The pecuniary and non-pecuniary costs of job displacement. The risky job of getting back to work *

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Abstract

This paper investigates the pecuniary and non-pecuniary costs of involuntary job loss by focusing on both post-displacement earnings losses and injury rates. To this end we employ a unique dataset. Administrative data from Italy describing individual work histories have been merged with individual data on workplace injuries. Propensity score matching techniques are employed to measure the causal effect of displacement on workplace injury rates. We find that in a period marked by tight labour market, re-employed displaced workers experience only moderate and short-lived earnings losses but are about 70 percent more likely to be injured at their subsequent jobs compared to the control group of non-displaced workers. These results suggest that re-employed displaced workers may trade pecuniary job attributes for non-pecuniary ones.

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1 Introduction

A vast literature has investigated the costs of involuntary job displacement - defined as job loss due to a firm's closure or downsizing - in such forms as post-displacement earnings losses, unemployment spells and human capital depreciation (see, among earlier contributions, Hamermesh, 1987 and Jacobson et al., 1993). However, additional important non-pecuniary job attributes exist that may be affected by displacement and have not yet received much attention in the literature. This paper aims to analyze the consequences of job displacement in terms of such attributes, namely job safety. In particular, we investigate whether and to what extent job displacement affects workers' safety by comparing displaced workers' outcomes in terms of workplace injuries (and other proxies for injury risk) with those of a control group of similar non-displaced workers.

An assessment of the effect of involuntary job loss on work-related injuries is important for several reasons. First, according to the theory of compensating differentials and equalizing differences (Brown, 1980) a complete evaluation of individual wealth should embody both the earnings from and non-pecuniary aspects of one's job. Many studies, especially those on wage premia for risks (for a comprehensive survey see Viscusi and Aldy, 2003), consider a job as being characterized by its monetary aspects (i.e. salary) and by other amenities, such as job safety measures provided by an employing firm. The simultaneity in the choice of the preferred combination of salary and injury risk implies that an expected worsening of working conditions after displacement should lead to a lower salary, a greater risk of injury or, most likely, both. To the extent that displaced workers are re-employed in other jobs with similar wages but higher (lower) job-related risks, a welfare analysis conducted exclusively on salaries would understate (overstate) the total loss for the displaced workers. Moreover, as emphasized by many studies, workers' pre-displacement characteristics have significant effects on post-displacement outcomes (Fallick, 1996, Kletzer, 1998).

Therefore, to evaluate the treatment effect of displacement, the treated (displaced) group has to be comparable with a control group (non-displaced) with respect to any relevant attribute of a job, including work-related injury risks. Therefore, taking into account pre-displacement workplace risk as an additional control variable allows us to refine the "conditional independence assumption" (CIA) on which identification strategy is based.[1]

Second, higher post-displacement injury rates might lead to substantial welfare losses and health costs through increase in the number of days of work lost or due to the payment of disability pensions. An additional long-run effect might be observed if serious injuries permanently reduce workers' production capacities.

The effect of job displacement and unemployment on health has been investigated in many studies. A first stream of this literature aims at understanding the negative effect of unemployment or job loss on health in the form of a higher incidence of stress-related and psychological diseases (Carr-Hill et al., 1996, Field and Briggs, 2001, Iversen and Sabroe, 1989, Keefe et al., 2002, Jin et al., 1995). These studies report that unemployed or displaced workers make more use of drugs and public health-care services (such as consultation of physician and hospitalization). A second body of literature assesses the long-run effect of job displacement on mortality rates for displaced workers (Eliason and Storrie, 2004, Morris et al., 1994, Moser et al., 1987, Sullivan and Wachter, 2009).

Two recent studies are particularly related to our work. Rege et al. (2009) investigate the consequences of downsizing on the probability of applying for and receiving a disability pension due to a reduction in "work capacity". As will be explained later, our approach differs substantially from that of Rege et al. (2009). In their study, disability pensions are granted due to illness, mental

[1]See section 3 for the definition of the CIA assumption.

disorders, injuries or defect. Because the decision to apply for a disability pension largely depends on workers' evaluation of the alternative opportunities available to them, the authors focus their analysis on the effect of displacement on this decision. In our study, the available data on job-related injuries allows us to analyze the direct impact of displacement on injury rates, by making the reasonable assumption that workers do not voluntarily become injured. A second study (Kuhn et al., 2009) analyzes the effect of plant closure on the taking-up of health provisions and on the utilization of sickness benefits by displaced workers comparing them to a control group of non-displaced workers. The authors report an increase in health costs for displaced workers, which is mainly caused by an increase in the amount of sickness benefits paid. This increase is explained by the fact that for unemployed workers, sickness benefits are greater than unemployment benefits. As a result, the authors find a small effect on number of days of sick leave among displaced workers. To our knowledge, however, no empirical work to date has examined the consequences for re-employed displaced workers in terms of workplace injuries.

To evaluate the effects of displacement, in this paper, we analyze post-displacement earnings and job safety using a unique dataset for the period of 1994-2002 that combines work histories from the Italian administrative "Work Histories Italian Panel" (WHIP) database and individual work-related injury data from the Italian Workers' Compensation Authority (INAIL). We focus on involuntary job losses of workers with at least three years of tenure to limit potential heterogeneity problems and self-selection issues. We restrict our analysis to workers displaced in 1997 due to firm closures. This strategy allows us to observe workers three years before displacement, thereby enabling the construction of reliable pre-displacement working histories and thus allowing us to match displaced workers with comparable controls (also in terms of injury rates). It also leaves a five-year interval in which to evaluate the consequences of the job loss. A longer time period after displacement allows us to more precisely reconstruct workers' career histories [2] and provides a reasonably sufficient window to observe rare injury episodes. To estimate the causal effect of displacement on earnings and on the subsequent risk of being injured in the workplace we combine industry-specific propensity score matching techniques with a Differences-in-Differences estimator (DID).

We find that, in a period marked by tight labour market, re-employed displaced workers in Italy experience only moderate and short-lived earnings losses, but that, as a consequence of displacement, they are about 70 percent more likely to be injured at their subsequent jobs than a control group of non-displaced workers. Moreover, this effect on job safety is not transitory and does not diminish in magnitude as time passes.These results seem to suggest that re-employed displaced workers, to avoid unemployment or earnings losses, trade-off pecuniary job attributes for non-pecuniary ones, even during a period of positive labour market performance.

The remainder of the paper is organized as follows. Section 2 provides an overview of the problem of multidimensionality in the evaluation of post-displacement outcomes. The identification strategy and econometric methodology are discussed in section 3. Section 4 describes the data in greater detail and provides some descriptive evidence. Estimation results are presented and discussed in section 5. Section 6 concludes.

2 Heterogeneity and multidimensional displacement outcomes

The study of the relationship between earnings and workplace risk is not new in the literature. Implicit market theory (Rosen, 1974) shows that the analysis of the relationship between salaries and

[2] An accurate reconstruction of career histories enables the tracking of movements of workers across different firms and increases the likelihood of detecting false firms' deaths. For a discussion of this phenomenon see section 4.

risk is complex, as these two job attributes are jointly determined in equilibrium with heterogenous agents on both the demand and supply sides of the labour market. Hamermesh (1999) jointly analyzed the trends in earnings and in workplace risk inequalities. In this section we borrow from his theoretical framework.

Although workers performing more hazardous jobs should be compensated with higher salaries, heterogeneity in employees' characteristics and in particular the inability to observe their productivity results in a negative correlation between injury rates and earnings (Brown, 1980, Garen, 1988, Hamermesh, 1999, Hwang et al., 1992). Therefore, if safety is a normal good, an income effect leads to workers with higher potential earnings choosing safer jobs.

Figure 1 illustrates this phenomenon. Panel (a) shows firms' isoprofit curves (Π_i)[3] and two types of workers with the same preferences over the wage-injury risk bundles (i.e., utility curves U_i) and different earnings potentials (i.e., intercepts ξ_i, due to human capital differences, other rents or match-specific determinants) that face the same trade-off between wage and injury risk (i.e., all isoprofit curves have the same slope). A type-A worker has higher potential earnings than a type-B individual (i.e., **a** higher intercept, $\xi_A > \xi_B$). Isoprofit curves are upward sloping; that is, firms offer higher salaries at increasing levels of risk. If job-safety is a normal good, because workers have the same preferences and are confronted with the same trade-off, a type A worker will choose a safer job than a type B worker (with respective injury risks equal to $I_A < I_B$), due to an income effect.

Combinations of salaries and injury risks in panel (a) of figure 1 represent the pre-displacement working conditions. To compare changes in job characteristics after displacement, displaced workers need to be matched to non-displaced individuals with similar observable working conditions and

[3]For simplicity's sake isoprofit curves are drawn as straight lines, although their slopes should be decreasing as **the** injury rate increases.

characteristics. Panel (a) shows that individuals with jobs described by point C are not good control subjects for individuals of type B, as they have lower skills or lower earnings potential ($\xi_C < \xi_B$). Therefore, comparing workers exclusively in terms of their wages can be misleading as similar wages could hide different earnings potentials. Therefore, it is important to choose appropriate controls in terms of both observed wages and injury rates. More generally, choosing controls only in terms of observable pre-displacement characteristics and standard labour market outcomes may not be sufficient to grasp important "non-ignorable" unobservables.

Let us now assume that appropriate controls were assigned to displaced workers of type B. Panel (b) of figure 1 displays a possible outcome for the displaced worker, B_{d2},relative to a non-displaced worker, B_{ND}. If displaced individuals experience a loss of earnings potential ($\xi_{Bd2} < \xi_{BND}$), for example, due to a loss of firm/industry-specific human capital (or other kind of rents), and, as a consequence, are re-employed in jobs on a lower isoprofit curve $\Pi_{B'}$, comparing their wages with a those of non-displaced individual B_{ND} could be misleading. Such an analysis would estimate a zero-welfare loss when comparing the earnings of B_{d2} to B_{ND}, ignoring the higher injury risk of the former.

The higher risk of injury compensates for the loss of earnings potential. Therefore, ideally, to correctly evaluate the impact of displacement, we need to take into account all possible labor market outcomes before and after displacement and to compare workers with similar observed and unobserved characteristics. This task is complex because, as shown by Rosen (1974), job attributes are determined in equilibrium and depend on the heterogeneity of individuals' preferences (e.g., their taste for risk) and heterogeneity on the labor demand side (e.g., the slope of an isoprofit curve indicating how firms reward risky jobs). The industry-specific propensity score-DID procedure described in the next section is aimed to reduce the problem of finding a proper counterfactual by assigning to each treated (displaced) individual an appropriate control (non-displaced) individual. By choosing controls through a sector-specific propensity score-matching procedure that takes into account any available non-ignorable job, firm and demographic characteristics, we hope to have enough coordinates to construct a credible counterfactual. Importantly, workers with analogous pre-displacement job histories (in terms of standard and non-pecuniary labor market outcomes) who work in similar firms belonging to the same industry are also likely to face similar remuneration-injury risk trade-offs and, thus, to be comparable in terms of their preferences for risk. In other words, imposing exact matching on sector while considering demographic, firm and job characteristics should deal simultaneously with the heterogeneity of individuals' preferences and with heterogeneity on the labor demand side. In turn, this accurate multidimensional strategy to build counterfactuals should reduce potential biases related to non-ignorable unobservables. Nevertheless, we will also complement this matching procedure with a DID estimator that further differences away any remaining individual unobserved characteristics that are fixed over time.

3 Identification Strategy and Estimators

As Jacobson et al. (1993) have pointed out, the main empirical problem when studying the effects of displacement is equivalent to that in the program-evaluation literature. One can observe the labour market outcome of the displaced workers (i.e., program participants) but not the outcomes for these workers had they not been displaced (i.e., not participated in the program).

Indeed, the goal of our analysis is to identify the average effect of displacement on the displaced workers with respect to various labour market outcomes. In the evaluation literature, this effect is known as the average treatment effect on the treated (ATT), which is simply a special case of the general notion of average partial effects computed for the treated part of the population

(Wooldridge, 2002). Let us define as D_i as a variable taking the value 1 if a worker has been displaced (i.e., the individual is exposed to the treatment) and 0 if he has not been displaced. Each individual has two potential outcomes: $Y_i(D_i = 1)$, in the case of treatment and $Y_i(D_i = 0)$ in the case of no treatment. The problem is that one is not able to observe both outcomes for the same individual, that is to directly compute $E(Y_i(0)|D_i = 1)$. It is possible to directly compute only $E(Y_i(0)|D_i = 0)$ and $E(Y_i(1)|D_i = 1)$.

Following this literature, our identification strategy is based on the conditional independence assumption (CIA). This assumption states that, conditional on workers' pre-treatment characteristics[4], the potential outcome in the non-treatment scenario is independent of the treatment status. In particular, expressions for the mean potential outcomes conditional on covariates are functions of participation status, observed outcomes, and covariates only: $E(Y_i(0)|D_i = 1, X) = E(Y_i(0)|D_i = 0, X)$.[5] Indeed, even if a plant closure can be seen as an exogenous shock at the plant level because all workers at the closing firm have to leave (irrespective of their ability, motivation and other characteristics that are unobserved by the researcher), it may still not constitute a natural experiment as: a) the structural change driving the closure of establishments is over-represented in certain sectors and regions of the economy; b) there could be systematic job matching between workers who have a low preference for job safety or are, in general, less risk-averse and establishments with low survival probability; c) the characteristics of the workers could be in principle one of the causes of the firms' closure; and d) some workers leave the firm before it closes down. More generally, the group of displaced workers cannot be expected to be a random sample in terms of

[4] These pre-treatment characteristics must be strictly exogenous, that is, it is assumed that they are not affected by the treatment, either ex-post or in anticipation of the treatment. The CIA will hold if these characteristics include all of the variables that affect both the selection into treatment (e.g., workers' displacement) and the outcomes of interest (e.g., earnings).

[5] It would be strictly sufficient to assume mean independence to recover the ATT. However, it is very difficult to credibly justify the validity of the stricter assumption but not of the more general one (see Imbens, 2004).

non-ignorable (observable and unobservable) characteristics. Therefore, our conditioning set X, see table A, is sufficiently rich and takes into account many important non-ignorable job, firm and demographic characteristics.

Different econometric techniques have been developed in observational studies to overcome the biases generated when computing the ATT based on the CIA. All available parametric, semiparametric, and nonparametric estimators are (implicitly or explicitly) based on the assumption that one can recover the counterfactual for every treated individual by taking into account all factors that jointly influence selection and outcomes. In this study, we employ propensity score matching estimators (PSM) (Rosenbaum and Rubin, 1983) to produce such comparisons. An advantage of these estimators is that they are semiparametric and thus allow for arbitrary individual-effect heterogeneity.[6] The aim of the propensity score-matching, and of matching estimators in general (Heckman et al., 1997), is first to reduce elements of the bias that are due to the non-overlapping support of treated and control subjects' characteristics (i.e., to avoid comparing workers who are already different in the pre-treatment period) and, second, the component that is due to misweighting on the common support of such characteristics (in fact, even in the common support, the distribution of the treated and of the untreated could be different). Therefore, the traditional econometric selection bias that stems from the "selection on unobservables" is assumed to be absent, that is, the matching method is based on the assumption of conditional independence (CIA).

Rosenbaum and Rubin (1983) showed that if potential non-treatment outcomes are independent of treatment status conditional on the covariates X , they are also independent conditional on a balancing score $b(X)$, and the propensity score, $P(X) = Pr(D = 1|X)$, constitutes one possible balancing score. This finding is important for solving the "curse of dimensionality" problem and

[6] For the difference between multivariate OLS and matching see, for example, Angrist and Krueger (1999).

to identify the ATT by using the propensity score even when, as in our case, many pre-treatment continuos variables have to be taken into account to build a credible counterfactual. Rosenbaum and Rubin (1983) have also stated the second assumption needed to identify the ATT under the CIA, the so-called "overlap" assumption: the support of the conditional distribution of X given $D = 0$, overlaps completely with that of the conditional distribution of X given $D = 1$. In practice, researchers assess this last assumption by comparing the descriptive statistics between the treated and the control groups and/or by inspecting the distribution of the propensity score for treated and control groups. At a minimum, matching can be used as a method for improving and checking the overlap in distributions of covariates (Rubin, 2006, Imbens and Woolridge, 2009).

We augment the robustness of the matching estimator by taking advantage of the panel structure of the data and by implementing a propensity score matching-difference-in-difference estimator (PSM-DID) (Heckman et al., 1997, Smith and Todd, 2005). Indeed, if the point-wise bias due to "selection on unobservables" $B(X)$ is constant over time, that is unobserved heterogeneity is fixed in time, we have:

$$B^{post}(X) - B^{pre}(X) = 0$$

Then, a typical PSM-DID estimator takes the form:

$$ATT^{PSM-DID} = \frac{1}{n_1} \sum_{i \in \{D_i=1\}} \left[(Y_{i,\ post} - Y_{i,\ pre}) - \sum_{j \in \{D_j=1\}} (w_{i,j}) \cdot (Y_{j,\ post} - Y_{j,\ pre}) \right]$$

where $w(i, j)$ is the weight placed on the jth observation in constructing the counterfactual for the ith treated observation, and n_1 is the number of treated observations. Matching estimators differ in the ways in which they construct the weights $w(i, j)$. To build the counterfactual in the non-treatment scenario for displaced workers, we have experimented with many alternative matching algorithms (nearest neighbor(s), Caliper, Radius, Kernel and Local Linear weights). In finite samples (with a high ratio of treated to untreated individuals and/or a limited overlap in the covariates' distributions), the choice of matching algorithm can be important (Heckman et al. (1997), Busso et al., 2009). Therefore, the performance of various estimators depends on the data structure in question. When there is overlap in the distribution of covariates between the comparison and treatment groups, matching algorithms should give similar results (Dehejia and Wahba, 2002). In this paper, we present only the results from Nearest Neighbour matching (NN) with replacement routine[7], given that the results for the other estimators are qualitatively equivalent.[8]

In addition, to estimate the average effect of job displacement on those who are displaced, we combine this PSM-DID strategy with exact covariate matching. We opted to exactly match on industry variable (i.e. to compare treated workers only with those non-treated workers who belong to the same industry) and to estimate a propensity score for each industry separately. According to the theory of matching, the independent variables that one should use in estimating the propensity score, i.e. the Xs, are all factors that affect both the selection into treatment (e.g., the displacement) and the outcomes under study (e.g., earnings, weeks worked, job safety). From our point of view, the importance of the determinants of job displacement that are correlated with the outcomes under scrutiny vary considerably among different sectors. This motivates our decision to devote special attention to the sectorial dimension. Besides, as is explained in the previous paragraph, imposing exact matching on sector is important to deal simultaneously with heterogeneity of individuals' preferences and with heterogeneity on the labour demand side. Although exact matching on all variables may be preferable, it is not feasible in our case due to the large number of continuous

[7] On the one hand (as is argued, for example, by Caliendo and Kopeinig, 2008 and Dehejia and Wahba, 2002), NN matching with replacement, by picking the closest control in terms of the estimated propensity score, favours bias reduction with respect to variance reduction (compared to other variants of NN matching and to other weighting schemes). Busso et al. (2009) explicitly investigate the finite sample properties of the most popular matching estimators and find that Nearest Neighbor Matching with replacement achieves the best performance in terms of bias reduction. On the other hand, if the closest neighbour is far away, NN matching faces the risk of bypassing the problem of the common support. This drawback can be avoided by imposing a tolerance level on the propensity score distance (e.g., a caliper). As shown in section 4.2, this problem seems not to be present in our case.

[8] Results are available upon request.

variables involved in the analysis. As discussed in Dehejia (2005), there is no reason to believe that the same specification of the propensity score will balance the covariates in different samples. In our case, we consider workers belonging to different sectors as belonging to different samples.

Our general specification of the propensity score can be represented as follows:

$P(Displacement_{i,\ 1997}) = \Phi\left\{h(WC_{i,1994}; FC_{i,1994}, H_{i,1994-1996})\right\}$

where $\Phi()$ is the normal cumulative distribution function. To free up the functional form of the propensity score we include higher-order polynomials and interaction terms, and search for a specification that balances the pre-treatment covariates between the treatment and the control groups conditional on the estimated propensity score (see section 4.2).

The variables used in the estimation of the propensity score are summarized in Table A.

TABLE A: Variables used in the propensity score estimation.

Variables	
$WC_{i,1994}$ = Workers' and job characteristics	Gender, age, tenure, log of aggregate annual earnings, aggregate annual weeks worked, main job function, number of employment relationships held in a year, region of birth, region of work.
$FC_{i,1994}$ = Firm characteristics	industrial sector, number of employees
$H_{i,1994-1996}$ = variables computed over the 1994-1996 period	number of injuries, number of years with a registered episode of sickness leave, number of serious injuries, number of episodes of "*Cassa integrazione*"

The set of variables $WC_{i,1994}$ and $FC_{i,1994}$ are computed for 1994, that is , three years before displacement. The set of variables $H_{i,1994-1996}$ is computed for the period three years before displacement, that is 1994, 1995 and 1996. If anticipation effects in the years preceding displacement are present, these variables will not completely satisfy strict exogeneity and, as a result, the CIA assumption might not hold. However, we have chosen to include these years, as episodes of injury, sickness absences and "Cassa Integrazione"[9] are rare events that proxy for job-safety, health status

[9] The "Cassa Integrazione" is a subsidy that is granted to manufacturing workers employed in firms in bad economic

and firm characteristics, respectively. The choice of a larger time-window for these covariates is aimed at smoothing them.[10]

4 Data and Descriptive Evidence

For our analysis we have merged the Work Histories Italian Panel (WHIP) dataset [11] and the administrative records from the Italian Workers' Compensation Authority (INAIL) for the period of 1994-2002. The resulting dataset provides a random sample of workers employed in the private sector of the Italian economy. It includes data on the beginning and ending dates and on the duration (number of weeks) of each employment relationship.[12] The WHIP files also provide information on workers' characteristics (age, sex, place of birth, place of work, type of occupation, maternity leaves, sick leaves), standard labor market outcomes (the number of weeks worked in a year and annual earnings) and characteristics of the firms at which the individuals in the dataset are employed (number of employees, firms' birth and death dates, sector).[13] The WHIP dataset contains a dummy variable that indicates whether the worker has been on sick leave lasting at least one week in a given year. The INAIL dataset contains the number of injuries and the duration of injury-related leaves at the employer-employee level in the private sector. It records all injuries leading to a leave of more than three days. Less serious injuries are not reported. In addition, this

situations, one that guarantees a wage replacement rate of 80%. It is a selective measure, in the sense that only firms of a certain size belonging to certain sectors are eligible.

[10] As a robustness check, we have repeated the empirical analysis with these variables at their 1994 values. The results were qualitatively the same.

[11] WHIP is a database of individual work histories, based on INPS administrative archives: http://www.laboratoriorevelli.it/whip/whip_datahouse.php?lingua=eng&pagina=home

[12] However, it is not possible to consistently recover the quarterly or monthly temporal pattern of earnings or weeks in employment as for each employment relationship we only observe the annual number of weeks in employment and annual earnings without additional information on their temporal distributions.

[13] The structure of the panel is such that we can observe the main characteristics of both employees and firms, but we cannot observe all employees belonging to a single firm. Therefore, we only observe the characteristics of a firm to the extent that some workers present in our sample are employed by it.

dataset also identifies serious injuries that lead to a permanent damage to an employee's health. Note that this last variable is highly correlated with the number of days lost due to injury-related leaves.

We retained in our sample full-time workers who had at least three years of tenure at their main job 1997 (i.e., the job with the highest yearly earnings). This choice was made for the following reasons. First, in this way we ensure comparability with other international studies. Second, tenured workers are also likely to experience greater losses from job-displacement than untenured workers as they may have accumulated firm -(or sector-) specific human capital and/or represent particularly good matches. Internal labor markets (policies for promotion from within) and incentive pay mechanisms are two other sources of earnings losses whose impacts increase with tenure. Moreover, we retain workers with at least three years of tenure because our identification of the effects of displacement is mainly based on the possibility of controlling for pre-treatment employees' and employers' characteristics. As is standard in the job displacement literature, we excluded the construction sector from our sample due to the high seasonality of these jobs. Also, the energy sector is left out due to its extremely low number of treated individuals (only two).

The main drawback of the WHIP dataset is that workers recorded as non-employed in the private sector could have found other jobs via self-employment, retired or ended up in the shadow economy. This is common in studies that use administrative data. For example, Jacobson et al. (1993) faced a similar problem when using administrative data on Pennsylvanian workers. To solve this problem, they decided to restrict their sample to workers with positive earnings during all years, and, as a consequence, they discarded about 40% of high-tenured displaced workers. In this paper, we follow this approach, which results in elimination of about 48% of the displaced workers in the sample. As a robustness check, we repeated the estimation procedure for the unbalanced sample (by also including workers who re-enter the private sector after 1997) and found qualitatively identical results.[14] Indeed, our estimates should be interpreted more conservatively as the effect of displacement on re-employed displaced workers. If the displaced workers are more likely to end up in the underground economy, where less attention is devoted to workers' job safety, our estimated losses constitute a lower-bound estimate of the true effects.

4.1 Definition and Identification of Closing Establishments and Displaced Workers

The aim of this work is to study the effects of job displacement by comparing the labor market outcomes of displaced workers with those of a control group of non-displaced workers. In particular, our treated group consists of workers who have been laid-off due to firm-closures. The following events are categorized as displacements related to firm-closures:

- all cases of workers' mobility accompanied by a registered closure of the reference firm;
- all cases of mobility associated with the absence of a workforce at the end of the reference year in the reference firm;
- separations from closing firms during the two years preceding firm-closure (pre-closing separators).

Data from WHIP include an indicator when a firm ceases its activity: a potentially closed firm is identified by the disappearance of a firm's identity number from tax returns. However, this variable often refers to administrative death (e.g., merges and/or legal transformations) and not

[14]Approximately 21% of displaced workers never re-enter the private sector. These results are available upon request.

to economic death (for a similar issue see Bender et al., 1999, and Kuhn, 2002). Workers in firms that are closing down only from an administrative point of view might become reemployed during subsequent years in the same firm or in entities that are somehow related to the former employer. To solve this problem, in addition to the procedure adopted for use with the WHIP dataset by Contini et al. (2009),we developed an algorithm to detect false deaths, which utilizes information on the connections between employers and employees for all available years. We identify the links between firms and employees by tracking down all possible connections between workers, firms and job relationships – all three of which have distinct identification number - in the years preceding and following 1997. An employer-employee relationship, that is interrupted by a firm's closure but is then followed by re-employment in a firm connected to the previous employer by any of the above-mentioned links is thus excluded from the sample of displacement events. Wrongly classifying non-displaced employees as treated individuals would lead to an under-estimation of the effects of displacement. To eliminate, or at least reduce, this bias, we exclude from the group of treated workers those individuals who, in spite of being "displaced" according to the WHIP firm demography variables, maintained the same employment relationship.

For the purpose of our study, it is important to exclude other cases of mass-layoffs (from both the treated and control groups) and to also include in the treated group pre-closing separators. Indeed, one can argue, as is common practice in the literature, that displacement approximates a "natural experiment" at the firm level as long as one is willing to assume that the firm-level processes behind layoffs are not determined by employers' or employees' decisions that are based on non-ignorable workers' characteristics. In fact, it is possible that selection effects are at work. On one hand, during mass layoffs (those that are not followed by firm closures) employers could select the "worse" workers to be laid-off and retain the "better" ones (Gibbons and Katz, 1991). On the other hand, if workers anticipate the future closure of their firm, another process of selection could take place: workers may try to find another job and separations registered in the years before firm-closure may thus constitute preemptive resignations (Pfann and Hamermesh, 2001). Therefore, those workers who succeed in this search process may tend to have comparatively "better" labour market characteristics (for example, they could simply have better job-searching abilities or labor market connections) than those remaining until the "bitter end", and thus, they will be comparatively less affected by the closure of the firm. However, in practice, we do not know if all pre-closure separators left their firms for a reason connected with the impending closure as the only information we have is the evolution of the number of employees in the firm during the years preceding the closure. Nevertheless, the empirical results of the paper are not sensitive to different definitions of pre-closing separators.[15] Therefore, for simplicity's sake, in our baseline specification we include pre-closing separators , that is, workers who left their firms within the two years immediately preceding the closure, in the treatment group.

In the analysis below, we will compare workers displaced in 1997 to a control group of workers who did not experience a mass layoff or a firm-closure (or a pre-closure separation) during 1997 or in the following years. The control group should represent the hypothetical (and unobserved) outcomes of the same displaced workers had they not experience an involuntary job loss, without additionally ruling out a job change. Thus, our control group also includes those employees whose separations were not related to mass-layoffs or firm closures. However, it is important to point out that among these movers there could also be workers who were laid-off on an individual basis, and whom we cannot take into account due to the administrative nature of the data. The inclusion

[15] We tried different definitions of pre-closing separators by enlarging the window to three years before closure and by restricting it to only one pre-closing year. Moreover, conditioning on the firm-level evolution of the number of employees (e.g., categorizing as a pre-closing separator a worker who leaves his firm in the year preceding its closure if and only if during this year there was a net reduction in the number of employees at the firm) leaves the main empirical results unaffected.

of employees who do not voluntarily separate in the non-displaced group would cause an underestimation of the effects of displacement. In practice, the main results of the paper do not change if we only include stayers in the control group.

4.2 Descriptive Statistics, Assessment of the Common Support and Propensity Score Estimation

As mentioned above, the aim of estimating the effect of displacement by matching is to choose a counterfactual group that is as similar to the treated group as possible (in terms of its non-ignorable characteristics) by properly selecting and reweighting control individuals. Several techniques are proposed in the literature to check the quality of the matching procedure according to the property that if $P(X)$ is the propensity score, then pre-treatment variables must balance given the propensity score, that is $D \perp X|P(X)$ (Rosembaum and Rubin, 1983). To test the effectiveness of our matching routine in balancing the covariates we first implement a balancing test proposed by Dehejia and Wahba, 2002, and Becker and Ichino, 2002.[16] We split the sample into intervals such that the average propensity scores for the treated and the control groups do not differ in each interval. Then, within each interval, we verify that the means of each characteristic do not differ between the treated and control groups. We verify that the balancing property is satisfied for every specification of the propensity score (and therefore for each sector separately). This procedure is thus also useful for determining which interactions and higher-order terms to include in the specification of the estimated propensity score (given a selected set of covariates X). Additionally, we perform a standard t-test for equality of means of the covariates to check whether significant differences

[16] We used the program written by Becker and Ichino (2002).

remain after matching on the propensity score and we show the standardized bias[17] before and after matching. The latter check is done by pooling all sectors together.

Table 1 reports the sample size before matching and different related post-matching statistics. The first column of Table 1 displays the number of observations by industry and in the economy as a whole before matching. Our aggregate sample is made up of 31,212 workers. In column 2, we show the ratio of the number of displaced workers to the number of controls. It is apparent that for every treated worker, we have a large pool of potential controls, even within each sector, which is an important pre-requisite to meaningful implementation of our matching strategy. Column 3 displays the percentage of treated individuals retained in the econometric analysis. As explained in paragraph 3, the overlap assumption is fundamental for the identification of the ATT. Our sector-specific propensity score matching strategy excludes from the treated group (and from the control group) those individuals who possess characteristics that perfectly predict success (or failure) in the sector-specific propensity score estimation. As a consequence, only 4% of displaced workers are disregarded. The representativeness of the treated sample used in the matching analysis is also supported by the fact that the means of the pre-treatment covariates for the treated sample remain practically unchanged (see Table 2).[18] Note that we do not additionally implement other trimming procedures (such as that proposed by Smith and Todd, 2005) given that, as shown in the remaining of the paragraph, the lack of overlap does not seem to represent a big issue in this sample, as our matching routine substantially improves the comparability of the two groups of workers (see table

[17]The standardized bias is the difference of the sample means in the treated and non-treated (full or matched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (formula from Rosenbaum and Rubin, 1985).

[18]In the presence of a homogeneous treatment effect discarding treated observations does not imply a redefinition of the estimand; rather, it simply indicates a loss in terms of efficiency. Instead, the identification of the ATT fails in the case of treatment effect heterogeneity, in particular when such heterogeneity occurs in the parts of the support where the treated are dropped. Therefore, our statement in the main text is based on the assumption that individual observable characteristics are the main cause of heterogeneity in treatment.

2). Finally, column 4 of Table 1 shows the average weights assigned to the matched observations. Given that NN matching with replacement selects for each treated individual the control subject with the most comparable propensity score, an average weight equal to one means that no control observation has been used more than one time and suggests that we have a sufficiently rich reservoir of controls. In our sample, this value equals 1.1; in fact, 92% of treated individuals were matched with a control that was not resampled, and only two controls were used three times as a match.[19]

Table 2 presents statistics for the unmatched and matched samples (U and M, respectively) during the period of 1994-1996. Column 1 shows the means of the lagged covariates for the treated group. Column 2 displays the means of the lagged covariates for the control group. The standardized bias is reported in column 3, while column 4 shows the p-values for the test of equality of means of the lagged covariates between the treated and the control workers. As can be seen from Table 2, the displaced workers are younger and less tenured than non-displaced individuals; they have lower earnings; they work fewer weeks per year; and they are more likely to have multiple jobs. Moreover, among the treated group, the percentages of women and blue-collar workers are larger. Regarding the geographical spread, the concentration of displaced workers is relatively lower in the central regions. These results are consistent with empirical evidence from other countries (Kuhn et al. (2002), Fallick (1996) and Kletzer (1998)). Finally, firms with displaced workers are overrepresented in the textile, apparel, leather and commerce industries (see Table 1) and are of relatively smaller size. No pre-treatment differences were detected with respect to injuries, sickness and Cassa Integrazione-related variables. Imbens and Woolridge (2009) suggest focusing on the standardized bias rather than on t-statistics.[20] In particular, as a rule of thumb when a standardized

[19] We also find that the median difference between the propensity score of the treated individuals and that of the matched controls is 0.0000193; its 95-th percentile is .0007671. These are very low values compared to the estimated probability of displacement.

[20] The reason is that t-statistics increase with sample size. However, simply increasing the sample size does not

bias is greater than 35, global linear regression methods are very sensitive to the specification and are not advisable. In our unmatched sample, the value of the standardized bias is very high for many important covariates (in the cases of tenure and earnings it is around 50). However, once we apply the matching routine described above, the majority of the above-mentioned differences are reduced or disappear.

Although in four cases (age, dummies for being born and working in the south, dummy for being born outside OECD), the t-test rejects the hypothesis of equal means we believe that this is a minor issue, as the values of the standardized bias are substantially reduced and that these differences are not profound.[21] As a robustness check, we also estimated the weighted regressions for the matched sample of workers (where the weights were those employed in the matching analysis).[22] Matching quality is then increased by exploiting the fact that these weighted regressions have the so-called double-robustness property (Rotnitzky and Robins, 1995, Lechner and Wunsch, 2009, Imbens and Woolridge, 2009, Busso et al., 2009). This property implies that the estimator remains consistent when either the matching is based on a correctly specified selection model or the regression model is correctly specified. To check the robustness of our matching procedure we applied this methodology to the linear DID estimator by regressing the difference between the post-treatment and the pre-treatment outcomes on a constant, the treatment dummy and other covariates used in the propensity score estimation.[23] Our main results remained robust to this alternative methodology.

make the ATT inference less problematic. Instead, the standardized bias is not systematically affected by the sample size. The authors refer to the "normalized difference" (ND), that is a transformation of the standardized bias: $ND = SB * (\sqrt{0.5}/100)$.

[21] The differences of the means of these variables between treated individuals and controls are not significantly different from zero inside each block of the estimated sector-specific propensity scores. The fact that at the aggregate level these differences become significant is an example of a situation in which increasing the sample size increases the value of the t-statistics but not the value of the differences. In other words, the denominator of the t-statistics decreases.

[22] As is shown in Busso et al. (2009), all propensity score matching estimators can be practically implemented as a weighted regression of the outcomes on a costant and a dummy indicating the treatment status.

[23] In the context of a linear DID estimator based on panel data, Imbens and Woolridge (2009) suggest adding

Finally, it is also useful to examine the density functions of the propensity scores for the treated group and the matched controls to develop a sense of the overlap between them. Figure 2 confirms that propensity score matching increased the comparability between the two groups. While prior to matching, the estimated kernel densities were quite different, after matching very similar values can be observed.

5 Econometric results and discussion

In this section, we investigate whether and to what extent the displaced workers suffer after displacement in terms of earnings, weeks worked, sick leave and measures of injury risk. To this end, we first employ the simple unweighted OLS estimator and the propensity score matching technique focusing on the post-1997 levels of the dependent variables. We then extend the standard propensity score analysis by using a PSM-DID strategy, which is our preferred estimator, and compare it with a linear unconditional DID estimator.[24] Our dependent variables are the logarithm of annual earnings, the number of weeks worked, the probability of being injured, the number of injuries, the number of out-of-work days because of injuries and the probability of absences due to sickness.

Tables 3 and 4 show the results from the two methods for the logarithm of annual earnings and the number of weeks worked. For the sake of comparability with other dependent variables (see

the pre-treatment outcomes as additional control variables . In their words (p. 70) "making treated and control units more comparable on lagged outcomes cannot make the causal interpretation less credible" as suggested by the standard DID assumptions (i.e., the treatment indicator may be correlated with the residual). Clearly, if the values of the lagged dependent variables are very similar for the treated and the control groups, the standard DID estimator and this augmented DID estimator will yield similar results. We experimented with various specifications in terms of the regressors included and flexibility of the functional form. For example, we first introduced a fourth-degree polynomial in age interacted with geographical dummies. Then, we regressed on all of the variables used in the propensity score estimation. The results of these various specifications were very similar, while the precision of the ATT-estimates improved.

[24] As an additional robustness check, we employed a mixed method that combines PSM and a linear conditional DID estimator. As explained above, this last empirical method is a weighted regressions (with the NN-matching weights) of the difference in outcomes on the treatment status and other controls. Results are available upon request.

below), we have computed the logarithms of the sum of annual earnings and the sum of annual weeks worked for the following periods: the year of displacement (year 0), the entire post-displacement period (years 1,2,3,4,5), the "short-run" period (years 1,2,3) and the longer-run period (years 4 and 5).[25] As expected and consistent with the existing literature, the estimates in Table 3 show that displaced workers experienced significant earnings loss during the year of displacement. This loss is evident when looking both at the unadjusted mean comparison which considers the entire sample, and at the estimation results of propensity score matching. The latter method suggests that the earnings loss equals 12 percent in the year of displacement and 5 percent during the five years after displacement (years 1,2,3,4,5). During the first three years after displacement, displaced workers experienced an earnings loss of 7 percent. This negative effect faded away thereafter. Estimates from the propensity score matching difference-in-difference model shown in table 4 display significant earnings losses in the year of displacement. Estimated coefficients are negative but not significant in the first three years after displacement and also in the fourth and fifth years. As can be seen from tables 3 and 4, unsurprisingly, there is a significant reduction in the number of weeks worked for the displaced workers in the three years after displacement, which becomes less relevant in the subsequent years. The small magnitude of earnings losses is probably due in part to the fact that we have selected individuals with at least three years of tenure, while other studies focus on more experienced workers. Moreover, previous studies (e.g., Eliason and Storrie, 2006 who use a PSM estimator and Jacobson et al. (1993)) have shown that earnings losses are sensitive to the business cycle, even in the long run. Eliason and Storrie (2006) associated this sensitivity to business cycle to the fact that displaced workers, holding relatively short-tenured jobs and therefore a relatively low level of human capital, are more likely to experience additional episodes of displacement because

[25] Coefficients estimated on a yearly basis are available upon request.

their skills are less valuable to the employer. This explanation, in turn, is based on the contribution by Stevens (1997), who found that displaced workers who incur additional job separations have substantially greater earnings losses. An alternative interpretation of this phenomenon relates the higher propensity of displaced workers to hold several short-lived jobs to the fact that transitions from job to job tend to be relatively longer during periods of recession (Hall, 1995). Holmlund and Storrie (2002) found that transitions from temporary jobs increased rapidly at the beginning of a recession. In fact, during the period under analysis the performance of the Italian labour market was improving.[26] The unemployment rate remained practically stable at around 11.3 % in the period of 1994-1998 and then declined monotonically to 8.7 percent in 2002.[27] Overall, the evidence from this study seems to be consistent with these conjectures.[28]

The novel and most interesting contributions of this paper are, however, the results for job safety. We have at our disposal three proxies for risk that the two groups of workers face at their workplaces: the probability of being injured, the number of injuries reported and the number of out-of-work days because of injuries. Injuries at the workplace are rare events; therefore, to smooth these outcomes, we consider three time windows: the entire post-displacement period (years 0,1,2,3,4,5), the first four years after displacement (years 0,1,2,3; the "short run") and the subsequent two years (4,5; the "*longer* run"). However, these measures of job risk are limited dependent variables and count variables, whose analysis is meaningful only if the control and treated groups have the same lengths of exposure to risk. Moreover, as we have just observed, the displaced workers tend to work fewer weeks than the control group. Therefore, all of the-above mentioned injury measures are normalized

[26] In 1997, the reform of the Italian labor market introduced flexibility at the margin.

[27] The employment to population ratio and the labor force participation rate had symmetrically opposite temporal patterns. They were relatively stable in the period **of** 1994-1998, at 42.2% and 47.5%, respectively, and then **increased** monotonically **to** 44.3% and 48.5% in 2002.

[28] Serti (2008) estimates positive and significant earning losses for Italian workers which were displaced in a period of recession. He employs the standard Jacobson et al. (1993) econometric model.

by the total number of weeks worked in the respective reference periods to account for the different lengths of exposure to risk. In short, the logic behind these measures of job-safety is as follows. An injury is a rare event and increasing the size of the window of observation increases the quality of the proxy. Then, a normalized variable is needed, as in the post-displacement period displaced individuals work less than non-displaced individuals.

Table 5 presents the results for the probability of being injured and the number of injuries in the post-displacement period, estimated by a linear regression and the nearest neighbour propensity score matching. The difference in the probability of being injured between the displaced and non-displaced workers is positive and highly significant in all years after displacement (year 0 included). The PSM estimated effect is equal to 0.087, implying a 72 percent increase in the workplace risk after displacement. The results for the normalized measure are qualitatively identical, and the estimated effect is equal to 0.0004, implying a 100 percent greater probability of being injured during subsequent employment relative to the control group. These positive and significant effects are also present in the fourth and fifth years after displacement for the non-normalized and normalized measures and are equal to 0.052 and 0.0006, respectively, suggesting that the effect of displacement on job-safety is relatively long-lasting. The results from the simple linear regression are very similar, although the losses are somewhat smaller. These findings are confirmed by the estimates obtained from the PSM-DID procedure (see Table 6). In this procedure, we implement PSM-DID only for the normalized variables for the following reasons. Because the outcomes of interest are computed over periods of different lengths before and after displacement, and because exposure to risk varies considerably with the number of weeks worked, we divide our dependent variables by the number of weeks worked. The pre-displacement normalized variables are computed over the three years before displacement. Once again, the results of the linear unconditional DID are very similar, although the intensity of the displacement effect is in some cases slightly lower.

The strong positive effect for the entire post-displacement period is also found for the total number of injuries after displacement and for the total number of days lost due to injuries, both non-normalized and normalized (Table 5 and Table 7, respectively). The estimated effect for the former non-normalized outcome is equal to 0.106, implying a 69 percent differential in the number of injuries, while the effect for the non-normalized days lost is equal to 2.86 and suggests a 89 percent increase in the number of days lost due to injuries. These findings are also confirmed by the estimates of the normalized variables which suggest a 100% increase in the number of injuries per week and a 116% increase in the number of days on injury leave per working week. Moreover, we again check the robustness of these results by employing a propensity score matching-difference-in-difference procedure. As can be seen from Tables 6 and 8, also in this case the displaced workers in the post-displacement period (years 0,1,2,3,4,5) face a significant increase in the number of injuries per week and out-of-work days per week after displacement, relative to the non-displaced workers (the estimated coefficients are equal to 0.0005 and 0.015 , respectively). In addition, the estimated coefficients on the fourth and fifth years show a positive and significant effect of displacement on the number of injuries and on the days lost because of injury, suggesting that the effect of displacement on job safety is relatively constant over time. Finally, it is interesting to note that a significant effect in terms of sickness absences emerges only during the first three years after displacement (see Table 9 and 10).

Overall, we found strong evidence of negative non-pecuniary effects of job displacement for the displaced workers. In particular, we have documented that the negative effect of displacement on job safety is robust to different outcome measures (and estimation techniques) and is not diminishing over time. These results, together with the modest losses in terms of earnings and weeks worked and the positive aggregate labor market trends, seem to suggest that re-employed displaced workers, to avoid unemployment or earnings losses, trade away pecuniary job attributes for non-pecuniary ones even during a period of positive labour market performance..

Workers can give up job safety by working at more hazardous jobs and/or by accepting job-instability, that is, several temporary and short-lived jobs that may be available in a period of economic expansion.[29] Indeed, as figure 3 shows, the monthly injury hazard rate[30] initially increases and reaches its peak three months after the beginning of a new job, and decreases thereafter and becomes relatively flat after the 20th month. In an additional exercise (see table 11), we also find evidence that the effect of job displacement on the number of new jobs begun by a worker (a proxy for job instability) is notably high during the short run (0,1,2,3), but this effect drammatically decreases during the last post-displacement years . We interpret these results as indicative of the relationship between more risky jobs and reductions in job safety.

6 Conclusion

This paper has analyzed an important dimension of the costs of job loss that has not yet received much attention in the literature, namely its effect on job-related injuries. It complements previous studies that have investigated the effects of job displacement in terms of standard labour market outcomes. We argue that, to provide a comprehensive picture of the effects of job displacement and to conduct a complete welfare analysis, it is crucial to also incorporate the non-pecuniary aspects of working conditions into the study.

We find that, in a period with a tight labor market, re-employed displaced workers in Italy

[29] All displaced workers **who** we consider in the analysis are eligible recipients of unemployment insurance.
[30] Monthly hazard rates for all observed job-relationships.

experience only moderate and short-lived earnings losses, but, as a consequence of displacement, they are also about 70 percent more likely to be injured while working at subsequent jobs compared to the control group of non-displaced workers. In addition, this effect on job safety is not transitory and does not simply depend on the fact that displaced workers pass through many temporary jobs and experience high injury hazard rates at the beginning of every new job. Considering that the effect of displacement on job instability is decreasing in time and that the effect on injuries is persistent over time, we argue that the effect of displacement on workplace injuries must be mainly ascribed to transitions to more hazardous jobs rather than to a mere duration effect. These results suggest that re-employed displaced workers may trade away pecuniary losses for non-pecuniary ones to reduce unemployment spells or avoid larger earnings losses, even during a period of positive labour market performance.

Our work is in line with and complements previous studies that have documented higher long-run mortality rates among displaced workers (Elliason and Storrie, 2009, Moser et al., 1987. Morris et al. 1994, Sullivan and Von Wachter, 2009) and those who claim to have observed a business-cycle sensitivity of earning losses (Eliason and Storrie, 2006, Jacobson et al., 1993).

Our results call for more attention to be devoted to policies designed to re-integrate displaced workers into the labor market. In particular, our results imply that labor market policies should also be concerned with job quality, particularly with job safety. On one hand, finding a new job could rapidly minimize losses in terms of human capital depreciation for the displaced workers and could reduce the use of unemployment benefits. On the other hand, a lower job safety level may imply other individual and social costs. The short-run and long-run costs of re-employment at a more hazardous job might outweigh the savings in terms of unemployment benefits and human capital depreciation. Therefore the reemployment of displaced individuals could be accompanied by training programs such as on-the job training that are aimed at reducing the risk of injuries through the development of specific safety-training methods.

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Table 1: Composition of the sample by industry

Industries	N. of obs. before matching	% ratio of treat/contr. before matching	% of matched treated	Av. weight of matched controls
Food, Beverages and Tobacco	1188	0.8	100.0	1.1
Textile, Apparel and Leather	2690	3.6	100.0	1.1
Wood, Paper, Printing and Publishing	1493	1.3	100.0	1.0
Cook, Chemical, Rubber and Plastic	2045	0.6	100.0	1.0
Non-metallic minerals, Metal and metallic products	4350	1.4	98.4	1.0
Machines manufacturing (including vehicles)	5475	0.8	100.0	1.0
Other manufacturing industries	784	1.7	100.0	1.1
Commerce, Hotels and Restaurants	5085	2.4	92.6	1.0
Transport and communications	2064	0.6	86.6	1.0
Financial intermediation and Business services	5088	0.9	100.0	1.1
Other community, social and personal service act.	428	1.9	100.0	1.0
All industries	31212	1.4	96.0	1.1

Table 2a: Quality of Matching

Variables	Sample	1) Mean Treated	2) Mean Controls	3) Stand. Bias	4) p>|t|
Sex	U	.553	.713	-33.5	.000
	M	.568	.541	3.5	.630
Age	U	35.111	37.653	-29.3	.000
	M	34.899	36.200	-15.0	.029
Tenure	U	7.939	9.105	-47.0	.000
	M	7.991	8.146	-6.3	.382
ln(aggregate earnings)$_{1994}$	U	4.853	5.120	-48.1	.000
	M	4.850	4.869	-3.2	.627
Worked weeks$_{1994}$	U	48.027	49.725	-17.5	.000
	M	48.442	48.264	1.8	.799
Dummy Prod. Worker	U	.659	.535	25.4	.000
	M	.656	.645	2.4	.719
Dummy Basic Non Prod. W.	U	.305	.402	-20.5	.000
	M	.306	.317	-2.5	.712
Dummy Adv. Non Prod. W.	U	.009	.038	-19.1	.002
	M	.009	.009	0.0	1.000
Dummy Manager	U	.002	.014	-13.4	.032
	M	.002	.004	-2.6	.564
Number of jobs$_{1994}$	U	1.036	1.023	6.9	.108
	M	1.035	1.035	0.0	1.000

U=unmatched samples; M=matched samples

Table 2b: Quality of Matching

Variables	Sample	1) Mean Treated	2) Mean Controls	3) Stand. Bias	4) p>|t|
Dummy working in North	U	.587	.545	8.4	.080
	M	.586	.564	4.3	.533
Dummy working in Center	U	.316	.289	5.8	.219
	M	.320	.296	5.1	.458
Dummy working in South	U	.097	.165	-20.3	.000
	M	.094	.139	-13.3	.042
Dummy born in North	U	.506	.458	9.5	.047
	M	.508	.489	3.8	.584
Dummy born in Center	U	.275	.257	4.1	.391
	M	.283	.271	2.7	.702
Dummy born in South	U	.169	.253	-20.6	.000
	M	.165	.219	-13.3	.045
Dummy born in OECD	U	.009	.009	-.3	.948
	M	.009	.005	4.9	.413
Dummy born in non-OECD	U	.038	.021	10.3	.011
	M	.035	.016	11.1	.084
Firm Employees$_{1994}$	U	147.68	4444.80	-39.4	.000
	M	153.5	308.46	-1.4	.438
Number of Injuries$_{1994-96}$	U	.113	.117	3.4	.473
	M	.136	.125	3.0	.688
N. of episodes of sickness leave$_{1994-96}$	U	.483	.457	3.3	.496
	M	.489	.475	1.8	.789
N. of days of injury leave$_{1994-96}$	U	1.589	2.220	-5.9	.332
	M	1.656	1.633	0.2	.961
N. of episodes of	U	.113	.120	-1.8	.710
"*Cassa integrazione*"$_{1994-96}$	M	.118	.082	7.7	.222

U=unmatched samples; M=matched samples

TABLE 3: The effect of displacement on the number of worked weeks and earnings for the initial sample and the matched sample.

LEVELS	All Sample			Matched Sample		
Variables	Mean Treated	Mean Controls	OLS	Mean Treated	Mean Controls	PSM
N. of Worked Weeks	23.46	49.95	-26.49***	23.71	48.20	-24.49***
0	(14.21)	(7.51)	[.37]	(14.25)	(10.04)	[.86]
N. of Worked Weeks	230.89	246.37	-15.48***	231.91	238.89	-6.98***
1,2,3,4,5	(37.00)	(28.12)	[.135]	(36.32)	(34.94)	[2.48]
N. of Worked Weeks	139.19	149.03	-9.84***	139.53	144.58	-5.05***
1,2,3	(25.48)	(17.94)	[.86]	(25.07)	(22.93)	[1.68]
N. of Worked Weeks	91.71	97.34	-5.64***	92.38	94.31	-1.93
4 and 5	(18.53)	(15.36)	[.74]	(17.97)	(17.99)	[1.26]
ln(Earnings)	4.83	5.23	-.40***	4.84	4.96	-.12***
0	(.64)	(.50)	[.02]	(.63)	(.52)	[.04]
ln(Earnings)	6.58	6.89	-.31***	6.59	6.64	-.05*
1,2,3,4,5	(.44)	(.48)	[.02]	(.44)	(.44)	[.03]
ln(Earnings)	6.05	6.37	-.32***	6.06	6.12	-.07**
1,2,3	(.46)	(.48)	[.02]	(.45)	(.45)	[.03]
ln(Earnings)	5.67	5.96	-.30***	5.67	5.71	-.04
4 and 5	(.52)	(.54)	[.03]	(.52)	(.51)	[.04]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01.
Std dev. in parentheses and std. err. in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

TABLE 4: The effect of displacement on the number of worked weeks and earnings for the initial sample and the matched sample.

DID	All Sample	Matched Sample
Variables	OLS	PSM
N. of Worked Weeks	-24.79***	-24.67***
0	[.49]	[1.08]
N. of Worked Weeks	-13.78***	-7.16***
1,2,3,4,5	[1.33]	[2.47]
N. of Worked Weeks	-8.15***	-5.23***
1,2,3	[.88]	[1.70]
N. of Worked Weeks	-3.94***	-2.11
4 and 5	[.80]	[1.41]
ln(Earnings)	-.12***	-.10***
0	[.02]	[.04]
ln(Earnings)	-.02	-.03
1,2,3,4,5	[.02]	[.03]
ln(Earnings)	-.04*	-.05
1,2,3	[.02]	[.03]
ln(Earnings)	-.01	-.02
4 and 5	[.02]	[.04]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01. Estimates from Differences-in-Differences and Propensity Score Matching Diff-in-Diff Standard errors in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

TABLE 5: The effect of displacement on the probability of injury and number of injuries for the initial sample and the matched sample.

LEVELS	All Sample			Matched Sample		
Variables	Mean Treated	Mean Controls	OLS	Mean Treated	Mean Controls	PSM
Probability of Injury	.205	.145	.061***	.207	.120	.087***
0,1,2,3,4,5	(.404)	(.352)	[.017]	(.406)	(.325)	[.026]
Probability of Injury	.151	.109	.042***	.151	.101	.049**
0,1,2,3	(.359)	(.313)	[.015]	(.358)	(.302)	[.023]
Probability of Injuries	.081	.052	.030***	.085	.033	.052***
4 and 5	(.274)	(.221)	[.011]	(.279)	(.179)	[.016]
Prob.Inj. per worked week	.0008	.0005	.0003***	.0008	.0004	.0004***
0,1,2,3,4,5	(.0017)	(.0012)	[.0000]	(.0017)	(.0011)	[.0001]
Prob.Inj. per worked week	.0010	.0006	.0004***	.0010	.0005	.0005***
0,1,2,3	(.0025)	(.0016)	[.0000]	(.0025)	(.0016)	[.0001]
Prob.Inj. per worked week	.0009	.0006	.0003***	.0009	.0003	.0006***
4 and 5	(.0029)	(.0024)	[.0001]	(.0030)	(.0018)	[.0002]
N. of Injuries	.260	.195	.065**	.259	.153	.106***
0,1,2,3,4,5	(.561)	(.557)	[.027]	(.557)	(.473)	[.036]
N. of Injuries	.176	.137	.039*	.172	.113	.059**
0,1,2,3	(.442)	(.443)	[.021]	(.430)	(.359)	[.028]
N. of Injuries	.084	.058	.026**	.087	.040	.047***
4 and 5	(.285)	(.260)	[.012]	(.290)	(.229)	[.018]
N. of Injuries per w.w.	.0010	.0007	.0004***	.0010	.0005	.0005***
0,1,2,3,4,5	(.0023)	(.0019)	[.0001]	(.0022)	(.0016)	[.0001]
N. of Injuries per w.w.	.0011	.0007	.0004***	.0011	.0006	.0005***
0,1,2,3	(.0029)	(.0023)	[.0001]	(.0029)	(.0019)	[.0002]
N. of Injuries per w.w.	.0009	.0006	.0003***	.0009	.0004	.0005***
4 and 5	(.0030)	(.0028)	[.0001]	(.0031)	(.0023)	[.0002]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01.
Std dev. in parentheses and std. err. in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

TABLE 6: The effect of displacement on the probability of injury and the number of injuries for the initial sample and the matched sample.

DID	All Sample	Matched Sample
Variables	OLS	PSM
Prob.Inj. per worked week	.0002**	.0004**
0,1,2,3,4,5	[.0001]	[.0002]
Prob.Inj. per worked week	.0003***	.0004**
0,1,2,3	[.0001]	[.0002]
Prob.Inj. per worked week	.0002	.0005**
4 and 5	[.0001]	[.0002]
N. of Injuries per w.w.	.0003**	.0005**
0,1,2,3,4,5	[.0001]	[.0002]
N. of Injuries per w.w.	.0004**	.0005**
0,1,2,3	[.0001]	[.0002]
N. of Injuries per w.w.	.0002	.0005*
4 and 5	[.0002]	[.0003]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01. Estimates from Differences-in-Differences and Propensity Score Matching Differences-in-Differences. Standard errors in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

TABLE 7: The effect of displacement on the days on injury leave for the initial sample and the matched sample

LEVELS	All Sample			Matched Sample		
Variables	Mean Treated	Mean Controls	OLS	Mean Treated	Mean Controls	PSM
Days on Inj. leave	5.94	5.13	.81	6.07	3.21	2.86**
0,1,2,3,4,5	(18.79)	(24.41)	[1.16]	(19.11)	(13.62)	[1.15]
Days on Inj. leave	3.84	3.40	.45	3.88	2.25	1.63**
0,1,2,3	(14.33)	(17.62)	[.84]	(14.55)	(10.64)	[.88]
Days on Inj. leave	2.10	1.73	.37	2.19	.96	1.23*
4 and 5	(10.99)	(15.83)	[.75]	(11.21)	(7.34)	[.66]
Days on Inj. leave per w.w.	.0232	.0177	.0055	.0236	.0109	.0127***
0,1,2,3,4,5	(.0733)	(.0849)	[.0041]	(.0745)	(.0467)	[.0043]
Days on Inj. leave per w.w.	.0243	.0175	.0068	.0245	.0113	.0132**
0,1,2,3	(.0944)	(.0932)	[.0045]	(.0959)	(.0526)	[.0053]
Days on Inj. leave per w.w.	.0240	.0188	.0052	.0250	.0096	.0153*
4 and 5	(.1383)	(.1802)	[.0086]	(.1412)	(.0728)	[.0078]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01.
Std dev. in parentheses. and std. err. in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

TABLE 8: The effect of displacement on days on injury leave for the initial sample and the matched sample.

DID	All Sample	Matched Sample
Variables	OLS	PSM
Days on Inj. leave per w.w.	.010*	.015**
0,1,2,3,4,5	[.006]	[.006]
Days on Inj. leave per w.w.	.011*	.016**
0,1,2,3	[.006]	[.007]
Days on Inj. leave per w.w.	.010	.018**
4 and 5	[.010]	[.009]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01. Estimates from Differences-in-Differences and Propensity Score Matching Differences-in-Differences. Standard errors in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

TABLE 9: The effect of displacement on the probability of sickness absence for the initial sample and the matched sample

LEVELS	All Sample			Matched Sample		
Variables	Mean Treated	Mean Controls	OLS	Mean Treated	Mean Controls	PSM
Prob. of sickness absences	.519	.439	.080***	.518	.489	.028
0,1,2,3,4,5	(.500)	(.496)	[.024]	(.500)	(.500)	[.035]
Prob. of sickness absences	.431	.368	.063***	.431	.395	.035
0,1,2,3	(.496)	(.482)	[.023]	(.496)	(.489)	[.034]
Prob. of sickness absences	.262	.249	.013	.259	.252	.007
4 and 5	(.440)	(.432)	[.021]	(.439)	(.435)	[.031]
Prob. of sickness abs. per w.w.	.0021	.0015	.0005***	.0020	.0018	.0003**
0,1,2,3,4,5	(.0020)	(.0018)	[.0000]	(.0020)	(.0018)	[.0001]
Prob. of sickness abs. per w.w.	.0028	.0019	.0009***	.0027	.0021	.0006***
0,1,2,3	(.0034)	(.0026)	[.0001]	(.0033)	(.0028)	[.0002]
Prob. of sickness abs. per w.w.	.0029	.0027	.0002	.0029	.0029	.0000
4 and 5	(.0052)	(.0052)	[.0002]	(.0051)	(.0056)	[.0004]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01.
Std dev. in parentheses and std. err. in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

TABLE 10: The effect of displacement on the probability of sickness absence for the initial sample and the matched sample.

DID	All Sample	Matched Sample
Variables	OLS	PSM
Probability of sickness abs. per w.w.	.0003**	.0003
0,1,2,3,4,5	[.0001]	[.0002]
Probability of sickness abs. per w.w.	.0006***	.0006**
0,1,2,3	[.0001]	[.0003]
Probability of sickness abs. per w.w.	-.0000	-.0000
4 and 5	[.0002]	[.0004]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01. Estimates from Differences-in-Differences and Propensity Score Matching Differences-in-Differences. Standard errors in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

TABLE 11: The effect of displacement on the number of new jobs

LEVELS	All Sample			Matched Sample		
Variables	Mean Treated	Mean Controls	OLS	Mean Treated	Mean Controls	PSM
Number of new jobs	1.38	.25	1.13***	1.96	.52	1.44***
0,1,2,3,4,5	(.66)	(.55)	[.02]	(1.24)	(.95)	[.075]
Number of new jobs	1.21	.18	1.06***	1.60	.31	1.28***
0,1,2,3	(.44)	(.40)	[.02]	(.91)	(.69)	[.06]
Number of new jobs	.18	.11	.07***	.36	.21	.15***
4 and 5	(.41)	(.33)	[.01]	(.75)	(.54)	[.05]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01.
Std dev. in parentheses and std. err. in square brackets. Standard errors from Nearest Neighbour Matching are computed analytically as in Lechner (2001).

Figure 1: Multidimensionality in job characteristics.

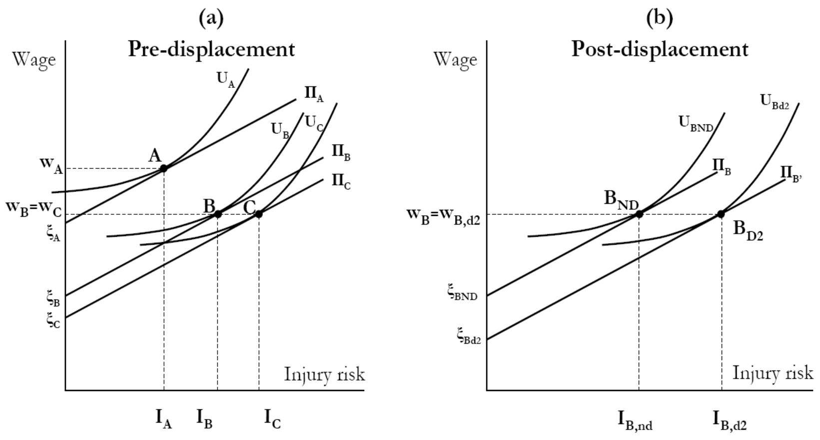

Figure 2: Comparison of Propensity Scores

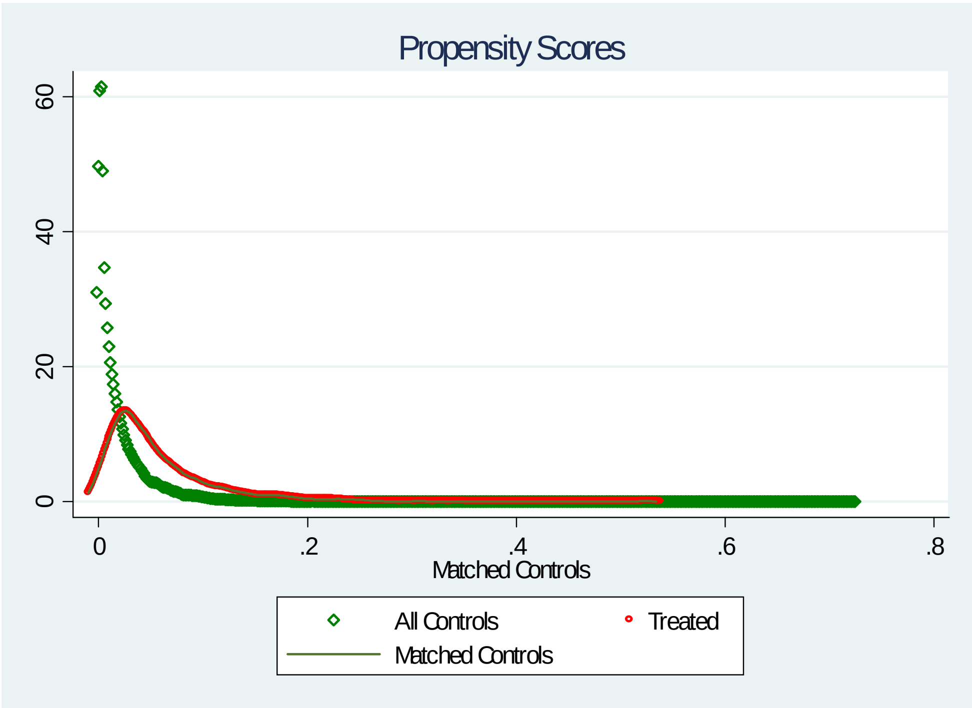

Figure 3: Monthly injury hazard rate for pooled flows over the 1994-1999 period.