The Echo of Job Displacement

Marcus Eliason and Donald Storrie

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Institute for Social and Economic Research
University of Essex
Wivenhoe Park
Colchester
Essex
CO4 3SQ UK
Telephone: +44 (0) 1206 872957
Fax: +44 (0) 1206 873151
E-mail: iser@essex.ac.uk
Website: http://www.iser.essex.ac.uk

ABSTRACT

In this paper we examine the long-term effects of job displacement due to establishment closures in Sweden on labor market status. Using linked employer-employee data we are able to identify all employees displaced in 1987 and follow them until 1999. The displaced employees are compared to a large random sample of non-displaced employees by using a difference–in–difference matching estimator. We find a rapid and almost total recovery of those displaced in 1987 compared to the control group up to 1990, both with respect to employment and unemployment. However, with the advent of the deep recession in 1990, the two groups again diverge and by the end of the century, the echo of the job loss 13 years earlier had still not subsided. We attribute the longer-term effects to recurrent displacements. Among the various possible explanations of this phenomenon, we focus on short tenure on subsequent jobs which makes the previously displaced vulnerable to further adverse shocks. We cannot precisely identify the significance of short tenure for recurrent displacement but loss of job specific capital or seniority lay-offs rules are the prime candidates.

JEL classification: J63, J64 & J65
Key Words: Displaced workers, plant closure, linked employer-employee data, propensity score matching, job specific capital.

1 Introduction[1]

The costs of job displacement have been defined by Hamermesh (1987) as the sum of adjustment costs incurred while the labor market moves to a new equilibrium and the difference between the value of resources before and after the labor market has adjusted to the shock that changes its value. It would appear quite reasonable that there are adjustment costs and indeed there is a substantial body of empirical research in the United States (Farber, 2003)[2] and Europe (Kuhn, 2002) that confirm this as regards earnings and joblessness. However, perhaps somewhat surprisingly, research has even found significant long-term effects on earnings. See Ruhm (1991), Jacobsen *et al.* (1993) and Stevens (1997) for the United States. While the long-term evidence from Europe is more sparse, Huttunen *et al*. (2003) find negative long-term (up to six years) earnings effects in Norway.

The short-term consequences of displacement are usually attributed to loss of firm specific capital (Hamermesh, 1987). The literature is much less explicit concerning the mechanisms behind the longer-term effects. The unemployment scarring literature (Ellwood, 1982, Arulampalam *et al*., 2001 and Böheim and Taylor, 2002) refers to a number of factors such as: permanent loss of firm specific human capital, the deterioration of general human capital during a spell of unemployment and that potential employers may use previous labor market history as a signal of productivity, but are not able to discriminate between the many hypothesis. However, Stevens (1997) shows that the long-term earnings effects are, to a considerable extent, related to multiple job losses. This leads us to focus on post-displacement labor market status. The risk of subsequent job loss is of course extremely sensitive to the state of the labor market (Hall, 1995). Unlike the previous literature on long-term effects of displacement, we control fully for the state of the labor market by exposing the displaced and control groups to the same labor market

We use linked employer-employee administrative data to examine the post-displacement labor market status, over a period of 13 years, of *all* workers who lost their job in 1987 due to the closure of an establishment in Sweden, compared to a matched control group.[3] Administrative data can overcome some of the weaknesses of survey data such as recall bias, sample size and, of particular importance when studying long-term effects, sample attrition (Jacobson *et al.,* 1993). However, a weakness of administrative data is that one typically observes not displacements but separations. Our exclusive focus on displacement due to plant closures is useful in this respect. Moreover, we have information on the closure process up to three years before final closure which Hamermesh and Pfann (2003) shows to be critical in properly evaluating the consequences of displacement.

[1] We thank Anders Björklund, Henry Ohlsson and participants at the conference "Microeconomic Analyses of Labor Reallocation" (W.E Upjohn Institute for Employment Research and the William Davidson Institute, University of Michican, August 2003) for useful comments. We gratefully acknowledge financial support from The Swedish Research Council. Part of this paper was written when Marcus Eliason was a visiting researcher at the Institute for Social and Economic Research, University of Essex, UK on a grant from the Swedish Council for Working Life and Social Research.

[2] See Kletzer (1998) for a review of the US literature.

[3] Previous Swedish research on the consequences of displacement has all been based on case studies. See Björklund and Holmund (1987), Edin (1988), Engström and Ohlsson (1985) and Storrie (1993).

Recent research on worker displacement has typically followed the individual fixed effects methodology as applied in Jacobsson *et al.* (1993), who point out that the methodology draws upon the labor market policy evaluation literature. Matching on propensity scores, originating in the field of medical statistics (Rosenbaum and Rubin, 1983 and 1984), has recently been applied to evaluation research in economics and is particularly suitable in this application.[4] Our focus on plant closures (with information on the closure process) and the availability of very rich pre-displacement information on individual characteristics, pre-displacement labor market and health status, sector, establishment and region permits us with some degree of confidence to consider that the conditional independence assumption, underpinning propensity score matching, is plausible.

In the next section we explain how all workers displaced in 1987 from *all* plant closures in Sweden are identified in the data. We also describe the development of the macroeconomic environment during the long 13 year follow-up period, showing that it provides an excellent framework for studying the effect of subsequent shocks as the initial displacement occurred when the labor market was good and remained so until, four years later, Sweden experienced a macroeconomic shock unparalleled since the Great Depression. Section 3 describes the matching technique and motivates its use in this context and in section 4 the matched control group is constructed. Section 5 presents the estimates of the impact of job displacement on the probability of being employed, unemployed and out of the labor force from 1987 to 1999. Section 6 concludes.

2 The Swedish labor market and the identification of the displaced workers.

2.1 The Swedish labor market

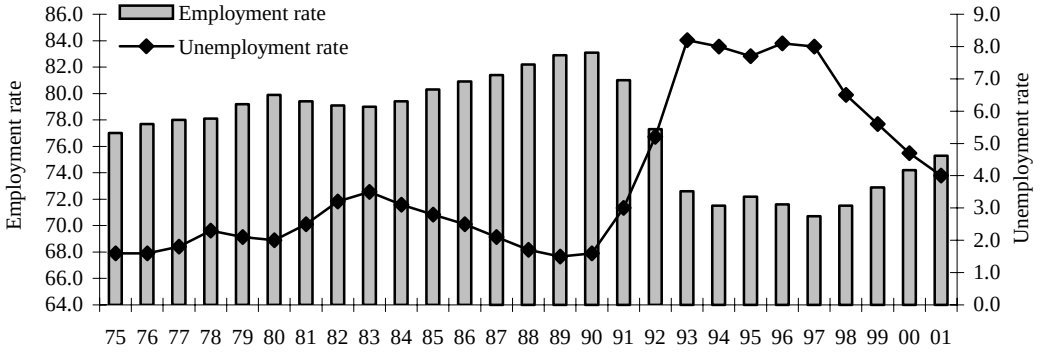
We trace the displaced workers over a very long period, four years before and thirteen years after displacement. As the state of the labor market appears to explain much of the variation of the employment rate differential between the displaced and control group over time, we briefly outline the state labor market between 1983 and 1999.

The initial displacement occurred in 1987. This was during an exceptionally good period in the Swedish labor market. Unemployment had been falling since 1983 and continued down to a low of 1.5 percent in 1989. Employment rose continually up to a peak in 1990. Indeed, by the end of the 1980s, these two measures indicated a more buoyant labor 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 between the first quarters of 1990 and 1994. After 1997 employment rose steadily and by 2001 unemployment had fallen to four percent. Thus, our displaced workers faced a very good labor market for the first four years after displacement with ample time and opportunity to find a new job.

[4] With administrative data, which typically has a large reservoir of control observations upon which to find good matches, matching on propensity scores becomes a feasible method for identifying the costs of displacement. To our knowledge this paper is the first use of matching in the displacement literature.

Then, at the beginning of the 1990s they faced the most severe recession since the 1930s. From 1997 onwards there was a sustained recovery. See Holmlund (2003) for more details.

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



Source: Labour Force Surveys, Statistics Sweden.

2.2 Identification of the closing establishments and displaced workers

The identification of persons who experienced an establishment closure is a vital and rather unique feature of the data and utilizes the possibility in Sweden to link establishment data to employee data. Procedures, developed by Statistics Sweden (Tegsjö, 1995 and Persson, 1999), are able to trace establishment births and deaths back to 1985 and distinguish establishment closures from mergers and dispersals and other ownership changes.[5] In this paper all the closures in 1987 and 1988 of establishments with at least ten employees have been identified. See Appendix A1 for details.

A plant closure is a process over time and while we can identify a closure and when the process ended, we cannot determine when it began. We set the upper limit for the duration of the closure to three years.[6] After careful inspection of each and every closing establishment during the three years prior to closure, we identified the probable duration of the closure process. It was defined to be one, two or three years, based on worker flows and establishment size.[7] While this procedure is hardly perfect, this *flexible* three-year-window is an improvement on the previous studies using administrative data that assume that the closing process begins and ends in the same year, or allow for a two-year process without examining the individual closure processes (i.e., all closures are defined as being of two years). See, for example Bender *et al*. (2002). Using such *rigid* time windows will presumably either over or under classify the displaced workers (Kuhn, 2002).

We identify separations by observing whether the worker was employed at the establishment in November in year *t*, but not in November year *t+1*, where year *t* and *t+1* are within the time window of the closure process. Thus, we cannot be sure that all separations were in fact due to the closure. There are three main possibilities;

[5] See Appendix A1 for details on these procedures.
[6] In Storrie (1993), at the closure of a large Swedish shipyard (i.e. with a long period of production), the closure process, from the public announcement to when the plant was finally closed, was just under three years.
[7] The precise *ad hoc* rules determining the classifications are found in Appendix A1.

voluntary quits unrelated to the closure, pre-emptive quits (i.e. quits due to expectation of closure) and actual displacements (i.e. where notice is served on the termination of the employment contact). The last two types of separations can be seen as direct consequences of the closure and should be included in the study. This is an important issue as there may be some unobserved criteria by both the firm when selecting whom to displace early and self-selection by workers through pre-emptive quits. One could speculate that while those who choose to leave early in the process are those with better labor market prospects than those staying until the bitter end, it is just these workers that the firm may wish to retain until the end of the closure process. Case study evidence in Hamermesh and Pfann (2003) indicate such mechanisms and also underline the complexities of the process through learning.

2.3 Sample retained for analysis

As one of the main issues in this paper is to investigate whether the displaced workers were more severely hit by the deep recession in the early 1990's, we require that both the study and the control group is selected in the same year. Our control group is comprised of a random sample of 200,000 persons employed in November 1986, who did not experience displacement due to a closure or downsizing in an on-going establishment in 1987 or 1988.[8] This requires us to restrict the study group retained for analysis to those employed in 1986 and displaced during the following year. Thus, for closures in 1987, we include those who were displaced at the closing year and, for closures 1988, those displaced a year before final closure.[9]

Table 1 The number of closing establishments and displaced workers.

Year of Closure	Year of separation	Number of displaced workers				Number of establishments
		Age 21-30	Age 31-40	Age 41-50	Total	
1988	1987	348	308	279	935	32
1987	1987	1,389	1,073	1,000	3,462	339
1987-1988	1987	1,737	1,381	1,279	4,397	371

We restrict our sample to individuals aged between 21 and 50 years in 1987.[10] We 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. We also exclude all self-employed workers and we require individuals to be in the sample for the entire observation period 1983-1999. This is not as restrictive as might appear since attrition, due only to death or emigration, is very low. We thereby obtain a balanced panel of 4,397 workers displaced from 371 closing establishments and 115,811 non-displaced workers. See Table 1.

[8] Note that much of the previous literature typically uses a control group that is *never* displaced from the firm. See, for example, Jacobson *et al*. (1993). However, we are somewhat concerned about the selection problems that this may entail, particularly when studying long-term effects.

[9] Thus, no workers displaced during the first year, in a three-year closing process, are included. This is not a serious problem, as very few processes were determined to be three years long, and the corresponding job losers separating were also few. In Eliason and Storrie (2003), we us the full three-year-window when examining the impact of displacement on mortality.

[10] The upper limit keeps our sample of working age (less than 65) for the whole observation period.

2.4 Who are the displaced workers?

All the descriptive variables, presented in Table 2, will subsequently be used in the matching process and are measured between 1983 and 1986.[11] However, the values for 1986 are not used in cases when they may be affected by the impending closure.[12] The descriptives show that displaced workers are slightly younger and more often single than the non-displaced. The displaced were more often unemployed, had lower earnings and with shorter tenure than the non-displaced. They also had a lower level of education, were less likely to have taxable wealth but were more prone to have received welfare benefits. The various measures of pre-displacement health show no systematic differences between the two groups. We see that the displaced were to a much greater extent employed in the private sector.[13] Finally, the displaced workers were employed at establishments with a lower educated work force, a higher share of non-Nordic citizens and with fewer employees.

Table 2 Descriptive statistics for the displaced and non-displaced workers, respectively, in the unmatched sample.

	Mean		
	Displaced	Non-displaced	*t*-stat
Socio-demographic variables (refer to 1986)			
Age	34.17	35.34	9.11
Women	0.50	0.53	3.18
Had children aged 0-6 year	0.22	0.25	4.60
Had children aged 7-17 year	0.32	0.37	6.53
Was married	0.40	0.49	11.99
Immigrant born in other Nordic country	0.07	0.05	-4.96
Immigrant born in non-Nordic country	0.06	0.04	-5.85
Labour market status			
Had non-zero earnings in 1983	0.95	0.97	7.78
Had non-zero earnings in 1984	0.96	0.98	8.23
Had non-zero earnings in 1985	0.98	0.99	6.51
Incidence of insured unemployment in 1983	0.16	0.10	-13.99
Incidence of insured unemployment in 1984	0.14	0.08	-13.14
Incidence of insured unemployment in 1985	0.13	0.07	-15.23
Incidence of insured labour market program in 1983	0.05	0.03	-8.64
Incidence of insured labour market program in 1984	0.05	0.03	-7.57
Incidence of insured labour market program in 1985	0.05	0.02	-10.96
Annual earnings in 1983	61,768	70,156	13.10
Annual earnings in 1984	70,739	79,971	13.51
Annual earnings in 1985	78,684	88,909	14.06
Income from unemployment insurance in 1983	1,772	995	-11.11
Income from unemployment insurance in 1984	1,723	940	-10.99
Income from unemployment insurance in 1985	1,911	930	-13.29
Income from labour market program in 1983	707	450	-4.62
Income from labour market program in 1984	725	445	-4.95
Income from labour market program in 1985	997	423	-9.71
Employed at the same establish in 1985 and 1986	0.54	0.75	30.95

[11] Details on the data sources are in Appendix A2.

[12] For the displaced workers the establishment variables are measured in the year prior to the onset of the closure process. For the non-displaced workers they are measured in 1986.

[13] Note that we observe displacement in the public sector, which amounts for roughly 30 percent of employment in Sweden.

Table 2 ***Continued***

	Mean		
	Displaced	Non-displaced	*t*-stat
Socio-economic variables			
Compulsory school, shorter than 9 years	0.16	0.12	-6.53
Compulsory school, 9 years	0.18	0.14	-7.39
Upper secondary school, shorter than 3 years	0.35	0.35	-0.97
Upper secondary school, 3 years or longer	0.11	0.12	1.96
Tertiary education, shorter than 3 years	0.08	0.12	8.04
Tertiary education, 3 years or longer	0.06	0.11	10.18
Graduate studies	0.00	0.01	3.38
Unknown education	0.06	0.04	-7.48
Owned a house in 1986	0.34	0.43	11.89
Had taxable wealth in 1986	0.44	0.51	9.28
Social assistant receiver in 1983	0.09	0.04	-16.04
Social assistant receiver in 1984	0.09	0.04	-17.08
Social assistant receiver in 1985	0.09	0.04	-16.32
Income from social assistance in 1983	899	290	-15.40
Income from social assistance in 1984	918	324	-14.51
Income from social assistance in 1985	988	341	-14.80
Health variables			
Incidence of hospital admission in 1983	0.05	0.06	1.19
Incidence of hospital admission in 1984	0.06	0.06	-0.68
Incidence of hospital admission in 1985	0.07	0.06	-1.86
Incidence of insured sickness in 1983	0.63	0.66	3.16
Incidence of insured sickness in 1984	0.66	0.67	1.33
Incidence of insured sickness in 1985	0.69	0.71	2.95
Received disability pension in 1986	0.00	0.00	1.13
Number of days admitted to hospital in 1983	0.79	1.00	2.49
Number of days admitted to hospital in 1984	0.80	0.65	-1.71
Number of days admitted to hospital in 1985	0.80	0.60	-2.24
Number of insured sickness days in 1983	14.68	13.40	-2.57
Number of insured sickness days in 1984	16.98	14.78	-4.01
Number of insured sickness days in 1985	19.76	16.72	-5.04
Sector variables (refer to 1986, a few of the smaller sectors are omitted from the table)			
Employed in public sector	0.25	0.46	28.69
Manufacture of food, beverages and tobacco	0.01	0.02	6.54
Textile, wearing apparel and leather industries	0.03	0.01	-14.51
Manufacture of wood and wood products	0.04	0.02	-7.51
Manufacture of paper and paper products	0.02	0.04	4.69
Manufacture of chemicals and chemical products	0.03	0.02	-1.45
Manufacture of non-metallic mineral products	0.01	0.01	1.67
Basic metal industries	0.02	0.02	-1.86
Manufacture of fabricated metal products	0.14	0.14	-0.10
Electricity, gas and heating	0.01	0.01	0.28
Wholesale trade	0.05	0.05	-1.87
Retail trade	0.04	0.05	3.03
Restaurants and hotels	0.11	0.02	-44.17
Transport and storage	0.06	0.05	-5.09
Post and telecommunication	0.02	0.03	5.45
Financial institutions	0.02	0.02	0.45
Real estate and business services	0.10	0.05	-15.17
Public administration and defence	0.03	0.06	7.83
Sewage and refuse disposal, and sanitation	0.04	0.01	-22.12
Education, research and scientific institutes	0.02	0.08	13.88
Medical, dental, welfare and veterinary services	0.13	0.24	17.26

Table 2 ***Continued***

	Mean		
	Displaced	Non-displaced	*t*-stat
Regional variables (refer to 1986)			
Resident in larger city	0.25	0.28	4.96
Resident in a suburban municipality	0.19	0.16	-5.50
Resident in a sparsely populated municipality	0.02	0.02	-0.18
Resident in a countryside municipality	0.03	0.03	-0.27
Resident in other small municipality	0.06	0.06	-1.04
Resident in medium-sized city	0.13	0.15	4.06
Resident in industrial municipality	0.05	0.07	5.42
Resident in big city	0.21	0.16	-7.69
Resident of large municipality	0.07	0.07	0.44
Local average annual income level	93,344	92,651	-5.23
Local employment rate	0.83	0.83	1.30
Local unemployment rate	0.04	0.04	0.45
Establishment variables			
Share of non-Nordic employees	0.06	0.04	-21.19
Share of female employees	0.48	0.51	7.39
Share of employees with short education	0.44	0.34	-33.34
Share of employees with medium-long education	0.42	0.44	8.30
Share of employees with long education	0.14	0.22	25.23
Number of employees	55.76	724.90	29.55

3. Empirical method

Jacobsen *et al* (1993) point out that the main empirical problem in displacement research is akin to that of the evaluation of labor market policy. One can observe the labor market outcome of the displaced workers, but not the outcome for these workers had they not have been displaced (denoted Y^1 and Y^0, respectively). Heckman *et al.* (1999) provides an overview of various methods to identify the treatment effect.[14] Matching on propensity scores is one such method. It originates in medical statistics, (Rosenbaum and Rubin, 1983, 1984) and has received increasing attention in economics. See Heckman *et al*. (1998a,b), Dehejia and Wahba (1999, 2002), Sianesi (2001a), Lechner (2002a,b), Larsson (2003) and Smith and Todd (2004).

3.1 Propensity score matching

By matching one tries to *ex post* mimic the properties of the randomization 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 subsequent labor market outcome can be attributed to displacement. The major difference between randomization and matching is that randomization "works" for all characteristics, observed as well as unobserved. Matching, on the other hand, only works for the set of observed characteristics X. Thus, to identify the treatment effect by matching a conditional independence assumption (CIA) has to be imposed. The CIA requires that $Y^0 \perp D|X$, i.e., that no variables other than X affect both the assignment to treatment D

[14] Here treatment is displacement and the terms displaced and treated will be used interchangeable.

and the outcome Y^0.[15] To identify the average effect of treatment on the treated, the CIA as stated above is unnecessarily strong. It is sufficient that $E[Y^0|D=1,X] = E[Y^0|D=0,X] = E[Y^0|X]$. If this assumption is valid, the unobserved counterfactual outcome of the treated can be estimated from the observed outcome of the matched non-treated.

With only a few covariates, matching directly on these covariates is straightforward, but when the number increases 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. $X \perp D|b(X)$, and if the CIA is valid for X, then the CIA is also valid for $b(X)$. Furthermore, they show that the propensity score, defined as $p(X) \equiv \Pr(D=1|X) = E[D|X]$, is a balancing score. Thus, matching on $p(X)$ corresponds to matching on X, with the advantage that the difficulties of high dimensional matching is eliminated.

3.2 The matching estimator

Smith and Todd (2004) provide an overview of the various methods of matching on propensity scores. We apply a simple nearest-neighbor (one-to-one) matching method, combined with a difference-in-difference (DiD) estimator.[16] With nearest-neighbor matching each displaced worker i is matched to a non-displaced worker j such that

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

The DiD matching estimator is analogous to the standard regression-adjusted difference-in-difference estimator, but does not impose a functional form on the outcome equation. The average effect can be estimated by

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

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,

$$E\left[Y_t^0 - Y_\tau^0 \middle| D=1, p(X) \right] = E\left[Y_t^0 - Y_\tau^0 \middle| D=0, p(X) \right].$$[17]

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

[15] Common support, $P(D=1|X)<1$, is also assumed, implying that a match can be found for each treated individual.

[16] Examples of studies using difference-in-difference estimators combined with matching are Heckman *et al*. (1998a, 1998b) and Smith and Todd (2004).

[17] If pre-displacement values of the outcome variable are included in the conditioning set of X, the identifying assumption is equivalent to the CIA, if the matching is perfect. In this application, pre-displacement values of the outcome variables do not enter the set of conditioning variables directly, but our measure of unemployment and out-of-the labour force does, as linear combination of other variables in the conditioning set. However, we will use this estimator to correct for the impact of any small differences in pre-displacement outcome values, as one never finds fully perfect matches.

DiD matching 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.

3.3 Why propensity score matching, and is the conditional independence assumption valid?

There are two main advantages of propensity score matching, over other non-experimental evaluation methods. Firstly, the treatment effect can be estimated non-parametrically, i.e. no assumptions on functional form on the outcome equation are required. Secondly, it highlights the common support problem, i.e. the problem of lack in overlap in X or $p(X)$ between the treated and non-treated. The problem of common support is often neglected in evaluations based on parametric estimators, but lack of common support, or distributional differences within the common support, may be an important source of bias. One major recommendation in Heckman *et al*. (1999) 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.

It is essential that we are able to convincingly argue that any bias due to selection on unobservables is not an issue in this application, i.e. that the CIA is valid. Here, this means that, experiencing displacement is not affected by any unobservable factors also affecting the post-displacement labor market outcomes. We argue that, due to the nature of the event of plant closure, the extensive set of available pre-displacement characteristics and information on the closure process, the CIA is indeed a plausible assumption.

A plant closure is not a random event but there are reasons to believe that, compared to, for example, cutbacks in on-going establishments it may greatly reduce the selection problem. All employees at the establishment are displaced irrespective of ability, motivation and other such factors that may be difficult to measure. Let us outline the determinants of plant closures and the characteristics of their employees that we are able to capture.

The structural change driving the closure of establishments is 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 the local unemployment level and wage level may also have an impact on the survival probability of establishments (Andersson and Vejsiu, 2001). Moreover, closing establishments are in general small and new. See Harris and Hassaszadeh (2001), Dunne *et al*. (1989) and Anderson and Vejsiu (2001). We have information on conceivably all these factors. See Table 2.

The stock of employees at closure is determined by the probability of becoming and remaining employed at the establishment until closure. It is conceivable that there may be systematic job matching between workers who have a low preference for job security or are less risk-averse and establishments with low survival probability. See Dunne *et al.* (1989) and Winter-Ebmer (2001). These workers may have a low opportunity cost of displacement, due to for example, a weaker attachment to the labor market or work in a sector with high turnover where not only job destruction but also job creation is high. However, even if the preference for job security is not

directly measurable, it is likely that this will be picked up in the employee's previous employment and unemployment history. It is also possible that an indication of lower risk-averseness (and productivity) may show up in medical history. We have information on both the employee's labor market and medical history up to four years prior to the displacement.

However, separations before closure appear to be a more obvious source of unobserved selection. One may have reason to believe that those that remain until the bitter end may either be those who had poorer outside options or showed less initiative in pursuing these options during the closure period compared to those who left earlier. Moreover, the firm's selection of which workers to displace first in a prolonged closure process is hardly random. It may be very difficult to account for the various processes at work in this situation. As these factors will probably be related to the post-displacement labor market outcome, it is important that we are able to also identify early separations in the closing process. Section 2.2 outlined our approach to this issue.

4. Empirical implementation of propensity score matching

The propensity scores are estimated, separately for three age groups (21-30, 31-40, 41-50), with a logit model with the probability of displacement due to establishment closure as dependent variable. 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.[18] This means that consistent modeling of the selection process by including covariates that only determine the selection process is not necessary. In fact, including such covariates could exacerbate the problem of common support (Smith and Todd, 2004). The choice of which interactions and higher-orders terms to include is determined solely by the need to achieve balance in covariate distributions (Dehejia and Wahba, 2002).

After estimation of the propensity scores, a comparison group is constructed by the nearest-neighbor method described in section 3.2.[19] However, we match on the logit of the propensity score instead of directly on the propensity score since matching on the logit should also generate better matches when the probability is close to zero or one (Lechner, 2001). Nearest-neighbor matching can be performed with or without replacement. Allowing each comparison to be matched more than once could improve the matching quality, and hence reduce bias, but will increase the variance especially if a few non-displaced are frequently used. In this application we match with replacement. Table 3 presents the number of individuals in the displacement and non-displacement group, before and after matching. The number of unique matches is in parenthesis. It shows that matching with replacement does not result in a small number of, frequently used, non-displaced workers.

The theory of propensity score matching outlined above assumes perfect matches on $p(X)$, to imply balance in X. In practice perfect matches will not be found for all the

[18] It should be noted, though, that no variables have been excluded from the estimation of the propensity score, on the basis that they are judged to be correlated *only* with the outcome.
[19] The matching procedure is performed in Stata 7.0 using *psmatch.ado.* See Sianesi (2001b).

treated, and so we allow for neighborhood matching. Hence, we apply a balancing criterion of no significant difference, at the 5 percent level, between the groups of displaced and matched non-displaced workers in any of the covariate means. If this criterion is not satisfied, the logit model is re-specified by adding higher-order terms and/or interactions of the covariates. This procedure was repeated until balance in means was achieved for each of the 108 covariates.

Table 3 Sample size before and after matching.

	Unmatched sample	Matched sample
Age 21-30		
Displaced	1,737	1,737 (1,737)
Non-displaced	37,714	1,737 (1,551)
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)

Note: The numbers in parenthesis indicate the number of unique matches

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

We can now examine the difference in labor market status of both the displaced and matched non-displaced workers in terms of employment, unemployment and being out-of-the labor force for those who were between the ages of 21 and 50 in 1986. The average effect of displacement is measured by the difference-in-difference estimator on page 6.[20] The estimates are presented with 99 percent bootstrapped confidence intervals.[21]

Figures 2a-d present the results for employment, measured by paid employment on average at least one hour per week in November, for the three age groups. The results for unemployment, the incidence of insured unemployment from the income registers,[22] is depicted in Figures 3a-d. Figures 4a-d show the results for being out-of-the labor force (i.e. no annual income from earnings or unemployment insurance).

[20] The base year for the difference-in-difference estimator is 1985.

[21] It is common practise to use bootstrapped confidence intervals when using one-to-one matching. See Smith (2000). Here the confidence intervals are based on 1000 replications of the whole matching process. In principle, this requires that we re-specify the logit model at each replication if the criterion of balanced covariates is not satisfied. In practice this is too time consuming. Therefore, we assume that the model specification, obtained for the original sample, is appropriate for all replications. We have, though, checked the balance in each replication, and on average 99 percent of the covariates are balanced. As an informal test of whether this small imbalance may have any impact, confidence intervals have also been computed using only those replications for which the balance criterion was satisfied, but the differences were found to be negligible.

[22] We have reason to believe that insured unemployment covers a very large proportion of unemployment as defined by the labour force survey (ILO definition). Between 1988 and 1992, roughly 70 percent of the ILO defined unemployed received benefit (Björklund, 1996). This is a high figure even in a European perspective (Standing, 2002). 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 and for participants in active labor market programmes.

The presentation of the results for employment and unemployment can be divided into four periods: the immediate effect, the recovery, the impact of the recession in the early 1990s and the long-term effects. The effects on being out-of-the labor force do not exhibit such cycles and are commented separately.

Figure 2a Matched DiD estimates, of the effect of job displacement on the probability of being employed, for those aged 21-50 years (pooled sample).

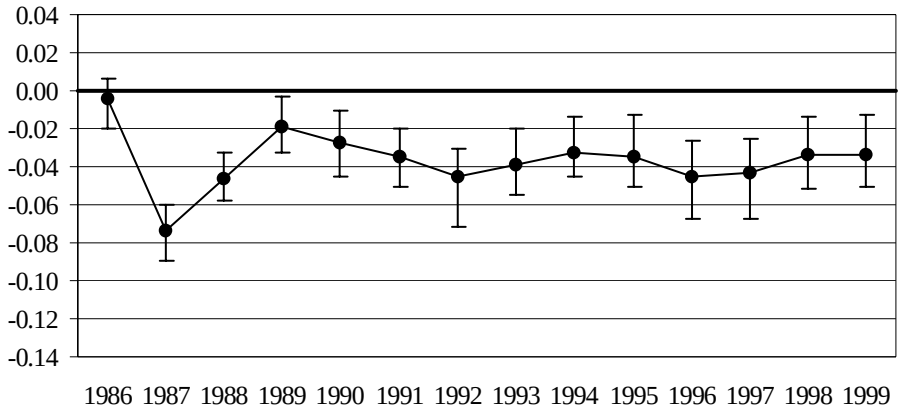


Figure 2b Matched DiD estimates, of the effect of job displacement on the probability of being employed, for those aged 21-30 years.

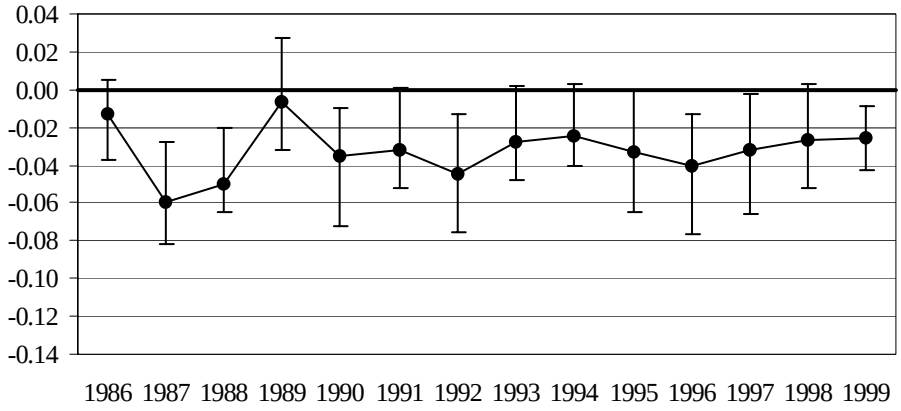


Figure 2c Matched DiD estimates, of the effect of job displacement on the probability of being employed, for those aged 31-40 years.

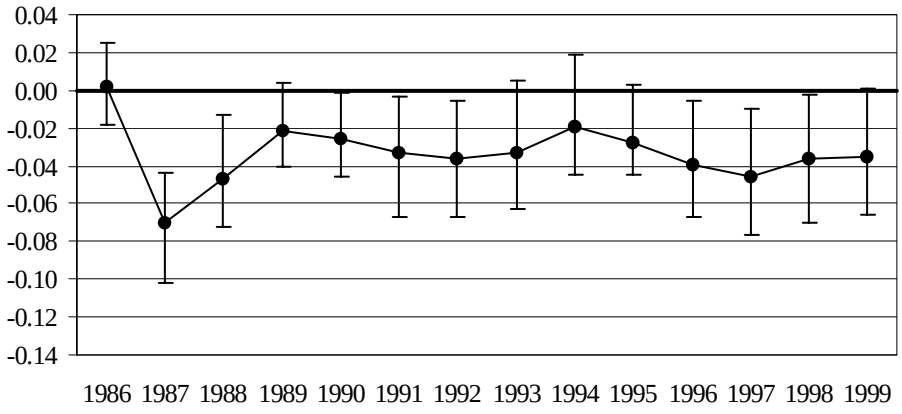


Figure 2d Matched DiD estimates, of the effect of job displacement on the probability of being employed, for those aged 41-50 years.

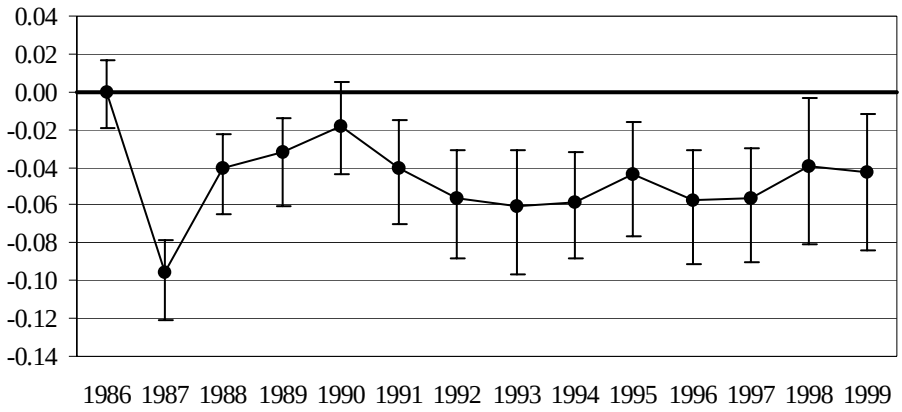


Figure 3a Matched DiD estimates, of the effect of job displacement on the probability of being unemployed, for those aged 21-50 years (pooled sample).

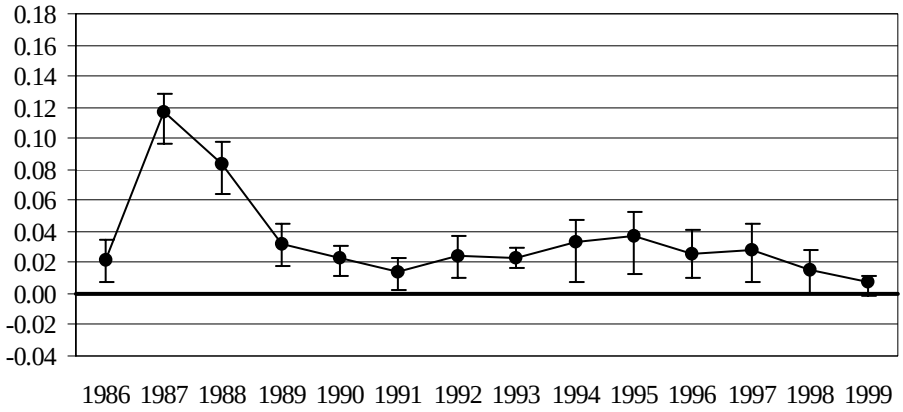


Figure 3b Matched DiD estimates, of the effect of job displacement on the probability of being unemployed, for those aged 21-30 years.

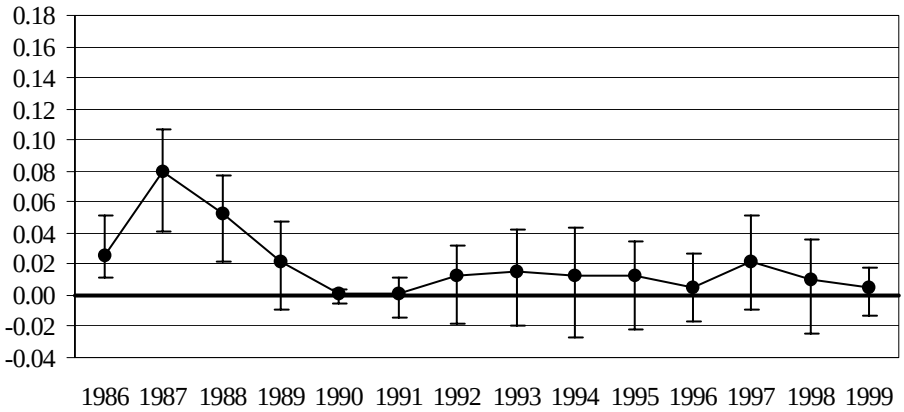


Figure 3c Matched DiD estimates, of the effect of job displacement on the probability of being unemployed, for those aged 31-40 years.

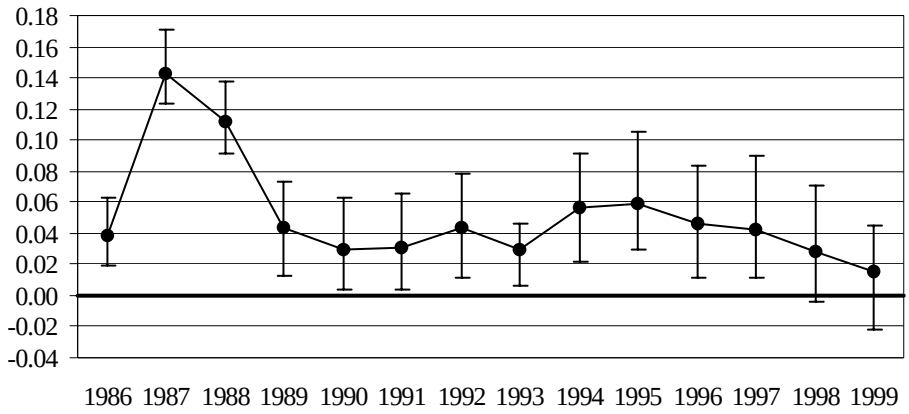
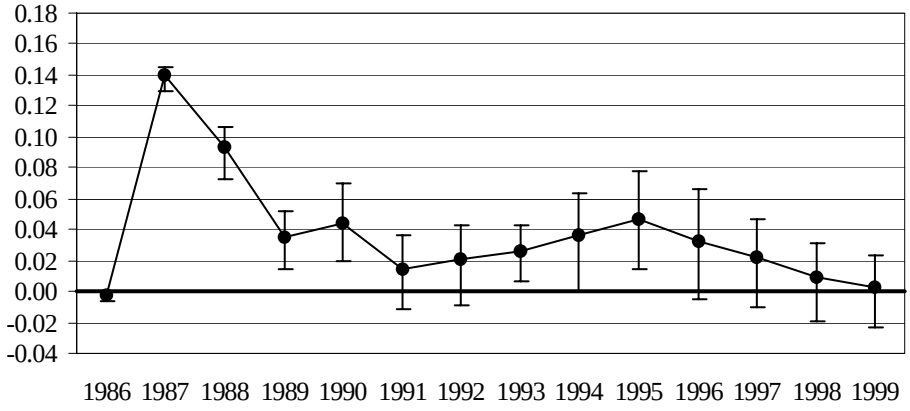


Figure 3d Matched DiD estimates, of the effect of job displacement on the probability of being unemployed, for those aged 41-50 years.



The immediate effect in 1987, the same year as the displacement, is very clear in terms of both employment and unemployment gap between those displaced in 1987 and the matched control.[23] We observe a total employment gap (Figure 2a) of just under 8 percent. The oldest age group exhibited the largest drop in employment, to just under 10 percent. The total unemployment gap (Figure 3a) was nearly 12 percent. Both gaps were appreciably lower for the 21 to 30 year olds.

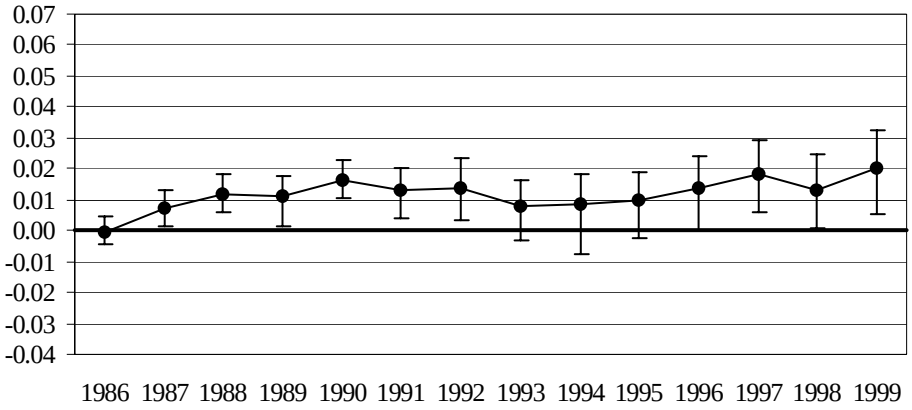
The recovery can be seen by the considerable narrowing of the total employment and unemployment gaps during 1988 and 1989, even if both the total gaps in 1989 are still statistically significant. The youngest age group makes the strongest recovery, as the point estimate of the employment gap is not statistically significant different from zero. The two older age groups recover up to roughly the same employment levels. However, the oldest group recovers more slowly and it is only by 1990 that the gap becomes statistically insignificant. As regards unemployment by age group, we note again a total recovery of the youngest, as the gap is statistically insignificant by 1989. For the older two groups this remains the case up to the end of the recovery period in 1989 or 1990.

[23] By "gap" we mean the average treatment effect on the treated as obtained by the difference-in-difference matching estimator.

The impact of the recession of the early 1990s is clearly revealed in that the previously observed narrowing of the employment and unemployment gaps ceases abruptly in the early 1990s. Indeed, the total employment gap increases again between 1989 and 1992. The widening of the gap is less pronounced for the 21 to 30 year olds but is very pronounced for the oldest group. The recession of the early 1990s hits the youngest two age groups first, between 1989 and 1990, with a larger initial loss for the 21 to 30 year-olds. When, one year later, the gap widens also for the 41 to 50 year-olds, it widens appreciably. We do not observe a widening of the unemployment gap around the time of recession but the previous trend of convergence between the two groups is arrested. While we cannot identify displacement events after 1987, it surely is far-fetched to suggest any other explanation than that the most severe shock in the Swedish labor market in living memory caused higher displacement rates for the previously displaced compared to the matched control group.[24]

There are significant long-term effects. For the entire period of 13 years the non-displaced have a statistically significant lower rate of employment. The largest long-term employment effects are found for the oldest group, followed by the 31 to 40 year olds. It is only after the sustained recovery, beginning in 1997 that the total unemployment gap becomes insignificant. There are no significant long-term effects for the youngest group (since 1990). The largest unemployment effects are found for the 31 to 40 year olds.

Figure 4a Matched DiD estimates, of the effect of job displacement on the probability of being out-of-the labor force, for those aged 21-50 years (pooled sample).



The last labor market state we investigate is being out-of-the labor force. As our definition of this state is quite restrictive, in that it requires no employment or unemployment over a full calendar year, it only includes a small fraction of the sample. For the total group we find that displacement increases the probability to be out of the labor force by 1 to 2 percentage points and is more or less constant during the whole period. As the most common exit routes should be due to sickness and disability it is not surprising that this increase is mainly explained by an increase among the 41 to 50 years old. For this group the gap is significant and increases up to

[24] The efficient separations literature, see for example McLaughlin (1991), views the labels of quits and layoffs to be devoid of economic content. A quit occurs when an upward revision of the wage is rejected by the employer and a layoff when a proposed downward revision of the wage is rejected by the worker. In highly unionised country like Sweden, revision of wages to the extent required to accommodate a severe macroeconomic shock does not occur.

1992 and then remains at a level of around 3 percentage points for the rest of the period. The two younger groups also show a slight increase, but we do not find the same stable results.

Figure 4b Matched DiD estimates, of the effect of job displacement on the probability of being out-of-the labor force, for those aged 21-30 years.

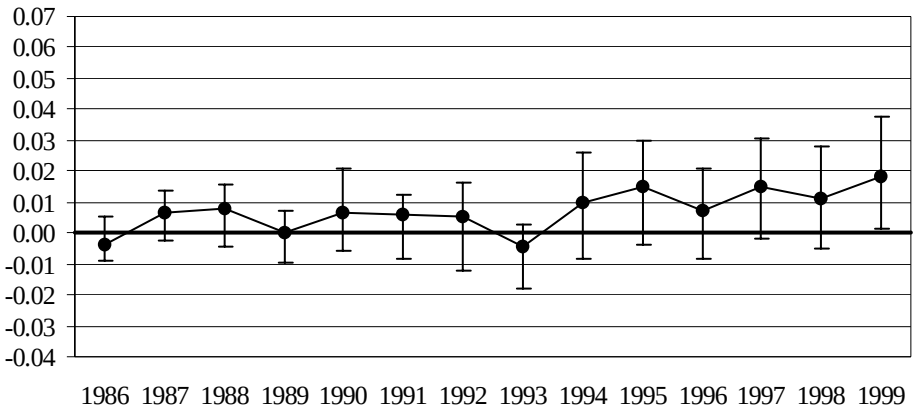


Figure 4c Matched DiD estimates, of the effect of job displacement on the probability of being out-of-the labor force, for those aged 31-40 years.

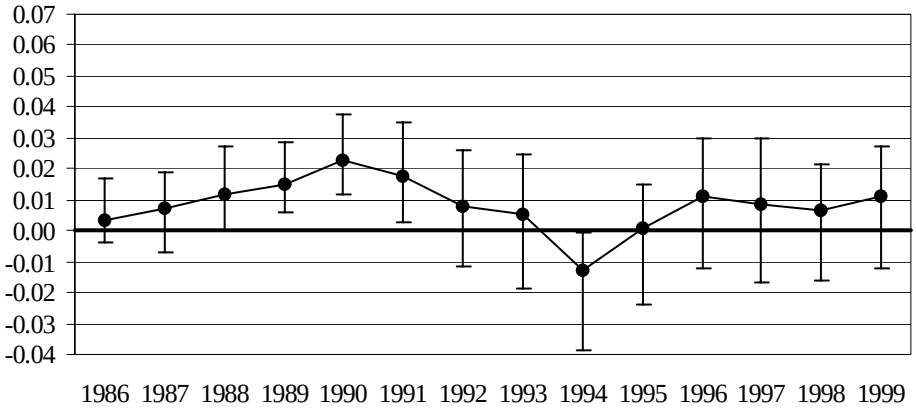
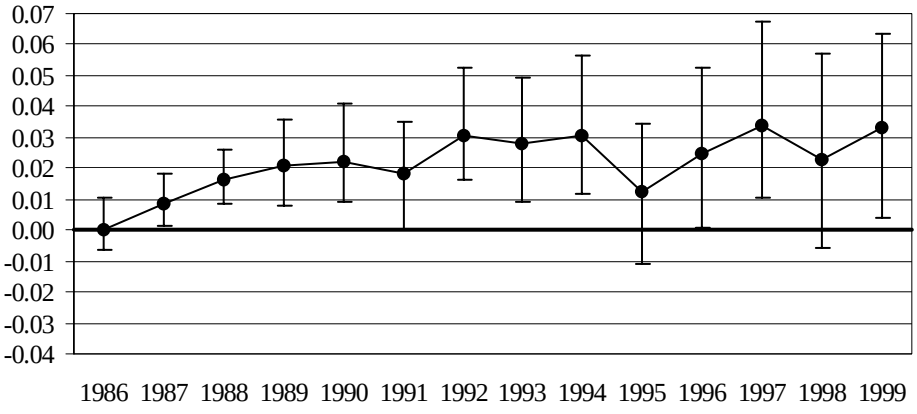


Figure 4d Matched DiD estimates, of the effect of job displacement on the probability of being out-of-the labor force, for those aged 41-50 years.



In summary, we have found that in "good times" displaced workers returns to the same levels of employment and unemployment as non-displaced workers quite

quickly, but that they seem to be more vulnerable to subsequent shocks. This increased vulnerability may cause long-lasting effects. We observe a quite different impact on different age groups. Over the entire period the most negative employment effects are found for the oldest group and smallest negative effects for the youngest group. The youngest group also experiences appreciably less unemployment over the period, with the 31 to 40 year olds exhibiting a slightly stronger impact than the older group. The effect of displacement on being out of the labor force is appreciably higher for the oldest displaced group but very small for the 21 to 30 year olds. These age differences are similar to the short-term effects found in many countries (Kuhn, 2002).[25]

6. Conclusions

We believe that we have been able to remedy many of the weaknesses of previous research on the consequences of displacement. This is primarily due to the possibility to link administrative employer and employee data of sufficiently high quality that is now available, at least, in Sweden and some other Scandinavian countries. From these registers we have identified displaced workers from all closing establishment (with more than 10 employees) in 1987 and 1988 as well as a large random sample of non-displaced workers. We have argued strongly that the possibility to correctly identify all plant closures and the displaced workers (even those leaving before final closure year), and with very extensive information on worker and establishment characteristics (perhaps most importantly pre-displacement labor market status, earnings and health) imply that we may reasonably exclude the possibility of selection bias. Our argument on the validity of the conditional independence assumption together with the very large control group makes the use of matching on propensity scores both valid and feasible.[26] Matching is a very suitable methodology as it permits estimation without the need of any assumptions on the functional form, deals directly with common support, and has attractive distributional implications.

We find lower employment and higher unemployment and inactivity for the displaced workers in both the short and long run. We observed that the initial gaps with the matched control group narrowed appreciably during the following two years. Indeed, by 1989 there was no significant difference in the employment rate for the youngest age group. However, with the advent of severe recession in 1990, the convergence is arrested and the employment rate gap widens again. By the end of our observation period, in 1999, the employment rate of the previously displaced was just under 4 percent lower than in the matched control group. Another significant long run effect was that the higher share of displaced workers, in the oldest age group, who left the labor force exhibited no sign of diminishing.

The evidence suggests that the observed initial convergence of employment rates was arrested and then reversed due to the very severe macroeconomic shock that hit the labor market in 1991. The most obvious difference between the two groups by this time was that the previously displaced held relatively new jobs. “New jobs end early”

[25] We note, however, that Jacobson *et al.* (1993) finds little difference for different age groups on earnings in the long term.

[26] In addition one should underline the negligible attrition rate (due to death or emigration only) in our administrative data. Attrition can be a very serious problem in studies of long-term effects,

is one of the stylized facts of the job stability literature (Farber, 1999a). Comparisons of the three age groups provide further support for our short tenure explanation. The difference in tenure between the study and control group, in the early 1990s, is presumably greatest for the oldest age group and we find an appreciably more negative labor market outcome for the older displaced workers.[27]

We cannot precisely specify what short tenure could signify in this context. The most obvious explanation is that it is a measure of job specific capital of value to the firm. It is comprised of firm specific human capital in the original sense of the word (Becker, 1962 and Parsons, 1972), and thus related to tenure. Long tenure may also reveal, through selection, a high quality match, as in the turnover model of Jovanovic (1979). In the efficient separations literature, (McLaughlin, 1991) displacement occurs when a negative demand or productivity shock is greater than the firm's evaluation of specific job capital and so displacement will be more prevalent among low relative to high tenure employees. This interpretation of the longer-term consequences of displacement is a neat extension of the theory of the shorter term costs as laid out in Hamermesh (1987) and relates the long-term adjustment process directly to the initial displacement event. The short-run costs are due to the loss of specific capital on the initially displaced job and adjustment takes time due to the exposure of the subsequent short tenure jobs to further adverse shocks.

However, the Swedish institutional context provides a powerful alternative explanation of the impact of short tenure. Last-in first-out (LIFO) seniority rules are stipulated in statutory law when displacing labor for economic reasons. However, the firm and the trade unions may negotiate derogations on seniority. While some case studies show that in practice seniority rules are often avoided, see for example Calleman (2000), there is no systematic evidence.

Another explanation could be that the displaced workers are matched with jobs with a high destruction propensity. Farber (1999b) shows that job losers are more likely than non-job losers to be in temporary employment arrangements. He also provides evidence that this is part of an adjustment process subsequent to job loss leading to regular full-time employment.

Regardless of which of these explanations is correct, the vulnerability of recently displaced workers to recurrent displacement provides us with a further conclusion; namely that post-displacement labor market status, even in the longer term, is extremely sensitive to the state of the labor market. This business cycle sensitivity feature should be borne in mind when comparing outcomes in different countries and time periods. Arulampalam *et al*. (2001) states that "a successful macroeconomic framework, with sustained high levels of employment, is the first-best attack on scarring at the aggregate level." Our results suggest, though, that even several years of good macroeconomic conditions are not enough to prevent the scarring effect at the *individual* level.

[27] Note that we state only that the older displaced workers had a worse outcome relative to the control group. This does not mean that older workers had worse times in the nineties than young and in fact the Labor Force Surveys show that employment rates for the young fell more than for the other two age groups used in this paper.

Our interpretation of the mechanism behind the longer-term effects of displacement are somewhat different to some of the mechanisms referred to in some of the recent scarring literature (see, for example, Arulampalam, 2000 and Gregg, 2001). There, it is the experience of unemployment through, for example, deterioration of human capital or as a signal of low productivity, that is understood to have left a long-term scar.[28] Given the relatively small initial unemployment effects and the rapid recovery found here, this does not appear to be a plausible explanation. The "new jobs end early" explanation is directly related to the initial displacement and may occur with or without an intervening period of unemployment.

The policy implications of these explanations of the long-term consequences of displacement are very different. The stigmatization or the deterioration of general human capital explanations leads one to focus on any policy that may find new jobs for the unemployed. At the micro level it implies active labor market policy measures such as matching services or training. As the "new jobs end early" explanation follows from the initial displacement *per se* the policy options here would appear to be very limited indeed. Few economists would call for a higher level of employment protection. Perhaps the most obvious labor law policy option would be to weaken the LIFO rules. However, this would presumably only shift the burden of subsequent displacement to others. Arulampalam *et al*. (2000) suggests though that employment services focus more on finding high quality matches rather than quickly finding a new job, might be a successful policy option.

Would similar results be found if, data permitting, one were to do this study in other times or institutional settings? The period studied was rather special in that the closures occurred in a very buoyant labor market which soon turned to the most severe recession in living memory. This must be a major factor behind the relatively minor initial effects, the rapid convergence and the negative impact in the longer term. While the sharply contrasting states of the labor market studied in this paper do serve to highlight the vulnerability of recently displaced to subsequent shocks, it will surely be the case that smaller long-term effects will be found in less turbulent times. Regarding institutions, at first glance the principle of statutory LIFO rules in Sweden may appear to be rather unique. However, as pointed out above, the unions may bargain away the LIFO principle and while there is no systematic evidence of the prevalence of such practices some case study evidence suggests that LIFO rules are often circumvented in Sweden. Moreover, while LIFO rules are seldom the sole criteria in statutory law in other European countries, they are often one such criteria (for example, in Germany) and they are common in collective agreements (Gerard, 2003). They also constitute a strong social norm in many countries. See Rousseau and Anton (1991) for evidence from the US.

On the other hand there are several features of our data and research design that may lead to our results being weaker than in much of the previous research. Gibbons and Katz (1991) argue and present empirical evidence for a better labor market outcome for workers displaced by plant closures compared to cutbacks in ongoing establishments, due to the negative signal of being selected for displacement. While some other US evidence does not support this result, recent evidence from Norway, (Huttunen *et al*. 2003) does. Moreover, administrative data will always capture some

[28] Note also that the employment effects are longer lasting than the unemployment effects.

who did not separate due to the impending closure, though presumably our exclusive focus on closures is helpful in this respect. Note also that we allow our control group to be displaced at any time after 1988 and presumable some of the matched control group were subsequently also displaced, especially since we have conditioned on some establishment characteristics. Neither have we constrained our displaced sample to long tenured workers, i.e. those who would be expected to be the main losers of displacement. Finally, we study displacement in all sectors of the economy and much previous research has been confined to the private sector and often only manufacturing.

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Appendix: Data sources

A1. Identification of establishment closures and definitions of length (time-window) of closing process

Identification of establishment closures

Statistics Sweden's Business Register (*Företagsdatabasen*) 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. 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.

Definition of the length of the closure process

The definition of the length of the closure process is based on the size of the establishment and its worker flows. The investigation of the worker flows is possible by the linking of employees to their establishment in the Register Based Labour Market Statistics (*Registerbaserad arbetsmarknadsstatistik*). This linkage is feasible because of the obligatory income statements, filed to the taxation authorities by the employer, which contain both the employee's civic registration number and the establishment's identity number. The precise definitions of the length of the closure process are given below.

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, of 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.

A2. The matching and outcome variables

The variables included in the matching process, briefly defined in Table 2, and the outcome variables, defined in the main text, are collected from four principal registers. Taxation and the administration of the universal Swedish welfare state provide the basis for practically all the variables and the registers cover every individual and every firm in the country. The principal registers, The Register Based Labour Market Statistics, The Income and Wealth Register (*Inkomst- och förmögenhetsstatistiken*), The Longitudinal Register of Education and Labour Market Statistics (LOUISE), and The Hospital Discharge Register (*Patientregistret*), are created by compiling data from several other registers.[29] Below give a brief account of the original data sources for the variables in this paper.

The Population and Housing Censuses (*Registret över totalbefolkningen*) contains basic demographic information on age, sex, marital status, number and age of children, and country of birth. The county of residence is also contained in this register and we have used a regional classification of counties (based on demography, urbanisation and employment structure) as defined by the Swedish Association of Local Authorities for 1985.

All data pertaining to income, labor market status and wealth is compiled in The Income and Wealth Register. This register is based on tax returns and income statements (*kontrolluppgifter*). Employers are obliged to file annual income statements, for each employee, which are used by the taxation authorities for individual income taxation. Moreover, since practically all transfers in the Swedish welfare state, such as disability pensions, and sickness and unemployment benefits, are liable to tax the National Social Insurance Board also files income statements on such transfers. The outcome variable "employment" is also derived from this source by Statistics Sweden by examining the income statement pertaining to November. Although, social assistance is not liable to tax this information has been gathered from a separate register, the Social Assistance Register *(Registret över ekonomiskt bistånd),* and included in the Income and Wealth register.[30]

The data on education are from The Register of Educational Attainment of the Population (*Registret över befolkningens utbildning*), which draws its information from several sources. These include the Population and Housing Censuses, the Higher Education Register (*Högskoleregistret*), the National Labour Market Board (AMS), and the National Board of Student Aid (CSN), and are updated annually.

The health data is extracted from two sources. The number of insured sickness days is taken from registers supplied by the National Social Insurance Board (RFV). In addition we have the number of days spent in hospital from The Hospital Discharge Register to which we have had full access. The County Councils are required to report all hospital visits (including date of admission and discharge, diagnosis, surgical procedures, etc) to the The Epidemiological Centre at the National Board of Health and Welfare that compile the register.

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[29] We thank the Unit for Register Based Labor Market Statistics, Statistics Sweden, and in particular Björn Tegsjö and Jan Andersson for the full access to this wealth of information.

[30] Since 1994 the National Board of Health and Welfare (*Socialstyrelsen*) has the principal responsibility of the social Assistance Register.

The regional labor market data was made available to us by The Institute for Labour Market Policy Evaluation (IFAU).[31] The regional income and employment data is a regional aggregation of the individual based taxable income data. The regional unemployment data is based on registered unemployment from the local labor market authorities.

The size of the establishments, the industrial classification and the variables on work force composition by sex and national background are taken from The Register Based Labor Market Statistics, i.e., the same register that identifies the displaced workers.

[31] We thank Anders Forslund for providing this material.