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The Echo of Job Displacement

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Abstract

This paper examines whether the loss of a job increases the likelihood of future difficulties on the labour market. We study displacement resulting from *all* plant closures (with ten or more employees) in Sweden in 1987 and follow their labor market outcome up to 1999. The control group is extracted from a random sample of non-displaced employees by matching on propensity scores. We find a rapid and almost total initial recovery of those displaced in 1987 compared to the control group up to 1990, both with respect to employment and unemployment measures. However, with the advent of the deep recession in 1990, the two groups again diverge. There is some relative recovery in the mid to late 1990s. However, by the end of the 1990s, the echo of the job loss 13 years earlier had still not subsided. We attribute the long-term effects as being either due to recurrent loss of match-specific capital or statutory seniority lay-offs rules.

JEL classification: J63, J64 & J65

Key Words: Plant closure, displaced workers, unemployment scarring, linked employer-employee data, propensity score matching.

1 Introduction

Job loss occurs in bursts and may result in a rapid increase in unemployment. However, while job loss exhibits no persistence, unemployment generally does. The literature has forwarded a number of reasons for the persistence of unemployment. Focus is predominantly placed on why those who lose their jobs do not secure new ones. This has been viewed as being due to loss of human capital, stigmatisation, welfare incentives, employment protection legislation and host of other factors. See, for example, Machin and Manning (1999). Of somewhat less prominence in the earlier literature is that even if the displaced does get a new job, the probability of also losing the new job may be high. Hall (1995) underlines the potential importance of this phenomenon for unemployment persistence. Thus, one may observe unemployment persistence resulting from displacement for those who did not even experience an initial period of unemployment.

The US literature on the longer-term effects of displacement, see Ruhm (1991), has mainly focused on permanent loss of income. Stevens (1997), found that an appreciable part of the

longer-term income loss was due to subsequent job loss. Similar "scarring" effects of unemployment are also found in the UK. See Arulampalam *et al*. (2000) and Böheim and Taylor (2002).

We examine the post-displacement labour market status of all workers who lost their job in 1987 due to plant closure in Sweden compared to a control group for 13 years. The control group is extracted from a large sample of employees not displaced in 1987 by matching on propensity scores. The focus on plant closures (and the availability of rich pre-displacement information between 1983 and 1987) permits us with some degree of confidence to exclude the possible effect of unobserved heterogeneity being correlated with the probability of displacement in 1987. As possible bias due to selection on unobservables is the main drawback of the, otherwise eminently suitable, methodology of matching on propensity scores, we believe that we have a very appropriate research design, data and econometric method.

In the next section we examine the reasons why displaced workers could be expected to have a relatively high probability of again experiencing job loss. In contrast to much of the scarring literature we focus on the theory of firm specific matches and the stylised fact that most new jobs end early. Section 3 shows how we identify the initial displacement, by plant closure, for all workers in Sweden in 1987. We also point out that the initial displacement occurred when the labour market was exceptionally good and remained so until, four years later, Sweden experienced a macroeconomic shock unparalleled since the Great Depression. Section 4 constructs a matched control group of persons employed in 1986 that did not experience a plant closure during 1987 and derives the difference-in-difference matching estimator used to identify the effect of displacement on subsequent labour market status. In section 5 we present the results in terms of the effect on employment, unemployment and out of the labour force for those of ages 20-30, 31-40 and 41-50, at the time of initial displacement. Section 6 concludes.

2 Why an echo?

Why should the effects of job displacement echo over time? This question is of course related to the persistence of unemployment, a topic that generated much research, particularly in Europe, since the oil shocks of the late 1970s and 1980s. However, as the data in this paper clearly shows a rapid initial convergence of the displaced group to the employment and unemployment levels of the control group, only to subsequently diverge again during the severe recession that started in early 1990, it is natural to focus on explanations of why the displaced group was more susceptible to subsequent displacement from the early 1990s and onwards than on issues related to an initial unemployment period, such as stigmatisation and discouragement.

The most obvious reason why jobs obtained by the group of displaced workers may be precarious is quite simply because they were new jobs. Farber (1997) gathers much of the US evidence on job stability, stating that; "The central facts regarding worker mobility in modern labour markets are clear … (1) long-term employment relationships are common, (2) most new jobs end early, and (3) the probability of a job ending declines with tenure". These three facts are empirically inter-related and can all be attributed to "specific job capital". This term encompasses not only that which is commonly equated with firm specific human capital (i.e. investment in training) but also the costly acquisition of information that can be termed "match specific capital".

The efficient separations literature, see for example Mortensen (1978), Hall and Lazear (1984) and Parssons (1986), shows that displacement occurs when a negative demand or

productivity shock is greater than the firm's evaluation of specific job capital. As one can reasonably assume that firm specific capital, acquired through experience or by training, increases with tenure, then displacement will be higher for low relative to high tenure employees.

In Jovanovic (1979) job specific capital is related to the informational problems for both parties in gauging the quality of the match. The matching process continues even after hiring as workers and firms continually reveal and learn more about job and worker quality. In this context new jobs end early due to one of the parties learning that the match was bad. As one could expect that match quality is revealed relatively early on the job (compared to the time taken to acquire significant levels of firm specific human capital in the more traditional meaning) matching capital may be a particularly plausible explanation of the extremely large number of very short jobs. Moreover, one could propose another search-based explanation for these new jobs ending early, as a conceivable strategy for the displaced worker is to take a temporary job in order to be able to search for other, better quality, jobs.

However, the Swedish institutional context provides a potentially powerful alternative explanation to the match specific capital hypothesis. To our knowledge Sweden is the only industrialised country in the world that stipulates seniority rules (last-in first-out) in statutory law when displacing labour for economic reasons. In addition, if the employer is to subsequently, within a year, re-employ, there is an obligation to first offer employment to the previously displaced employees in accordance with seniority. However, the firm and the trade unions may negotiate derogations on seniority. While some case studies show that seniority rules may be avoided in practice, see for example Calleman (2000), there is no systematic evidence.

Moreover, seniority rules at layoff can be viewed as fair in terms of reward for services rendered to the firm. This can be seen from a sociological perspective where the group (i.e. the work force) may consider that those who have spent many years at the firm may have secured a "job right" and should be rewarded by preferential treatment at lay-offs ("the ethics of desert"). Elster (1992) find this argument spurious as "to devote one's life to a task is meritorious only when it involves forgoing other activities that should have been more satisfying personally". However, whether spurious or not, the argument appears to be persuasive, as shown by empirical research in Rousseau and Anton (1992) and earlier US research surveyed therein.

Thus our hypothesis is that the workers displaced in 1987 will be more exposed to subsequent risks of job losses compared to similar individuals who did not experience displacement during this year. This is simply due to the fact that they have, compared to the matched control group, relatively new jobs. This follows from the economic theory of specific job capital, but also possibly from Swedish labour law and social norms. We will not be able to discriminate between these three potential explanations.

3 The labour market 1987-1999 and the definition of displacement in 1987

3.1 The Swedish labour market between 1987 and 1999

The initial job loss occurred in 1987. This was during an exceptionally good period in the Swedish labour market. Unemployment had been falling since 1983 and continued down to a low of 1.5% in 1989 and employment increased up to a peak in 1990. Indeed, by the end of the 1980s, the two basic measures of the state of labour market indicated a more buoyant labour market than at any time since 1975. See Figure 1.

However, in the early 1990s, Sweden experienced a macroeconomic downturn unparalleled in the post-war period. GDP fell by six percent from the cyclical peak in the first quarter of 1990 to the trough in the first quarter of 1993. By 1993 unemployment had risen to 8.2 percent. Total employment fell by 13 percent, close to 600 000 persons, between the first quarters of 1990 and 1994. After 1994 there was a minor recovery, which soon petered out. Signs of a sustained labour market recovery did not appear until the end of the decade. The period from 1997 and onwards has seen a large decline in unemployment as well as rising employment.[1]

Thus, our displaced workers faced a very good labour market for the first four years after displacement with ample time and opportunity to find a new job. At the beginning of the 1990s they faced the most severe recession since the 1930s. From 1997 and onwards there was a sustained recovery.

3.2 Identification of the closing establishments and displaced workers

The identification of persons who experience an establishment closure is a vital and rather unique feature of the data used in this paper and should be clearly defined. While register data of acceptable quality on plant closures in Sweden only became available in the mid 1990s, see Tegsjö (1995), the new procedures at Statistics Sweden enable us to trace establishments back to 1985 and match them to their employees.[2] We identify plant closures in 1987 and 1988 with at least ten employees.

Statistics Sweden's Business Register (*Företagsregistret*) is the basic frame for the identification of all establishments in both the private and public sectors. The first step in determining whether an establishment has closed is to find non-matches between the establishment identity number in the Business Register and the obligatory annual payroll tax returns, which are submitted by establishment. However, non-matches are only potential closures as they may occur due to a change in the identity number due to, for example, a change in legal status of the firm, change in ownership, or simply due to errors.[3] An incorrect change in the identity number has occurred if any two of the following criteria apply: 1) it has the same owner; 2) it has the same geographical location; 3) it conducts the same type of economic activity. To assure that this is not the case, Statistics Sweden surveys the firms when non-matches occur in multi-establishment firms or in establishments of at least 10 employees (as is the case in this study). We are, thus, quite convinced that the steps taken here vouch for a low risk of over-classification of closed establishments.[4]

A plant closure is a process over time and while the procedure above can identify a closure and when the process ended, it cannot determine when it began. We set the upper limit for the duration of the closure to three years.[5] After careful inspection of each and every closuring establishment up to three years prior to closure, we identified a probable duration of the closure process. The process was defined to be one, two or three years, based on worker flows and establishment size.[6] One should note that while this procedure is hardly perfect, this

[1] See Holmlund (2003) for a detailed account of the development of the Swedish labour market during this period.
[2] This data was first used in Persson (1999) in a study of job creation and destruction.
[3] Kuhn (2002) recognizes that failing to correct for false deaths, could have a significant impact on the results.
[4] For a more detailed description of these surveys and additional criteria, see Appendix B.
[5] In Storrie (1993), at the closure of a large Swedish shipyard (i.e. a sector with a long production time), the closure process, from the initiation of negotiations which are mandatory according to the Codetermination Act before redundancies may commence, to when the plant was finally closed, was just under three years.
[6] The precise *ad hoc* rules determining the classifications are found in Appendix C.

flexible three-year-window is a substantial improvement on previous plant closure literature. Many US studies on displaced workers use the Displaced Worker Surveys, which identify the displaced workers by retrospective survey questions. It is most likely that this kind of survey only identifies actual displacements and not the quits due to expectation of closure. See the discussion below. Studies using administrative data more similar to the data in this study, usually assume (explicitly or implicitly) that the closing process begins and ends in the same year. While some allow a two-year process, this is done without an examination of the individual processes and all closures are defined as being of two years (Bender, 2002). Using such *rigid* time windows will presumably either over or under classify the displaced workers. This problem is highlighted in Kuhn (2002).

According to the classification of the length of the closing process, we then identify the workers separating from the establishments. However, we cannot be sure that all these separations were in fact due to the observed closure, since we only observe that the worker is employed at the establishment in November in year t, and not in November year t+1. There are three main possibilities; voluntary quits unrelated to the closure, pre-emptive quits (i.e. quits due to expectation of closure) and actual displacements. The last two types of separations can be seen as direct consequences of the closure. With a long rigid time-window there is a risk of including a large fraction of quits unrelated to the impending closure. When applying a short time-window, one will presumably miss a large proportion of pre-emptive quits related to the impending closure and possibly some actual displacements. The implications are that an over-classification of displaced workers will probably underestimate any adverse effects of the displacement, and an under-classification of displaced workers leaving early in the closing process will have the opposite effect.[7] The logic behind the latter is that, presumably most separations early in the process are pre-emptive quits. We expect those workers to have a better labour market situation, than those staying to the bitter end, and therefore not affected to the same extent by the closure. The more flexible time-window applied in this paper will be a step towards minimizing these problems.

3.3 Sample retained for analysis

The flexible three-year-window implies that the year when the job loss occurs will differ between displaced workers within the same establishment according to when during the closing process the displacement occurs. Our control group is comprised of a random sample of 200,000 persons employed in November 1986, who did not experience a closure during the same period, i.e. 1987 and 1988. Since one main issue in this paper is to investigate whether the displaced workers are more severely hurt by the deep recession in the early 1990's, it is most important that both the study and the control group is selected in the same year, otherwise we cannot separate time effects from calendar effects. This requires us to restrict the study group retained for analysis to those employed in 1986 and displaced during the following year. Thus, no workers displaced during the first year, in a three-year closing process, are included. We do believe, though, that this is not a severe problem, since very few processes were determined to be three years long, and the corresponding job losers separating early in the process were also few. See Eliason and Storrie (2003).

In the estimations we restrict our sample to individuals aged 20-50 years. We also exclude those working in the construction sector or a sector not adequately defined, since for these sectors the concept of establishment, may be somewhat peculiar, and the identification of displaced workers is not considered reliable. Moreover, we use a balanced panel, i.e. all individuals are required to be in the sample during the observation period 1983-1999. Attrition is very low as it occurs only through emmigration or death. Finally, we exclude all

[7] Displaced workers are here defined as both those who actually were dismissed by the employer and those who left the establishment in expectation of an impending closure.

self-employed workers. After applying the sampling restrictions 4,612 displaced workers in 371 establishments, and 119,241 non-displaced workers remain (see Table 3).

4 Econometric method and empirical implementation

The objective of this paper is to determine whether job displacement has long-term effects on labour market status. The main empirical problem is akin to that in the evaluation of labour market policy or medical treatment. One can observe the labour market outcome of the displaced workers but not the outcome for these workers had they not have been displaced. Heckman (1999) provides an overview of much of the literature trying to deal with these problems. One such method, originating in medical statistics, is matching on propensity scores (Rosenbaum and Rubin, 1983, 1984, and Rosenbaum, 1989). Recently propensity score matching has also received increasing attention in economics. See Heckman *et al*. (1997), Dehejia and Wahba (1998), Sianesi (2001), Lechner (2002a), Lechner (2002b) and Larsson (2003).

By matching one tries to *ex post* mimic the randomisation of individuals into the treatment group in experimental studies. Intuitively this is a very appealing method, since if the displaced and the non-displaced workers are alike in all relevant pre-displacement characteristics then any difference in labour market outcome can be attributed to the job displacement.[8] There are several reasons why we believe that, in this application, matching is a most appropriate methodology. One obvious advantage is that, appropriately matched, it permits non-parametric estimation of the treatment effect. Moreover, matching addresses directly problems of common support and distributional issues. Finally, and probably of most importance, we argue that the major drawback of matching, i.e. that it cannot address unobserved selection, is practically negligible in this application. This is due to the nature of the event (plant closure) and the large number of relevent pre-displacement variables.

4.1 The method of propensity score matching[9]

Let Y^1 and Y^0 denote the potential outcome of displacement (D=1) and non-displacement (D=0), respectively. Then, for the displaced workers Y^1 is observed, and Y^0 is observed for the non-displaced workers. Under certain conditions it is possible to estimate the treatment effect, even if Y^0 is not observed for those experiencing displacement. The assumption underlying the identification of the treatment effect, on the treated, by matching is the conditional independence assumption (CIA). The CIA requires that, given the observed characteristics X, the non-treatment outcome Y^0 is independent of treatment status (D), i.e. no variables other than X affect both the assignment to treatment D and the outcome Y^0.[10] More formally

$$(1) \qquad Y^0 \perp D \mid X .$$

To identify the *mean* average effect of treatment on the treated, the CIA as stated in (1) is unnecessarily strong. It is sufficient that

$$(2) \qquad E\left[Y^0 \mid D=1, X\right] = E\left[Y^0 \mid D=0, X\right] = E\left[Y^0 \mid X\right].$$

[8] Here the treatment is displacement and the terms displaced and treated will be used interchangeable.
[9] This and the following section draw from Smith and Todd (2003), Lechner (2001), Heckman et al. (1996), and Rosenbaum and Rubin (1983).
[10] It is also assumed that $P(D=1|X)<1$, implying that a match can be found for each treated individual.

If this assumption is valid, the unobserved contrafactual outcome of the treated can be estimated from the observed outcome of the matched non-treated.

When only a few relevant covariates exist, matching directly on these covariates is straightforward, but when the number increases (or are continuously distributed) it is extremely unlikely that matches will be found. Rosenbaum and Rubin (1983) shows that if a function $b(X)$, is a balancing score, i.e.

$$(3) \qquad X \perp D \big| b(X),$$

and if the CIA is valid for X, then the CIA is also valid for $b(X)$. Thus, matching on $b(X)$ is equivalent to matching directly on X, with the advantage that the difficulties with matching on a large set of covariates is eliminated. Furthermore, Rosenbaum and Rubin (1983) shows that the propensity score is a balancing score. The propensity score,

$$(4) \qquad p(X) \equiv \Pr(D = 1 \big| X) = E\big[D \big| X\big],$$

is defined as the conditional probability of receiving treatment given pre-treatment characteristics.

4.2 Matching estimators

In the literature several different methods of matching on propensity scores have been proposed. In this paper we will apply a simple nearest-neighbour matching method (NNM). With NNM each displaced worker i is matched to a non-displaced worker j such that:

$$(5) \qquad \left| p_i(X) - p_j(X) \right| = \min_{k \in \{D=0\}} \left| p_i(X) - p_k(X) \right|$$

This estimator can be applied both with and without invoking a common support requirement. Formally, the common support requirement means here that all displaced workers i such that

$$(6) \qquad p_i(X) < \min_{k \in \{D=0\}} p_k(X) \vee p_i(X) > \max_{k \in \{D=0\}} p_k(X)$$

are excluded. Imposing common support is inefficient but decreases bias, and also implies that what we really estimate is the treatment effect on the treated that falls within the common support. In all estimations in this paper the common support is required. This has only implications for one of our estimations, and then only 2 individuals are excluded. See Appendix H. Applying this estimator, the average effect of displacement on those displaced is

$$(7) \qquad \Delta_t^D = \frac{1}{N_{D=1}} \sum_{i \in \{D=1\}} Y_{it}^1 - \frac{1}{N_{D=0}} \sum_{j \in \{D=1\}} Y_{jt}^0$$

An alternative to this estimator, proposed in Heckman *et al*. (1996), when both pre and post-displacement data is available (as it is in this study) is a difference-in-difference (DID) matching estimator.[11] This estimator compares the difference in pre and post displacement

[11] This estimator differs somewhat from the one in Heckman *et al*. (1995) in that we condition on $p(X)$ and not on X.

labour market status between the displaced and non-displaced workers. The average effect can then be estimated by

$$(8) \qquad \Delta_{t,\tau}^{DID} = \frac{1}{N_{D=1}} \sum_{i \in \{D=1\}} \left(Y_{it}^{1} - Y_{i\tau}^{0} \right) - \frac{1}{N_{D=0}} \sum_{j \in \{D=1\}} \left(Y_{jt}^{0} - Y_{j\tau}^{0} \right),$$

where t is a time period after the displacement and τ is a time period before the displacement. The advantage with the DID matching estimator is that the CIA as stated previously can be relaxed. The identifying assumption is instead

$$(9) \qquad E\left[Y_{t}^{0} - Y_{\tau}^{0} \middle| D = 1, p(X) \right] = E\left[Y_{t}^{0} - Y_{\tau}^{0} \middle| D = 0, p(X) \right].$$

That the CIA is valid is sufficient, but not necessary for the validity of this assumption. Even though CIA may not hold we could still identify the effect by the DID estimator if the bias due to a violation of CIA is the same in both the pre displacement period τ and the post displacement period t. However, if we try to minimize bias due to the failure of CIA, then we should probably include lagged values of the outcome variable in the conditioning set of X. Since, (9) could be reformulated as

$$(10) \quad E\left[Y_{\tau}^{0} \middle| D = 1, p(X) \right] - E\left[Y_{\tau}^{0} \middle| D = 0, p(X) \right] = E\left[Y_{t}^{0} \middle| D = 1, p(X) \right] - E\left[Y_{t}^{0} \middle| D = 0, p(X) \right]$$

The difference on the left-hand side must, by definition, be equal to zero if the matching process is perfect, implying that also the right-hand side is equal to zero. The right-hand side equal to zero is equivalent to the CIA, thus we do not gain anything using the DID matching estimator. However, we will use this estimator to correct for the impact of any small differences in lagged outcome values as one never will find fully perfect matches.

In econometrics much attention has been placed on reducing the selection bias resulting from differences in unobservables, when estimating the treatment effect, but the bias due to comparisons of groups, which are non-comparable in observable characteristics has largely been ignored. Even in the absence of unobserved differences, and if using model-based methods adjusting for observable differences in characteristics, the estimated treatment effect may not be valid for any causal inference, due to a lack of overlap in the explanatory variables in the treatment and control groups and/or from differences in the distribution of the explanatory variables between treated and non-treated within the region of the common support. See Rubin (1997). Heckman *et al.* (1999) examines the same sources of bias and reports that differences in the support of $p(X)$ and differences in the distributions of $p(X)$, within the common support, between the treatment and non-treatment group, are both important sources of bias. One of their major recommendations is that non-experimental comparison groups should be designed so that they have the same set of X or $p(X)$ values as the treatment group.

4.3 The validity of the CIA

Although propensity score matching may reduce two of three sources of bias, it is essential that we are able to convincingly argue that selection on unobservables is not an important issue in this application, i.e. that the CIA is valid. Here, CIA means that, experiencing displacement is not affected by any unobservable factors also affecting the post displacement labour market outcomes. We believe that the CIA is indeed very reasonable in this case. This is due to the nature of the event of plant closure, the very extensive set of available pre-displacement characteristics and information on the closure process up to three years before closure.

The event of plant closure has been studied widely since it is believed that any selection problems are reduced or eliminated. The motivation is that there is no selection in separations, since all workers have to leave in case of a closure. We stress the same argument in this paper, but only to the extent that it will reduce potential selection problems. The probability of experiencing plant closure cannot be viewed as a random event (if it could there would, of course, be no need for any conditioning variables). The closing firms are not a random sample of all firms nor are the displaced workers a random sample of all workers. The question is then whether we are able to observe all characteristics, which are jointly correlated with the probability of displacement and the labour market outcomes.

There are several reasons why we should believe that workers at closing establishments differ from workers in general. Closing establishments are in general small and new. See Harris and Hassaszadeh (2001), Dunne *et al*. (1989) and Anderson and Vejsiu (2001). This implies of course that the workers have less tenure and probably also that they are younger. The structural change driving the closure of establishments is also over-represented in certain sectors of the economy. These sectors may in turn have distinctive profiles as regards, for example, region, gender, age, and education level. Regional conditions such as local unemployment level and wage level may also have an impact on the survival probability of establishments (Andersson and Vejsiu, 2001). These differences will not be a problem here. As can be seen from Appendix D, we have information on conceivably all these factors, both the factors influencing the probability of a closure and the differences in individual characteristics that they will result in.

A matter of greater concern would be if the displaced workers differed from the non-displaced even after conditioning on the factors mentioned above. The stock of employees at closure is determined by the probability of becoming and remaining employed at the establishment. Hiring occurs through matching in the labour market and according to Dunne (1989) and Winter-Ebmer (2001), there may be systematic matching between workers who have a low preference for job security or less risk-averse and establishments with low survival probability. These workers may have a low opportunity cost of displacement, due to, for example, a weaker attachment to the labour market or work in a sector with high turnover where not only job destruction but also job creation is high, for example, in the restaurant and construction sectors. If such matching occurs then we could conceivably have a problem. On the other hand, even if the preference for job security, for example, is not directly measurable, it is likely that any such difference will show up in the workers' employment/ unemployment history.[12] Since our data contain information about the workers attachment to the labour market up to four years prior to the displacement this may not be as severe as first stated.

However, it is perhaps more obvious to search for unobserved selection in separations. While the employee is, of course, free to quit there are some restrictions on the employer right to dismiss particular individuals.[13] We see a potential selection problem to be if some of the workforce leaves the establishment before the closing year due to prior knowledge of an impending closure. One could speculate that those that remain until the bitter end may either be those who had poorer outside options or showed less initiative in pursuing outside options during the closure period compared to those who left earlier. This may be a problem as, particularly the initiative factor, may be difficult to quantify and control for. These factors will definitely be related to the post-displacement labour market outcome. Thus, our ability to

[12] It is also possible that a indication of lower risk-averseness may show up in medical or sickness history.

[13] Swedish employment protection legislation stipulates that lays-offs for economic reasons are to proceed in accordance with seniority. However, the employer can unilaterally decide if the establishment is to close.

also identify those who were displaced earlier in the closing process and pre-emptive quits, is important.[14]

4.4 Choice of matching variables

The propensity scores are not known, but have to be estimated, which can be done with some standard probability model. The statistical literature does not give clear guidance on which variables to include in the estimation of the propensity scores. However, the balancing score property and the CIA imply, that it is sufficient to only include covariates that are jointly correlated with the selection into treatment and the outcome, i.e. displacement and labour market status. This means that consistent modelling of the selection process by including covariates, which only determine the selection process, is not necessary. In fact, some authors claim that it could be dangerous to include instruments, since this could exacerbate the problem of common support. See Coniffe *et al*. (2000), Smith and Todd (2000), Augurzky (2001) and Lechner (2001). When it comes to the choice of which interactions and higher-orders terms to include, this is determined solely by the need to achieve balance in propensity scores and covariate distributions. See Dehejia and Wahba (1998) and Coniffe *et al*, (2000). In the following we will outline the variables included in the estimations, as well as a brief motivation.

Basic socio-demographic variables – The included socio-demographic variables are age, sex, marital status, number of children, and immigrant status.

Labour market status – Pre displacement labour market status should be correlated with post displacement labour market status. As discussed in Section 4.3, it may also approximate worker' preferences for job security, which could be correlated with probability of being employed at a closing establishment, if there is a matching of less stable workers to establishments with a higher exit rate, as suggested in Winter-Ebmer (2001). Therefore, we include an indicator of unemployment and employment. The degree of employment and unemployment is represented by the income from employment and unemployment, respectively. Finally, we include a limited measure of tenure, only indicating whether the worker has been employed at the same establishment in both November 1985 and November 1986.

Socio-economic variables – Apart from the variables presented above, we also distinguish between six education levels. We include three dummies indicating that the worker has taxable wealth, owns a house, and has received social assistance (the extent of social assistance is measured by a continuous variable similar to the ones measuring the degree of employment/unemployment). House ownership could indicate a preference for stability, which we argued in the previous section might be correlated with the likelihood to be employed at a more stable establishment, i.e. one that has a lower exit probability. House owners may also be more reluctant to move, which could have implications for labour market outcome. Similar reasoning could motivate the inclusion of wealth. It could also be that workers' taxable wealth is an accumulation of a steady stream of previous high income. A high income may be interpreted as high labour market quality, which will be reflected in a favourable labour market situation even in the future.

Health variables – There is no doubt that there is a correlation between ill-health and unemployment. Most studies on this matter have, though, tried to establish a causal impact of unemployment on ill-health. See Jin (1995) for an overview of this literature. In this literature it is well recognized that there is a reverse mechanism, i.e. persons with ill-health are more

[14] It should be noticed, though, that we only include workers who separated in the same year as the closure or within the year prior to the closure.

prone to both become unemployed and remain unemployed. It is more doubtful whether there is a correlation between displacement due to a plant closure and pre-displacement health. Possibly, pre-displacement health could pick up differences in risk-averseness or productivity, which may be correlated with the probability of being employed at a closing establishment (see the discussion in Section 4.3). From the National Social Insurance Board's register we know the length of hospital stays, as well as the number of sickness insurance days and the incidence of disability pension.

Sector variables – The structural change driving the closure of establishments is over-represented in certain sectors of the economy and the business cycle varies between sectors. Thus, the employment prospects will also differ between sectors. We use a rather detailed classification of the economic sectors, according to ISIC, containing 35 sector dummies.

Establishment variables – From the Swedish Business Register we extract some information at the establishment level. We include measures of establishment size, the share of Swedish citizens, females and low and highly educated workers. As was discussed in Section 4.3, establishment size is a strong predictor of a closure. The other measures could be seen as measures of the human capital structure of the establishment. See Andersson and Vejsiu (2001).

Regional variables – The municipalities have been grouped into nine classifications by the Swedish Association of Local Authorities. These are not based on the geographical locations, but on the population density in the municipally, nearness to big cities and on some dominating industry activities. We also include the local unemployment and employment rate as well as the local wage level. The local conditions may have an impact on the establishments' exit probability as well as on future labour market outcomes. The channel by which these local measures may impact on the exit probabilities is, for example, through wage pressure (the worker's outside option value increases with higher local wage levels and employment rates and lower unemployment rates).

4.5 Who are the displaced workers?

The descriptive statistics are presented in table 2. As was discussed in the previous section the displaced and non-displaced workers differ significantly. The largest differences are not found in individual characteristics, but in establishment characteristics. The displaced workers are employed in much smaller establishment. The average establishment size for the displaced workers is 55 employees. The corresponding figure for the non-displaced is 700. Moreover, the displaced workers are employed at establishments with a higher fraction of immigrants and low educated workers. Not surprisingly the displaced workers are overrepresented in the private sector, but there are differences in industrial sectors as well. In accordance with the stylized facts of displaced workers, we also find that more displaced workers have only short tenure and are less educated. The shorter tenure is also reflected in pre-displacement labour market status. The displaced workers have significantly longer periods of unemployment and consequently a lower degree of employment. Concerning socio-demographics and pre-displacement health status the differences are less prominent, with one exception, the displaced workers are to a much lesser extent married. It is also notable that we do not find any large regional differences.

4.6 Estimation of the propensity scores and the matching quality

The probability of displacement due to establishment closure is the dependent variable in the estimation of the propensity scores. As independent variables we include all the variables described in Section 4.4, as well as various interactions and higher orders, if necessary, to

balance the covariates.[15] Thereafter, each unit in the study group is matched to the closest unit within the control group, with respect to the *logit* of the propensity score, i.e. the log-odds ratio.[16] We match on the logit of the propensity score instead of directly on the propensity score because our sample is choice-based. With a choice-based sample the propensity score cannot be consistently estimated without re-weighting the sample. However, if one does not re-weight a choice-based sample, matching can still be performed but on the odds ratio or the log odds ratio. See Smith and Todd (2003).

The balancing criteria applied here is that there is no significant difference, at the 5 percent level, in covariate means between the displaced and non-displaced workers. Focusing on the covariate means or the standardized difference in means is in line with several other applications of propensity score matching. See, for example Smith and Todd (2000), Sianesi (2002), Vuri (2002) and Larsson (2003). A stronger criterion would have been preferable, such as balance also in second and higher moments. Although, in theory with exact matches and with infinite samples the covariate distributions should be balanced, in practise this has been difficult to obtain. Despite a large reservoir of controls and numerous attempts with different specifications of the logit model, we have not been able to obtain balance in higher moments for all the covariates. On the other hand, most included variables are dummies, and for those obviously a first moment test is sufficient.

To further assess the covariate balance gained by the matching, we calculate the standardized differences of the means both for the unmatched and matched samples.[17] As can be seen from Table 7, the matching has considerably improved the covariate balance. In the unmatched samples the absolute standardized difference is, in the extreme cases more than 1,000 percent, which should be compared to 7 percent in the matched samples. In all samples the average absolute standardized difference has been reduced from around 24 percent to 2.5 percent.

5 The effect of displacement in 1987 on subsequent labour market status.

5.1 Measures of labour market status

We examine the labour market status of both the displaced and matched non-displaced workers in terms of employment, unemployment and being out of the labour force. Employment is measured by paid employment on average at least one hour per week in November. We have two measures of unemployment. For the entire period data is available on the incidence of unemployment benefit payments from the income registers.[18] This is from the same source as in the employment data mentioned above. From 1992 onwards we also have data from the National Labour Market Board (NLMB-data) which provides us with the number of days of registered (full-time) unemployment per person and year. The measure of

[15] We estimate the propensity scores separately, for three age categories, The logit estimates are presented in Table 4. The estimated coefficients are not directly comparable, since the included higher orders and interactions, differ between the three specifications and are not presented in the table.

[16] The matching procedure is performed in Stata. The program used is a modification of *psmatch.ado.* See Sianesi (2001b).

[17] The standardized difference in means is calculated as $d = \left(\bar{x}_{D=1} - \bar{x}_{D=0}\right) \Big/ \sqrt{\left(s^2_{D=1} + s^2_{D=0}\right)/2}$.

[18] We have reason to believe that payment of unemployment benefit covers a very large proportion of unemployment as defined by the labour force survey (ILO definition). Roughly 70 percent of the ILO defined unemployed in Sweden received benefit in the early 1990s. This is a high figure even in a European perspective. See Standing (2000). Moreover, a large proportion of those not receiving benefit are those without an employment record. Both the displaced and the matched control group were employed in 1986. Note also that this data includes benefit payments for part-time unemployment.

being out of the labour force is defined as zero annual income from employment and unemployment.

5.2 Differences in labour market status

We examine the labour market outcome following displacement for our chosen population i.e. those between the age of 20 and 50 in 1986. The average effect of displacement in 1987 on those displaced is measured by equation (7), i.e. we only refer to the difference-in-difference estimates, although the simple difference estimates can be found in the Appendix D.6. These treatment effects will be referred to as "gaps" henceforth.

Employment – Looking first at the degree of employment (paid employment in November) for the 20 to 50 year-olds in Figure 2, we observe an initial decline to just less than 0.08 in the same year as displacement which signifies that the employment rate was 8 percentage points lower for the displaced-1987 group. See also Table 11. We then observe a clear tendency to a narrowing of the gap with the matched control group, so that by November 1989 the difference in employment rates is 2.2 percent. However, in 1990 with the advent of the severe recession, we observe a distinct widening of the gap. The gap is at its widest in 1992. By the end of the period, in 1999, we still observe a significantly lower employment rate among the displaced workers amounting to 3.8 percentage units.

Turning now to the employment results for the three age groups (see Figure 3 and Tables 8-10) we observe the largest initial drop in employment for the 41 to 50 year olds, relative to the matched control group. However, all three age groups recover up to roughly a two-percentage point difference by 1989 (1990 for the oldest group). Indeed, we observe an almost total narrowing of the gap for the youngest group. The recession of the early 1990s hits the youngest two age groups first, between 89 and 90, with a larger initial loss for the 20 to 30 year-olds. When, one year later, the gap widens also for the 41 to 50 year-olds, it widens appreciably. The post 1991 development is more diverse. The gap starts to narrow for the young groups as early as 1992, the middle group in 1993 and the oldest group in 1994. A new peak in the figure occurs in 1996 for the young, 1994 for the middle and 1995 for the oldest group. The widening of the gap again in the mid-1990s, most clearly observed for the 31 to 40 year-olds coincides, roughly with the dip in aggregate employment between 1995 and 1997. See Figure 1. From 1997 onwards, as aggregate employment increases, the employment gap narrows for the two oldest age groups, while the gap remains the same for the 20 to 30 year olds.

Unemployment – Figure 2 shows the gap in the incidence of unemployment (some unemployment benefit payment during the year), for all between the ages of 20 and 50 year. See also Table 15. It reveals more or less the mirror image of the employment graph. The immediate effect of the job loss led to just over 11 percentage point higher gap of the incidence of unemployment some time during 1987 for the displaced workers.[19] The gap declined up to 1989, remained quite steady between 1989 and 1991 and increased monotonically up to 1995 and then monotonically declined to the end of the period. Compared with employment, the echo in the troubled early 1990s is not so loud at the beginning but increases all the way up to 1995. After 1995 the echo subsides down to a gap of just over 1 percentage point. Figure 4 presents this data broken down into the three age groups. They show an initial widening of the gap and subsequent convergence up to 1989 for

[19] This difference during the displacement year might be considered as rather low an in Table 15 we see that only 25 percent of the displaced workers experience any unemployment during the displacement year. Remembering the good Swedish labour market situation it is not an exceptionally low figure in a European perspective. Similar figures (around 30 percent) for joblessness have been observed in several European countries, although the incidence rate is much higher in the US, see Kuhn (2002).

the youngest group, 1990 for the middle one and 1991 for the oldest, apart from a blip in 1990. These local minimum points were lowest for the youngest group, followed by the oldest and then the middle group.

For the period, from 1992 onwards we also use the NLMB-data to estimate the effect not only on incidence of unemployment, but also on the number of days unemployed. See Figures 6 and 7. As the data measures different units it is not possible to link the two series.[20] The NLMB-data tells us that, in 1992, the young (20 to 30) displaced workers were, on average, unemployed for 3½ more days than the matched non-displaced. The corresponding difference for the middle age group was 18 days and 12 days for the oldest. By the end of the period, the unemployment gap for both the oldest and youngest groups was 4 days and was not statistically significant. The older group fell down to this level from, at the most, 15 days in 1994. Over the period 1992 to 1999, the youngest group exhibited no clear downward trend. A much higher initial (18 days) and final gap (12 days) was observed for the 31 to 40 year olds and for the entire period the gaps were statistically significant.

We wish to examine whether the difference in average days of unemployment is due to longer (or more frequent) spells of unemployment or if it is only attributed to the greater number of unemployed persons among the displaced workers, as we could see in Figure 2. It is not possible to decompose the average gap, as defined previously, on days in unemployment into these two components. It is, however, possible to do so if we redefine as a relative effect, i.e.

$$(11) \qquad \Delta_t^D(Y) = \frac{1}{N_{D=1}} \sum_{i \in \{D=1\}} Y_{it}^1 \Bigg/ \frac{1}{N_{D=0}} \sum_{j \in \{D=0\}} Y_{jt}^0$$

Defining the effect as an ratio instead of a difference, implies that the effect can be decomposed as,

$$(12) \qquad \Delta_t^D(U) = \Delta_t^D(U \mid U > 0) \times \Delta_t^D(\Pr(U > 0)),$$

where U is the number of unemployed days. The first term on the right hand side is the relative effect on unemployment days (given that they were unemployed), and second one is the relative effect on incidence of unemployment. The decomposition of the relative treatment effect is presented in Table 24, and we can interpret from this table that most of difference in average unemployment between the displaced and matched non-displaced can be attributed to a higher probability of being unemployed during a year among the displaced workers rather than longer (or more frequent) unemployment spells for those actually unemployed.

Out of the labour force – When turning to our measure of being out of the labour force, the figures clearly differ, from what we previous have seen for employment and unemployment, as we do not observe any business cycle effect. The state of being out of the labour force increases over the period, both for the displaced and non-displaced workers. See Table 19. There is no large immediate gap, see Figure 5, but a small one that increases up to 1990-92 and no tendency to an eradication of the gap towards the end of the period. Instead, what we see is a convergence during the recession, and then during the second half of the 1990's a return to the levels in 1990-92, which now seems to become permanent. These differences are significant and correspond to, on average, around 2 percentage points. In particular, we observe an appreciable gap for the oldest age group, which increases up to the end of the 1990 and shows no sign of diminishing at the end of the observation period.

[20] However, we can observe that there is some compatibility in the two series in terms of the ranking of the three age groups. In both series, the widest gap can be found for the 31 to 40 year olds, followed by the 40 to 50 year olds and with the youngest group exhibiting a very small gap.

Summing up – Not surprisingly, we observe the greatest difference in labour market status between the displaced and non-displaced in the same year as the displacement. During the first years after displacement the gap with the matched control group narrowed appreciably and by 1989 there were only small differences in both employment and unemployment rates. It is equally clear that the convergence was arrested upon the advent of the recession at the beginning of the 1990s and then reversed. Thus there is clear evidence of an echo. One can even see tendency of another smaller echo when the minor recovery up to 1995 petered out. Thus, bad times hit the displaced group harder than the non-displaced. It would appear difficult to interpret this, in particular the pre-1993 experiences, in any way other than that they first found new jobs and subsequently lost them to a greater extent than the control group in the recession. Even the post-1992 break-down of the unemployment data from the NLMB suggest a more frequent incidence of unemployment as opposed to longer periods, suggesting recurrent displacement. Being out of the labour force does not exhibit the same business cycle pattern as the other two states.

Over the entire period the greatest negative employment effects are found for the oldest group and smallest negative effects for the youngest group. The youngest group also experiences appreciably lower unemployment over the period with the 31 to 40 year olds exhibiting slightly higher levels than the older group. Being out of the labour force is appreciably higher for the oldest displaced group but with very small displacement effects for the 31 to 40 year olds.

6 Conclusions

We have studied the long-term effect of displacements in 1987, due to establishment closures, on labour market outcomes and have found significant long-term effects compared to the matched control group. Not surprisingly, the largest gap between the displaced and non-displaced group is found in the year of displacement, i.e. in 1987, even if the gap could be viewed as relatively small. Moreover, this relatively small initial narrow gap is almost totally closed within three years. Both the low initial gap and the rapid and almost total catch-up are, presumably due to the very good labour market up to 1990. Regardless of the reason, one important point here is that we cannot expect that the experience of these first three years would have been "scarring" in terms of discouragement, stigmatisation and other factors often cited in the unemployment persistence literature.

However, very early in the severe recession of the early 1990s the picture changes dramatically. This is initially most obvious in the employment gap and somewhat later on in the unemployment gap. The stark contrasting states of the labour market, before and during the recession, and the divergent experiences of the displaced group relative to other similar but non-displaced workers, provides us with our first conclusion, namely that post-displacement labour market status is extremely sensitive to the general state of the labour market. This result appears well established particularly as we have argued quite strongly that we do not expect any selection on unobservables to be much of a problem in this application. This business cycle sensitivity feature should be borne in mind when evaluating policy measures directed to displaced workers.

The question thus arises why the displaced group experienced lower employment rates during the recession? While we cannot explicitly identify displacement after the initial displacement in 1987, it would appear somewhat far-fetched to propose any process other than involuntary job loss. As we observe a good labour market development for these individuals for the first three years prior to the recession, without the potentially scarring effects mentioned above and similar employment rates before the recession, there would appear to be only one possible explanation for a higher subsequent displacement probability. These jobs were lost because

they were, compared to the matched control group, new jobs. “New jobs end early” is one of the stylised facts of the job mobility literature. While in the Swedish institutional context one could attribute this as simply a strict application of last-in first-out clause in statutory labour law, it may also be related to higher separation probabilities for those with low levels of specific capital, under the reasonable assumption that specific capital varies inversely with tenure. Specific capital is comprised of two elements. Firm specific human capital in the usual meaning of the term, i.e. that accumulated by training and experience. But it is also something acquired in a matching process that continues even after hiring.

The lowest negative employment effects were largest for the older two age groups. Presumably the younger displaced group did not have appreciably shorter jobs than the equally young control group. Also somewhat in line with our interpretation of the persistence story is that the greater average number of unemployment days in the displaced group appears to be more related to more displaced workers being unemployed rather than longer duration of unemployment or more frequent unemployment spells within a given year.

We were also able to find some long-term effects lasting for the entire observation period of 13 for years. Such effects were found for the employment gap for both the 20 to 30 and 41 to 50 year olds. A very significant long run effect was that the high out of the labour force gap for the oldest age group that exhibited no sign of subsiding by 1999. Rather smaller very long run effects were found for unemployment. Interestingly, unemployment has been the main labour market status studied in the scarring literature. The very long run effects found here do not appear to be found in most other studies. See, for example Fallick (1996) review of the literature, which shows that the effects fade away after roughly four years.

If our explanation is correct this has implications for the unemployment persistence literature which has predominately been on why workers who lose their jobs may not secure new ones. This has been viewed as being due to long-term scarring effect of the loss of human capital, stigmatisation, welfare incentives etc. We interpret the long run effects as being that displaced workers are more likely to experience new job loss and possibly new spells of unemployment. Thus as the use of the word echo in the title indicates, the sound of the initial job loss echoes as time passes, albeit diminishing in volume, but lasting for a long time.

Perhaps the most obvious policy implications following from the specific capital explanation story is that it is not only important that displaced workers find a match but that they find a *good* match. However, it is far from clear how active labour market policy, at least in Sweden, can do more than currently is the case.

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A Data sources

To create this data set we have used several different registers and databases. To identify the study- and control groups the Register based Labour Market Statistics (RAMS), have been utilized. From this register, we have also collected a set of variables for the period 1985-99. During the period 1990-99, the Longitudinal database of Education, Income and Employment (LOUISE) is the main source. This database do not cover, though, the year prior to 1990, therefore we have tried to collect the corresponding information for 1983-89 from the Income and Wealth registers (IoF), which is a set of registers, to create longitudinal data for the whole period 1983-99. All these sources will be briefly described below.

The Register based Labour Market Statistics (*RAMS*) – This annual data is based on several registers, including all individuals, firms and establishments in Sweden, and cover the period 1985-99. The main source is the employers' payroll reports to the tax authorities. By law, all employers are annually obliged to file an income statement for each payee to the Swedish national tax board. This makes it possible to link employees to establishment and firms. This feature is most important for the underlying study since it enables us to identify individuals on closing establishments. In addition to establishment information linked to the individual, a lot of socio-demographic information as well as income information are collected from this register.

The Income and Wealth registers **(***IoF***) –** The income and wealth registers contain the whole Swedish population and provides detailed annual income information. The statistics are mainly based on administrative records from the Swedish National Tax Board, and the variables, relevant for this study, in particular from the tax forms and the employers' payroll reports.

The Longitudinal database of Education, Income and Employment **(***LOUISE***) –** LOUISE is an individual based longitudinal database containing all individuals in Sweden aged 16 and above. It is an integration of a large number of register and its variables cover several different areas, such as demographics, education, employment/unemployment, income, and establishments, for the period 1990-99.

B Statistic Sweden's surveys and the demographic method

When no survey is addressed to the firm, Statistics Sweden performs a different control of the closing establishments. By a demographic method they check whether the establishment have disappeared due to mergers or dispersals. We apply the same method, constituted by the following two conditions.

Condition 1 $N_{ij} / N_j \leq 0.5$

Condition 2 $N_{ij} / N_i \leq 0.5$

Where N_{ij} is the number of individuals employed at the closing establishment *i* in year *t*-1 and at establishment *j* in year *t*. N_i is the total number of individuals employed at the closing establishment *i* in year *t*-1 and N_j is the total number of individuals employed at establishment *j* in year *t*.

If not both conditions are fulfilled, this indicates that the establishment has been wrongly classified as a closure. In case that condition 1 is not satisfied the establishment is likely to have ceased to exist due to dispersal. On the other hand, if condition 2 is not satisfied this indicates that the establishment has been merged into a larger unit. In our sample, all establishments satisfy both these conditions.

C Definitions of length (time-window) of closing process

Definition 1 For an establishment closed in year *t* the closing process 3 years if
a) the number of employees in *t*-3 was 50 or more,
b) there was a reduction of the workforce, between both *t*-3 and *t*-2, and between *t*-2 and *t*-1, by at least 20 percent.

Definition 2 For an establishment closed in year *t* the closing process is 2 years if
a) the closing process is not 3 years according to definition 1,
b) the number of employees in *t*-2 was 25 or more,
c) there was a reduction of the workforce, between *t*-2 and *t*-1, by at least 10 employees, and
d) the reduction in the number of employees corresponded to at least a 20 percent reduction of the workforce.

Definition 3 For an establishment closed in year *t* the closing process is 1 year if the closing process is not 2 or 3 years according to definition 1 and 2.

D Tables

D.1 Variable definitions

Table 1 Variable definitions and descriptions.

Variable	Definition/Description
Socio-demographic variables	
AGE_86	Age in 1986
D_FEMALE	Women
D_CHILD06_86	Has child/-ren aged 0-6 year.
D_CHILD717_86	Has child/-ren aged 7-17 year.
D_MARRIED_86	Was married in 1986.
D_IMMIGRANT_N	Born in other Nordic country.
D_IMMIGRANT_O	Born in non-Nordic country.
Labour market status	
D_EMPL_yy	Had non-zero earnings in 19yy.
D_UNEMPL_yy	Incidence of insured unemployment in 19yy.
D_LMP_yy	Participation in labour market program.
EMPL_yy	Annual earnings in 19yy.
UNEMPL_yy	Amount of unemployment insurance in 19yy.
LMP_yy	Income from labour market program.
D_TENURE	Was employed at the same establishment in both 1985 and 1986
Socio-economic variables	
D_EDUC1_86	Compulsory school, less than 9 years.
D_EDUC2_86	Compulsory school, 9 years.
D_EDUC3_86	Upper secondary school, shorter than 3 years.
D_EDUC4_86	Upper secondary school, equal to or longer than 3 years.
D_EDUC5_86	Tertiary education, shorter than 3 years.
D_EDUC6_86	Tertiary education, equal to or longer than 3 years.
D_EDUC7_86	Graduate studies.
D_EDUC0_86	Unknown education.
D_HOUSE_86	Owned a house in 1986.
D_WEALTH_86	Had taxable wealth in 1986.
D_SA_yy	Social assistant receiver in 19yy.
SA_yy	Amount of social assistance in 19yy.
Health variables	
D_HOSPITAL_yy	Spend at least on night in hospital in 19yy.
D_SICK_yy	Incidence of insured sickness in 19yy.
D_DISABILITY_86	Received disability pension in 1986.
HOSPITAL_yy	Number of days spent in hospital in 19yy.
SICK_yy	Number of insured sickness days.
Sector variables	
D_PUBLIC_86	Employed in public sector in 1986.
D_SNI0_86	Activities not adequately defined
D_SNI1_86	Agriculture and hunting
D_SNI2_86	Forestry
D_SNI3_86	Fishing
D_SNI4_86	Coal mining
D_SNI5_86	Metal ore mining
D_SNI6_86	Other mining
D_SNI7_86	Manufacture of food, beverages and tobacco
D_SNI8_86	Textile, wearing apparel and leather industries
D_SNI9_86	Manufacture of wood and wood products
D_SNI10_86	Manufacture of paper and paper products
D_SNI11_86	Manufacture of chemicals and chemical products
D_SNI12_86	Manufacture of non-metallic mineral products
D_SNI13_86	Basic metal industries

Table 1 Continued.

Variable	Definition/Description
Sector variables	
D_SNI14_86	Manufacture of fabricated metal products
D_SNI15_86	Other manufacturing industries
D_SNI16_86	Electricity, gas and heating
D_SNI17_86	Water supply
D_SNI18_86	Construction
D_SNI19_86	Wholesale trade
D_SNI20_86	Retail trade
D_SNI21_86	Restaurants and hotels
D_SNI22_86	Transport and storage
D_SNI23_86	Post and telecommunication
D_SNI24_86	Financial institutions
D_SNI25_86	Insurance
D_SNI26_86	Real estate and business services
D_SNI27_86	Public administration and defence
D_SNI28_86	Sewage and refuse disposal, and sanitation
D_SNI29_86	Education, research and scientific institutes
D_SNI30_86	Medical, dental, other health and veterinary services, and welfare institutions
D_SNI31_86	Business, professional and labour associations
D_SNI32_86	Other social and related community services
D_SNI33_86	Recreational and cultural services
D_SNI34_86	Personal and household services
D_SNI35_86	International and other extra-territorial bodies
Regional variables	
D_KKSS_86	Resident in larger city in 1986.
D_KKF_86	Resident in a suburban municipality in 1986.
D_KKG_86	Resident in a sparsely populated municipality.
D_KKL_86	Resident in a countryside municipality in 1986.
D_KKMK_86	Resident in other small municipality in 1986.
D_KKMS_86	Resident in medium-sized city in 1986.
D_KKN_86	Resident in industrial municipality.
D_KKS_86	Resident in big city in 1986.
LOC_WAGE_86	Local average annual income level.
LOC_EMPL_86	Local employment rate.
LOC_UNEMPL_86	Local unemployment rate.
Establishment variables	
EST_NONNORDIC	Share of non-Nordic employees.
EST_FEMALES	Share of female employees.
EST_EDUC120	Share of employees with short education.
EST_EDUC34	Share of employees with medium long education.
EST_EDUC567	Share of employees with long education.
EST_SIZE	Number of employees.

D.2 Descriptive statistics

The descriptive statistics are only given for the full sample retained for the analysis. All dummy variables have the prefix D_.

Table 2 Descriptive statistics.

	Displaced		Non-displaced		
Variable	Mean	Std.Dev.	Mean	Std.Dev.	*t*
Socio-demographic variables					
AGE_86	33.508	8.951	34.895	8.607	-10.35
D_FEMALE	0.508		0.529		-2.74
D_CHILD06_86	0.214		0.247		-5.33
D_CHILD717_86	0.306		0.358		-7.61
D_MARRIED_86	0.382		0.478		-13.20
D_IMMIGRANT_N	0.064		0.049		4.17
D_IMMIGRANT_O	0.055		0.038		5.11
Labour market status					
D_EMPL_83	0.946		0.966		-6.03
D_EMPL_84	0.963		0.980		-6.07
D_EMPL_85	0.980		0.990		-4.85
D_UNEMPL_83	0.154		0.094		11.09
D_UNEMPL_84	0.137		0.083		10.46
D_UNEMPL_85	0.130		0.071		11.74
D_LMP_83	0.048		0.027		6.54
D_LMP_84	0.044		0.026		5.94
D_LMP_85	0.050		0.023		8.25
EMPL_83	59,218.520	44,566.270	68,347.510	42,348.440	-13.67
EMPL_84	68,310.650	46,843.880	78,268.920	44,962.380	-14.19
EMPL_85	76,803.560	48,700.560	87,631.690	47,408.280	-14.83
UNEMPL_83	1,690.026	6,089.270	966.274	4,416.114	7.99
UNEMPL_84	1,644.644	6,064.135	914.201	4,507.268	8.09
UNEMPL_85	1,828.578	6,932.950	908.334	4,636.034	8.94
LMP_83	675.417	4,066.848	437.609	3,547.503	3.91
LMP_84	702.329	4,500.820	433.305	3,600.228	4.01
LMP_85	980.715	5,916.146	416.493	3,691.804	6.43
D_TENURE	0.522		0.737		-28.77
Socio-economic variables					
D_EDUC1_86	0.149		0.119		5.55
D_EDUC2_86	0.181		0.140		7.15
D_EDUC3_86	0.359		0.351		1.17
D_EDUC4_86	0.111		0.119		-1.74
D_EDUC5_86	0.078		0.118		-9.87
D_EDUC6_86	0.060		0.108		-13.47
D_EDUC7_86	0.002		0.006		-5.80
D_EDUC0_86	0.060		0.039		6.01
D_HOUSE_86	0.322		0.415		-13.23
D_WEALTH_86	0.438		0.507		-9.30
D_SA_83	0.082		0.037		11.09
D_SA_84	0.093		0.041		11.96
D_SA_85	0.092		0.041		11.70
SA_83	857.567	4,771.011	282.542	2,415.147	8.14
SA_84	889.094	4,407.111	321.688	2,548.687	8.69
SA_85	968.907	4,860.831	344.700	2,733.966	8.67

Table 2 Continued.

	Displaced		Non-displaced		
Variable	Mean	Std.Dev.	Mean	Std.Dev.	*t*
Health variables					
D_HOSPITAL_83	0.052		0.056		-1.42
D_HOSPITAL_84	0.063		0.061		0.59
D_HOSPITAL_85	0.066		0.058		2.11
D_SICK_83	0.610		0.640		-4.08
D_SICK_84	0.645		0.658		-1.84
D_SICK_85	0.685		0.708		-3.23
D_DISABILITY_86	0.002		0.003		-1.43
HOSPITAL_83	0.752	4.480	0.970	5.510	-3.22
HOSPITAL_84	0.773	6.593	0.633	5.843	1.41
HOSPITAL_85	0.790	6.538	0.593	5.776	2.02
SICK_83	14.080	31.867	13.056	31.981	2.14
SICK_84	16.427	37.719	14.457	35.317	3.49
SICK_85	19.471	43.414	16.507	38.704	4.57
Sector variables					
D_PUBLIC_86	0.242		0.460		-33.78
D_SNI0_86	0.000		0.000		
D_SNI1_86	0.002		0.004		-2.90
D_SNI2_86	0.027		0.004		9.62
D_SNI3_86	0.003		0.000		3.82
D_SNI4_86	0.000		0.000		-1.42
D_SNI5_86	0.000		0.003		-19.65
D_SNI6_86	0.000		0.001		-9.65
D_SNI7_86	0.007		0.022		-11.66
D_SNI8_86	0.035		0.011		8.88
D_SNI9_86	0.036		0.019		6.10
D_SNI10_86	0.022		0.036		-6.35
D_SNI11_86	0.028		0.025		1.32
D_SNI12_86	0.005		0.007		-1.62
D_SNI13_86	0.020		0.017		1.36
D_SNI14_86	0.143		0.145		-0.30
D_SNI15_86	0.000		0.002		-15.08
D_SNI16_86	0.010		0.010		-0.47
D_SNI17_86	0.000		0.000		-6.86
D_SNI18_86	0.000		0.000		
D_SNI19_86	0.055		0.049		1.78
D_SNI20_86	0.042		0.053		-3.51
D_SNI21_86	0.115		0.017		20.78
D_SNI22_86	0.061		0.045		4.46
D_SNI23_86	0.018		0.033		-7.47
D_SNI24_86	0.018		0.019		-0.57
D_SNI25_86	0.008		0.014		-4.50
D_SNI26_86	0.097		0.046		11.66
D_SNI27_86	0.034		0.063		-10.37
D_SNI28_86	0.035		0.007		10.50
D_SNI29_86	0.022		0.078		-24.16
D_SNI30_86	0.126		0.240		-22.58
D_SNI31_86	0.007		0.004		2.33
D_SNI32_86	0.000		0.006		-26.02
D_SNI33_86	0.016		0.014		1.31
D_SNI34_86	0.007		0.007		-0.28
D_SNI35_86	0.000		0.000		-1.00

Table 2 Continued.

	Displaced		Non-displaced		
Variable	Mean	Std.Dev.	Mean	Std.Dev.	*t*
Regional variables					
D_KKSS_86	0.250		0.284		-5.11
D_KKF_86	0.186		0.157		5.10
D_KKG_86	0.019		0.018		0.47
D_KKL_86	0.029		0.029		-0.07
D_KKMK_86	0.064		0.060		0.89
D_KKMS_86	0.130		0.154		-4.62
D_KKN_86	0.049		0.069		-6.22
D_KKS_86	0.204		0.161		7.11
D_KKSK_86	0.069		0.068		0.08
LOC_WAGE_86	932.954	89.806	926.276	86.156	4.96
LOC_EMPL_86	0.834	0.034	0.835	0.030	-1.44
LOC_UNEMPL_86	0.041	0.022	0.041	0.019	-0.22
Establishment variables					
EST_NONNORDIC	0.063	0.089	0.043	0.061	15.14
EST_FEMALES	0.478	0.288	0.510	0.301	-7.50
EST_EDUC120	0.441	0.232	0.337	0.206	30.13
EST_EDUC34	0.426	0.171	0.445	0.163	-7.64
EST_EDUC567	0.133	0.183	0.218	0.219	-30.70
EST_SIZE	55.352	53.204	726.664	1,507.724	-151.33

D.3 The number of closing establishments and displaced workers

Table 3 The number of closing establishments and displaced workers.

Year of closure	Year of separation	Age 20-30	Age 31-40	Age 41-50	Total	Establishments
1988	1987	392	308	279	979	32
1987	1987	1,560	1,073	1,000	3,633	339
1987-1988	1987	1,952	1,381	1,279	4,612	371

D.4 The logit estimates

Table 4 Logit estimates

	Age 20-30		Age 31-40		Age 41-50	
	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Socio-demographic variables						
AGE_86	-0.128	0.154	0.356	0.287	0.169	0.381
D_FEMALE	**0.190**	0.065	-0.116	0.086	-0.069	0.087
D_CHILD06_86	-0.099	0.079	-0.067	0.071	-0.033	0.125
D_CHILD717_86	0.005	0.123	-0.003	0.069	0.070	0.068
D_MARRIED_86	-0.110	0.084	-0.020	0.070	**-0.324**	0.070
D_IMMIGRANT_N	-0.144	0.145	**0.236**	0.113	0.196	0.115
D_IMMIGRANT_O	-0.031	0.160	0.043	0.146	-0.110	0.145
Labour market status						
D_EMPL_83	0.068	0.118	-0.115	0.167	-0.108	0.220
D_EMPL_84	0.005	0.149	0.072	0.195	-0.091	0.260
D_EMPL_85	0.193	0.184	0.083	0.232	0.080	0.321
D_UNEMPL_83	0.042	0.089	**0.570**	0.165	**0.645**	0.165
D_UNEMPL_84	0.178	0.093	**-0.609**	0.213	0.051	0.199
D_UNEMPL_85	-0.091	0.098	**0.821**	0.179	0.093	0.197
D_LMP_83	0.101	0.172	-0.115	0.278	0.070	0.285
D_LMP_84	-0.083	0.180	0.177	0.293	-0.115	0.314
D_LMP_85	0.003	0.160	-0.352	0.276	-0.089	0.299
EMPL_83/1000	0.002	0.002	-0.003	0.003	**0.005**	0.002
EMPL_84/1000	-0.002	0.002	0.002	0.002	0.000	0.001
EMPL_85/1000	0.000	0.001	-0.004	0.002	**-0.003**	0.001
UNEMPL_83/1000	0.004	0.006	-0.031	0.020	-0.016	0.009
UNEMPL_84/1000	-0.009	0.006	**0.054**	0.022	-0.011	0.010
UNEMPL_85/1000	0.004	0.005	**-0.049**	0.019	0.014	0.008
LMP_83/1000	-0.010	0.010	0.038	0.030	-0.006	0.013
LMP_84/1000	-0.016	0.009	-0.031	0.027	0.019	0.013
LMP_85/1000	**0.016**	0.006	0.003	0.022	0.000	0.014
D_TENURE	**-0.522**	0.054	**-0.658**	0.063	**-0.647**	0.067
Socio-economic variables						
D_EDUC2_86	0.119	0.373	0.045	0.108	**-0.317**	0.122
D_EDUC3_86	-0.019	0.372	0.193	0.102	0.005	0.083
D_EDUC4_86	-0.131	0.378	0.007	0.136	0.019	0.120
D_EDUC5_86	-0.069	0.384	0.071	0.143	-0.044	0.140
D_EDUC6_86	0.097	0.397	0.156	0.155	-0.072	0.157
D_EDUC7_86			0.440	0.611	-0.409	0.512
D_EDUC0_86	-0.054	0.378	-0.177	0.153	-0.042	0.171
D_HOUSE_86	0.075	0.074	-0.045	0.068	-0.067	0.068
D_WEALTH_86	-0.044	0.053	-0.120	0.063	-0.026	0.066
D_SA_83	0.068	0.129	-0.047	0.174	-0.235	0.229
D_SA_84	0.121	0.125	-0.039	0.175	0.306	0.224
D_SA_85	0.058	0.118	0.223	0.164	-0.151	0.232
SA_83/1000	0.008	0.010	0.012	0.018	**0.036**	0.011
SA_84/1000	-0.012	0.011	0.017	0.016	-0.027	0.017
SA_85/1000	0.010	0.008	-0.019	0.013	0.023	0.014

Table 4 continued.

	Age 20-30		Age 31-40		Age 41-50	
	Coef.	Std. Err.	Coef.	Std. Err.	Coef.	Std. Err.
Health variables						
D_HOSPITAL_83	**0.360**	0.178	0.025	0.207	0.342	0.292
D_HOSPITAL_84	0.134	0.121	-0.021	0.179	-0.241	0.155
D_HOSPITAL_85	0.040	0.116	0.220	0.169	0.026	0.144
D_SICK_83	-0.011	0.065	-0.082	0.076	-0.019	0.072
D_SICK_84	-0.021	0.064	0.139	0.077	-0.090	0.072
D_SICK_85	-0.072	0.063	0.031	0.077	-0.087	0.073
D_DISABILITY_86	-0.689	1.030	-1.126	0.603	-0.571	0.402
HOSPITAL_83	0.001	0.001	0.001	0.002	0.000	0.001
HOSPITAL_84	-0.002	0.001	**0.004**	0.002	0.000	0.001
HOSPITAL_85	0.002	0.001	0.000	0.002	-0.001	0.001
SICK_83	-0.014	0.008	0.026	0.039	-0.027	0.025
SICK_84	-0.006	0.007	-0.013	0.023	0.001	0.005
SICK_85	0.003	0.004	-0.033	0.020	0.000	0.005
Sector variables						
D_PUBLIC_86	0.020	0.113	**-0.501**	0.128	**-0.419**	0.133
D_SNI1_86	-0.549	0.432	-0.834	0.610		
D_SNI2_86	**1.871**	0.227	**2.413**	0.226	**2.414**	0.233
D_SNI3_86	**3.426**	0.561			**3.033**	1.251
D_SNI7_86	**-0.653**	0.278	**-1.145**	0.364	**-1.179**	0.407
D_SNI8_86	**1.236**	0.195	0.438	0.228	**0.905**	0.209
D_SNI9_86	**0.769**	0.171	**0.495**	0.208	**0.829**	0.203
D_SNI10_86	-0.213	0.239	0.045	0.231	**0.654**	0.198
D_SNI11_86	**0.952**	0.184	**0.800**	0.203	**0.501**	0.229
D_SNI12_86	**0.693**	0.297	0.004	0.413	-1.021	0.600
D_SNI13_86	**1.268**	0.303	**2.112**	0.248	**2.139**	0.240
D_SNI14_86	**0.734**	0.129	**0.929**	0.141	**0.885**	0.151
D_SNI16_86	**0.844**	0.290	**0.912**	0.285	0.352	0.339
D_SNI19_86						
D_SNI20_86	**-0.408**	0.149	**-1.306**	0.210	**-1.095**	0.213
D_SNI21_86	**1.325**	0.132	**0.730**	0.175	**0.682**	0.191
D_SNI22_86	**0.412**	0.145	**0.337**	0.167	**0.481**	0.179
D_SNI23_86	**0.693**	0.213	-0.429	0.357	-0.154	0.343
D_SNI24_86	**0.577**	0.206	-0.161	0.271	0.207	0.257
D_SNI25_86	-0.563	0.406	-0.557	0.333	0.240	0.296
D_SNI26_86	**0.805**	0.133	**0.513**	0.149	**0.421**	0.162
D_SNI27_86	**1.086**	0.192	-0.280	0.255	-0.249	0.265
D_SNI28_86	**1.140**	0.183	**0.825**	0.208	**1.090**	0.227
D_SNI29_86	**-0.510**	0.255	**-1.528**	0.269	**-1.181**	0.260
D_SNI30_86	-0.077	0.180	-0.374	0.202	-0.028	0.210
D_SNI31_86	0.622	0.395	0.436	0.349	0.069	0.346
D_SNI33_86	**0.552**	0.193	-0.266	0.277	**-1.671**	0.527
D_SNI34_86	-0.504	0.317	-0.491	0.354	-0.376	0.394
Regional variables						
D_KKSS_86	0.187	0.123	0.211	0.140	0.112	0.137
D_KKF_86	**0.608**	0.151	**0.435**	0.182	0.135	0.184
D_KKG_86	0.102	0.192	-0.465	0.259	**-1.276**	0.371
D_KKL_86	-0.277	0.196	0.228	0.189	0.270	0.199
D_KKMK_86	0.052	0.153	0.144	0.163	0.230	0.161
D_KKMS_86	0.097	0.126	-0.006	0.143	0.003	0.142
D_KKN_86	**-0.405**	0.155	-0.299	0.170	-0.041	0.161
D_KKS_86	**0.376**	0.149	**0.522**	0.178	0.248	0.177
LOC_WAGE_86	**0.001**	0.001	0.001	0.001	0.001	0.001
LOC_EMPL_86	-0.382	1.648	-3.623	34.513	0.731	1.910
LOC_UNEMPL_86	3.025	4.797	0.684	6.244	5.445	3.024

Table 4 continued.

	Age 20-30		Age 31-40		Age 41-50	
	Coef.	Std. Err.	Coef.	Std. Err	Coef.	Std. Err
Establishment variables						
EST_NONNORDIC	0.463	0.309	**1.171**	0.364	**2.380**	0.403
EST_FEMALES	-0.003	0.136	**1.018**	0.171	**0.998**	0.178
EST_EDUC567	**-1.327**	0.212	**-0.899**	0.234	**-0.856**	0.246
EST_EDUC34	**-0.886**	0.185	**-1.720**	0.235	**-1.572**	0.241
EST_SIZE	**-0.009**	0.001	**-0.004**	0.001	-0.002	0.001
Number of obs	42628		42349		37304	
Log L	-6232		-4689		-4384	
Pseudo R2	0.214		0.229		0.213	

D.5 Sample sizes and matching quality

Table 5 Sample size before and after matching

	Unmatched sample	Matched sample
Age 20-30		
Displaced	1,952	1,952 (1,952)
Non-displaced	41,144	1,952 (1,731)
Age 31-40		
Displaced	1,381	1,379 (1,379)
Non-displaced	41,457	1,379 (1,250)
Age 41-50		
Displaced	1,279	1,279 (1,279)
Non-displaced	36,640	1,279 (1,165)

Table 6 Summary statistic of the propensity of becoming displaced (log-odds-ratio).

	Unmatched sample			Matched sample		
	Mean	Min	Max	Mean	Min	Max
Age 20-30						
Displaced	-2.105	-5.227	1.102	-2.105	-5.227	1.102
Non-displaced	-29.215	-611.449	1.213	-2.105	-5.227	1.107
Age 31-40						
Displaced	-2.317	-5.385	1.083	-2.317	-5.385	1.083
Non-displaced	-56.810	-1,667.430	1.665	-2.317	-5.385	1.052
Age 41-50						
Displaced	-2.348	-4.999	0.954	-2.348	-4.999	0.954
Non-displaced	-63.237	-2,365.273	1.024	-2.348	-4.999	1.003

Table 7 Absolute standardized difference before and after matching.

	Unmatched sample			Matched sample		
	Mean	Min	Max	Mean	Min	Max
Age 20-30	24.477	0.007	1525.507	2.001	0.000	5.696
Age 31-40	24.739	0.442	1208.690	2.594	0.000	5.957
Age 41-50	20.609	0.387	1038.979	2.521	0.000	7.560

D.6 Results

Table 8 Estimated average effect of displacement on the employment probability for workers aged 20-30.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	1.000	1.000	0.000	.	-0.015	-1.20
1987	0.864	0.917	**-0.053**	-5.09	**-0.068**	-4.52
1988	0.877	0.921	**-0.044**	-4.36	**-0.059**	-3.91
1989	0.910	0.910	-0.001	-0.05	-0.015	-1.03
1990	0.884	0.910	**-0.027**	-2.61	**-0.042**	-2.68
1991	0.844	0.877	**-0.033**	-2.85	**-0.048**	-2.98
1992	0.803	0.834	**-0.031**	-2.36	**-0.046**	-2.66
1993	0.730	0.753	-0.023	-1.55	**-0.038**	-2.04
1994	0.755	0.786	**-0.031**	-2.19	**-0.046**	-2.54
1995	0.769	0.790	-0.022	-1.53	**-0.036**	-2.01
1996	0.780	0.795	-0.015	-1.07	-0.030	-1.64
1997	0.779	0.801	-0.023	-1.62	**-0.037**	-2.09
1998	0.800	0.833	**-0.034**	-2.57	**-0.049**	-2.79
1999	0.803	0.825	-0.022	-1.67	**-0.037**	-2.13

Table 9 Estimated average effect of displacement on the employment probability for workers aged 31-40.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	1.000	1.000	0.000	.	0.002	0.20
1987	0.888	0.960	**-0.073**	-7.09	**-0.070**	-5.02
1988	0.901	0.951	**-0.049**	-4.81	**-0.047**	-3.34
1989	0.913	0.937	**-0.024**	-2.31	-0.022	-1.51
1990	0.902	0.930	**-0.028**	-2.61	-0.026	-1.78
1991	0.867	0.902	**-0.036**	-2.82	**-0.033**	-2.10
1992	0.831	0.870	**-0.038**	-2.71	**-0.036**	-2.13
1993	0.777	0.812	**-0.035**	-2.17	-0.033	-1.77
1994	0.792	0.813	-0.021	-1.33	-0.019	-1.04
1995	0.779	0.809	-0.030	-1.86	-0.028	-1.50
1996	0.771	0.813	**-0.042**	-2.62	**-0.040**	-2.15
1997	0.760	0.808	**-0.048**	-2.93	**-0.046**	-2.45
1998	0.776	0.814	**-0.038**	-2.41	**-0.036**	-1.96
1999	0.778	0.815	**-0.037**	-2.33	-0.035	-1.89

Table 10 Estimated average effect of displacement on the employment probability for workers aged 41-50.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	1.000	1.000	0.000	.	0.000	0.00
1987	0.889	0.984	**-0.095**	-10.01	**-0.095**	-7.52
1988	0.932	0.973	**-0.041**	-4.70	**-0.041**	-3.36
1989	0.928	0.960	**-0.032**	-3.40	**-0.032**	-2.54
1990	0.920	0.938	-0.019	-1.77	-0.019	-1.40
1991	0.877	0.917	**-0.041**	-3.24	**-0.041**	-2.78
1992	0.830	0.887	**-0.056**	-3.95	**-0.056**	-3.52
1993	0.776	0.837	**-0.060**	-3.70	**-0.060**	-3.41
1994	0.768	0.826	**-0.059**	-3.54	**-0.059**	-3.25
1995	0.768	0.812	**-0.044**	-2.60	**-0.044**	-2.37
1996	0.737	0.794	**-0.057**	-3.27	**-0.057**	-3.02
1997	0.708	0.764	**-0.056**	-3.08	**-0.056**	-2.87
1998	0.689	0.728	**-0.039**	-2.08	-0.039	-1.93
1999	0.652	0.694	**-0.042**	-2.17	**-0.042**	-2.03

Table 11 Estimated average effect of displacement on the employment probability for workers aged 20-50. The estimates are based on the results of the estimations stratified by age categories.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	1.000	1.000	0.000	.	-0.006	-0.85
1987	0.878	0.949	**-0.071**	-11.81	**-0.076**	-9.09
1988	0.900	0.944	**-0.045**	-7.73	**-0.050**	-6.02
1989	0.916	0.932	**-0.016**	-2.82	**-0.022**	-2.61
1990	0.899	0.924	**-0.025**	-4.07	**-0.031**	-3.50
1991	0.860	0.896	**-0.036**	-5.05	**-0.042**	-4.49
1992	0.819	0.859	**-0.040**	-5.02	**-0.046**	-4.61
1993	0.757	0.794	**-0.037**	-4.05	**-0.043**	-3.94
1994	0.769	0.805	**-0.036**	-4.00	**-0.041**	-3.88
1995	0.772	0.802	**-0.030**	-3.36	**-0.036**	-3.33
1996	0.765	0.800	**-0.035**	-3.85	**-0.040**	-3.73
1997	0.753	0.793	**-0.040**	-4.30	**-0.045**	-4.15
1998	0.762	0.798	**-0.037**	-4.07	**-0.042**	-3.91
1999	0.754	0.786	**-0.032**	-3.53	**-0.038**	-3.49

Table 12 Estimated average effect of displacement on the unemployment probability for workers aged 20-30.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	0.217	0.203	0.014	1.04	0.013	1.01
1987	0.267	0.195	**0.072**	5.10	**0.071**	4.37
1988	0.207	0.183	0.024	1.80	0.023	1.39
1989	0.160	0.154	0.006	0.50	0.005	0.32
1990	0.139	0.127	0.012	1.07	0.011	0.70
1991	0.175	0.159	0.015	1.21	0.014	0.85
1992	0.242	0.226	0.017	1.17	0.016	0.87
1993	0.288	0.257	**0.031**	2.06	0.030	1.60
1994	0.282	0.264	0.019	1.25	0.018	0.94
1995	0.277	0.252	0.026	1.71	0.025	1.31
1996	0.253	0.244	0.010	0.66	0.009	0.47
1997	0.256	0.238	0.018	1.22	0.017	0.92
1998	0.232	0.217	0.015	1.08	0.014	0.79
1999	0.216	0.197	0.019	1.38	0.018	1.00

Table 13 Estimated average effect of displacement on the unemployment probability for workers aged 31-40.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	0.153	0.109	**0.044**	3.33	**0.038**	2.87
1987	0.241	0.092	**0.149**	10.35	**0.143**	8.49
1988	0.205	0.087	**0.118**	8.57	**0.112**	6.69
1989	0.137	0.088	**0.049**	3.96	**0.044**	2.69
1990	0.112	0.077	**0.035**	3.02	0.029	1.81
1991	0.141	0.104	**0.037**	2.84	0.031	1.79
1992	0.202	0.153	**0.049**	3.24	**0.044**	2.30
1993	0.241	0.206	**0.035**	2.10	0.029	1.45
1994	0.254	0.191	**0.062**	3.78	**0.057**	2.83
1995	0.242	0.176	**0.065**	4.04	**0.060**	2.97
1996	0.223	0.172	**0.052**	3.26	**0.046**	2.31
1997	0.220	0.173	**0.048**	3.04	**0.042**	2.14
1998	0.200	0.167	**0.033**	2.17	0.028	1.41
1999	0.183	0.161	0.022	1.45	0.016	0.83

Table 14 Estimated average effect of displacement on the unemployment probability for workers aged 41-50.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	0.088	0.082	0.006	0.55	-0.002	-0.21
1987	0.220	0.071	**0.149**	10.69	**0.140**	9.08
1988	0.163	0.060	**0.102**	8.16	**0.094**	6.40
1989	0.092	0.049	**0.044**	4.27	**0.035**	2.55
1990	0.096	0.043	**0.053**	5.15	**0.045**	3.16
1991	0.091	0.067	**0.024**	2.12	0.015	1.00
1992	0.133	0.104	**0.029**	2.19	0.020	1.24
1993	0.181	0.147	**0.034**	2.27	0.026	1.45
1994	0.194	0.149	**0.045**	2.95	**0.037**	2.04
1995	0.186	0.131	**0.055**	3.68	**0.046**	2.58
1996	0.173	0.131	**0.041**	2.83	0.033	1.87
1997	0.178	0.147	**0.031**	2.02	0.022	1.21
1998	0.168	0.151	0.017	1.15	0.009	0.48
1999	0.162	0.151	0.011	0.74	0.002	0.13

Table 15 Estimated average effect of displacement on the unemployment probability for workers aged 20-50.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	0.162	0.141	**0.021**	2.73	**0.016**	2.18
1987	0.246	0.130	**0.116**	13.97	**0.112**	11.72
1988	0.194	0.120	**0.074**	9.43	**0.069**	7.27
1989	0.134	0.105	**0.030**	4.20	**0.025**	2.71
1990	0.119	0.089	**0.030**	4.60	**0.026**	2.82
1991	0.141	0.117	**0.024**	3.30	**0.020**	2.01
1992	0.200	0.170	**0.030**	3.54	**0.025**	2.40
1993	0.244	0.211	**0.033**	3.63	**0.029**	2.57
1994	0.249	0.210	**0.039**	4.29	**0.035**	3.09
1995	0.241	0.196	**0.046**	5.07	**0.041**	3.68
1996	0.222	0.191	**0.031**	3.52	**0.026**	2.42
1997	0.224	0.193	**0.030**	3.43	**0.026**	2.35
1998	0.205	0.184	**0.021**	2.47	0.017	1.54
1999	0.191	0.174	**0.018**	2.09	0.013	1.21

Table 16 Estimated average effect of displacement on the probability of being out of the labour force for workers aged 20-30.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	0.000	0.000	0.000	.	-0.001	-0.13
1987	0.016	0.007	**0.009**	2.55	0.009	1.64
1988	0.026	0.012	**0.014**	3.00	**0.013**	2.21
1989	0.031	0.021	**0.010**	1.96	0.010	1.50
1990	0.038	0.025	**0.014**	2.38	0.013	1.89
1991	0.047	0.030	**0.017**	2.61	**0.016**	2.16
1992	0.056	0.038	**0.019**	2.58	**0.018**	2.20
1993	0.057	0.053	0.004	0.53	0.004	0.42
1994	0.062	0.057	0.005	0.65	0.005	0.53
1995	0.061	0.047	0.014	1.88	0.014	1.63
1996	0.062	0.046	**0.015**	2.01	0.015	1.74
1997	0.065	0.051	0.014	1.74	0.013	1.51
1998	0.062	0.049	0.013	1.72	0.013	1.48
1999	0.072	0.055	**0.017**	2.12	0.017	1.87

Table 17 Estimated average effect of displacement on the probability of being out of the labour force for workers aged 31-40.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	0.000	0.000	0.000	.	0.004	0.78
1987	0.014	0.010	0.004	0.86	0.007	1.19
1988	0.023	0.015	0.008	1.53	0.012	1.73
1989	0.030	0.018	0.012	1.93	**0.015**	2.05
1990	0.044	0.025	**0.019**	2.63	**0.023**	2.69
1991	0.049	0.035	0.014	1.78	**0.017**	1.97
1992	0.052	0.047	0.004	0.51	0.008	0.82
1993	0.062	0.060	0.002	0.15	0.005	0.49
1994	0.056	0.073	-0.017	-1.74	-0.013	-1.23
1995	0.069	0.072	-0.003	-0.29	0.001	0.07
1996	0.077	0.070	0.007	0.71	0.011	0.98
1997	0.083	0.078	0.005	0.48	0.009	0.75
1998	0.094	0.091	0.003	0.25	0.007	0.54
1999	0.106	0.099	0.007	0.61	0.011	0.86

Table 18 Estimated average effect of displacement on the probability of being out of the labour force for workers aged 41-50.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	0.000	0.000	0.000	.	0.000	0.00
1987	0.011	0.002	**0.009**	2.61	0.009	1.66
1988	0.024	0.007	**0.016**	3.27	**0.016**	2.55
1989	0.041	0.020	**0.021**	3.00	**0.021**	2.62
1990	0.049	0.027	**0.022**	2.75	**0.022**	2.46
1991	0.060	0.042	0.018	1.96	0.018	1.79
1992	0.078	0.048	**0.031**	3.04	**0.031**	2.80
1993	0.095	0.067	**0.028**	2.48	**0.028**	2.33
1994	0.110	0.079	**0.031**	2.51	**0.031**	2.38
1995	0.111	0.099	0.013	0.97	0.013	0.93
1996	0.131	0.106	0.025	1.85	0.025	1.76
1997	0.153	0.119	**0.034**	2.35	**0.034**	2.25
1998	0.169	0.146	0.023	1.49	0.023	1.44
1999	0.203	0.170	**0.033**	2.02	0.033	1.96

Table 19 Estimated average effect of displacement on the probability of being out of the labour force for workers aged 20-50.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t	Δ^{DID}	t
1986	0.000	0.000	0.000	.	0.001	0.35
1987	0.014	0.007	**0.007**	3.38	**0.008**	2.55
1988	0.024	0.011	**0.013**	4.48	**0.014**	3.70
1989	0.033	0.020	**0.014**	3.98	**0.015**	3.48
1990	0.043	0.026	**0.018**	4.45	**0.018**	4.01
1991	0.051	0.035	**0.016**	3.70	**0.017**	3.43
1992	0.061	0.043	**0.018**	3.62	**0.018**	3.40
1993	0.069	0.059	0.010	1.87	0.011	1.86
1994	0.073	0.068	0.006	1.02	0.006	1.08
1995	0.077	0.069	0.009	1.54	0.010	1.55
1996	0.086	0.070	**0.016**	2.68	**0.017**	2.63
1997	0.095	0.078	**0.017**	2.73	**0.018**	2.67
1998	0.101	0.089	**0.013**	2.02	**0.014**	2.01
1999	0.119	0.100	**0.019**	2.78	**0.020**	2.75

Table 20 Estimated average effect of displacement on number of registered unemployment days for workers aged 20-30.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t
1992	50.10	46.61	3.49	1.00
1993	64.58	58.18	6.41	1.63
1994	61.48	55.46	6.02	1.58
1995	62.88	54.06	**8.82**	2.30
1996	55.98	55.26	0.72	0.19
1997	51.06	49.41	1.66	0.46
1998	43.02	37.82	5.20	1.63
1999	38.50	34.07	4.43	1.41

Table 21 Estimated average effect of displacement on number of registered unemployment days for workers aged 31-40.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t
1992	50.91	33.12	**17.79**	4.51
1993	61.21	44.73	**16.49**	3.66
1994	62.23	46.49	**15.74**	3.51
1995	61.00	46.94	**14.07**	3.05
1996	61.27	43.54	**17.73**	3.85
1997	57.13	40.74	**16.39**	3.71
1998	48.73	35.27	**13.46**	3.28
1999	45.02	33.57	**11.46**	2.91

Table 22 Estimated average effect of displacement on number of registered unemployment days for workers aged 41-50.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t
1992	32.29	19.66	**12.63**	3.90
1993	44.49	32.88	**11.61**	2.86
1994	50.76	35.74	**15.02**	3.47
1995	49.37	35.30	**14.07**	3.24
1996	47.86	36.96	**10.90**	2.48
1997	43.17	37.17	6.00	1.42
1998	38.97	31.59	7.38	1.83
1999	39.12	34.73	4.39	1.06

Table 23 Estimated average effect of displacement on number of registered unemployment days for workers aged 20-30. The figures are calculated from the samples stratified by age categories.

Year	$E[Y_{D=1}]$	$E[Y_{D=0}]$	Δ^D	t
1992	45.40	35.10	**10.31**	4.93
1993	58.00	47.13	**10.87**	4.49
1994	58.73	47.30	**11.43**	4.73
1995	58.57	46.72	**11.84**	4.84
1996	55.31	46.67	**8.64**	3.54
1997	50.69	43.42	**7.27**	3.12
1998	43.61	35.33	**8.28**	3.87
1999	40.62	34.10	**6.52**	3.08

Table 24 Unemployment differential (ratio) by incidence and days per year

Year	Age 20-30			Age 31-40			Age 41-50		
	$\Delta_t(U)$	$\Delta_t(U\|U>0)$	$\Delta_t(\Pr(U>0))$	$\Delta_t(U)$	$\Delta_t(U\|U>0)$	$\Delta_t(\Pr(U>0))$	$\Delta_t(U)$	$\Delta_t(U\|U>0)$	$\Delta_t(\Pr(U>0))$
1992	1.07	0.97	1.11	1.54	1.13	1.36	1.64	1.14	1.45
1993	1.11	1.04	1.07	1.37	1.10	1.24	1.35	1.00	1.36
1994	1.11	1.10	1.01	1.34	1.06	1.26	1.42	0.98	1.44
1995	1.16	1.05	1.11	1.30	1.05	1.24	1.40	1.00	1.40
1996	1.01	0.97	1.04	1.41	1.08	1.30	1.30	1.01	1.28
1997	1.03	0.97	1.06	1.40	1.10	1.27	1.16	0.96	1.21
1998	1.14	1.05	1.08	1.38	1.11	1.24	1.23	1.07	1.15
1999	1.13	1.00	1.13	1.34	1.09	1.23	1.13	1.01	1.12

E Figures

Figure 1: The employment and unemployment rate in Sweden, 1975-2001.

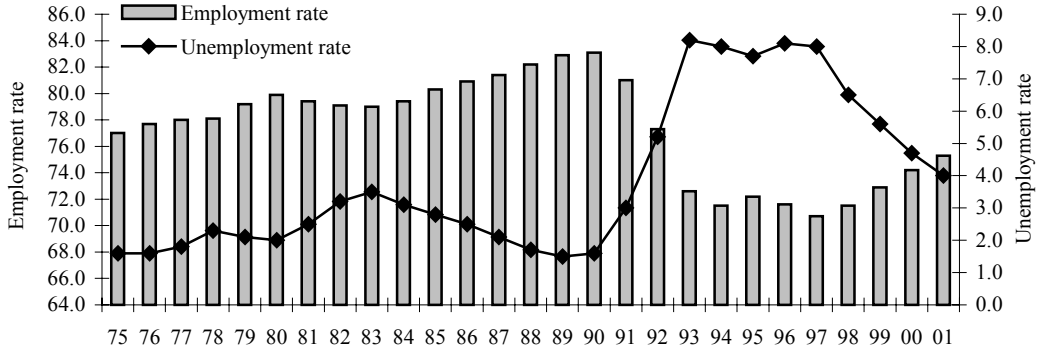


Figure 2 Estimated average effect (Δ^{DID}) of displacement on the probability of being employed,(in November) unemployed or out of the labour force. The estimates are based on the results of the estimations stratified by age categories. Dots denote estimates significant at 5 percent level.

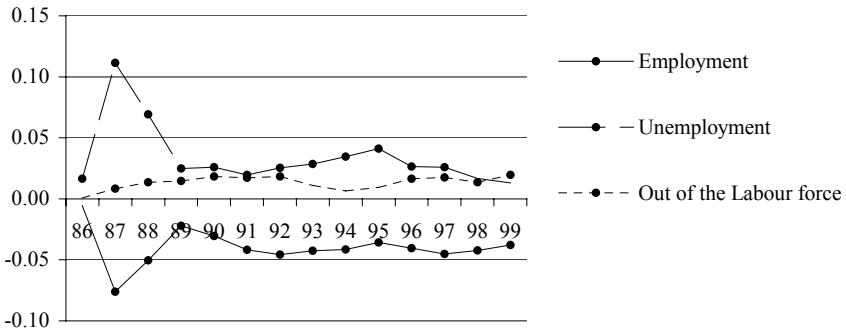


Figure 3 Estimated average effect (Δ^{DID}) of displacement on employment (in November) probability by age category. Dots denote estimates significant at 5 percent level.

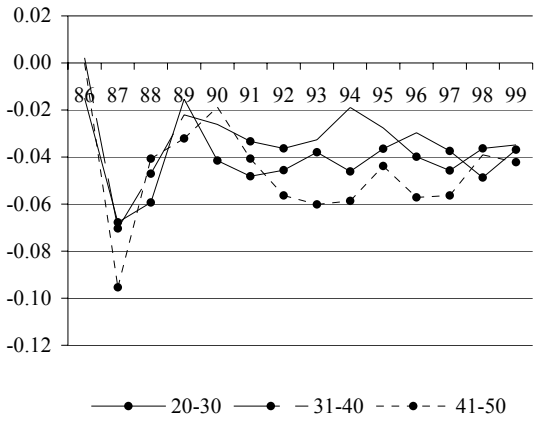


Figure 4 Estimated average effect (Δ^{DID}) of displacement on unemployment probability by age category. Dots denote estimates significant at 5 percent level.

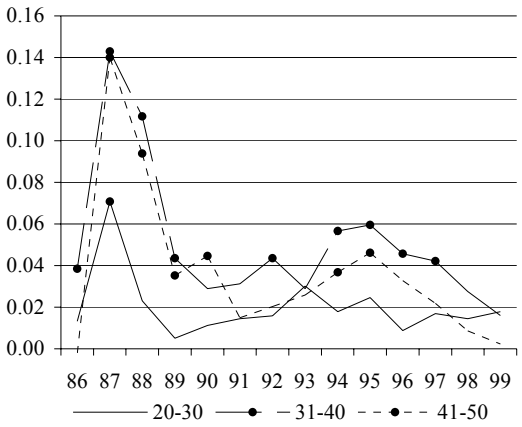


Figure 5 Estimated average effect (Δ^{DID}) of displacement on the probability of being out of the labour force, by age category. Dots denote estimates significant at 5 percent level.

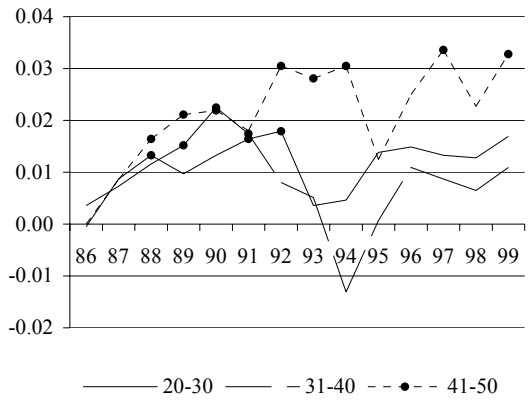
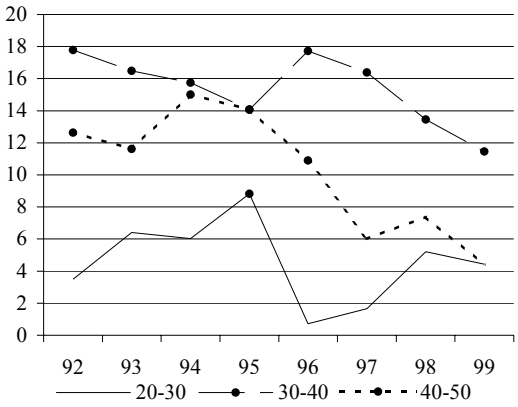


Figure 6 Estimated average effect (Δ^{D}) of displacement on annual number of registered unemployment days. Dots denote estimates significant at 5 percent level.



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