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Marcus Eliason and Donald Storrie

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SCHOOL OF BUSINESS, ECONOMICS AND LAW, GÖTEBORG UNIVERSITY

Department of Economics
Visiting adress Vasagatan 1,
Postal adress P.O.Box 640, SE 405 30 Göteborg, Sweden
Phone + 46 (0) 31 786 000

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Marcus Eliason[†], Donald Storrie[‡]

[†] *Centre for European Labour Market Studies (CELMS), Department of Economics, Göteborg University, Sweden*
[‡] *The Institute for Labour Market Policy Evaluation (IFAU), Uppsala, Sweden*
[§] *European Foundation for the Improvement of Living and Working Conditions, Dublin, Ireland*

Abstract

This paper examines the causal effect of job loss on overall and cause-specific mortality. Using linked employer-employee register data, we identified the job losses due to all establishment closures in Sweden in 1987 and 1988. Hence, we have extended the case study approach, which has dominated the plant closure literature, and also been able to remedy most weaknesses associated with previous studies. We found that the overall mortality risk for men was increased by 44 percent during the first four years following job loss. For women and in the longer run we found no effects. The short-run excess mortality was mainly attributed to increased risk of suicides and alcohol-related causes of death. For both sexes, the increase in suicides was about twofold for both men and women, while the increase in alcohol-related causes of death was somewhat less.

JEL Classification: I12, J63, J65

Key Words: Plant closure, displaced workers, mortality

1. Introduction

Job loss is an inevitable feature of a well-functioning market economy, but may have severe consequences for those losing their jobs. Among the adverse consequences are not only immediate and lasting earnings losses (e.g., Eliason & Storrie, 2006; Jacobson *et al*., 1993) but as one of the more stressful life events (Miller & Rahe, 1997), job loss is also believed to severely affect health. However, although the association between job loss, or unemployment, and ill-health is unquestionable, the existence of a causal link is still debated (Goldney, 1997). Despite extensive attention from various academic disciplines, the empirical issue of whether there is a causal relation running from job loss and unemployment to ill-health is fraught with so many methodological difficulties that the relationship is still somewhat unclear. The main obstacle is to disentangle whether it is job loss and unemployment that leads to ill-health (i.e., the causality hypothesis) or if an observed association is due to those with poor health being more likely to lose their jobs and/or remain unemployed (i.e., the selection hypothesis). Reviews of previous research (e.g., Morris & Cook, 1991; Weber & Lehnert, 1997) have claimed that no study satisfies the requirements for establishing causality as opposed to merely an association. However, two recent studies claim to nearly fulfill the requirements in the earlier review; using better data than previously available, Browning, Dano, and Heinesen (2005) found no impact of job displacement on hospital admission for stress-related diseases, whilst Keefe et al. (2002) found increased risk of self-harm leading to hospital admission or death. Recent research has also provided evidence of increased aggregate mortality when the unemployment rate goes down (Gerdtham & Ruhm, 2006; Ruhm, 2000). Although the latter is not evidence that job loss and unemployment do not have detrimental effects on health for those affected, it has nonetheless fueled the debate on the impact of economic conditions on health.

The postulated mechanisms through which job loss may affect health include stress associated to financial strain and the loss of psychosocial assets such as time structure, personal status, and work relationships; triggering of, or increased vulnerability to, subsequent adverse life events; and destructive coping strategies or risky behaviors. Stress associated with financial strain does not only encompass the 'acute stressor' of immediate earnings losses during a period of unemployment but there is also consistent evidence that job loss inflicts earnings losses in the longer run (e.g., Eliason & Storrie, 2006; Jacobson et al., 1993) and thus constitutes a 'chronic stressor.'[1] The earnings losses in the longer run has partly been explained by increased risk of subsequent job losses (Stevens, 1995), but repeated job loss is not the only adverse life event that can follow. For example, some studies have shown that marriages may end in divorce as a consequence of unemployment (Hansen, 2005; Kraft, 2001). Moreover, both financial and psychological strain following job loss may undermine the resources needed to cope with other adverse life events (Kessler, Turner & House, 1987). The strategies to cope with the stressors may also be destructive and directly harmful as, for example, increased smoking (Falba et al., 2005; Lee et al., 1991), alcohol abuse (Catalano et al., 1993; Dooley & Prause, 1998) and, perhaps most prominently, suicidal behavior (Blakely, Collings & Atkinson, 2003; Kposowa, 2001; Lewis & Sloggett, 1998).

These mechanisms are consistent with the diseases and causes of death, which in the literature have been associated with job loss and unemployment. Stress, smoking, and excessive alcohol consumption are linked to cardiovascular diseases,[2] smoking is strongly associated with lung cancer and several other cancers,[3] and both psychological distress and alcohol abuse are associated with suicide. Suicides constitute a considerable part of the premature deaths in industrialized countries and about a twofold increase has been found in fatal suicides among unemployed (e.g., Blakely et al., 2003; Gerdtham & Johannesson, 2003; Kposowa, 2001). This may in part be due to selection, but can also be explained by an increased vulnerability to subsequent adverse life events or increases of risk factors for suicide (Blakely et al., 2003). The

[1] It must be viewed unlikely, however, that the loss of income, in developed economies and particularly in a well-developed welfare state like Sweden, would lead to a degree of material deprivation that would directly affect on physical health due to malnutrition, bad housing, or lack of access to health care.

[2] See Weber and Lehnert (1997) for a review of the literature on unemployment and cardiovascular diseases.

[3] See Lynge (1997) for a review of the literature on unemployment and cancer.

loss of social networks and daily time-structure may also imply less surveillance, which can be an important factor especially for people with an already troubled mind (Kposowa, 2003).

The objective of the present study is to investigate the causal impact of job loss on mortality. Like many other studies on the health effects of job loss, we exploit plant (i.e., establishment) closures as a strategy to establish causality and overcome the problem of health selection.[4] To some extent, one can view a plant closure as a natural experiment, since all workers are laid off irrespective of their individual characteristics (e.g., health status) and behavior (e.g., alcohol abuse). However, in contrast to previous plant closure studies the present study is not a case study. In fact, using linked employee-employer register data, we identify all establishment closures in Sweden in 1987 and 1988. Thus, we are able to remedy weaknesses such as unrepresentativeness, small samples, lack of pre-closure health status, and lack of an appropriate control group, which has hampered most previous studies. The use of register data also allow us to follow these workers for a long period (i.e., three pre-displacement years and up to 12 post-displacement years) with negligible attrition. This may allow enough time to observe deaths due to diseases that only become manifest in the longer run.

2. Data

To create the linked employee-employer data used in this paper, containing information from 1983 to 1999, six registers were merged: the Cause of Death Register (*Dödsorsaksregistret*), the Hospital Discharge Register (*Patientregistret*), the Business Register (*Företagsdatabasen*), the Register Based Labor Market Statistics (*Registerbaserad arbetsmarknadsstatistik*), and the Income and Wealth Register (*Inkomst- och förmögenhetsstatistiken*). [5] The linkage of registers is possible since every resident and every establishment in Sweden has a unique identity number, i.e., a civic registration number or an organization number. Moreover, as the obligatory income statements, filed to the taxation authorities by the employer, contain both the employee's civic registration number and the establishment's organization number, we can link all employees to their establishment; a feature of the data that enabled the identification of both the closing establishments and the workers who were displaced.

2.1. Definition and identification of closing establishments and displaced workers

Establishment 'births' and 'deaths' can be traced back to 1985 in Statistic Sweden's Business Register. A potentially closed establishment can be identified by the disappearance of its identity number from the tax returns. As Kuhn (2002) emphasizes, this identification procedure may lead to 'false firm deaths,' i.e., a change of organization number due to, for example, a new legal form or a new owner is incorrectly interpreted as a closure. To eliminate this problem, Statistics Sweden surveys the firm if the establishment was part of a multi-establishment firm or had at least 10 employees. By this means, all establishments with at least 10 employees that shut down in 1987 or 1988 were identified.

In administrative data one can observe separations between employees and employers, but cannot distinguish between quits and layoffs. Thus, one has to define displacements as separations in connection to the closure. However, a closure is typically a process over time and the final shutdown may be preceded by both pre-emptive quits in expectation of the impending

[4] Early examples of factory closure studies are Kasl, Gore and Cobb (1975) and Beale and Nethercott (1985), while a more recent is Keefe et al. (2002).
[5] These registers are in turn created by compiling data from several other registers.

closure and displacements initiated by the employer. Thus, previous plant closure studies using administrative data have assumed a time-window preceding the closure within which all separations have been defined as displacements (i.e., job losses).[6] [7] The crucial question is then how wide the time-window should be, i.e., how close in time to the actual shutdown one should assume a separation to be a displacement. There is a trade-off in that the closer one is to the shutdown the more likely a separation will in fact be a displacement and not normal turnover, but also the more likely that these workers constitute a non-representative sample of all workers affected by the closure. One has reason to suppose that those with better outside options will be more likely to quit, but on the other hand, the firm will be more likely to first lay off its less valuable workers.[8] [9] Hence, to be able to take advantage of the experimental situation that closures offer, it is essential to identify all workers affected by the closure and not only those laid-off at the time of the actual shutdown.

The procedure applied here recognizes both this selection issue and that individual closing processes can be both lengthy and of various lengths. We have improved the time-window procedure by letting the width of the window vary among the closures. An upper limit of the probable duration of the closing processes was first set to three years.[10] Then, based on establishment size and worker flows during the three years prior to closure, the duration of each individual closing process was defined to be either one, two, or three years.

Thereafter, we defined the displaced workers to be those employed at a closing establishment in November in one year, within the defined time-window for that particular establishment, but not in November of the following year. We identified 13,943 displaced workers, in the age of 25-64 yrs, corresponding to 760 establishment closures, and a control group comprised of a random sample of about 165,000 workers of the same ages and employed in November of 1986, but not at a closing establishment. After deleting observations with missing information for any of the baseline variables 12,337 displaced and 146,687 non-displaced workers remained. All these workers could be followed during first a pre-displacement period of three years and then a post-displacement period of a maximum of 12 years.

2.2. Outcome variables

The Cause of Death Register comprises all deaths of Swedish residents, irrespective of both citizenship and whether they occurred in Sweden or not. During the time-period under study, the causes of deaths were classified according to three different revisions of the International Classifications of Diseases and causes of death (ICD). Prior to 1987 causes of deaths were classified by ICD-8, while ICD-9 was used for the period 1987-1996, and ICD-10 thereafter. We examined all-cause mortality as well as deaths from three major categories of underlying causes of death: neoplasms (ICD-8/ICD-9 codes: 140-239; ICD-10 codes: C00-C97), cardiovascular

[6] We will use ‘job loss’ and ‘displacement’ interchangeably.

[7] Among studies on health-related outcomes, however, we are aware of only one (i.e., Browning et al., 2006) that uses administrative data.

[8] Ill-health is not in itself a valid basis for layoff. In practice, as long as the employee is receiving the publicly financed sickness benefit, ill-health may not be used as a criteria to lay off an employee. It is only when ill-health leads to a deterioration of capacity for work that may be judged to be permanent, and no other suitable work can be found at the workplace, that dismissals on the basis of ill-health may occur (Lunning, 1989).

[9] Case study evidence in Pfann and Hamermesh (2001) indicate such complex mechanisms.

[10] Storrie (1993) found that the closure process, of a large Swedish shipyard (i.e., a plant with a long period of production), from the public announcement to when the plant was finally closed, was just under three years. This is also the longest time-window applied in Bender et al. (2002).

diseases (ICD-8/9: 390-459; ICD-10: I00-I99), and external causes (ICD-8/9: 807-999; ICD-10: S00-Y91). In addition we examined deaths from ischemic diseases (ICD-8/9: 410-414; ICD-10: I20-I25), cerebrovascular diseases (ICD-8/9: 430-438; ICD-10: I21, I22, I60-I69, G45), smoking related cancer (ICD-8: 197.8; ICD-8/9: 140-151, 155, 157, 160-162, 188, 189.0, 189.1, 205.0; ICD-10: C0-C16, C22, C25, C30-C34, C64-C67, C929), alcohol-related mortality (ICD-8/9: 291, 303, 571.0, 577.0, 577.1, 980; ICD-9: 305.0, 357.5, 425.5, 535.3, 571.1-571.3; ICD-10: F10, G31.2, G62.1, K29.2, I42.6, K70, K85, K86.0, K86.1, X45, X65, Y15) and suicides (ICD-8/9: E950-E959, E980-E989; ICD-10: X60-X64, X66-X84, Y10-Y14, Y16-Y34).

2.3. Baseline variables

The data also contain rich information on personal characteristics for three pre-displacements years. However, many measures for the calendar year immediately preceding the job loss will not be used as they may already have been affected by the impending closure.

Most of the variables included to control for baseline health status were derived from the Hospital Discharge Register, which contains information on hospital inpatient stays (both for somatic and psychiatric care), including the number of stays, total length of stays, and diagnosis.[11] The particular diseases, or categories of diseases, we controlled for were the following with ICD-8 codes in parentheses: smoking-related cancer (140-151, 155, 157, 160-162, 188, 189.0, 189.1, 197.8, 205.0), other malign neoplasms (152-154, 156, 158, 159, 163-187, 189.2-189.9, 190-197.7, 197.9-209), benign neoplasms (210-229), undefined neoplasms (230-239), diabetes (250), psychoses and neuroses (290-309), multiple sclerosis (340), Parkinson's disease (342), other diseases of the nervous system (320-324, 330-333, 341, 343-349), hypertonic diseases (400-404), ischemic diseases (410-414), other heart diseases (390-398, 420-429, 746), cerebrovascular diseases (430-438), other circulatory diseases (440-458), chronic respiratory diseases (490-493, 515-518, 748), ulcers and gastritis (531-535), diseases of the liver (570-573), diseases of the pancreas (577), nephritis and nephrosis (580-584), alcohol-related conditions (291, 303, 571.0, 577.0, 577.1, 979, 980, E860), self-harm (950-959, 980-989), and accidents (807-949, 960-978, 990-999). In addition, we also controlled for recorded disability and annual number of insured sick-leave days.

The other baseline, or pre-displacement, variables can be categorized into demographic, socioeconomic, regional, and occupational variables. The demographic variables include age, marital status, number of children, and world region of origin (11 categories). The regional variables include a regional classification of municipality of residence (9 categories), local unemployment rate, and local income level.[12] Socio-economic factors that we controlled for are attained educational level (8 categories), indicators of house possession and incidence of taxable wealth, two years of annual earnings, social benefits, and disposable income, as well as days of unemployment. The occupational variables include type of industry sector (30 categories) and the educational level at the workplace.

[11] This information is reported by the County Health Authorities to the National Board of Health and Welfare that compile the register. Unfortunately, a few counties did not report during our defined pre-displacement period; persons living in these counties were therefore excluded from the analysis.

[12] The classification is defined by the Swedish Association of Local Authorities and based on demography, urbanization, and employment structure.

3. Empirical method and estimations

In epidemiological mortality studies, the standard method is a logistic regression or a Cox proportional hazards model. Recently, however, propensity score methods and the related method of inverse-probability-of-treatment-weighting have been increasingly popular. We will adopt a propensity score weighted regression estimator similar to the estimators proposed in Hirano and Imbens (2001) and Robins, Hernán, and Brumback (2000).

3.1. The propensity score weights

By propensity score weighting (PSW) one will ideally obtain a pseudo-sample where the distribution of observed characteristics is the same in the samples of treated (displaced workers) and non-treated (non-displaced workers). The propensity score (p or PS) is the probability of treatment (Rosenbaum & Rubin, 1983), which is to be estimated. Our estimates will be based on a logistic regression model: $p = \Pr[D_{baseline}=1|X_{baseline}] = \{1+\exp(-\alpha_0-\alpha_1 X_{baseline})\}^{-1}$, where $D_{baseline}$ is an indicator taking the value 1 if the worker were displaced at baseline and 0 otherwise, and $X_{baseline}$ is a set of baseline variables.

To estimate the effect on those actually displaced (i.e., the treatment effect on the treated) the weight assigned to worker i is defined as $w_i = D_i+(1-D_i)\cdot p_i/(1-p_i)$.[13] Hence, all the displaced workers are assigned a weight equal to one, while each non-displaced worker i is assigned a weight equal to $p_i/(1-p_i)$, i.e., the odds ratio of being displaced at the baseline.

3.2. The estimators

The PS weights were then used in a weighted discrete-time logistic regression (DTLR): $\ln[h(t)/(1-h(t)] = \lambda(t)+\beta Z_{baseline}+\delta(t|D_{baseline})$, where $h(t)$ is the hazard rate, or the conditional probability of death, $\lambda(t)$ is a function of time, $Z_{baseline}$ is a set of baseline variables, and $\delta(t|D_{baseline})$ is a time-varying effect of job loss at the baseline.[14] [15] Here $\lambda(t)$ will be modeled fully non-parametrically, while $\delta(t|D_{baseline})$ will take a piecewise constant form with three periods each of four years of length (i.e., 1–4 yrs, 5–8 yrs, and 9–12 yrs). Hence, $\delta(t|D_{baseline}) = \sum_{k=1}^{3} \delta_k I_k(t) D_{baseline}$, where $I_k(t)$ is an indicator taking the value 1 if t falls in interval k and 0 otherwise.

The above constitutes the general framework for the estimation strategy. The choices of X and Z can produce four different estimators. If we let $\varnothing$ denote the empty set, then $X=\varnothing$ and $Z=\varnothing$ (i.e., no covariates are included in neither the PS estimation nor the following DTLR) yields the crude, or unadjusted, estimates. If $X\neq\varnothing$ and $Z=\varnothing$ (i.e., covariates are included only in the PS estimation) then we will have a PSW estimator without further covariate adjustment, and if $X=\varnothing$ and $Z\neq\varnothing$ (i.e., covariates are included only in the DTLR) we will instead have the usual unweighted DTLR. Finally, $X\neq\varnothing$ and $Z\neq\varnothing$ (i.e., covariates are included in both the PS estimation and the following DTLR) results in a PS weighted DTLR.

Incorporating a covariate-adjusted analysis also after the PSW may control for any remaining differences between the treated and non-treated. However, as death is a rare outcome, not many

[13] See Hirano and Imbens (2001).

[14] We derived robust standard errors by allowing for clustering.

[15] The odds ratios produced by a pooled logistic regression are equivalent to the hazard ratios obtained from a Cox proportional hazard model given that the hazard of death is small in every time period (D'Agostino, Lee & Belanger, 1990).

covariates can be incorporated in neither a weighted nor an unweighted logistic regression model. Thus, we are in favor of the PSW estimator, without further covariate adjustment after the weighting, and in the following analysis, we will only present estimates from this estimator. However, in a section on robustness checks we will present estimates on overall mortality using the alternative estimators above.

3.3. Assessment of the covariate balance

Before presenting the results, we will assess the degree of covariate balance. To compare comparable people, has been shown to be important in reducing selection bias in evaluation studies (Heckman et al., 1998) and as other propensity score methods, PSW aims to obtain samples of treated and non-treated with the same distribution of observed covariates.

To determine whether we have obtained comparable samples with respect to the baseline covariates we computed the standardized differences in means (SDM) before and after the PSW.[16] [17] For brevity, the SDMs are only summarized here, while the SDMs for each single baseline covariate can be found in the Appendix (Table A1).

Table 1. Summary statistics of the covariate balance in the unweighted and weighted samples of men and women.

	Men			Women		
Sample	Mean	Min	Max	Mean	Min	Max
Unweigthed sample	8.804	0.178	31.778	7.939	0.038	49.387
Weighted sample	0.307	0.000	1.523	0.263	0.000	1.057

As we can see from Table 1 the average (of the absolute values of the) SDMs have decreased considerably. Before the PSW the average was 8.8 for the sample of men and 7.9 for the sample of women. After the PSW, however, it decreased to about 0.3 and the largest SDM for a single covariate was no more than 1.5, indicating that the weighting generated pseudo-samples of non-displaced workers on average almost identical to the samples of displaced workers.

4. Results

4.1. The effect of job loss on overall and cause-specific mortality

Table 2 shows the estimated hazard ratios, with 95% confidence intervals, of job loss on overall mortality and three major categories of causes of death, i.e., malign neoplasms, circulatory diseases, and external causes, as well as all remaining causes. For overall mortality, the only striking result is the increased risk for men over the first four years following job loss; during these first years the mortality risk was 44% higher (HR: 1.44; 95% CI: 1.19-1.76) for male workers who were displaced at baseline compared to those who were not. Although one may hypothesize that job loss may lead to diseases that only become manifest in the longer run, or that job loss may trigger an accumulation of adversities that only would be apparent in mortality data after many years, this is not supported by the estimates. The hazard ratios, in both

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[16] The standardized difference in means is the difference in covariate means between the displaced workers and the weighted non-displaced workers, in percentage of the pooled standard deviation (before weighting) of that covariate.
[17] Various balancing tests have been suggested in the literature (Smith & Todd, 2005), but there is no consensus on which of them to apply or on what degree of balance is satisfying. However, in Rubin and Rosenbaum (1985) a SDM of 20 was judged substantial.

subsequent four-year periods, are close to one and statistically insignificant, as is the hazard ratio for women in all three periods.

Table 2. The estimated impact of job loss on overall mortality and death from some major causes, by sex, expressed as hazard ratios with 95% confidence intervals.

Sex and time period	Overall		Malign neoplasms		Circulatory diseases		External causes		All other causes	
	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)
Men	(n=474/453)[a]		(n=127/130)[a]		(n=172/166)[a]		(n=78/62)[a]		(n=97/95)[a]	
≤ 4 yrs	1.44	(1.19-1.76)	1.39	(0.96-2.00)	1.24	(0.90-1.71)	2.07	(1.42-3.02)	1.31	(0.75-2.30)
5 – 8 yrs	0.98	(0.82-1.17)	0.84	(0.60-1.18)	1.20	(0.91-1.58)	0.84	(0.52-1.37)	0.92	(0.60-1.40)
9 – 12 yrs	0.91	(0.77-1.07)	0.93	(0.69-1.24)	0.84	(0.64-1.10)	0.99	(0.59-1.63)	0.98	(0.69-1.39)
Women	(n=200/189)[a]		(n=111/100)[a]		(n=46/41)[a]		(n=14/14)[a]		(n=29/34)[a]	
≤ 4 yrs	1.01	(0.74-1.37)	0.86	(0.55-1.35)	1.30	(0.69-2.45)	1.48	(0.61-3.59)	0.85	(0.36-1.98)
5 – 8 yrs	1.04	(0.79-1.38)	1.19	(0.83-1.70)	1.05	(0.58-1.90)	0.84	(0.26-2.77)	0.68	(0.31-1.50)
9 – 12 yrs	1.10	(0.88-1.38)	1.20	(0.89-1.63)	1.10	(0.69-1.77)	0.76	(0.24-2.39)	0.95	(0.55-1.64)

[a] Propensity score weighted number of deaths among the displaced and non-displaced workers, respectively.

For men, the excess mortality in the first four years applies to all of the major categories of causes of death, although the hazard ratio for external causes of death is considerably larger and the only estimate that is statistically significant. For women, the estimates also indicate an increased initial risk of death from external causes (HR: 1.48; 95% CI: 0.61-3.59) and circulatory diseases (HR: 1.30; 95% CI: 0.69-2.45); none of these estimates are statistically significant, however, and neither are any of the estimates of the longer-term impact

Although job loss potentially may have a broad impact on health, some diseases and causes of death can be considered especially likely. Mortality from stress-related diseases such as ischemic diseases is one example, as is causes of death related to health damaging behavior such as smoking, excessive alcohol consumption, and suicidal behavior.[18] Therefore, we will extend the analysis by exploring the impact of job loss on a number of specific causes of death: ischemic diseases, cerebrovascular diseases, smoking-related cancer, alcohol-related conditions, and suicides.

Table 3. The estimated impact of job loss on death from some specific causes, by sex, expressed as hazard ratios with 95% confidence intervals.

Sex and time period	Smoking-related cancer		Ischemic diseases		Cerebrovascular diseases		Alcohol-related diseases		Suicide	
	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)
Men	(n=62/66)[a]		(n=116/107)[a]		(n=37/39)[a]		(n=33/28)[a]		(n=42/31)[a]	
≤ 4 yrs	1.48	(0.92-2.40)	1.33	(0.91-1.93)	1.43	(0.54-3.74)	2.21	(1.14-4.31)	2.15	(1.28-3.59)
5 – 8 yrs	0.76	(0.46-1.25)	1.29	(0.93-1.80)	0.59	(0.25-1.39)	0.44	(0.17-1.13)	0.82	(0.41-1.67)
9 – 12 yrs	0.84	(0.54-1.30)	0.84	(0.60-1.17)	1.02	(0.68-1.55)	1.38	(0.73-2.60)	1.28	(0.67-2.44)
Women	(n=32/34)[a]		(n=27/24)[a]		(n=12/13)[a]		(n=5/6)[a]		(n=11/7)[a]	
≤ 4 yrs	0.94	(0.42-2.12)	1.50	(0.67-3.37)	0.36	(0.05-2.72)	1.63	(0.34-7.81)	2.03	(0.73-5.63)
5 – 8 yrs	1.26	(0.67-2.37)	0.89	(0.40-1.99)	0.79	(0.19-3.38)	0.48	(0.06-3.64)	1.54	(0.44-5.39)
9 – 12 yrs	0.77	(0.43-1.40)	1.14	(0.61-2.15)	1.17	(0.57-2.39)	0.66	(0.13-3.37)	0.78	(0.18-3.29)

[a] Propensity score weighted number of deaths among the displaced and non-displaced workers, respectively.

[18] The risk of ischemic diseases is also increased by smoking and excessive alcohol consumption.

The estimates in Table 3 indicate that the mortality patterns found above for male job losers largely applies also to these specific causes of death. For alcohol-related deaths and suicides, the hazard ratio is 2.21 (95% CI: 1.14-4.31) and 2.15 (95% CI: 1.28-3.59), respectively, during the first four-year period, whilst for the other causes of death the hazard ratio ranges from 1.33 for ischemic diseases to 1.48 for smoking-related cancer. In the two following four-year periods the estimates vary more among the causes; for example, the hazard ratio for ischemic diseases is almost as large in the second period (HR: 1.29; 95% CI: 0.93-1.80) as in the first period, while the hazard ratio for alcohol-related deaths is only 0.44 (95% CI: 0.17-1.13). This seemingly inconsistent impact on alcohol-related mortality may be explained by the samples of both displaced and non-displaced workers containing a number of workers with a long history of excessive drinking for which increased consumption only hastened an inevitable process.

The impact of job loss on suicides, alcohol-related mortality, and deaths due to ischemic diseases, seem to be similar for both sexes. However, the overall lower premature mortality among women results in lower power of the statistical tests. Thus, none of the estimates for women are statistically significant although they are almost as large as those for men. For death due to cerebrovascular disease, however, female mortality actually seem to be lower in the first years following job loss (HR: 0.36), but the confidence intervals are again very wide (95% CI: 0.05-2.72).

4.3. Heterogeneity

In this section, we will further explore the heterogeneity in the impact of job loss by age, marital status, and health status.[19] [20] Previous studies have shown that the excess mortality caused by unemployment is the highest for middle-aged men (Mathers & Schofield, 1998). This is to some extent supported here by a large hazard ratio for men in the age of 55-64 yrs (HR: 1.60; 95% CI: 1.22-2.10), but we find an even higher risk for male workers aged 25-34 yrs. For these young men, job loss more than doubled the risk of dying within the first four years (HR: 2.20; 95% CI: 1.20-4.01). Men aged 35-44 yrs did not at all seem to experience a similar excess risk and one may speculate, for example, that workers of this age, the prime working age, easily got new jobs while the younger and the older workers experienced continued difficulties on the labor market.

Table 4. The estimated impact of job loss on overall mortality, by sex and age, expressed as hazard ratios with 95% confidence intervals.

Sex and	Age 25-34 yrs		Age 35-44 yrs		Age 45-54 yrs		Age 55-64 yrs	
time period	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)
Men	(n=44/36)[a]		(n=85/87)[a]		(n=122/111)[a]		(n=223/210)[a]	
≤ 4 yrs	2.20	(1.20-4.01)	1.00	(0.60-1.69)	1.36	(0.92-1.99)	160	(1.22-2.10)
5 – 8 yrs	1.20	(0.66-2.18)	1.11	(0.74-1.66)	1.02	(0.72-1.45)	0.98	(0.75-1.28)
9 – 12 yrs	0.82	(0.43-1.55)	0.86	(0.57-1.28)	1.07	(0.79-1.46)	0.88	(0.69-1.12)
Women	(n=20/15)[a]		(n=43/36)[a]		(n=51/51)[a]		(n=86/92)[a]	
≤ 4 yrs	0.74	(0.21-2.58)	0.74	(0.35-1.57)	0.82	(0.41-1.62)	1.17	(0.75-1.81)
5 – 8 yrs	2.95	(1.36-6.41)	1.10	(0.56-2.15)	1.02	(0.60-1.74)	0.75	(0.47-1.19)
9 – 12 yrs	0.80	(0.30-2.11)	1.64	(1.02-2.65)	1.07	(0.70-1.63)	0.95	(0.66-1.35)

[a] Propensity score weighted number of deaths among the displaced and non-displaced workers, respectively.

[19] The PS weights were re-estimated for each of the samples in this section. For brevity, however, no assessment of the covariate balance is presented.

[20] This kind of subgroup analysis, however, is most often underpowered and by repeating the analysis within several subgroups one will also increase the risk of false significant findings. Thus, the findings should be interpreted with caution.

For women, just as for men, the impact on the youngest seems to have been the greatest. However, the excess mortality for young displaced women is not, as for displaced men, found in the first four years but later on. The hazard ratio in the second four-year period following the job loss is 2.95 (95% CI: 1.36-6.41), while the hazard ratio in the first four years is only 0.74 (CI: 0.21-2.58). For women aged 45-64 yrs, however, there seem to be no impact at all.

We also performed an equivalent subgroup analysis by marital status. Several studies have shown that marriage is associated with better health[21] and a spouse may offer valuable support, both financially and emotionally in difficult times, which might ameliorate any negative impact. On the other hand, a spouse can also be a burden in such times, depending on how the couple is coping with the job loss, and for married job losers marital instability and divorce can subsequently follow in a chain of adversities (Hansen, 2005; Kraft, 2001). Analogously, job loss is an additional adverse life event for persons who are divorced or widowed; although the job loss is then not causally linked to the divorce, or the death of the spouse, it might still be the accumulation of life events that affect health adversely.

Table 5 shows that only the immediate impact of job loss on married men (HR: 1.50; 95% CI: 1.16-1.93) and divorcés/widowers (HR: 1.65; 95% CI: 1.12-2.43) is statistically significant. For married women the impact also seems to be somewhat larger, as well as more enduring, but the estimates are neither considerably large nor statistically significant.

Table 5. The estimated impact of job loss on overall mortality, by sex and marital and health status, expressed as hazard ratios with 95% confidence intervals.

	Marital status						Health status			
Sex and	Single		Married		Divorced/widowed		Good health		Poor health	
time period	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)
Men	(n=133/126)[a]		(n=253/233)[a]		(n=88/95)[a]		(n=336/315)[a]		(n=138/139)[a]	
≤ 4 yrs	1.14	(0.70-1.86)	1.50	(1.16-1.93)	1.65	(1.12-2.43)	1.31	(1.01-1.70)	1.71	(1.25-2.34)
5 – 8 yrs	1.04	(0.73-1.49)	1.10	(0.87-1.38)	0.67	(0.42-1.06)	1.09	(0.89-1.34)	0.74	(0.51-1.07)
9 – 12 yrs	1.02	(0.73-1.43)	0.91	(0.73-1.13)	0.78	(0.52-1.15)	0.94	(0.78-1.14)	0.83	(0.60-1.16)
Women	(n=34/34)[a]		(n=116/94)[a]		(n=50/59)[a]		(n=141/135)[a]		(n=59/54)[a]	
≤ 4 yrs	0.79	(0.34-1.86)	1.19	(0.79-1.79)	0.93	(0.52-1.66)	1.00	(0.67-1.49)	1.02	(0.61-1.69)
5 – 8 yrs	1.01	(0.51-1.97)	1.18	(0.82-1.71)	0.83	(0.48-1.44)	0.86	(0.60-1.22)	1.61	(1.01-2.56)
9 – 12 yrs	1.15	(0.66-2.01)	1.31	(0.98-1.76)	0.80	(0.50-1.27)	1.18	(0.92-1.52)	0.80	(0.46-1.39)

[a] Propensity score weighted number of deaths among the displaced and non-displaced workers, respectively.

Finally, we performed a subgroup analysis by health status at baseline. Health status was based on disability and hospital in-patient care during the two first baseline years; workers with a record of either disability or hospital in-patient care were classified as of ‘poor health’ and all others as of ‘good health.’ The immediate impact found above, may suggest that the stress from job loss rather exacerbated or aggravated already existing disease processes than initiated any new disease or disorder. If this were the case, we would expect a larger immediate impact on those who initially had worse health. The estimates also support this to some extent, but the difference is not that large and there is a statistically significant immediate excess mortality also among male workers of ‘good health.’ For women, however, there is a statistically significant excess mortality only among those with worse health, but not until the second four-year period following job loss.

[21] See Wilson and Oswald (2005) for a review.

4.4. Robustness check

As a check of robustness, we will compare the estimates on overall mortality in the main analysis to the unadjusted estimates, and the estimates from both the unweighted and the propensity score weighted (PSW) discrete-time logistic regression (DTLR).[22] Moreover, we will also explore the sensitivity to the width of the time-window defining the job losses by narrowing it down to one year.

Table 6. Comparison of the estimated impact of job loss on overall mortality, by sex, using various estimators and widths of the time-window defining the job losses, expressed as hazard ratios with 95% confidence intervals.

	Three-year-window								One-year-window	
Sex and	Unadjusted		DTLR		PSW+DTLR		PSW			
time period	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)	HR	(95% CI)
Men										
≤ 4 yrs	1.61	(1.36-1.92)	1.42	(1.19-1.70)	1.48	(1.22-1.81)	*1.44*	*(1.19-1.76)*	1.41	(1.10-1.79)
5 – 8 yrs	1.14	(0.96-1.35)	1.02	(0.86-1.21)	1.01	(0.85-1.21)	*0.98*	*(0.82-1.17)*	0.95	(0.77-1.17)
9 – 12 yrs	1.00	(0.85-1.17)	0.91	(0.77-1.07)	0.94	(0.80-1.11)	*0.91*	*(0.77-1.07)*	0.92	(0.76-1.11)
Women										
≤ 4 yrs	1.12	(0.84-1.51)	1.05	(0.77-1.42)	1.01	(0.74-1.39)	*1.01*	*(0.74-1.37)*	1.15	(0.83-1.59)
5 – 8 yrs	1.09	(0.83-1.42)	1.02	(0.77-1.35)	1.05	(0.80-1.40)	*1.04*	*(0.79-1.38)*	0.97	(0.70-1.33)
9 – 12 yrs	1.19	(0.96-1.47)	1.12	(0.89-1.41)	1.11	(0.88-1.41)	*1.10*	*(0.88-1.38)*	1.07	(0.83-1.38)

NOTE. – The estimates in italics are the estimates from the main analysis, which can be compared to both the estimates using the one-year-window and the estimates obtained by the other estimators using the same three-year window.

Table 6 shows that the estimates from both the weighted and the unweighted DTLR are very close to those based only on PSW. All covariate-adjusted estimates are more conservative than the unadjusted estimates, but even those are quite similar. Moreover, although we believe our three-year-window, defining the displaced workers, to be essential for viewing plant closure as a natural experiment, narrowing it to one year (i.e., including only those who separated from their establishment within the same year as the closure) does not alter the estimates much.

4.5. Causality or selection bias

We have presented evidence that displaced workers experience a higher risk of premature death in the first four years following job loss. Especially, we found higher mortality from alcohol-related conditions and suicides, and to some extent from ischemic diseases. Although the estimates seem to be robust to the choice of estimator, a key issue is whether they are causal effects of job loss or a result of selection bias. Our first main argument for that causality has been established is that a plant closure is close to a natural experiment. When a plant shuts down all workers are separated from their jobs irrespective of their individual characteristics. Hence, the strategy to focus exclusively on job loss due to plant closure will greatly reduce any selection bias and has also previously been a popular strategy in the literature.

However, most plant closure studies have had serious shortcomings. In their review of factory closures, Morris and Cook (1991) described an 'ideal study' as having the following characteristics: a large number of workers, an adequate comparison group, high response rate, pre-closure information, both self-reported and objective measures of health, a post-closure period of ideally 10 years, and minimal attrition. Our second argument then is that by utilizing

[22] The set of variables included in the DTLR was the same as in the PS estimation.

high-quality administrative registers this study satisfies almost all the requirements for being an 'ideal study.'

Our only remaining concern is related to the fact that closing establishments in general are new establishments (Persson, 2004). Possibly, workers at new establishment have characteristics or behaviors associated with higher risk of morbidity and mortality, as new businesses may have less developed hiring and screening processes. However, while administrative registers do lack subjective measures, their richness does not leave much else unobserved and the importance of unobserved characteristics can therefore be played down somewhat.

There are also some reasons to believe that our estimates are underestimates of the true impact of job loss. First, the comparison group was not restricted to continuously employed workers. Second, the time-window procedure used to define the job losses is likely to have included also some normal turnover. Third, at the time of the job loss, the displaced workers faced a very buoyant labor market and many of the workers got new jobs without an intervening period of unemployment.

5. Summary and conclusions

That an association between job loss, or unemployment, and ill-health exists is unquestionable, but whether this link is causal has not yet been established. Some reviews have claimed that no study satisfies the requirements for establishing causality as opposed to merely an association. Using linked employer-employee register data we have extended the case study approach, which has dominated the plant closure literature, by identifying the job losses due to all establishment closures in Sweden in 1987 and 1988. Hence, we have also been able to remedy weaknesses such as unrepresentativeness, small samples, lack of pre-closure health status, and lack of an appropriate control group, which has hampered previous research.

Our estimations reveal first a higher overall mortality risk for displaced men (but not women), concentrated to the first four years following job loss. Although one could speculate that the loss of a job would imply, for example, increased levels of stress and the onset of a chain of adversities that only in the long run would lead to higher mortality risk, this is not supported by the estimates.[23] On the contrary, we find a rather immediate impact, indicating a faster process or that the job loss for some is not the onset of this chain of adversities but rather a link closer to the end of the chain. Much of the immediate excess mortality is even reversed in the following years suggesting that the stress from job loss rather exacerbated or aggravated already existing disease processes than initiated any new disease or disorder.

A large part of the immediate excess mortality following job loss was due to an about twofold increase in fatal suicides, consistent with much of the previous research, as well as a somewhat less increase in deaths from alcohol-related conditions. The lack of a statistically significant increased risk of death due to other causes is also consistent with Keefe et al. (2002) that found adverse health effects of job loss only in form of fatal and non-fatal self-harm.

The limited number of deaths, despite the 12-year follow-up, also implies limited statistical power to establish whether the impact of job loss was disproportionately pronounced among certain socio-demographic subgroups. A subgroup analysis indicated, nonetheless, that most of the excess mortality among male job losers was found among the youngest (25-34 yrs) and

[23] This is not to say, however, that such effects do not exist, only that they do not show up in mortality data during the years we observe. We can neither rule out that such detrimental effects on health would be revealed if an even longer post-displacement period was covered by the data nor that job loss have adverse non-fatal health effects.

oldest (55-64 yrs). The workers in these ages were possibly the ones who experienced the most difficulties on the labor market following the job loss. It is especially likely that many of the oldest workers never returned to employment. Moreover, in line with our previous hypothesis that job loss may be a link in the end of a chain of adverse life events, we found a higher mortality risk ratio among divorcés and widowers. First losing a beloved one and then a job may simply have been too much to bear.

The lack of statistically significant effects of job loss on mortality for women does not necessarily mean that women's health is not affected by job loss. First, the overall low premature mortality among women results in lower power of the statistical tests. Second, the point estimates indicate a similar impact for both sexes on mortality from causes that should be viewed especially likely to be a consequence of job loss, such as ischemic diseases, alcohol-related conditions, and suicides. Moreover, for women there seem to be a negative impact limited to those in the age of 25-44 yrs, possibly indicating that although women traditionally have placed less importance on work relative to, for example, family life, later cohorts of women may have been more adversely affected by job loss due to a changing attitude to work over time.

The scientific debate on whether job loss has causal adverse effects on physical health is still open. While most of the early research claimed that such a relationship exists, some of the more recent research has both questioned the causal direction of the effects found earlier and produced new evidence that does not support a causal relationship from job loss to ill-health. We claim, however, that this study has eliminated most of the weaknesses of previous studies. In fact, it is very close to what Morris and Cook (1991) defined as an ideal study. Hence, it is reasonable to believe that the higher short-run mortality risk following job loss found in this study, mainly attributed to increased risk of death from alcohol-related conditions and suicides, is a causal effect of job loss.

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Table A1. Sample characteristics of the displaced (*D*=1) and non-displaced (*D*=0) workers after propensity score weighting, and SDMs before and after weighting, by sex.

	Men				Women			
	Mean/%		|SDM|[a]		Mean/%		|SDM|[a]	
Baseline characteristics	*D*=1	*D*=0	Pre-PSW	Post-PSW	*D*=1	*D*=0	Pre-PSW	Post-PSW
Demographic variables (*t*-1)								
Age (yrs)	41.08	41.07	9.45	0.11	41.14	41.17	6.62	0.32
No. of children aged 0-6 yrs	0.25	0.25	5.87	0.29	0.28	0.28	1.69	0.23
No. of children aged 7-17 yrs	0.39	0.39	12.38	0.07	0.52	0.53	1.79	0.12
Marital status								
Single	36.8%	36.7%	14.29	0.29	27.4%	27.3%	8.97	0.31
Married	50.3%	50.4%	18.82	0.11	53.7%	53.8%	13.27	0.21
Divorced	12.1%	12.2%	8.25	0.31	16.4%	16.4%	8.85	0.06
Widowed	0.8%	0.8%	0.18	0.14	2.5%	2.5%	3.00	0.06
World region of birth								
Sweden	85.2%	85.1%	15.26	0.14	84.4%	84.4%	16.06	0.08
Other Nordic country	7.9%	7.7%	12.35	0.58	9.4%	9.3%	12.35	0.29
All other regions (9 cat.)	7.0%	7.2%	2.35	0.36	6.2%	6.3%	2.87	0.27
Regional variables (*t*-1)								
Local unemployment rate (log)	-3.40	-3.40	13.95	0.25	-3.45	-3.45	21.35	0.59
Local income level (SEK)	167,09	167,738	15.93	0.78	168,844	168,858	18.99	0.09
Type of region								
Stockholm, Gothenburg, Malmö	23.4%	23.4%	13.66	0.01	24.1%	24.0%	12.26	0.19
Other larger cities	22.9%	22.6%	9.22	0.78	23.6%	23.2%	8.38	0.97
All other municipalities (7 cat.)	53.7%	54.0%	4.81	0.43	52.3%	52.8%	6.02	0.50
Socio-economic variables								
Attained educational level (*t*-1)								
Unknown education	7.7%	7.8%	13.04	0.45	5.3%	5.3%	10.90	0.27
Compulsory school < 9 yrs	23.3%	23.1%	2.28	0.26	24.2%	24.1%	12.90	0.14
Compulsory school 9 yrs	12.2%	12.3%	4.78	0.40	15.0%	15.1%	9.13	0.11
Upper secondary school < 3 yrs	28.9%	28.7%	7.67	0.54	31.9%	31.8%	4.98	0.08
Upper secondary school 3-4 yrs	13.3%	13.3%	3.48	0.10	7.6%	7.6%	2.65	0.01
Tertiary education < 3 yrs	7.5%	7.5%	6.26	0.12	8.9%	9.0%	11.24	0.42
Tertiary education 3+ yrs	7.0%	7.0%	17.65	0.14	7.0%	6.9%	18.09	0.08
Ph.D. studies	0.3%	0.3%	10.84	0.04	0.2%	0.2%	3.66	0.13
Employment/unemployment (*t*-2)								
Employment incidence	98.1%	98.0%	12.23	0.63	97.8%	97.7%	9.20	0.30
Earnings (SEK)	195,908	196,411	21.58	0.46	127,635	127,862	15.29	0.34
Unemployment incidence	13.6%	14.0%	31.04	1.35	10.6%	10.7%	14.92	0.33
Unemployment days[b]	9.84	10.15	24.63	1.14	6.90	6.92	13.50	0.09
Employment/unemployment (*t*-3)								
Employment incidence	97.2%	97.1%	12.81	0.65	96.1%	96.1%	10.80	0.40
Earnings (SEK)	188,878	189,088	24.23	0.19	122,495	122,393	21.04	0.15
Unemployment incidence	13.1%	13.3%	23.39	0.81	11.0%	11.3%	13.83	1.05
Unemployment days[b]	9.60	9.74	19.32	0.49	6.88	7.05	12.72	0.75
Wealth, social assistance, etc (*t*-2)								
Taxable wealth[c]	4.9%	5.0%	5.54	0.22	5.7%	5.9%	5.05	0.54
Disposable income (SEK)	210,095	210,521	20.50	0.44	214,765	214,980	15.55	0.23
Social assistance recipient	8.3%	8.3%	21.57	0.07	6.10%	6.04%	15.52	0.28
Social assistance amount (SEK)	1,804	1,830	14.98	0.32	1,243	1,234	12.33	0.13

Table A1. Cont'd.

	Men				Women			
	Mean/%		|SDM|[a]		Mean/%		|SDM|[a]	
Baseline characteristics	*D*=1	*D*=0	Pre-PSW	Post-PSW	*D*=1	*D*=0	Pre-PSW	Post-PSW
Socio-economic variables								
Wealth, social assistance, etc (*t*-3)								
Taxable wealth[c]	3.9%	3.9%	5.37	0.21	4.5%	4.7%	5.23	0.51
Disposable income (SEK)	201,177	201,444	21.07	0.28	207,924	208,150	15.88	0.24
Social assistance recipient	8.2%	8.2%	20.83	0.07	6.4%	6.4%	16.77	0.12
Social assistance amount (SEK)	1,828	1,869	15.85	0.53	1,424	1,402	13.25	0.31
House-owner (*t*-1)	44.2%	44.3%	22.04	0.14	35.3%	35.4%	11.29	0.24
Baseline health status								
Diagnosed disease or condition (*t*-2, *t*-3)								
Smoking related cancer	0.0%	0.0%	2.69	0.00	0.1%	0.1%	0.98	0.06
Malign neoplasms	0.2%	0.3%	1.96	0.54	0.5%	0.6%	1.82	0.20
Benign neoplasms	0.2%	0.2%	1.20	0.10	0.9%	1.0%	0.21	0.42
Undefined neoplasms	0.1%	0.1%	0.84	0.09	0.4%	0.3%	1.83	0.49
Diabetes	0.2%	0.2%	1.01	0.05	0.2%	0.2%	0.25	0.44
Psychoses and neuroses	2.4%	2.3%	10.46	0.31	1.0%	0.9%	1.42	0.40
Multiple sclerosis	0.0%	0.0%	1.46	0.00	0.0%	0.0%	0.04	0.16
Parkinson's disease	0.0%	0.0%	1.03	0.00	-	-	-	-
Other diseases of the nervous system	0.3%	0.3%	3.75	0.17	0.1%	0.1%	0.44	0.17
Hypertonic diseases	0.0%	0.0%	2.05	0.04	0.1%	0.1%	0.13	0.10
Ischemic diseases	0.6%	0.6%	0.59	0.02	0.2%	0.1%	1.56	0.36
Other heart diseases	0.2%	0.2%	0.39	0.04	0.1%	0.1%	0.23	0.15
Other circulatory diseases	0.6%	0.6%	0.31	0.21	0.5%	0.6%	1.35	0.27
Cerebrovascular diseases	0.1%	0.1%	0.48	0.01	0.1%	0.1%	1.04	0.41
Chronic respiratory diseases	0.2%	0.2%	0.65	0.04	0.2%	0.2%	0.95	0.09
Ulcers and gastritis	0.2%	0.2%	0.60	0.05	0.1%	0.1%	0.32	0.00
Diseases of the liver	0.1%	0.1%	1.20	0.25	0.0%	0.1%	0.30	0.62
Diseases of pancreas	0.2%	0.2%	2.98	0.33	0.0%	0.0%	0.44	0.09
Nephritis and nephrosis	0.0%	0.0%	0.18	0.28	0.0%	0.0%	1.46	0.23
Self-harm	0.2%	0.2%	3.13	0.38	0.2%	0.2%	1.62	0.41
Accidents	1.8%	1.8%	1.69	0.25	1.0%	0.9%	0.22	0.31
Alcohol-related conditions	1.8%	1.7%	9.97	0.94	0.3%	0.3%	1.87	0.53
Disability[d]	0.6%	0.6%	1.35	0.17	0.6%	0.6%	3.02	0.14
Hospital in-patient care and insured sickness (*t*-2)								
Hospital inpatient stays	0.13	0.12	6.00	0.29	0.15	0.15	1.53	0.18
Hospital inpatient days	1.37	1.39	5.83	0.26	1.06	1.03	2.65	0.35
Insured sick-leave incidence	64.1%	63.9%	5.03	0.39	72.3%	72.2%	6.34	0.30
Insured sick-leave days	21.38	21.55	9.63	0.35	25.70	25.52	6.32	0.34
Hospital in-patient care and insured sickness (*t*-3)								
Hospital inpatient stays	0.12	0.12	5.19	0.05	0.17	0.17	1.30	0.04
Hospital inpatient days	1.12	1.11	3.40	0.11	1.37	1.35	2.16	0.28
Insured sick-leave incidence	62.9%	62.8%	1.08	0.28	70.5%	70.4%	0.30	0.15
Insured sick-leave days	19.15	19.29	11.42	0.35	21.24	21.33	4.38	0.19

Table A1. Cont'd.

	Men				Women			
	Mean/%		|SDM|[a]		Mean/%		|SDM|[a]	
Baseline characteristics	*D*=1	*D*=0	Pre-PSW	Post-PSW	*D*=1	*D*=0	Pre-PSW	Post-PSW
Occupational variables								
Type of industry sector (*t*-1)								
Agriculture, fishing, and forestry	2.3%	2.1%	12.36	1.52	0.8%	0.8%	9.50	0.16
Mining and quarrying (3 cat.)	0.0%	0.0%	5.34	0.00	0.0%	0.0%	2.24	0.00
Manufacturing (9 cat.)	31.7%	31.8%	8.27	0.22	23.2%	23.5%	6.87	0.18
Electricity, gas, water supply (2 cat.)	0.8%	0.8%	5.99	0.06	0.3%	0.3%	2.06	0.08
Construction	12.2%	12.0%	15.48	0.81	1.4%	1.4%	3.74	0.05
Trade, restaurants and hotels (3 cat.)	17.6%	18.0%	14.64	0.63	20.2%	20.3%	15.31	0.22
Transport, storage, communication (2 cat.)	10.4%	10.5%	11.47	0.12	4.1%	4.1%	14.62	0.02
Financing, insurance, real estate, etc (3 cat.)	11.7%	11.7%	8.19	0.19	12.5%	12.3%	14.32	0.47
Community, social/personal services (6 cat.)	12.8%	12.6%	12.04	0.29	37.2%	37.0%	14.87	0.12
Educational level at the workplace[e] (*t*-1)								
Compulsory schooling	44.2%	44.2%	27.81	0.08	41.0%	41.0%	49.39	0.10
Upper secondary schooling	44.0%	44.0%	0.26	0.38	42.0%	41.9%	5.86	0.88
Tertiary education	11.8%	11.8%	31.78	0.43	17.0%	17.2%	44.51	0.58

NOTE – For brevity some categorical variables have been collapsed from a larger number of categories (the particular number within parentheses) included in the estimations and the corresponding (absolute values of the) SDMs represents averages over these categories. *t*-1, *t*-2, *t*-3, indicates one, two, or three years from the baseline year (i.e., year of job loss). 1 SEK ≊ 0.15 USD.

[a] |SDM| denotes the absolute value of the standardized difference in means, i.e., the absolute value of the difference in covariate means between the two samples, in percentage of the pooled standard deviation of that covariate.

[b] Unemployment days is a measure derived by dividing the annual income from unemployment insurance by the maximum daily amount. This will underestimate the true number of days for the few who do not reach the ceiling.

[c] Taxable wealth is an indicator of having a wealth over the threshold amount.

[d] Disability is measured as incidence of disability pension.

[e] For closing workplaces these figures correspond to the year prior to the onset of the closing process.