outcomes. *Health Economics*, 15: 1061-1075.

Daly, L.E. & G.J. Bourke (2000): *Interpretation and uses of medical statistics*. Oxford: Blackwell.

Eliason, M. & D. Storrie (2006): Lasting or Latent Scars? Swedish Evidence on the Long-Term Effects of Job Displacement. *Journal of Labor Economics*, 24(4): 831-856.

Eliason, M. & D. Storrie (2009a): Does job loss shorten life? *Journal of Human Resources*, 44(2): 277-302.

Eliason, M. & D. Storrie (2009b): Job loss is bad for your health – Swedish evidence on cause-specific hospitalisation following involuntary job loss. *Social Science and Medicine* 68: 1396-1406.

Eliason, M. & D. Storrie (2010): Inpatient psychiatric hospitalization following involuntary job loss. *International Journal of Mental Health*, 39(2): 32-35.

Hijzen, A.; R. Upward & P.W. Wright (2010): The income losses of displaced workers. *Journal of Human Resources*, 45(1): 243-269.

Hirano, K. & G.W. Imbens (2001): Estimation of causal effects using propensity score weighting: An application to data on right heart catheterization. *Health Services & Outcomes Research Methodology*, 2: 259-278.

Huttunen, K.; J. Møen & K.G. Salvanes (2010): How destructive is creative destruction? Effects of job loss on job mobility, withdrawal and income. *Journal of the European Economic Association*, forthcoming.

Jacobson, L.S.; R.J. LaLonde & D.G. Sullivan (1993): Earnings losses of displaced workers. *American Economic Review*, 83(4): 685-709.

Jahoda, M. (1982): *Employment and unemployment – a social psychological analysis*. Cambridge: Cambridge University Press.

Kasl, S. & B. Jones (2000): The Impact of Job Loss and Retirement on Health, chapter 6 in L. Berkman & I. Kawachi (eds.): *Social Epidemiology*. Oxford: Oxford University Press.

Kessler, R.C.; J.B. Turner & J.S. House (1987): Intervening processes in the relationship between unemployment and health. *Psychological Medicine*, 17(4): 949-961.

Kuhn, P.J. (ed.) (2002): *Losing work, moving on – International perspectives on worker displacement*. Kalamazoo, Michigan: W.E. Upjohn Institute for Employment Research,.

Morris, J.K. & D.G. Cook (1991): A Critical Review of the Effect of Factory Closures on Health. *British Journal of Industrial Medicine*, 48: 1-8.

Pearlin, L.I.; M.A. Lieberman, E.A. Menaghan & J.T. Mullen (1981): The Stress Process. *Journal of Health and Social Behavior*, 22: 337-356.

Rege, M.; M. Votruba & K. Telle (2009): The effect of plant downsizing on disability pension utilization. *Journal of the European Economic Association*, 7(4): 754-785.

Ruhm, C. (1991): Are workers permanently scarred by job displacements? *American Economic Review*, 81: 319-323.

Sullivan, D. & T. von Wachter (2009): Job displacement and mortality: An analysis using administrative data. *Quarterly Journal of Economics*, 124(3): 1265-1306.

Warr, P. (1987): *Work, unemployment and mental health*. Oxford: Clarendon Press.

Wooldridge, J.M. (2002): *Econometric analysis of cross section and panel data*. Cambridge, Massachusetts: MIT Press.

Dansk sammenfatning

Effekten på risiko for død og hospitalsindlæggelse af at miste sit job pga. virksomhedslukning

Det undersøges, om det at miste sit job i forbindelse med virksomhedslukning øger risikoen for død og hospitalsindlæggelse for mandlige lønmodtagere med en stærk arbejdsmarkedstilknytning.

Hovedresultaterne er, at afskedigelse pga. virksomhedslukning har negative helbredseffekter i form af øget dødelighed generelt, samt større sandsynlighed for at dø pga. hjertekarsygdomme, selvmord, alkoholrelaterede sygdomme og psykiske sygdomme (hvis sekundære dødsårsager medregnes); og øget risiko for at blive indlagt på hospital pga. alkoholrelaterede og psykiske sygdomme; og øget risiko for at dø eller blive indlagt på hospital pga. trafikulykker og selvmord/selvmordsforsøg. De negative helbredseffekter er størst umiddelbart efter, at man har mistet sit job, men de er også betydelige på mellemlangt sigt (op til ca. fire år efter afskedigelse), og for især generel dødelighed, dødelighed pga. hjertekarsygdomme, indlæggelse på hospital pga. alkoholrelaterede eller psykiske sygdomme og død eller indlæggelse pga. trafikulykker, er der betydelige effekter op til 20 år efter, at man har mistet sit job. Mens effekterne ikke varierer meget mht. alder eller anciennitet i virksomheden, tyder analysen på, at de er større, når den lokale arbejdsløshed er høj. Der er ikke statistisk signifikante effekter på risiko for død eller hospitalsindlæggelse som følge af kræft.

Risikoen for at dø (uanset årsag) øges med 84% i op til et år efter afskedigelse/virksomhedslukning, med 36% op til fire år efter, med 17% op til 10 år efter, og med 10% op til 20 år efter afskedigelse. Effekterne er altså størst lige efter afskedigelsen, men de er også betydelige (og statistisk signifikante) over en lang opfølgningsperiode på op til 20 år. De store kortsigtseffekter repræsenterer derfor ikke kun en fremskyndelse af dødsfald, der ville være indtruffet inden for en kort årrække alligevel. De nævnte effekter er store relativt set, men det er vigtigt at være opmærksom på, at den grundlæggende risiko for at dø er lille. Når fx risikoen for at dø inden for fire år efter afskedigelsen øges med 36%, er der tale om en stigning i dødeligheden fra 1,01% til 1,37%.

Effekten af afskedigelse på dødelighed som følge af hjertekarsygdomme har omtrent samme mønster som effekten på dødelighed generelt, dog er effekterne noget større; fx øger afskedigelse risikoen for at dø pga. en hjertekarsygdom inden for fire år med ca. 54%. Afskedigelse øger risikoen for hospitalsindlæggelse pga. alkoholrelaterede sygdomme på både kort og langt sigt; effekten er 28% i perioden op til 10 år efter afskedigelsen. Effekten på risikoen

for død pga. alkoholrelaterede sygdomme er stor på kort sigt (risikoen øges med 62% inden for fire år efter afskedigelse). Sandsynligheden for at begå selvmord øges markant i årene umiddelbart efter afskedigelse – med 86% inden for fire år.

Risikoen for hospitalsindlæggelse med psykiatriske diagnoser øges markant på både kort sigt (med 61% inden for et år efter afskedigelse), men også på langt sigt (med 20% i perioden op til 20 år efter afskedigelse).

De negative helbredseffekter er konsistente med teorier om, at afskedigelse – med deraf følgende øget risiko for arbejdsløshed eller en ringere eller mere ustabil jobsituation – for nogle mennesker kan føre til økonomisk og socialt betinget stress, depression og reduceret selvkontrol.

Analysen er baseret på danske registerdata for perioden 1980-2006 og alle lukninger af virksomheder i den private sektor i 1986-2002. Der fokuseres alene på virksomheder med kun et arbejdssted og mindst fem ansatte. Godt 33.000 mandlige lønmodtagere med stærk arbejdsmarkedstilknytning (fuldtidsbeskæftigede uden arbejdsløshed 3-4 år før virksomhedslukningen) mistede deres job pga. virksomhedslukninger. Deres helbred i en opfølgningsperiode på op til 20 år sammenlignes med helbredet for en kontrolgruppe af ca. 630.000 lønmodtagere, der var ansat i tilsvarende virksomheder, men som ikke mistede deres job pga. virksomhedslukninger. I analysen tages højde for de enkelte personers alder, uddannelse, familieforhold, anciennitet på virksomheden, branche, virksomhedsstørrelse samt tidligere helbred, arbejdsmarkedskarriere og indkomst.

Når der fokuseres på afskedigelser pga. virksomhedslukninger (og ikke afskedigelser eller arbejdsløshed generelt), er det fordi, alle ansatte mister deres job i forbindelse med en virksomhedslukning, mens der ved andre typer af afskedigelser kan være en vis selektion, således at fx ansatte med helbredsproblemer har en højere risiko for at miste deres job. Den statistiske metode (propensity score weighting) er baseret på, at observationerne i kontrolgruppen vægtes, således at den så godt som muligt ligner de lønmodtagere, der mistede deres job pga. virksomhedslukninger. Denne metode kombineres med (ikke-parametrisk) varighedsanalyse.

Den beskrevne analyse er mere omfattende end tidligere danske og udenlandske analyser af samme type. Resultaterne svarer i store træk til resultaterne fra disse tidligere undersøgelser, men det bedre datagrundlag betyder, at resultaterne er statistisk bedre bestemt.

Effect of Job Loss Due to Plant Closure on Mortality and Hospitalisation

We investigate whether job loss due to plant closure causes an increased risk of (cause-specific) mortality and hospitalisation for full-time male workers having strong labour-market attachment. We use unique administrative data: A panel of all persons in Denmark in the period 1980-2006, containing full records on demographics, health and work status, and a link from workers to plants. We use propensity score weighting combined with non-parametric duration analysis. We find that job loss increases the risk of overall mortality and mortality caused by circulatory disease; of suicide and suicide attempts; and of death and hospitalisation due to traffic accidents, alcohol-related disease and mental illness.

AKF
Anvendt KommunalForskning