The pecuniary and non-pecuniary costs of job displacement – The risky job of being back to work*

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March 29, 2013

ABSTRACT

This paper investigates the effect of displacement on workplace injury risk and earnings using Italian administrative data on work histories merged with data on individual job-related accidents. Compared to a control group of non-displaced workers selected with propensity score matching techniques, re-employed displaced workers are exposed to moderate earnings losses and experience approximately a 79% increase in workplace injuries. This sizeable reduction in the quality of non-pecuniary working conditions is driven by the transition to new occupations and the risk imposed by new work environments.

JEL Codes: I18, J28, J63

Keywords: Job displacement, post-displacement injury rates, propensity score matching

1 Introduction

The costs of involuntary job displacement have been extensively documented in terms of post-displacement earnings losses, lower employment rates and negative health effects (Couch and Placzek, 2010; Browning and Heinesen, 2012). In contrast, non-pecuniary job attributes have received relatively little attention in the literature, although these amenities

* We are grateful to LABOR (Collegio Carlo Alberto-Torino) for access to firm-level data. We gratefully acknowledge the financial support provided by the Ministerio de Ciencia e Innovación and FEDER under project SEJ-2007-62656. We wish to thank the following individuals and organisations for their helpful comments and suggestions: Pedro Albarran, Lola Collado, Giuseppe De Luca, Andrea Ichino, Iñigo Iturbe-Ormaetxe, Fabian Lange, Michael Lechner, Sonia Oreffice, Climent Quintana Domeque, Marcello Sarterelli, Francesco Turino, Luis Ubeda, Anzelika Zaiceva and the participants at the seminars of the Ninth IZA/SOLE Transatlantic Meeting of Labor Economists, the IZA, the Applied Econometrics Association Conference on the Econometrics of Healthy Human Resources, the EALE-SOLE Conference, the Conference on The Economic and Social Implications of Health and Safety at Work, the University of Alicante, the University of Bologna, the University of Florence and the University of Siena. We also thank the editors, including an associate editor and two anonymous referees, who suggested several ways to improve the analysis. The usual disclaimer applies.

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might represent an equally important component of the compensation package in several European countries, including Italy, where wages are regulated by strict institutional rules.

This paper is the first study to investigate the changes in workplace safety conditions after displacement together with standard outcomes, such as employment and wage dynamics. Toward this end, the Italian administrative "Work Histories Italian Panel" (WHIP, for the 1989-2003 period) database has been merged with individual workplace injury data on the same workers from the Italian Workers' Compensation Authority (INAIL, for the 1994-2003 period). The post-displacement outcomes of workers displaced in 1997 due to firm closure are compared to those of a control group of similar non-displaced individuals by combining the use of propensity score matching techniques with a difference-in-differences estimator (PSM-DID). This comparison reveals that in addition to moderate earnings losses, re-employed displaced workers experience approximately a 79% increase in workplace injuries. This effect on job safety is not transitory and cannot be fully explained by lower experience in the new jobs. Indeed, our results indicate that displaced workers are re-employed in occupations that are riskier on average.

The empirical finding that ignoring non-pecuniary losses understates the effects of job displacement is consistent with the theory of equalising differences (Rosen, 1974). If job safety is a normal good and job displacement generates a negative shock to workers' earnings potential (i.e., to human capital or other kind of rents), laid-off workers might reasonably be expected to trade-off part of their job safety to reduce their wage losses. Indeed, the relevance of this income effect has been emphasised in many studies (Hwang et al., 1992; Hamermesh, 1999). The results of this study also complement the existing evidence on the negative consequences of unemployment and involuntary job displacement

on health conditions. Several studies have documented higher rates of mental and stress-related diseases among unemployed (Paul and Moser 2009; Roelfs et al. 2011) and laid-off workers (Sullivan and von Wachter, 2009; Eliason and Storrie, 2009, 2010; Browning and Heinesen, 2012), in addition to higher mortality rates for both groups. Lower levels of job safety in new jobs represents an unexplored channel through which displacement may negatively affect workers' health conditions. Moreover, this deterioration of job safety might lead to substantial costs that result from an increase in the number of working days lost and a (permanent) reduction of workers' productive capacity.

The remainder of this paper is organised as follows. Section 2 discusses the identification strategy and econometric methodology. Section 3 describes the data and provides descriptive evidence. Section 4 discusses the empirical results and Section 5 concludes.

2 Identification Strategy and Estimators

To estimate the effect of displacement (i.e., the treatment) on displaced workers, every treated individual "*i*" is matched to the closest control in terms of the estimated propensity score in the same sector "*j*" by employing a one-to-one nearest-neighbour (NN) matching (with replacement) routine. Propensity scores (i.e., the probability of being displaced in 1997) are estimated separately by industry using the following general specification:

$$P_j(Displacement_{i,1997}) = \Phi\left(h_j(WC_{i,1994}, FC_{i,1994}, H_{i,1994-1996})\right) \quad [1]$$

where $\Phi(.)$ is the normal cumulative distribution function. The balancing test proposed by Dehejia and Wahba (2002) is used to check the effectiveness of the matching routine in balancing the covariates (Rosenbaum and Rubin, 1983) and to determine which interactions and higher-order terms to include in $h_j(.)$ (Becker and Ichino, 2002). The argument of $h_j(.)$

includes pre-displacement characteristics that may simultaneously affect the outcomes under study and the selection into treatment. $WC_{i,1994}$ represents workers' pre-displacement attributes and job characteristics, such as gender, age, tenure, log of annual earnings, annual weeks worked, type of occupation, number of employment relationships held in a year, region of birth and region of work. $FC_{i,1994}$ denotes the industrial sector of the firm and its number of employees. Both $WC_{i,1994}$ and $FC_{i,1994}$ are computed for 1994 (i.e., three years before displacement).[1] The set of variables $H_{i,1994\text{-}1996}$ includes safety-related aspects of a workplace[2] (i.e., the number of injuries and the number of days on injury leave), an indicator of health status (the number of years with a registered episode of sickness leave) and the number of episodes of "*Cassa integrazione*" (which is a subsidy granted to workers employed at poor-performing firms in selected industries). The use of a larger time window is intended to smooth the rare events on which the proxies are based.

Although plant closure can be understood as an exogenous shock at the plant level and the conditioning set in [1] includes a large number of important job, firm and demographic characteristics, the PSM identification assumption of "selection on observables" is rather demanding. Therefore, the additional DID procedure is employed to augment the robustness of the PSM estimator by differencing away individual unobserved characteristics that are constant over time (Heckman et al., 1997; Smith and Todd, 2005; Couch and Placzek, 2010).

[1] In 1995, the impending displacement begins to negatively affect weekly earnings dynamics (evidence is available upon request). For this reason, the values of earnings and of the number of weeks worked for 1995 and 1996 are excluded from the set of control variables.

[2] In principle, pre-displacement injuries might also be already affected by the future firm closure. On the one hand, employers might try to cut the costs associated with job safety and/or they could lay off workers and leave the remaining employees with more demanding jobs. On the other hand, the fear of being laid off could make workers reluctant to report less serious accidents. However, no difference is detected between treated and untreated individuals in terms of the average injuries and average days lost because of injuries for each of the pre-displacement years 1994-1996.

3 Data and Descriptive Evidence

The analyses in the paper are based on a random sample of full-time workers employed in the private sector in Italy with at least three years of tenure at the main job (i.e., the job with the highest annual earnings) in 1997.[3] The WHIP dataset provides information on earnings, employment spells and characteristics of both firms and employees. The INAIL dataset contains the number of workplace injuries (i.e., the number of accidents that occurred during a work task) and the duration of injury-related leaves at the employer-employee level. It records all injuries that lead to a leave of more than three days. The diagnosis and the prognosis of these accidents are reported and certified by physicians.

Workers displaced due to firm closure in 1997 are compared to a control group of workers that did not experience a mass layoff or a firm closure (or a pre-closure separation) during 1997 or subsequent years. The following events are categorised as displacements related to firm closure: registered closure of the reference firm,[4] absence of a workforce at the end of the reference year at the reference firm and separations from closing firms during the two years preceding firm closure (i.e., pre-closing separators). Thus, to minimise selection problems, workers displaced due to mass layoffs are excluded from the analyses whereas pre-closing separators are included in the treated group (Gibbons and Katz, 1991; Eliason and Storrie, 2006; Pfann and Hamermesh, 2008).[5]

[3] Because of its high degree of seasonality, the construction sector is excluded. The energy sector is not considered due to the negligible number of treated individuals (three). Because firm closures in the Italian public sector are difficult to identify and unlikely to be observed, only transitions from private to public sector in the post-1997 period are included.

[4] The algorithm of Contini et al. (2009) has been extended to detect cases of false displacements (Kuhn, 2002) by exploring all possible links between workers, firms and job relationships (all three entities have distinct identification numbers) in the years preceding and following 1997.

[5] A mass layoff is a reduction in an employer's workforce that affects at least 30% of the employees in a given year. The empirical results are not sensitive to different definitions of pre-closing separators.

As in other administrative data, employees who appear as non-employed may have found other sources of income via self-employment, quasi-dependent employment (i.e., atypical contracts), unemployment benefits or pensions. Following Jacobson et al. (1993), we assume zero income and zero injuries for periods of non-employment. Workers who were not re-employed after 1997 (19% of displaced workers) are excluded from the baseline sample. As a robustness check, these individuals are then included in the analyses.

Table 1 reports the sample size before matching and various post-matching statistics for each sector. For each treated worker, there is a large pool of potential controls (columns 1 and 2). The lack of overlap does not appear to represent a significant issue in this sample (columns 3 and 4).[6]

[Insert Table 1 about here]

Table 2 reports statistics for the unmatched and matched samples for the pre-displacement period. After matching on the propensity score, the means of the pre-treatment variables look similar for the two groups of workers (columns 1 and 2). This observation is confirmed by comparing the standardised bias (Rosenbaum and Rubin, 1985) before and after matching (column 3) and by the results of a standard *t*-test for the equality of means (column 4).[7] In the unmatched sample, the value of the standardised bias is high for many important covariates (e.g., for tenure, it is approximately 60), and the difference between means is often significantly different from zero. However, after implementing the matching routine, the majority of these differences is reduced or disappears.

[6] See Imbens and Wooldridge (2009) for a discussion. Those individuals with characteristics that perfectly predict success (or failure) in the sector-specific propensity score estimation are excluded from the analysis. As a result, only 5% of displaced workers are disregarded.

[7] 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. When the standardised bias is greater than 35, global linear regression methods are sensitive to the specification and are not advisable (Imbens and Rubin, forthcoming).

[Insert Table 2 about here]

4 Results

This section reports the estimated effects of displacement (in terms of earnings, weeks worked, injuries and job instability) and investigates the possible mechanisms that lead displaced workers to experience more injuries.

4.1 Effect of displacement on worked weeks and earnings

Table 3 reports estimates for annual weeks worked, annual earnings and weekly earnings in columns 1 to 3, respectively. Earnings are expressed in hundreds of Euros at 1995 prices using the Consumer Price Index provided by the Italian Institute of Statistics (ISTAT). For comparability with other dependent variables (see below), the average annual earnings (i.e., the sum of earnings divided by the number of years under consideration) and the average annual weeks worked (i.e., the sum of weeks worked divided by the number of years under consideration) have been computed for the year of displacement (year 0),[8] the entire post-displacement period (years 1 to 6), the "short-run period" (years 1 to 3) and a more extended period (years 4 to 6). Weekly earnings are defined as the sum of earnings during the period divided by the corresponding number of weeks worked, which represents the average weekly earnings of the considered time window. The pre-displacement baseline measure of the PSM-DID estimator is calculated for the 1989-1994 time window (from eight to three years before displacement).[9]

[8] During the year of displacement, estimates are contaminated by the fact that displaced workers are still employed in the pre-displacement firm for part of the year, depending on the timing of events.

[9] There is evidence of an Ashenfelter's dip in weekly earnings for 1995 and 1996 (see also note 1). Therefore, these two years are excluded from the base period of the differences.

As shown in the first two columns of Table 3, displaced workers experience economically and statistically significant losses in terms of average annual weeks worked and average annual earnings for the year of displacement (-26% and -21%, respectively) and in the three years after displacement (-12% and -15%, respectively).[10] In subsequent years, these negative effects remain statistically significant but decrease in magnitude by approximately half. It is also notable that the magnitude of earnings losses (in percentage terms) and their temporal patterns are similar to those for weeks worked. Moreover, there is no significant loss in terms of weekly earnings. Thus, consistent with estimates for other European countries (Kuhn, 2002; Hijzen et al., 2010), these findings suggest that the bulk of earnings losses for re-employed workers is attributable to a decline in time worked rather than to losses attributable to reduced wage rates. The absence of weekly earnings losses for re-employed workers may be caused by the relatively stringent institutional regulations governing wages in Italy and the possibility that workers may exit the labour force in response to earnings losses.

[Insert Table 3 about here]

The estimates in Table 3 are based on the baseline sample (i.e., workers who were re-employed at least once in the post-displacement period) and do not account for whether workers may have found other sources of income via self-employment, quasi-dependent employment (i.e., atypical contracts), unemployment benefits or pensions. Although the WHIP dataset does not quantify such monetary compensations, it is possible to estimate the effects of displacement on the probability that an individual will fall into one of the above

[10] Earnings losses in percentage terms represent losses of displaced workers (the estimated parameter) as a percentage of the mean earnings of the matched control group. The same holds for the other dependent variables.

states/categories (see Table A.1 in the Appendix).[11] In the short term, displaced workers have an economically and statistically higher probability of receiving unemployment benefits than the matched controls. However, the estimated effects of job displacement on the probability of an individual's falling into one of these categories are not statistically and/or economically significant over the long term. Moreover, the difference between the treated and matched controls in terms of the probability of their being absent from all administrative data at our disposal is equal to 0.13 in the year after displacement and gradually decreases to 0.08 six years after displacement.

Table 4 presents the PSM-DID estimated losses with respect to average annual weeks worked and average annual earnings for all displaced workers by assuming zero weeks worked and zero earnings for workers who were never re-employed after 1997 (in columns 1 and 2). In the short term, average annual earnings losses amount to approximately 34% and average annual weeks worked decrease by approximately 30%. Because displacement increases the probability of receiving unemployment benefits only in the years immediately after the loss of a job, the difference between estimated earnings losses and income losses is likely to be quantitatively relevant particularly in the short run. In the long term, the estimated losses in terms of average annual earnings amount to approximately 26% (20% in terms of average annual weeks worked). Overall, this evidence suggests that the bulk of the displacement effect on earnings is determined by the difference between the treated and controls in terms of the probability of being non-employed.

[Insert Table 4 about here]

[11] Table A.1 reports PSM estimates because pre-1997 information on unemployment benefits is not available, which makes it impossible to compute PSM-DID estimates. The PSM and PSM-DID results are similar for the other dependent variables.

4.2 Effect of displacement on workplace injuries

The effect of displacement on job safety is estimated by analysing two proxies for the risk that workers face at their workplaces: the number of injuries reported and the number of days that workers are absent from their jobs because of injuries. An injury is a rare event, so enlarging the size of the observation window increases the quality of the proxies. To smooth these outcomes, the following four time windows are considered: the year of displacement (year 0), the entire post-displacement period (years 1 to 6), the first three years after displacement (years 1 to 3, or the "short term") and the subsequent three years (years 4 to 6, or the "long term"). However, these measures of job risk are limited dependent variables and count variables whose analysis is meaningful only if the control and treated groups show the same time of exposure to risk (e.g., displaced individuals work fewer weeks than the control group). Thus, the two proxies for risk have been normalised by the total number of weeks worked during the respective reference periods. The estimated parameters are reported on a yearly basis (i.e., the number of injuries and number of days on injury leave per year) to improve the readability of the estimates.[12]

Table 3 reports the PSM-DID results for the number of injuries per year and the number of days on injury leave per year in columns 4 and 5. To proxy the level of job safety before displacement, all of the available pre-displacement years (1994-1996) have been used together as the base period of the differences.[13] In the entire post-displacement period, the

[12] For example, the dependent variable “normalised number of injuries in years 1 to 3” is the sum of the injuries registered during the first, second and third years after displacement divided by the number of weeks worked in the same time window. To report the estimated parameters on a yearly basis (i.e., in terms of the variable “number of injuries per year in years 1 to 3”), the dependent variable described above (i.e., the outcome “normalised number of injuries in the years 1 to 3”) is multiplied by 52.14 (i.e., by the notional number of working weeks in a year).

[13] The results are not sensitive to restricting the base period of the differences and the covariates used in the propensity score specification to the year 1994 (see note 2).

estimated effect of job displacement on the number of injuries per year amounts to 0.026, which implies a 79% increase in workplace injuries compared to the control group. Moreover, the estimates in Table 3 also indicate a 71% increase in workplace injuries over the long term (i.e., in the final three post-displacement years), which suggests that the effect of displacement on job safety might be relatively long lasting. The results for the number of days on injury leave per year provide evidence of a considerable increase in absences because of workplace accidents, but only over the long term. The effect of displacement on absences in the first three years after displacement is not statistically significant, although it is positive and economically relevant. A set of non-reported estimations suggests that the probability of normal sickness absences is not affected by displacement (results available upon request).

In principle, the results of Table 3 are uniquely representative of the population of re-employed displaced workers. However, another interesting policy parameter is the effect of dismissal on labour market outcomes if all dismissed individuals find a job. In the worst-case scenario, re-employment patterns observed in this study might be the result of a self-selection process in which relatively more risk-averse (and less productive) displaced workers leave the workforce permanently. Therefore, as a robustness check, the effects of job displacement have been re-estimated for all displaced workers, including those who were never re-employed after 1997. These workers are assumed to be so risk averse that they would have experienced zero injuries (and zero days on injury leave) had they been re-employed. Because controls have a relatively low probability to be permanently out of employment, the estimated parameters are close to being lower-bound estimates. As expected, the estimated effects on injuries and days on injury leave decrease in magnitude

(columns 3 and 4 of Table 4); however, they remain statistically significant, and their values continue to be relevant.

Overall, these results indicate that displaced workers experience more job-related injuries than their matched controls. Workers may relinquish job safety by accepting more hazardous jobs and/or by accepting job instability (i.e., temporary jobs that may be available during a period of economic expansion).[14] Indeed, as Figure 1 shows, the monthly injury hazard rate for all observed job relationships initially increases and peaks three months after the beginning of a new job; it decreases thereafter and becomes relatively flat after the 20th month. The effect of displacement on job instability (which is proxied by the average annual number of new jobs that a worker begins) is high in the short term but decreases significantly thereafter (column 6 of Table 3). The (percentage) effect of displacement on job instability is approximately 15 times greater during the year in which the firm closes down than in the final three years after displacement (years 4 to 6). Nevertheless, the (percentage) effect of displacement on injuries per year, which is estimated for the final three post-displacement years, remains approximately one-half of that found for the year of displacement (and for the first three post-displacement years). Thus, the temporal pattern of the estimated effects of displacement on injuries is not consistent with the argument that job instability (i.e., workers passing through many temporary jobs and experiencing high injury-hazard rates at the beginning of every new job) is the main explanatory factor.

[Insert Figure 1 about here]

[14] During the period under analysis, the performance of the Italian labour market was improving. The unemployment rate remained fairly stable at approximately 11.3% during the period 1994-1998 and then declined monotonically to 8.7% in 2002.

4.3 Discussion and heterogeneity analysis

A post-displacement increase in workplace injuries might theoretically be explained by factors other than the risk imposed by the new work environment but that are otherwise connected to workers' displacement status. Higher risk might also depend on changes in attitudes on the supply side (such as less cautious behaviour that results from stress-related and psychological illness). Moreover, on the demand side, displaced workers may be allocated to the most risky tasks because of stigma effects or the use of screening devices by new employers.

Ideally, a measure of the risk imposed by the working environment may be constructed by calculating the likelihood of workplace injury for each firm (using data on injuries from the colleagues of individual "*i*" at individual *i*'s plant), but this strategy is not feasible because the WHIP dataset does not contain information on all workers employed at a single firm. However, at a level of greater aggregation, a measure of the risk imposed by the working environment may be developed by computing the injury incidence rates in different strata defined by relevant observables that predict injury risk. These incidence rates are constructed by using injury data for all non-displaced workers during the 1994-2003 period and weighting the number of injuries by the number of weeks worked in each strata.[15] The value of the index calculated for non-displaced workers is then imputed to displaced workers belonging to the same cell. The strata used in the construction of the index are defined by characteristics at the firm level (industry, size and geographical location) and at the job level

[15] Displaced workers are not considered so that this measure remains independent of other potential factors that are different from the risk imposed by the work environment and that are connected to the displacement status.

(type of occupation and gender).[16] Indeed, within firms belonging to the same sector and size class, workers in different occupations (i.e., blue- and white-collar workers) face different levels of injury risk. Moreover, within the same occupation, workers of different genders are typically assigned different tasks. To detect the variation of workplace risk irrespective of the possible learning effect (i.e., high hazard rates in the first months of a job) shown in Figure 1, workers' tenure is excluded from the variables defining the strata.

Table 5 reports the effect of displacement on the incidence rate in the first column. The PSM-DID estimate indicates that displaced workers experience riskier job transitions than the matched control group. To assess whether the estimated effect of displacement on injuries reflects transitions to more hazardous occupations, the sample of displaced workers is divided into two groups according to the riskiness of their transitions. The transitions that imply variations in the index (post-displacement minus pre-displacement incidence rates) that are greater than the median variation are considered risky. The effect of displacement on the number of injuries per week worked is estimated separately for these two groups of displaced workers. Table 5 displays the per-year parameter estimates for the two groups in columns 2 and 3. Only those displaced workers who make transitions that are riskier show a statistically significant effect on the number of job-related injuries. The estimated effect for the other group of displaced workers is positive, but the magnitude of the effect is lower and not statistically significant.

[Insert Table 5 about here]

Although compensation for risk cannot be quantified because of the impossibility to observe the full set of non-pecuniary job characteristics and to compare *ceteris paribus* (e.g.,

[16] The sample of non-displaced workers used to calculate the index is comprised of 125,383 workers and 37 million weeks worked. There are 563 strata, and the median number of weeks worked per strata is 159,309.

productivity and tenure) wage and job-safety variations among treated workers, the evidence presented in this paper points to the existence of an income effect. If job safety is a normal good and if displaced individuals experience a loss of earnings potential (for example, because of a loss of firm/industry-specific human capital or of other types of rent), they might reasonably be expected to lose in terms of both job safety and wages. The finding that re-employed workers on average do not experience wage losses but do lose in terms of job safety might be explained by the fact that they earn relatively low pre-displacement wages with respect to the entire set of non-displaced workers and because of downward wage rigidity.[17] On the one hand, the implied average pathway to re-employment for displaced workers is a job that is similar to their pre-displacement job in terms of wages but with a relatively lower level of job-safety. On the other hand, displaced workers who do not re-enter the labour market might be those workers who cannot be profitably re-employed in any firm because their low productivity level cannot be compensated by any increase in injury risk (e.g., because of legal or technological constraints at the firm level).[18]

Tables 6 and 7 report the results on the heterogeneity analysis of the effect of displacement that were obtained by performing the following regressions:

$$Y_{i,t} - Y_{i,1994} = D_i\,\gamma + D_i\,X_{i,1994}\delta + X_{i,1994}\beta + u_{it} \qquad [2]$$

The dependent variables are the variations in weekly earnings and in the injury incidence rate with respect to 1994, for each year after 1994. At each post-displacement year, the sample of re-employed displaced workers is compared to the corresponding matched

[17] According to Dolado el al. (1996), the minimum wage as a fraction of average earnings in Italy is 0.71, which is the highest value across western European countries and the USA during the 1990s.

[18] It is not possible to account for re-employment in the informal sector. Schneider et al. (2000) estimated that the Italian shadow economy constitutes 27% of GDP. According to the segmented labour market theory, job quality tends to be lower in the underground than in the formal sector. Therefore, displaced workers re-employed in the informal sector are likely to lose in terms of both wages and job safety.

controls. The regressors include a dummy indicating whether an individual is displaced (D_i=1, 0 otherwise), a set of pre-displacement workers and firms characteristics, $X_{i,1994}$, and the interaction of the dummy with the set of pre-displacement workers and firms characteristics. These variables, which were measured in 1994, include the incidence rate, weekly earnings, number of employees, gender (the dummy is equal to one for men), tenure and blue-collar job (the dummy is equal to one for blue-collar workers).

The main message of Tables 6 and 7 is that the pre-displacement wage appears to be the most relevant source of treatment effect heterogeneity. Re-employed workers with low pre-displacement wages face greater effects on job safety and lower effects on weekly earnings. This finding is consistent with binding downward wage rigidity; low-wage displaced workers who succeed in becoming re-employed must accept riskier jobs because wages cannot fully adjust. In addition, displaced workers with safer pre-displacement jobs have, ceteris paribus, higher probability to be re-employed in riskier jobs. The same can be said for men, who tend to have access to a broader spectrum of risky jobs than women. Moreover, it is also interesting to note that men tend to have lower weekly earnings losses than women. Finally, type of occupation, age, firm size and tenure are not, ceteris paribus, relevant determinants of the heterogeneity of the effects of job displacement.

[Insert Tables 6 and 7 about here]

4.4 Robustness check

This section analyses the robustness of the previous results to an alternative formulation of the propensity score in which the normal distribution is not assumed and to another method for the estimation of the effect of displacement on displaced workers. The distributional assumption of the error term is relaxed by adopting the semi-nonparametric

(SNP) procedure developed by Gallant and Nychka (1987) and subsequently adapted to the estimation of binary-choice models by Gabler et al. (1993). This method can approximate a broad class of density functions with arbitrary skewness and kurtosis (De Luca, 2008). An alternative estimation method based on the conditional independence assumption is the propensity score weighting (PSW) estimator (Hirano and Imbens, 2001). With this estimation method, a counterfactual for the treated individuals is obtained by weighting the untreated observations with the estimated propensity score. The PSW estimator is applied to the differenced outcomes, i.e., in the computation of the DID estimator, the observations are weighed using the estimated propensity score. As shown by Abadie (2005), the PSW may be combined with the DID estimator leading to an even more robust estimator.

An online supplementary appendix contains the full details of these additional estimation strategies and the corresponding estimates.[19] The main results of the paper remain unchanged.

5 Conclusions

The empirical findings of this paper point toward important implications. Although transitions to riskier tasks do not necessarily entail the creation of new risks and do not necessarily affect welfare in the entire economy (i.e., total accident rates), they may induce a more unequal distribution of risk. A policy maker seeking to reduce the disparities between treated and control individuals might be interested in devoting more attention to policies designed to re-integrate displaced workers into the labour market and policies concerned

[19] This online supplementary appendix also reports the estimated effects of displacement obtained by using: a) the PSM technique, b) the OLS estimator, and c) the OLS-DID estimator. The PSM results are similar to the PSM-DID estimates presented here, whereas the other two estimators tend to find relatively greater earnings losses.

with their job safety. For such policy makers, there is a trade-off between the minimisation of earnings and employment losses and the level of job safety that such individuals experience.

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Figure 1: Monthly injury hazard rate for pooled flows over the 1994-1999 period.

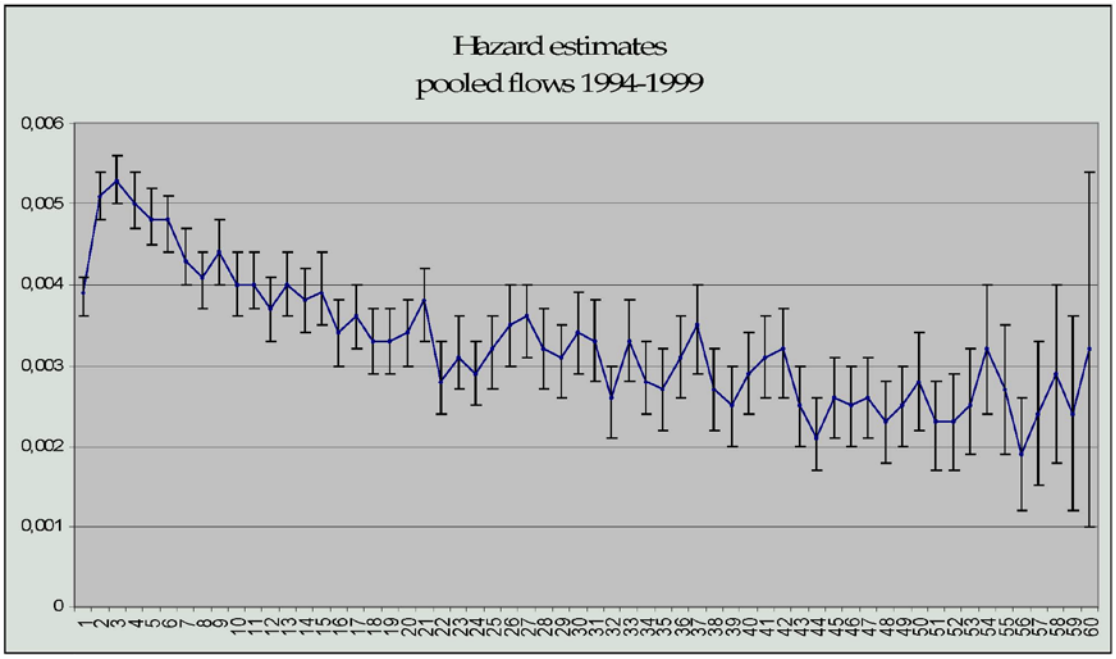

Table 1: Composition of the sample by industry.

Industries	Number of observations before matching	Number of displaced workers as a percentage of the number of controls (before matching)	Percentage of displaced workers retained in the matched sample	Average weight assigned to matched controls
Food, Beverages and Tobacco	1699	2.1	97.1	1.1
Textile, Apparel and Leather	3809	4.8	96.0	1.1
Wood, Paper, Printing and Publishing	1895	2.6	97.9	1.0
Cook, Chemical, Rubber and Plastic	2589	0.9	95.7	1.1
Non-metallic minerals, Metal and metallic products	5540	1.8	96.9	1.0
Machines manufacturing (including vehicles)	6995	1.0	94.1	1.0
Other manufacturing industries	979	2.5	100	1.2
Commerce, Hotels and Restaurants	6513	3.9	93.1	1.1
Transport and communications	3312	0.9	82.8	1.1
Financial intermediation and Business services	6186	1.2	100	1.1
Other community, social and personal service act.	619	5.1	96.7	1.4
All industries	40153	2.2	95.3	1.1

Note: An average weight equal to one means that no control observation has been used more than once. The median difference between the propensity score for the treated individuals and that of the matched controls is 0.000026; its 95th percentile is 0.0031. These values are very low compared to the estimated probability of displacement.

Table 2: Quality of matching

Variables	Sample	1) Mean Treated	2) Mean Control	3) Standardized Bias	4) p>| t |
Sex	U	0.552	0.713	-33.839	0.000
	M	0.550	0.578	-5.986	0.249
Age	U	36.357	39.519	-31.431	0.000
	M	36.382	36.015	3.654	0.456
Tenure	U	8.210	10.103	-58.253	0.000
	M	8.335	8.303	0.990	0.842
Earnings_{1994}	U	139.912	197.277	-41.885	0.000
	M	142.902	140.075	2.064	0.623
$\text{Weeks worked}_{1994}$	U	47.330	49.897	-25.423	0.000
	M	48.270	48.631	-3.579	0.464
$\text{Weekly earnings}_{1994}$	U	2.504	3.307	-49.059	0.000
	M	2.480	2.493	-0.783	0.820
$\text{Average annual earnings}_{1989\text{-}1994}$	U	117.201	175.687	-51.604	0.000
	M	116.680	112.393	3.783	0.293
$\text{Average annual weeks worked}_{1989\text{-}1994}$	U	39.623	44.621	-35.037	0.000
	M	40.521	39.260	8.844	0.154
$\text{Weekly earnings}_{1989\text{-}1994}$	U	2.692	3.424	-45.667	0.000
	M	2.620	2.612	0.525	0.903
Dummy Apprentice	U	0.038	0.009	19.027	0.000
	M	0.037	0.028	5.753	0.329
Dummy Production Worker	U	0.685	0.550	27.978	0.000
	M	0.678	0.660	3.857	0.428
Dummy Basic Non Prod. Worker	U	0.262	0.384	-26.354	0.000
	M	0.269	0.298	-6.406	0.185
Dummy Adv. Non Prod. Worker	U	0.011	0.038	-17.893	0.000
	M	0.011	0.010	0.806	0.808
Dummy Manager	U	0.005	0.018	-12.805	0.003
	M	0.005	0.004	1.161	0.705
Dummy working in North	U	0.502	0.542	-7.918	0.022
	M	0.507	0.490	3.470	0.486
Dummy working in Centre	U	0.298	0.287	2.553	0.459
	M	0.302	0.306	-0.816	0.871
Dummy working in South	U	0.199	0.171	7.186	0.033
	M	0.191	0.204	-3.505	0.492
Dummy born in North	U	0.426	0.449	-4.788	0.169
	M	0.432	0.401	6.237	0.207
Dummy born in Centre	U	0.258	0.256	0.440	0.899
	M	0.265	0.277	-2.830	0.576
Dummy born in South	U	0.274	0.262	2.511	0.466
	M	0.264	0.265	-0.279	0.955
Dummy born in OECD	U	0.007	0.009	-2.215	0.546
	M	0.007	0.014	-6.917	0.223
Dummy born in non-OECD	U	0.033	0.022	7.059	0.023
	M	0.032	0.043	-6.841	0.240
$\text{Number of jobs}_{1994}$	U	1.041	1.025	8.625	0.006
	M	1.038	1.046	-3.963	0.483
$\ln(\text{Firm Employees}_{1994})$	U	2.296	5.132	-126.242	0.000
	M	2.299	2.284	0.625	0.853
$\text{Number of Injuries}_{1994\text{-}96}$	U	0.119	0.118	0.348	0.923
	M	0.123	0.136	-3.555	0.480
N. of years with registered sickness leaves $_{1994\text{-}96}$	U	0.499	0.485	1.696	0.635
	M	0.507	0.498	1.250	0.801
$\text{N. of days on injury leave}_{1994\text{-}96}$	U	2.000	2.372	-2.856	0.472
	M	2.032	2.552	-3.990	0.485
Probability of episodes of	U	0.066	0.087	-7.993	0.030
$\text{"Cassa Integrazione"}_{1994\text{-}96}$	M	0.068	0.059	3.258	0.476

Note: U=unmatched sample; M=matched sample. These statistics are based on the entire sample of workers observed in the year of displacement (see the row “year 0” in Table A.2). The balancing property is satisfied for all the samples of Table A.2.

Table 3: The effect of displacement on annual weeks worked, annual earnings, weekly earnings, number of injuries, number of days on injury leave and number of new jobs.

PSM-DID Time-window	Average annual weeks worked	Average annual earnings	Weekly earnings	Number of injuries per year	Number of days on injury leave per year	Average n. of new jobs per year
Year 0	-12.493***	-32.115***	0.034	0.035**	0.573	0.595***
	(1.110)	(4.470)	(0.119)	(0.015)	(0.511)	(0.025)
Years 1 to 3	-5.274***	-20.835***	-0.076	0.037**	1.346	0.231***
	(1.297)	(5.670)	(0.198)	(0.016)	(1.193)	(0.022)
Years 4 to 6	-3.193**	-12.656*	-0.276	0.027*	1.802*	0.105***
	(1.374)	(6.711)	(0.204)	(0.016)	(0.977)	(0.026)
Years 1 to 6	-3.973***	-17.698***	-0.086	0.026**	0.749	0.177***
	(1.378)	(6.143)	(0.183)	(0.011)	(0.708)	(0.018)

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01. Estimates from Propensity Score Matching Difference-in-Differences. Standard errors (in parentheses) are computed analytically as in Abadie and Imbens (2011). Sample sizes for each time window are described in Table A.2.

Table 4: The effect of displacement on annual weeks worked, annual earnings, number of injuries and number of days on injury leave including non-re-employed workers (missing observations imputed to zeros).

PSM-DID Time-window	Average annual weeks worked	Average annual earnings	Number of injuries per year	Number of days on injury leave per year
Years 1 to 3	-12.473***	- 47.958***	0.034**	1.134
	(1.194)	(5.869)	(0.013)	(0.876)
Years 4 to 6	-7.114***	- 33.266***	0.021*	1.552*
	(1.325)	(6.632)	(0.012)	(0.864)
Years 1 to 6	-9.793***	- 40.612***	0.022**	0.672
	(1.201)	(6.001)	(0.010)	(0.494)

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01. Estimates from Propensity Score Matching Difference-in-Differences. Standard errors (in parentheses) are computed analytically as in Abadie and Imbens (2011). The number of observations used in the analysis is indicated in the first row of Table A.2 (Year 0). Indeed, by the sample definition, during the year of displacement all individuals are present in the sample for at least one week worked. Therefore, the estimates for the year 0 are the same as those presented in Table 3.

Table 5: The effect of displacement on the injury incidence rate and on the number of injuries per year for two subgroups (entire post-displacement period: Years 1 to 6).

	Incidence Rate	Risky Transitions	Non-risky Transitions
PSM-DID	0.003** (0.001)	0.035** (0.016)	0.019 (0.015)

Note: * p-value <0.1, ** p-value<0.05, *** p-value<0.01. Estimates from Propensity Score Matching Difference-in-Differences. Standard errors (in parentheses) are computed analytically as in Abadie and Imbens (2011). The temporal average of the incidence rate is calculated as follows: (Sum of the values corresponding to each main annual job) / (Number of main annual jobs). The number of the main annual jobs corresponds to the number of years that the worker is present in the sample during the considered interval. See the text for the definition of the incidence rate. The number of observations used in the analysis is indicated in the last row of Table A.2 (Years 1 to 6).

Table 6: Heterogeneity analysis of the variations in weekly earnings with respect to year 1994.

			Years						
Δ Weekly earnings	-2	-1	0	1	2	3	4	5	6
Treated	0.006	-0.052	0.038	-0.010	-0.067	-0.019	-0.212	0.020	-0.016
(no interactions)	(0.015)	(0.040)	(0.069)	(0.050)	(0.044)	(0.064)	(0.179)	(0.069)	(0.062)
Treated	-0.049	-0.380**	-0.013	0.279	0.127	0.307	2.052*	0.110	0.175
	(0.069)	(0.189)	(0.322)	(0.231)	(0.209)	(0.312)	(0.886)	(0.352)	(0.317)
Treated*Risk 1994	1.107	-2.363	-0.400	-5.377**	-4.149*	-3.122	2.208	0.771	-3.075
	(0.836)	(2.291)	(3.898)	(2.728)	(2.450)	(3.517)	(9.942)	(3.801)	(3.421)
Treated*Week. earn. 1994	0.059***	0.039	-0.015	-0.210***	-0.209***	-0.266***	-0.564***	-0.171***	-0.171*
	(0.015)	(0.040)	(0.068)	(0.057)	(0.052)	(0.076)	(0.226)	(0.094)	(0.089)
Treated*firm size 1994	-0.001***	-0.000	-0.001	-0.001	-0.001	-0.001	-0.004	-0.002	0.000
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)
Treated*male	-0.067	0.143	-0.106	0.205	0.249**	0.441**	0.207	0.232	0.389**
	(0.041)	(0.113)	(0.193)	(0.139)	(0.122)	(0.176)	(0.497)	(0.191)	(0.171)
Treated*tenure	0.004	0.022	0.021	0.042***	0.024	0.005	0.014	0.036	0.020
	(0.005)	(0.014)	(0.023)	(0.016)	(0.015)	(0.021)	(0.061)	(0.023)	(0.021)
Treated*age	-0.003**	-0.003	0.003	-0.001	0.005	0.008	-0.015	0.002	0.000
	(0.002)	(0.005)	(0.008)	(0.006)	(0.005)	(0.008)	(0.023)	(0.009)	(0.008)
Treated*blue collar	0.016	0.260**	-0.155	0.038	-0.013	-0.120	-0.744	-0.215	-0.062
	(0.045)	(0.124)	(0.211)	(0.152)	(0.135)	(0.196)	(0.553)	(0.214)	(0.191)
Male	0.081***	-0.029	0.146	0.261***	0.259***	0.210*	0.488	0.362***	0.390***
	(0.029)	(0.080)	(0.136)	(0.095)	(0.085)	(0.123)	(0.346)	(0.133)	(0.119)
Age	-0.000	0.000	-0.006	-0.004	-0.008**	-0.012**	0.005	-0.015**	-.016***
	(0.001)	(0.003)	(0.006)	(0.004)	(0.004)	(0.005)	(0.016)	(0.006)	(0.006)
Tenure	-0.004	-0.026***	-0.017	-0.025**	-0.020*	-0.007	-0.010	-0.025	-0.013
	(0.004)	(0.010)	(0.017)	(0.012)	(0.011)	(0.015)	(0.043)	(0.016)	(0.015)
Blue collar	-0.061	-0.291***	-0.139	-0.335***	-0.326***	-0.307**	0.456	-0.443***	-0.359***
	(0.032)	(0.088)	(0.150)	(0.107)	(0.095)	(0.139)	(0.391)	(0.152)	(0.135)
Risk 1994	-0.572	1.893	-0.390	0.492	0.308	0.212	-8.891	-1.015	-1.768
	(0.589)	(1.615)	(2.744)	(1.902)	(1.714)	(2.488)	(7.000)	(2.673)	(2.376)
Weekly earnings 1994	-0.047***	-0.041	0.034	-0.024	-0.055	-0.047	0.367**	-0.102	-0.084
	(0.011)	(0.031)	(0.052)	(0.039)	(0.037)	(0.052)	(0.161)	(0.071)	(0.068)
Firm size 1994	0.001***	0.000	0.001	0.001	0.001*	0.001*	0.005***	0.002***	.002***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.002)	(0.001)	(0.001)
Constant	0.191***	0.524***	0.435**	0.670***	0.916***	0.930***	-0.903	1.408***	1.298***
	(0.048)	(0.133)	(0.226)	(0.160)	(0.146)	(0.219)	(0.624)	(0.252)	(0.226)

Note: The first row reports the effect of job displacement obtained excluding from the explanatory variables the interaction terms. Risk 1994 is the injury incidence rate in 1994. Standard errors in parentheses.

Table 7: Heterogeneity analysis of the variations in injury incidence rates with respect to year 1994.

	Years								
Δ Injury incidence rate	-2	-1	0	1	2	3	4	5	6
Treated	0.000	-0.000	-0.002*	0.003***	0.004***	0.004***	0.004***	0.003**	0.003**
(no interactions)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated	0.000	0.002	-0.001	0.010**	0.011**	0.008	0.007	0.007	0.004
	(0.001)	(0.002)	(0.001)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)
Treated*Risk 1994	0.026	-0.005	-0.144***	-0.202***	-0.216***	-0.137**	-0.137**	-0.191***	-0.184**
	(0.001)	(0.019)	(0.014)	(0.049)	(0.055)	(0.060)	(0.061)	(0.067)	(0.071)
Treated*Week. earn. 1994	0.000	0.000	0.000	-0.002**	-0.002**	-0.002*	-0.004***	-0.003*	-0.002
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Treated*firm size 1994	0.000	0.000*	-0.000***	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*male	-0.001	-0.000	0.005***	0.008***	0.009***	0.008***	0.005	0.006*	0.005
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Treated*tenure	-0.000	-0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*age	0.000	0.000	0.000	0.000	0.000	0.000	0.000*	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated*blue collar	-0.000	0.000	0.004***	0.001	0.003	-0.001	0.000	0.002	0.003
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Male	0.003***	0.003***	0.002***	0.003**	0.005**	0.005**	0.007***	0.009***	0.010***
	(0.001)	(0.000)	(0.000)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age	-0.000	-0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Blue collar	0.004***	0.004***	0.002***	0.004*	0.004*	0.007***	0.007***	0.008***	0.009***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Risk 1994	-0.099***	-0.101***	-0.046***	-0.111***	-0.125**	-0.198***	-0.211***	-0.254***	-0.278***
	(0.001)	(0.013)	(0.010)	(0.034)	(0.038)	(0.042)	(0.043)	(0.047)	(0.049)
Weekly earnings 1994	0.0000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Firm size 1994	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	-0.002	-0.003	-0.001	0.000	0.001	0.002	0.002	0.005	0.004
	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)

Note: The first row reports the effect of job displacement obtained excluding from the explanatory variables the interaction terms. Risk 1994 is the injury incidence rate in 1994. Standard errors in parentheses.

Appendix

Table A.1: The effect of displacement on the following probabilities: to retire, to receive Unemployment benefits, to receive *Mobilitá*, to have an Atypical Contract (quasi-dependent employment), to be Self-Employed, to be an employee and to be non-employed. Means and PSM estimates.

years	0	1	2	3	4	5	6
Retirement T	0.022	0.025	0.032	0.035	0.037	0.037	0.037
Retirement C	0.022	0.024	0.031	0.033	0.037	0.040	0.041
Diff	0.000	0.001	0.001	0.001	0.000	-0.002	-0.004
UB T	0.204	0.125	0.048	0.038	0.027	0.028	0.038
UB C	0.046	0.051	0.040	0.027	0.030	0.031	0.048
Diff	**0.158**	**0.074**	0.009	0.011	-0.002	-0.002	-0.010
Mobilità T	0.099	0.098	0.066	0.033	0.017	0.016	0.017
Mobilità C	0.001	0.002	0.006	0.006	0.004	0.004	0.004
Diff	**0.098**	**0.095**	**0.059**	**0.027**	**0.014**	*0.012*	*0.014*
Atypical Contracts T	0.014	0.026	0.030	0.024	0.025	0.028	0.037
Atypical Contracts C	0.012	0.012	0.015	0.021	0.020	0.022	0.028
Diff	0.001	*0.014*	*0.015*	0.002	0.005	0.006	0.009
Self-Employment T	0.001	0.000	0.000	0.000	0.000	0.000	0.000
Self-Employment C	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Diff	0.001	0.000	0.000	0.000	0.000	0.000	0.000
INPS T	1.000	0.679	0.696	0.681	0.676	0.665	0.652
INPS C	1.000	0.944	0.863	0.813	0.782	0.759	0.733
Diff	0.000	**-0.265**	**-0.167**	**-0.132**	**-0.106**	**-0.094**	**-0.080**
Non-Employment T	0.000	0.171	0.193	0.217	0.223	0.230	0.235
Non-Employment C	0.000	0.037	0.080	0.116	0.127	0.144	0.155
Diff	0.000	**0.134**	**0.113**	**0.100**	**0.095**	**0.087**	**0.080**

Note: Standard errors of PSM are computed analytically as in Abadie and Imbens (2011). The means for the treated (T) and control groups (C) are calculated by using the matched sample. The corresponding differences between means (Diff) represent the effects of displacement estimated by applying the Propensity Score Matching technique. The differences in bold are significant at 0.001 level. The differences in italics are significant at 0.100 level. The other differences are not statistically significant. The number of observations used in the analysis is indicated in the first row of Table A.2 (by the sample definition, during the year of displacement all individuals are present in the sample for at least one week worked). *Mobilità* is an Italian labour market policy comparable to Unemployment Benefits (UB). The main differences between these two policies are the following: 1) eligibility rules (*Mobilità* is more selective); 2) the replacement rates (for *Mobilità* 80% the first year and 60% the following years, for UI about 30%); 3) the maximum duration (for UB is six months, for *Mobilità* between one year and four years depending on age). INPS is the probability to be employed in the private or public sector; Non-employment is the probability to be absent from all the administrative data (i.e., not to be in the states mentioned above). Non-employment might include cases of death, emigration and employment in the shadow economy.

Table A.2: Number of observations for the samples used in Tables 3-10.

		All unmatched sample			Matched sample	
Years	Treated	Non displaced	Total	Treated	Controls	Total
0: year of displacement	848	39305	40153	808	746	1554
Years 1 to 3	659	37391	38050	624	592	1216
Years 4 to 6	608	32407	33015	577	542	1119
Years 1 to 6	687	37515	38202	652	613	1265