The pecuniary and non-pecuniary costs of job displacement. An evaluation of the post-displacement injury rate *

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Abstract

This paper investigates the cost of involuntary job loss by focusing both on post-displacement earnings losses and injury rates. To this aim, administrative data from Italy containing individual work histories have been merged with individual data on workplace injuries. We employ propensity score matching techniques to measure the causal effect of displacement on workplace injury rates. We find that re-employed displaced workers compared to non-displaced individuals experience an increase of 72 percent in the probability of being injured in the post-displacement period.

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1 Introduction

A vast literature has investigated the costs of involuntary job displacement - i.e. a job loss due to firm's closure or downsizing - along several dimensions, such as post-displacement earnings losses, unemployment spells and human capital depreciation (see, among earlier contributions, Fallick 1996, Hamermesh (1987), Kletzer 1998). However, there exist additional equally important job attributes that may be affected by displacement, such as non-pecuniary job characteristics, which have not yet received much attention in the literature. This paper aims to fill the gap by analyzing the consequences of job displacement also in terms of such attributes. In particular, we investigate whether and to what extent job displacement affects workers' safety by comparing displaced workers' outcomes in terms of job-related injuries (and other proxies for injury risk) with those of a control group of similar non-displaced workers.

The relation between wages and workplace risk is not new in the literature. For instance, Hamermesh (1999) analysed jointly the trends in earnings and workplace risk inequalities. An assessment of the effect of involuntary job losses on work-related injuries is important for the following reasons. First, following the theory of compensating differentials and equalising differences (Brown (1980)) a complete evaluation of individual wealth should embody both earnings and non-pecuniary aspects of her job. Several studies, especially those on wage premia for risks (for a comprehensive survey see Viscusi and Aldy (2003)), consider a job as being characterized by monetary aspects (i.e. the salary) and by other amenities, such as job safety provided by the firm against work-related injuries. 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 lower salaries, higher injury risk or, most likely, to both. To the extent that displaced workers are re-employed in other jobs with similar wages but higher (lower) job risk, a welfare analysis conducted exclusively on salaries would understate (overstate) the total loss for the displaced workers. Moreover, as is emphasized by many studies based on survey data, pre-displacement workers' characteristics have significant effects on post-displacement outcomes (Fallick (1996), Kletzer (1998)). Therefore, in order to evaluate the treatment effect of displacement, the treated group (displaced) has to be comparable with a control group (non-displaced) with respect to any relevant attribute of a job, including work-related injury risks. Thus, 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 increases in the number of lost days of work or due to the payment of disability pensions. An additional long-run effect might occur if serious injuries entail a permanent reduction in the production capacity of workers.

The fact that displacement-related stress has a detrimental effect on health has been investigated in many studies, especially in the medical field. A first stream of this literature aims at identifying the negative effect of unemployment or job loss on health provoked by 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)). These studies report that unemployed or displaced workers make more use of drugs or public health care services (consultation of physician, hospitalization rate, etc.). A second body of the 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 Von Wachter (2007)).

Two recent studies are particularly related to our work. Rege et al. (2009) investigate the con-

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

sequences of downsizing on the probability to apply and receive a disability pension for a reduction in "work capacity". As it will be explained later on, our approach differs substantially from the study of Rege et al. (2009). In their study, disability pension is granted for illness, mental disorders, injury or defect. Since the application decision largely depends on workers' evaluation of the alternative opportunities, the authors focus their analysis on the effect of displacement on disability participation. Conversely, in our study, the data at our disposal on job-related injuries allows us to analyze a direct impact of displacement on injury rates, by reasonably assuming that workers do not get voluntarily involved into injury events. The study by Kuhn et al. (2009) reports an increase in health costs for displaced workers, which is mainly caused by a raise in the amount of sickness benefits. This increase is explained by the fact that for the unemployed workers, sickness benefits are higher than unemployment benefits. As a result, the authors find no significant effect on days of sick leave for displaced workers. To our knowledge, however, no empirical work up to date examine the consequences for the re-employed displaced workers in terms of job-related 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 1994-2002 that combines working histories from the Italian administrative data (WHIP) and individual work-related injuries from the Italian Workers' Compensation Authority (INAIL). We focus on involuntary job losses of workers with at least three years of tenure in order to restrain potential heterogeneity problems and self-selection issues. We restrict our analysis to workers displaced in 1997 due to firms' closure. This strategy allows us to observe workers three years before displacement, thereby permitting to construct reliable pre-displacement job 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 to evaluate the consequences of the job loss. A longer time period after displacement allows us to reconstruct the workers' career history more precisely[2] and provides a reasonably sufficient window to observe rare injury episodes. To estimate the causal effect of displacement on earnings and on the risk of being injured at the subsequent workplace we combine industry-specific propensity score matching techniques with a Differences in Differences estimator.

We find that, in a period of tight labor market, re-employed displaced workers in Italy experience only moderate and short lived earnings losses, but, as a consequence of displacement, they are more likely to get injured at the subsequent job 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 by. Since the aggregate Italian economy was growing during the period under analysis, these results suggest that re-employed displaced workers, in order to avoid unemployment or earnings losses, trade-off pecuniary job attributes for the non-pecuniary ones, even during period of positive labour market performance.

The remaining 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 provide some descriptive evidence. Estimation results are presented and discussed in section 5. Section 6 concludes.

2 Heterogeneity and multidimensional displacement outcomes

Implicit market theory (Rosen (1974)) shows that the analysis of the relationship between salaries and risk is complex, since these two job attributes are jointly determined in equilibrium with

[2] An accurate reconstruction of career history permits to track movement of workers across different firms and increases the likelihood of detecting false firms' deaths. For a discussion of this phenomenon see section 4.

heterogenous agents on the demand and supply sides of the labour market.

Although more hazardous jobs should be compensated with higher salaries, heterogeneity in employees' characteristics and, in particular, 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)). Thus, 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). Type-A worker has higher potential earnings than type-B individual (i.e. higher intercept $\xi_A > \xi_B$). Isoprofit curves are upward sloping; that is, the firms offer higher salaries at an increasing level of risk. If job-safety is a normal good, since workers have the same preferences and confront with the same trade-off, type A worker will choose a safer job than type B one (with an injury risk equal to $I_A < I_B$), due to an income effect.

Combinations of salaries and injury risk in panel (a) of figure 1 represent the pre-displacement working conditions. Willing to compare changes in job characteristics after displacement, displaced workers need to be matched to non-displaced individuals with similar observable working conditions and characteristics. Panel (a) shows that individuals with jobs described by point C are not good control subjects for individuals of type B, since they have lower skills or lower earnings potential ($\xi_C < \xi_B$). Therefore, comparing workers exclusively on their wages could be very misleading. Similar wages could hide different earnings potentials. Thus, it is important to choose appropriate

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

controls both in terms of observed wages and injury rates. More generally, choosing controls only in terms of observable pre-displacement characteristics and standard labor market outcomes possibly could not be sufficient to grasp important "non-ignorable" unobservables.

Let's assume now 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 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 injury risk compensates for the loss of earnings potential. Therefore, ideally, to correctly evaluate the impact of displacement, we need to take into account all possible labour market outcomes before and after displacement and to compare workers with similar observed and unobserved characteristics. This task is complex since, as showed by Rosen (1974), job attributes are determined in equilibrium and depend upon heterogeneity of individuals' preferences (e.g., taste for risk) and heterogeneity on the labour demand side (e.g., the slope of isoprofit curve indicating how firms reward risky jobs). The industry specific propensity score-DID procedure described in the next section is aimed to reduce such counterfactual problems by assigning to each treated (displaced) 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, therefore, to be comparable also in terms of preferences for risk. In other words, imposing exact matching on sector and considering also demographic, firm and job characteristics should deal simultaneously with heterogeneity of individuals' preferences and with heterogeneity in the labour 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 individual unobserved characteristics that are fixed over time.

3 Identification Strategy and Estimators

As Jacobson et al. (1993) 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 labor market outcome of the displaced workers (i.e., program participants) but not the outcome for these workers had they not been displaced (i.e., not participated in the program).

Indeed, the object of our analysis is to identify the average effect of displacement on the displaced workers with respect to various labor 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's indicate as D_i a variable taking the value 1 if a worker has been displaced (i.e. the individual is exposed to the treatment) and 0 if it is not displaced. Each individual has two potential outcomes: $Y_i(D_i = 1)$, if he has been exposed to the treatment and $Y_i(D_i = 0)$ if not. The problem is that in observational (non-experimental) studies one is not able to observe both outcomes for the same individual, i.e to compute directly $E(Y_i(0)|D_i = 1)$. What one is able to compute directly are

$E(Y_i(0)|D_i = 0)$ 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, since all workers at the closing firm have to leave (irrespectively of their ability, motivation and other characteristics that are unobserved for the researcher), it is not a natural experiment since: 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 firm closure; 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 non-ignorable (observable and unobservable) characteristics. Therefore, our conditioning set X, that is shown in the table at the end of this paragraph, takes into account many important non-ignorable job, firm and demographic characteristics.

Different econometric techniques have been developed in observational studies to overcome the bias generated when computing the ATT based on the CIA. All available parametric, semi-, and

[4] These pre-treatment characteristics must be strictly exogenous, namely 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, as made clear by Imbens (2004), in practice it is very unlikely to credibly justify that the stricter assumption is valid while the more general assumption is not .

nonparametric estimators are (implicitly or explicitly) based on the assumption that for every treated individual one can recover her counterfactual 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 to reduce, first, the component of the bias that is due to non-overlapping support of treated and control workers' characteristics (i.e. we avoid to compare workers that 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. The traditional econometric selection bias that stems from "selection on unobservables" is assumed to be absent, i.e. 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 $b(X)$. the propensity score, $P(X) = Pr(D = 1|X)$, is one possible balancing score. This finding is important to solve the "curse of dimensionality" problem and, therefore, 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) also stated the second assumption needed to identify the ATT under the CIA, the "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

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

last assumption by comparing the descriptive statistics between the treated and the control group and/or by inspecting the distribution of the propensity score for the treated and the control group. As a minimum, matching can be used as a method for improving and checking the overlap in covariates distributions (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-Differences-In-Differences estimator (PSM-DID) (Heckman et al. (1997), A. Smith and E. Todd (2005)). Indeed, if the point-wise bias due to "selection on unobservables" $B(X)$ is constant over time, i.e. unobserved heterogeneity is fixed in time, we have:

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

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 observations in constructing the counterfactual for the j treated observation, and n_1 is the number of treated observations. Matching estimators differ in how they construct the weights $w(i,j)$. To build the counterfactual in the non-treatment scenario for displaced workers, we experimented with the various available matching algorithms (Nearest neighbour(s), Caliper, Radius, Kernel and Local Linear weights). In finite samples (with a high ratio of treated individuals and/or a limited overlap in the covariate distributions), the choice of the 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 the covariates between the comparison and treatment groups, the various matching algorithms should give similar results (Dehejia and Wahba (2002)). In this paper, we present only the results from the Nearest Neighbour Matching (NN) with replacement routine[7], given that the results for the other estimators are qualitatively equivalents.[8]

Moreover, to estimate the average effect of job displacement on those displaced, we combine this PSM-DID strategy with exact covariate matching. We opted to exactly match on the 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 the 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 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 have been preferable, this was not possible due to the large number of continuous 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 akin to belonging to different samples.

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

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

[7] On the one hand (as 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 the other weighting schemes). Busso et al. (2009) explicitely investigate the finite sample properties of the most popular matching estimators and find that the Nearest Neighbour Matching with replacement achieves the best performance in terms of bias. 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 with the imposition of a tolerance level on the the propensity score distance (e.g., a caliper). As shown in section 4.2, this problem seems not to be our case.

[8] Results are available upon request.

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 group conditional on the estimated propensity score (see section 4.2).

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

TABLE A: Variables used in the propensity score estimation.

Variables used in the propensity score estimation	
$WC_{i,1994}$ = Workers' and job characteristics	Gender, age, tenure, log of aggregate annual earnings, aggregate annual worked weeks, 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 in 1994, i.e. three years before displacement. The set of variables $H_{i,1994-1996}$ are computed over three years before displacement, i.e. 1994, 1995 and 1996. If anticipation effects in the years preceding displacement are present, these variables could not satisfy completely strict exogeneity and therefore the CIA assumption might not hold. However, we have chosen to include these years, since episodes of injury, sickness absences and Cassa Integrazione[9] are rare events that respectively proxy for job-safety, health status and firm characteristics. The choice of a larger time-window for these covariates is aimed at smoothing them.[10]

[9] The "Cassa Integrazione" is a subsidy that is granted to manufacturing workers employed in firms in bad economic situations, that guarantees a wage replacement rate of 80%. It is a selective measure, in the sense that only firms of a certain size belonging and belonging to certain sectors are eligible.

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

4 Data and Descriptive Evidence

For our analysis we merge the Work Histories Italian Panel (WHIP)[11] and the administrative records from the Italian Workers' Compensation Authority (INAIL) for the period 1994-2002. It is a random sample of workers employed in the private sector of the Italian economy. It includes data on the calendar beginning and closing dates and on the duration (number of weeks) for 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 the firms where they are employed (number of employees, date of birth and of death, sector).[13] The WHIP dataset contains a dummy variable that indicates whether the worker has been on a sick leave during 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 3 days. Less serious injuries are considered as usual episodes of sickness. In addition, this dataset also identifies serious injuries that lead to a permanent health damage. Note that this 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 3 years of tenure in the main job in 1997, i.e. the job with highest yearly earnings. This choice is made due to the following reasons: first, in this way we ensure comparability with other international studies. Second, tenured workers are also the most likely to suffer from job-displacement, since they have higher probability to have

[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 because for each employment-relationship we only observe the annual number of weeks in employment and the annual earnings without additional information on their temporal distribution.

[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 the 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 in it.

accumulated firm (or sector) specific human capital and/or to maintain their jobs simply because they are particularly good matches. Internal labor markets (promotion from within policies) and incentive pay mechanisms are other two sources of earnings losses that increase their impacts with tenure. Moreover, the identification of the effects of displacement is mainly based on the possibility to control for pre-treatment employees' and employers' characteristics. As is standard in the job-displacement literature, we excluded from the sample the Construction sector due the high seasonality of these jobs. Also the Energy sector is left out due to the 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 or working in the Agricultural or Public sectors. Moreover, there is also the possibility that workers simply retire or end up in the shadow economy. Jacobson et al. (1993) faced a similar problem having administrative data on Pennsylvanian workers. To get around this problem, they decided to restrict their sample only 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 will follow their approach, which results in eliminating about 48% of displaced workers. Indeed, our estimates should be interpreted more conservatively as the effect of displacement on re-employed displaced workers. As a robustness check we repeated the estimation procedure for the unbalanced sample (by including also workers that re-enter the private sector after 1997) finding qualitatively identical results.[14]

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

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 treated workers with a control group of non-displaced workers. In particular, our treated group is formed by workers who have been laid-off due to firm-closure. The following events are categorized as displacements related to firm-closure:

- all cases of workers' mobility accompanied by a registered closure of the reference firm;
- all cases of mobility associated with the absence of 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 contains an indicator when a firm ceases its activity: a potentially closed firm is identified by the disappearance of a firm's identity number from the tax returns. However, this variable often refers to the administrative death (e.g., merges and/or legal transformations) and not to economic death (for a similar issue see Bender et al. (1999), Kuhn (2002)). Workers in firms that are closing down only from administrative point of view might be reemployed during the following years in the same firm or in entities that are somehow related to the former employer. To solve this problem, we develop an algorithm to detect false deaths, which utilizes information on the connections between employers and employees in 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 having distinct identification number - in the years preceding and following 1997. An employer-employee relationship, which is interrupted by firm's closure but it 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, maintain the same employment relationship.

For the purpose of our study, it is important to exclude other cases of mass-layoffs (both from the treated and the control groups) and to include in the treated group also 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, there is the possibility that selection effects are at work. On the one hand, during mass layoffs (that are not followed by firm closures) employers could select the "worse" workers to be laid-off and retain the "better" ones. 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, consequently, separations registered in the years before firm-closure could constitute preemptive quits. Therefore, it can be the case that those workers who succeed in this search process will tend to have comparatively "better" labor market characteristics (as an example they could simply have better job-search ability or labor market connections) than those remaining till the "bitter end" and thus that they will be affected comparatively less by the closure of the firm (see Serti (2008)). 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, we include in our baseline specification pre-closing separators , i.e. workers who have left their firms two years preceding the closure, in the treatment group.

In the analysis below, we will compare displaced workers to a control group formed by workers who did not experience a mass layoff or a firm-closure (or a pre-closure separation) during all the sample period. We think this is a better choice than that of using only workers who additionally maintain their initial jobs for all the years under scrutiny (or who do not experience periods of non-employment), because the comparison group is aimed to be representative of the counterfactual situation of displacement. Therefore, the control group should represent the hypothetical (and not observed) outcomes of the same displaced workers had they not experience the involuntary job loss, without additionally (and arbitrarily) ruling out the opportunity of a job change. However, it is also important to point out that the control group described above could also include individuals that were laid-off on an individual basis, and whom that we cannot take into account due to the administrative nature of the data. The inclusion of employees who do not leave voluntarily in the non-displaced group would cause an under-estimation of the effects of displacement. In any case, the estimates provided below are not sensitive to the exclusion from the control group of those employees, whose separations are not related to mass-layoffs or firms' closures.

[15] We have 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, also conditioning on the firm-level evolution of the number of employees (e.g., categorizing as pre-closing separator a worker that 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) leaves the main empirical results unaffected.

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 as similar as possible to the treated group (in terms of its non-ignorable characteristics) by properly selecting and reweighting control individuals. Several procedures were proposed in the literature in order to check the quality of the matching procedure based on the property that if $P(X)$ is the propensity score, then it must be that pre-treatment variables balance given the propensity score, i.e. $D \perp X|P(X)$ (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),Becker and Ichino (2002).[16] We split the sample in intervals such that the average propensity score for the treated and the control does not differ in each interval. Then, within each interval, we test that the means of each characteristics do not differ between treated and control units. We verify that the balancing property is satisfied for every specification of the propensity score (and therefore for each sector separately). This procedure is therefore also useful to determine which interactions and higher order terms to include in 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 if significant differences remain after matching on the propensity score and we show the standardized bias[17] after and before 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

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

[17] The standardised 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)).

as a whole before matching. Our aggregate sample is made up of 31212 workers. In column 2 we show the ratio of the number of displaced workers over the number of controls. It is apparent that for every treated worker we have a large pool of potential controls, even within each sector. This is an important pre-requisite to meaningfully implement our matching strategy. Column 3 displays the percentage of treated individuals that are 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 A. Smith and E. Todd (2005)) given that, as shown in the remaining of the paragraph, the lack of overlap do not seem to represent a big issue in this sample, since our matching routine substantially improves the comparability of the two groups of workers (see table 2). Finally, column 4 of Table 1 shows the average weight assigned to the matched observations. Given that NN matching with replacement selects for each treated individual the control subject with the minimum distance in terms of the 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 have been matched with a not resampled control and only two controls

[18] In the presence of homogeneous treatment effect discarding treated observations do not imply a redefinition of the estimand, but simply 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 treated are dropped. Therefore our statement in the main text is based on the assumption that individual observable characteristics are the main cause of treatment heterogeneity.

have been used three times as a match.[19]

Table 2 presents the following statistics for the unmatched and the matched samples (U and M, respectively) during the 1994-1996 period. 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 controls. As can be seen from Table 2, the displaced workers are younger and less tenured than the non-displaced, have lower earnings, work fewer weeks per year and have a greater chance of having multiple jobs. Moreover, among the treated, the percentage of women and blue-collar workers is larger. Regarding the geographical spread, the concentration of displaced workers is relatively lower in the central regions. These results are consistent with the empirical evidence on other countries (Kuhn et al. (2002), Fallick (1996) and Kletzer (1998)). Finally, firms where there are employed 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 are 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 bias 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 case of tenure and earnings it is about 50).

[19] We also find that the median difference between the propensity score of the treated individuals and the matched controls is 0.0000193, its 95-percentile is .0007671. This are very low values if compared with the estimated probability of being displaced.

[20] The reason is that t-statistics are decreasing with sample size. However, simply decreasing the sample size does not make the ATT inference problem more easy. 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: $ND = SB * (\sqrt{0.5}/100)$.

However, once we apply the matching routine described above the majority of the above mentioned differences are reduced or disappear.

Although in four cases the t-test rejects the hypothesis of equal means (age, dummy for being born and working in the south, dummy for being born outside OECD) we believe this is a minor issue, since the values of the standardized bias are substantially reduced and these differences are not profound.[21] As a robustness check, we have 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 apply this methodology to the linear DID estimator by regressing the difference between the post-treatment and the pre-treatment outcomes on a constant, on the treatment dummy and on the other covariates used in the propensity score estimation.[23] Our main results remained robust to this alternative methodology.

[21] The difference of the means of these variables (between treated and controls) are not significantly different from zero inside of each block of the estimated sector-specific propensity scores. The fact that at the aggregate level these differences become significant is an example of the fact that increasing the sample size the value of the t-statistics increases though the value of the differences do not. 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 to add as control variables also the pre-treatment outcomes. 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 group, the standard DID estimator and this augmented DID estimator will give similar results. We experimented with various specifications in terms of the regressors included and its flexibility. For example, we first introduced a fourth degree polynomial in age interacted with the geographical dummies. Then, we regressed on all 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.

Finally, it is also useful to look at the density functions of the propensity scores for the treated and the matched controls to get a sense of the overlap between them. Figure 2 confirms that the propensity score matching increases the comparability between the two groups. While prior to matching, the estimated kernel densities were quite different, after matching we can observe very similar values.

5 Econometric results and discussion

In this section, we investigate whether and to what extent the displaced workers after displacement lose in terms of earnings, weeks worked, sick leave and measures of injury risk. To this aim, 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 that 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 sickness absences.

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 below), we have computed the logarithm of the sum of annual earnings and the sum of annual worked weeks for the following periods: the year of displacement (year 0), all post-displacement period (years 1,2,3,4,5), the "short-run" period (years 1,2,3) and the longer-run period (years 4,5).[25] As expected and consistent with the existing literature, estimates in Table 3 show that displaced

[24] As an additional robustness check, we employ 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.

[25] Coefficients estimated year by year are available upon request.

workers experience a significant earnings loss during the year of displacement. This loss is evident when looking both at the unadjusted mean comparison which considers all the sample, and at the propensity score matching estimation results. The latter method suggests that an earnings loss equal to 12 percent in the year of displacement and is equal to 5 percent during the five years after displacement (years 1,2,3,4,5). During the first three years after displacement displaced workers experience a 7 percent earnings loss. This negative effect fades away thereafter. Estimates from the Propensity Score Matching Differences-In-Differences shown in table 4 display significant earnings losses in the year of displacement. Estimated coefficients are negative although not significant in the first three years after displacement and in the 4th and 5th years. As can be seen from tables 3 and 4, unsurprisingly, there is a significant reduction in the number of worked weeks 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 in part due to the fact that we have selected individuals with at least 3 years of tenure, while other studies focus on more experienced workers. Previous studies (e.g., Eliason and Storrie (2006) that uses 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) associate this business cycle sensitivity 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 their skills are less valuable to the employer. This explanation, in turn, is based on the contribution of Stevens (1997), who finds that displaced workers who incur in additional job separations have substantially higher earnings losses. An alternative interpretation of this phenomenon relates the higher probability of displaced workers to hold several short lived jobs to the fact that transitions from job to job tend to be relatively longer in the periods of recession (Hall (1995)). Holmlund and Storrie (2002) find that transitions from temporary jobs increased rapidly at the beginning of a recession. In fact, during the period under analysis the performances of the italian labor market were improving.[26] The unemployment rate remained practically stable at around 11.3 % in the period 1994-1998 and then declined monotonically to the value of 8.7 percent in 2002.[27] Overall, evidence from this study seems to be consistent with these conjectures.

The novel and the most interesting contribution 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 faced at their workplace : the probability of being injured, the number of injuries 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 if the control and the treated groups have the same length of exposure to risk. Moreover, as we have just observed. the displaced workers tend to work fewer weeks than the control group. Therefore, all the above mentioned injury measures are normalised by the total number of worked weeks in the respective reference period to account for the different lengths of exposure to risk. In short, the logic behind these measures for job-safety is the following. An injury is a rare event, increasing the window of observation increases the quality of the proxy. Then we need a normalized variable because 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

[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 simmetrically opposite temporal patterns. They were relatively stable in the period 1994-1998, at 42.2% and 47.5%, respectively, and then incresead monotonically until reaching 44.3% and 48.5% in 2002.

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 normalised measure are qualitatively identical, and the estimated effect is equal to 0.0004 implying a 100 percent greater probability to be injured at the subsequent job 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 permanent. The results from the simple linear regression are very similar, although the size of the losses is somewhat lower. 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. Since the outcomes of interest are computed over period of different length before and after displacement and since 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 normalised 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 confirmed also by the estimates of the normalised variables that suggest a 100% increase in the number of injuries per week and a 116% increase in the number of days on injury leave per worked week. Moreover, we also check the robustness of these results employing a Propensity Score Matching-Differences In Differences 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 worked week and out-of-work days per worked 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 4th and 5th 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 was relatively constant in 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 decreasing over time. These results, together with the modest losses in terms of earnings and weeks worked combined with the positive aggregate labor market trends, suggest that re-employed displaced workers, in order to avoid unemployment or earnings losses, trade-off pecuniary job attributes for the non-pecuniary ones.

In the periods of tight labour market, workers can give up job safety in exchange for lower pecuniary losses by working in more hazardous tasks and/or by accepting job-instability, i.e. several temporary and short lived jobs that could be available in a period of economic expansion.[28] Indeed, as figure 3 shows, the monthly injury hazard rate[29] is initially increasing and reaches its peak three months after the beginning of a new job, then it decreases thereafter and becomes relatively flat after the 20th month. In the additional exercise that is not reported here, we also find evidence that job instability (proxied by the number of jobs in an year) is notably higher for the displaced workers only in 1997 (in the year of displacement the displaced workers have almost twice more jobs than the control group), while from 1998 to 2002 the gap is reduced to economically insignificant levels (reaching at most 3%). Therefore, we interpret these results as indicative for the relation that runs from more risky jobs to lower earning losses.

6 Conclusion

This paper has analyzed an important dimension of the costs of job loss, namely its effect on job-related injuries. It complements the previous analyses that has studied the effects of job displacement in terms of standard labor market outcomes. We argue that, in order to provide a comprehensive picture of the effects of job displacement and to conduct a complete welfare analysis, it is crucial to incorporate the non-pecuniary aspects of working conditions into the analysis of the effects of job loss.

We find that, in a period of tight labor market, re-employed displaced workers in Italy experience only moderate and short lived earnings losses, but, as a consequence of displacement, they are also 72 percent more likely to get injured on a subsequent job compared to control group of non-displaced workers. In addition, this effect on job safety is not transitory and does not decrease in intensity as

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

time passes by. These results suggest that re-employed displaced workers trade-off pecuniary losses for non-pecuniary ones in order to reduce unemployment spells or larger earning losses. Given that displaced workers experience only a temporary increase in job instability and that the effect on injuries is constant over time, we speculate that the effect of displacement on job safety has to be ascribed more to transitions to more risky jobs rather than to the fact that displaced workers pass through many temporary jobs (and the injury hazard is higher at the beginning of a new job).

This work is in line with and complements previous studies that document higher long-run mortality rates among the displaced workers (Elliason and Storrie, 2009, Moser et al., 1987. Morris et al. 1994, Sullivan and Von Wachter, 2006) and those that attested a business-cycle sensitivity of earning losses (Eliason and Storrie (2006), Jacobson et al. (1993)). Our findings are also consistent with Gerdtham and Ruhm (2002) and Ruhm (2000) who find a positive correlation between fatalities and economic upturns. The increase in accidents reported by the authors as unemployment decreases might partly be the result of transitions to more hazardous jobs or tasks.

Our findings 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 be concerned also with job quality, namely with job safety. On the one hand, finding a new job rapidly could minimize the losses in terms of human capital depreciation for the displaced workers and could reduce the use of unemployment benefits. On the other hand, we have also shown that lower job safety may imply other individual and social costs. The short-run and the long-run costs of re-employment in more hazardous job might outweigh the savings on unemployment benefits. Therefore the reemployment of displaced individuals could be accompanied by training programmes, and in particular, by the on-the job training aimed at reducing the risk of injuries by developing 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 $\text{Injuries}_{1994-96}$	U	.113	.117	3.4	.473
	M	.136	.125	3.0	.688
N. of episodes of sickness $\text{leave}_{1994-96}$	U	.483	.457	3.3	.496
	M	.489	.475	1.8	.789
N. of days of injury $\text{leave}_{1994-96}$	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)	[2.47]
N. of Worked Weeks	139.19	149.03	-9.84***	139.53	144.58	-5.05***
1,2,3	(25.48)	(17.94)	[.87]	(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***	4.83	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.
Standard errors 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	(.0022)	(.0019)	[.0000]	(.0022)	(.0016)	[.0001]
N. of Injuries per w.w.	.0011	.0007	.0004***	.0011	.0006	.0005***
0,1,2,3	(.0029)	(.0022)	[.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.
Standard errors 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)	[.0073]

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01.
Standard errors 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)	[.030]
Prob. of sickness abs. per w.w.	.0021	.0015	.0005***	.0020	.0018	.0002**
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.
Standard errors 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).

Figure 1: Multidimensionality in job characteristics.

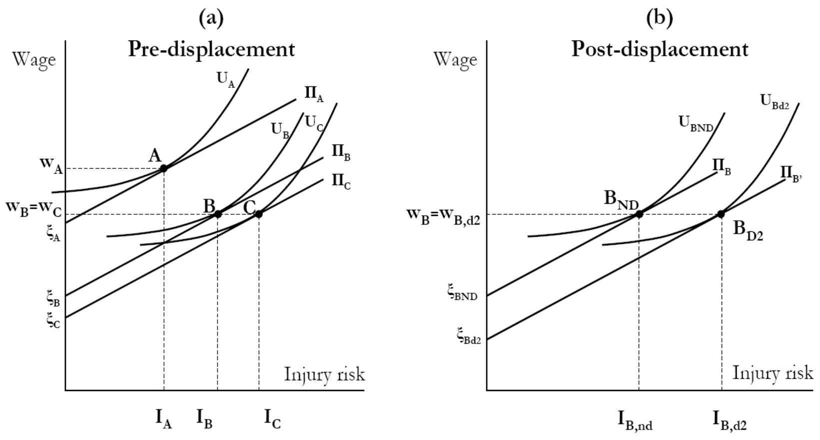

Figure 2: Comparison of Propensity Scores

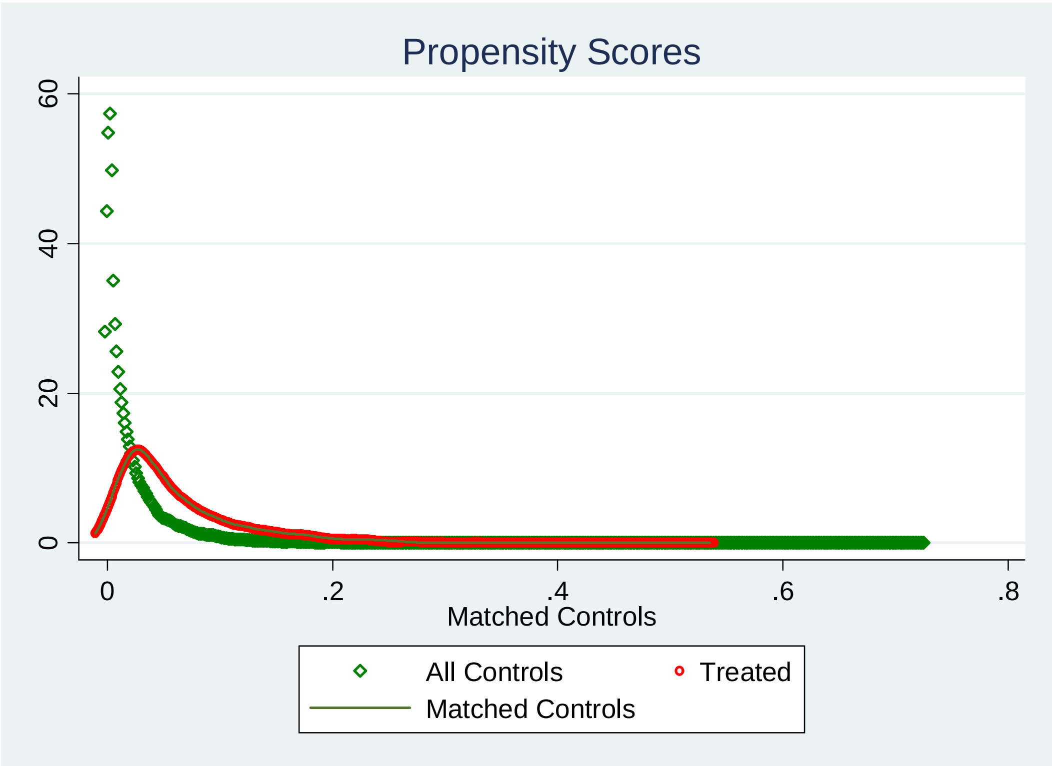

Figure 3: Monthly injury hazard rate for pooled flows over the 1994-1999 period.