Studying the impact of job mobility on wage growth at the beginning of the employment career in Spain.

Maria A. Davia
Universidad de Castilla La Mancha
Facultad CCEE y EE
Plaza de la Universidad s/n
02071
tel: 967599200 (ext 2380)
fax: 967599220
e-mail: mangeles.davia@uclm.es

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Abstract

The beginning of the employment career is very often defined by a period of intensive job mobility which influences a path of wage progression and may have an important impact on individual future income. We aim at measuring the impact of different types of job moves on the subsequent hourly wage and identifying the factors that make a transition across jobs successful or, on the contrary, which circumstances may have scarring effects on subsequent wages. For that purpose we use the Spanish section of the European Community Household Panel and work on a sample of youths. We use Difference-in-Differences Propensity Score Matching techniques to explore the impact of direct and indirect, voluntary and involuntary job mobility on subsequent wages during the nineties. We observe not only a positive impact of both direct and voluntary moves but also a non scarring effect of both involuntary and indirect job moves.

Keywords: job mobility, wage mobility, propensity score matching

JEL codes: J31, J63

RESUMEN

El inicio de la vida laboral es un periodo de intensa movilidad laboral que puede influir las trayectorias de progresión salarial en el futuro. Pretendemos aquí medir el impacto de distintos tipos de movilidad laboral sobre el salario, así como identificar los factores que convierten a una transición entre empleos en exitosa o, al contrario, las circunstancias que pueden tener efectos estigmatizadores sobre los salarios futuros. Para ello usamos la versión española del Panel de Hogares de la Unión Europea y trabajamos sobre una muestra de jóvenes. La técnica utilizada es Propensity Score Matching combinado con Diferencias en Diferencias para explorar el impacto de la movilidad directa, indirecta, voluntaria e involuntaria sobre los salarios. Observamos un impacto positivo de la movilidad directa y la voluntaria, y no detectamos un efecto estigmatizador de la movilidad involuntaria y la que se produce a través del desempleo.

Palabras clave: movilidad laboral, movilidad salarial, propensity score matching.

Clasificación JEL: J31, J63

0. Introduction

The impact of job mobility on wages is one of the most relevant and controversial issues in labour economics. In most of the theoretical approaches (human capital, job search and job-matching as well as the career mobility models) youths are supposed to get positive wage gains from mobility, which is voluntary. Hence, in much of the seminal literature on the topic, initial job mobility is referred to as "job shopping" (Johnson, 1978). Yet in many countries young people do not always move voluntarily across jobs. This holds particularly true in Spain. Given the high temporality rates in Spain during the nineties, Spanish youths bear with the highest rates of job turnover across the European Union, and are severely affected by unemployment and involuntary job interruptions. This should make them very vulnerable towards movements. Moreover, it has been often argued that an excessive job turnover at the beginning of employment careers may seriously damage labour market outcomes in the mid or long term.

This piece of work is aimed at studying the rewards to different types of job mobility at the beginning of the employment career in Spain. To that aim, a sub-sample of young people (under 25 in 1994) has been drawn from the European Community Household Panel. Since endogeneity in the decision to move and unobserved heterogeneity are very relevant issues in the job mobility and wage mobility literature, the selected empirical strategy will consist of difference-in-differences propensity score matching (DID-PSM).

Results show that, after observed and unobserved heterogeneity is controlled for and when we compare stayers with their more similar mover counterparts, returns to recent mobility are quite positive. When observing just one move in our observation window we find positive significant effects of both direct and voluntary moves for both young men and young women, and no scarring effects of neither involuntary non unemployment. Nevertheless, our initial descriptive analysis shows as job moves become frequent they do not generate positive results any longer, so that our very optimistic results should be tingled under this evidence.

The contents of the paper are displayed as follows: Section 1 surveys the main theoretical approaches to the relation between job mobility and wage dynamics and a discussion of the expected effects of different institutional frameworks. Section 2 is used to survey empirical evidence and methodological problems. After that, the data base is presented in Section 3; Section 4 is devoted to some basic descriptive analysis about the link between job mobility and wage dynamics. Section 5 displays the empirical strategy. Section 6 shows the main results from the propensity score matching and finally some conclusions are drawn from those results.

1. Job mobility and wage mobility, a theoretical survey

The connection between job mobility and wage mobility has driven much attention in the empirical literature in the recent years. In most cases, research has focused on the scarring effect of unemployment spells on the subsequent jobs and job careers, having the effects of both layoffs and quits been compared. Moreover, since tenure or seniority is the flip side of the coin of job mobility, the effect of mobility on wages is often studied from the perspective of tenure, that is, stability in employment. We will essentially focus on the first type of contributions for the sake of concreteness, though.

In this section, we intend to offer a broad picture of what the main issues from a theoretical point of view are in the study of the link between job mobility and wage mobility. We leave for the next section the review of some methodological issues and previous empirical evidence on the topic.

The first approach to the problem was given by Blumen et al, (1955) with a hypothesis on workers being inherently movers or stayers. Movers are less stable and less productive workers, and this ends up in lower wages. Thus, the problem gets reduced to a matter of individual heterogeneity.

Human capital models point at the investment in employer-specific human capital, part of which is not transferable (Becker, 1962; Parsons, 1972; Hashimoto, 1981). Through on-the-job experience and any formal training, workers accumulate firm-specific skills which, as they rise with tenure in the firm, rise wages as well. This reduces profitability of job mobility: if specific skills on the job rise faster than general skills, the probability that an outside offer exceeds the worker's own wage (adjusted for the cost of movement) declines with tenure. Therefore, if firm-specific skills are an important determinant of earnings, then movers are likely to experience earning losses.

In this context, willingness in movement makes a difference as regards expected wage effects. In most cases, involuntary movements are also featured by a spell of unemployment, and there are several hypotheses (rooted in human capital theory) that point at a wage loss by displaced employees, which is often labelled in the related literature as "wage scar" of unemployment. On the one hand, there is a risk of deterioration of human capital and skills with time. This would erode re-employment chances and re-employment wages as a result of a decrease in productivity. On the other hand, if employers take past unemployment experience as a signal of productivity (Vishwanath, 1989; Pissarides, 1992), they may develop a practice of statistical discrimination against workers who have gone through unemployment spells and offer them lower wages[1].

In a very different vein, internal labour markets and segmentation hypotheses (Doeringer and Piore, 1971; Edwards, 1975) would predict different effects of mobility according to the direction of the movement: if from the external/secondary labour market into the internal/primary one, wages should experience an increase as a result of mobility; the same applies to voluntary movements between primary jobs; and the opposite holds true for movements from internal/primary segments out to the external/secondary, which are, no wonder, involuntary.

Job matching models would predict a positive effect of job mobility on wages: workers will quit jobs in the search for better matches (Jovanovic, 1979a); should they succeed in their search, wages will be higher in the new jobs. Moreover, workers explore their own productivity and will tend to quit those jobs where they do not experience strong increases in productivity with tenure and, therefore, more stable matches will be a signal of productivity and should be paid in accordance (Jovanovic, 1979b). Should a worker experience a separation, if her following match is better than the previous, her wage will be higher. None of these arguments mean that wages for movers will be higher than those for workers in stable positions. They predict a steeper age-wage profile, but not necessarily final higher wages amongst movers. This conclusions can also be applied to the training approach considered by Mortensen (1988) and to the job-shopping theory (Stigler, 1962) according to which younger

[1] There are other possibilities, though. Antel (1991) shows that unemployment spells amongst voluntary movers are a way to look more intensively for a job and allow for better matches as a result of the search process. The alternative hypothesis is that unemployed workers search intensity may be weaker as links to the labour market while in unemployment tend to fade.

workers are more likely to try a variety of jobs to acquire knowledge about the labour market and their own tastes, being age and job tenure positively correlated.

Job search models suggest that voluntary mobility will generate positive wage gains. According to Burdett (1978), if the cost of off-the-job search is higher than the one on-the job, unemployed workers will adopt two reservation wages during search. In this case, jobs offering more than the low reservation wage (while unemployed) but not as much as the high reservation wage (while employed) will be accepted, but the worker will continue searching on the job after acceptance. This implies that on-the-job searchers will be looking for better wage offers and will not move until a better position is found.

A more recent approach that roots in job matching and search theories would also expect higher wage increases from movers than from stayers as a result of simultaneous options for occupational mobility across jobs and employers (Sicherman and Galor, 1990). Other views (contract models and models where employers poach for good workers) would also forecast wage increases as a result of mobility, but this time they would be demand-driven: firms poach the best employees (Lazear 1986). Therefore, mobility in those models is a signal for productivity and generates automatic wage gains.

And last but not least, although one change across employers may be expected to provide better employment conditions, when movements are cumulative, the initial profits may vanish, and scarring effects may be stronger if separations are employer-initiated (Keith and McWilliams, 1995; Stevens, 1997). Indeed, initially positive effects turn negative when mobility is seen not as a discrete past decision but as a cumulative process (Munasinghe and Sigman, 2004).

None of these approaches may fully describe the link between mobility and wage dynamics and they are observationally equivalent as regards the duration of workers-jobs matches. They complement each other, given that neither job search or human capital alternatives alone may explain the link between job and wage mobility. Empirical evidence is vital to disentangle which approach fits reality better.

2. Job mobility and wage mobility: empirical evidence and methodological issues.

When tackling the effect of job mobility on wage dynamics a first concern is the endogenous nature of the main explanatory variable, job mobility (or job tenure, the other flip side of the coin), which cause bias in the OLS estimators in a typical Mincerian wage equation. There are several strategies for overcoming this problem. One of the most widespread ones is instrumental variables approach, as an alternative to the simultaneous resolution of equation systems such in switching endogenous models. The former has been heavily used in empirical literature, particularly in the study of the effects of tenure on wages. Althonji and Shakotko (1987) and Topel (1991) developed smart techniques that remove the effects of correlation between wages and job duration without trying to estimate the extent of correlation, via instrumental variables (the former) or two-step procedures for differentiating between returns to experience and tenure (the latter). The same idea was used afterwards by Light and McGarry (1998), Topel (2001), Perticara (2002), Le Grand and Tåhlin (2002), Lefranc (2003), Dustman and Pereira (2003) and Naticchioni and Panigo (2004) amongst many others.

Attempts to integrate the search for an explanation to job mobility and wage mobility come from Flinn (1986), who considers simultaneously job turnover processes and wage growth in the study of labour market experiences of young people by developing a discrete time version of Jovanovic's job-match model. Antel (1991) uses mobility choice dummies that are determined by a probit function by to assure consistent estimates of the effect of job mobility on wages. Another very well-known piece of evidence is Topel and Ward (1992). The authors analyse both the effect of past job mobility on current wages and past wages growth on current decisions of job mobility. Perticara (2002) evaluates a hazard model for both voluntary and involuntary job separations.

More sophisticated strategies have been developed by Lillard (1999) and Abowd and Kang (2002). The proposal of Lillard tries to encounter simultaneously for job mobility and wage mobility through a multilevel estimation with three levels of sources of unobserved heterogeneity: the individual level, and employer level and the job (employer-employee match) level. He models job turnover and job duration in continuous time jointly with the wage time series for that job. And finally, Abowd and Kang (2002) resume and revise the results of the three aforementioned papers (namely, Altonji and Shakotko (1987), Topel (1991) and Lilliard (1999)) in a new simultaneous estimation of wages and tenure.

The second key methodological issue deals with unobserved heterogeneity, which is another problem that questions the causal nature of the link between job mobility and wage mobility. If we were to accept the hypothesis that some individuals are just more prone than others to be mobile and this wanderlust results in a lower productivity, then we should accept that, after controlling for unobserved heterogeneity amongst individuals should cancel significance of the variables reporting mobility. If, on the contrary, control for unobserved heterogeneity does not cancel explanatory power of mobility, we should accept a causal link between both variables. Anyway, even if we accept that there is a causal link between them, the rationale behind this relation, this is, the causal mechanism, is a third issue to be tackled. This is beyond the scope of this paper[2].

The most heavily deployed techniques to control for unobserved heterogeneity are fixed effects estimations, recent examples being Light and McGarry (1998), Arulampalam (2001), Gregory and Roberts (2001), Le Grand and Tåhlin (2002), Naticchioni and Panigo (2004) and Munasinghe and Sigman (2004). The idea is to observe not wage levels but the relative distance between wage levels in a given moment and the average across the period of observation for each individual instead of taking the previous wave as a "mobile" reference period. It is therefore a proxy for a before-after estimator.

Empirical evidence on job mobility focuses on youth very often, since it is during the early stages in the working life that workers experience more intensive job mobility and wage mobility. The availability of data-sets on early careers have also contributed to this. The seminal pieces of evidence as Bartel and Borjas (1978, 1981) observe higher mobility returns amongst youths, as well as Mincer (1986). Later on Antel (1991), Topel and Ward (1992), Light and McGarry (1998) and Perticara (2002) focus only on youths, whereas Le Grand and Tåhlin (2002) comprise young adults, aged 26 to 35, when both job and wage mobility are crucial.

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2 The causal mechanism in the job mobility-wage mobility relationship may come from either job search, job match and/or specific human capital considerations. The three arguments are not self exclusionary and it is difficult to disentangle which is the explanatory power of each of them. Generally researchers deploy information about the three possible explanatory factors: for instance, tenure in previous jobs is often used as a proxy for specific human capital accumulated in former jobs, satisfaction and wages in former jobs may be used as proxy for initial quality of the previous job match, and job search intentions should play the same role as regards job search strategies.

Very often authors deal with voluntary and involuntary mobility at the same time, such as Bartel and Borjas (1978), Mincer (1986), Perticara (2002) and Munasinghe and Sigman (2004) although sometimes this distinction cannot be done or attention is simply driven towards unemployment spells after dismissals, such as Arulampalam (2001), Gregory and Roberts (2001) and Lefranc (2003), who study the scarring effect of unemployment, and Antel (1991), who finds out positive effects of unemployment on the basis that young unemployed search more intensively for jobs than those who register job interruptions but no unemployment.

Wage and job mobility has been explored in a large amount of countries: U.S. (Bartel and Borjas (1978, 1981), Topel and Ward (1992), Light and McGarry (1998), Perticara (2002), Munasinghe and Sigman (2004) among many others) U.K (Campbell (2001), Dustman and Pereira (2003)) and Germany (Dustman and Pereira, 2003) have been more studied than other cases due to the existence of longitudinal surveys such as BHPS and GSOEP. Exceptionally other countries have been provided with employee-employer longitudinal data-sets, such as Italy (Contini *et al* (2004), Natichioni and Pagani (2004) and France (Lefranc, 2003). The Spanish case has been studied in comparative pieces of work (Davia (2005), Arranz, Davia and García-Serrano (2005) and García Pérez and Rebollo Sanz, 2005).

Here we contribute to the literature with a new empirical strategy with which we aim at dealing with both the endogeneity and the unobserved heterogeneity, namely differences-in-differences propensity score matching and focus on the Spanish case. We have found only two pieces of research that use this technique to study wage growth: Gash and McGinnity (2005) and Ham, Li and Reagan (2005). The former compare the wages, wage growth and labour market outcomes of fixed term contract workers relative to a matched sample of permanent workers with similar characteristics in Germany and France. The latter apply PSM to measure the effect of internal job migration on subsequent wages of men in the U.S.

3. *The data base: the European Community Household Panel*

In order to gain evidence on both determinants of and rewards to job mobility, the data-base used here will be the European Community Household Panel (hereinafter, ECHP). This survey gathers information on several socio-economic aspects in the European Union, being labour market related issues one of the most important fields considered in the survey. This data-base, produced by *Eurostat*, has two very important features which make it particularly interesting and useful for the study of labour market dynamics in Spain: it is longitudinal in nature and the information about the job is extraordinarily rich. In this version of the paper we do not take advantage of the comparative nature of the survey, but it is one of the points in the future research agenda.

As for the type of information we will need in our analysis, the ECHP is provided with information on characteristics of jobs such as occupation, industry, size of the firm, public or private employer, monthly (both gross and net) wage and length of working week, among many others. It is also possible to estimate tenure at the moment of the interview from the distance between the date of the interview and the beginning of the relation with the employer; and hourly net wage can be imputed from the working week and the monthly salary.

As for our main explanatory variable, there is no explicit question in the survey about recent changes across employers. Therefore, job mobility is detected when an individual who was employed in t, reports in t +1 a shorter tenure than the distance between the date of the interview in t and the following interview in t +1[3]. Tenure is computed from the date of the beginning of the relationship with the current employer. Should an interviewee sign several contracts in a row with the same employer s/he would report the date when the labour relationship began and therefore no job change would be registered.

Every employed person will report as well whether s/he experienced unemployment before accessing the current job and why s/he left the previous one. This information for job movers will derive in more complete variables combining movements across jobs with willingness in job mobility with the presence of unemployment spells between jobs. This will allow us to distinguish between voluntary and involuntary, direct and indirect moves detected since 1995[4] to the current interview. We are aware that our way of computing job mobility may

[3] For instance, if two interviews are in subsequent years, a job move is detected when the individual is employed in both interviews but in the second one his/her tenure in the current job is less or equal to one year, meaning that there has been necessarily an interruption in the employment relation between the two interviews. Should tenure in the second interview be longer than in the first interview, we would understand that no job change has taken place.

[4] We start the analysis in wave 2 because wave 1 has no information on the type of contract, which is a very relevant variable to define the probability to leave a job.

underestimate it, since whenever more than one movement occurs between two subsequent interviews, only one is computed.

As regards the chosen sample, we are observing workers who were under 25 years old in 1994 and register at least two positive wages during the period 1995-2001. Otherwise wage increases would not be observed. We tried in previous specifications to select only those in their first jobs at the beginning of the observation period so that we really observe the impact of the first job move, but this implied a very strong reduction of the sample size. This enables us to work with a relatively homogeneous and very mobile sample of workers, which is exactly what we want in order to match "equal with equal" in the empirical strategy presented in section 5.

The main characteristics of the sample are displayed in table 1A which gathers descriptive values of several explanatory variables. We may observe that young Spanish women are more educated than men and, accordingly, register a higher International Socio-Economic Index of occupational status. Women are as well more often employed at the public sector and have specific training previous to current jobs than their male counterparts. Anyway, men register initially higher hourly wages and work more hours per week than women.

4. Some first evidence on wages and job mobility

This first section aims at giving a flavour of how frequent job mobility is and how large wage growth is amongst the different types of young movers in Spain. Table 1 shows the main year in year transition rates from employment, split in two types of movements. On the one hand, during the period of observation (1995-2001) around 20% of all observations in employment change to a different position every year. More than half of the movers are involuntary movers, namely, are those who, in their subsequent job, report having left the previous job obliged by the employer, because of the end of a temporary contract or for family reasons[5]. On the other hand, those

[5] The latter is arguable, but as long as it is not due to a personal choice like the one women make when a better job is available, and as long as the wage increase for women who move because of family reasons is the lowest for job movers, we consider it plausible than family reasons are not a voluntary option.

who move voluntarily are those who move because a better job has been achieved or for other reasons[6].

Table 1.

Year in year transition rates, wage increases and wage increase in the first move						
	No move	Voluntary move	Involuntary move	No move	Direct move	Indirect move
Year in year transition rates				Year in year transition rates		
men	0.80	0.09	0.11	0.78	0.12	0.10
women	0.83	0.06	0.11	0.80	0.09	0.11
Wage increase				Wage increase		
men	0.04	0.27	0.20	0.05	0.23	0.26
women	0.05	0.22	0.23	0.05	0.21	0.23
Wage increase after the first movement				Wage increase after the first movement		
male	0.07	0.30	0.27	0.07	0.30	0.34
female	0.07	0.30	0.28	0.08	0.33	0.32

Source: ECHP (1995-2001)

Net real hourly wage increase for those who leave willingly their jobs is, in average, during the period, 27% for women and 22 % for men. As for direct movements versus indirect ones, interestingly, we see that nearly half of the moves imply a transition through unemployment, and that, in the case of young workers, unemployment does not seem to have a negative impact on wages. On the contrary, there is hardly any difference between workers who move directly and those who move through unemployment. This may hide a wide variety of situations, from those who actually lose in their wages to those who invest in information during their period of unemployment and, therefore, end up with a better job than the one they lost/quitted. These straightforward comparisons may be biased as long as they are not comparing equivalent individuals but the average wage growth of individuals who may be pretty different.

Moreover, we have checked not all movements across jobs have the same impact. There is a positive impact of mobility when workers are observed moving across jobs for the first time, but this initially positive effect is decreasing the more movements

[6] This choice is also arguable, but the alternatives explanations to “other reasons” we may think about are, for instance, conflicts with the employer or co-workers or low satisfaction with some aspects of the job which force the worker to quit the job and look for a different alternative. This type or situations end up in extraordinarily large wage increases, so that we may infer than the new job has advantages compared to the former and, therefore, the movement has been voluntary.

we observe. This holds true for both voluntary and involuntary, direct and indirect movements, as Figures 1 to 4 show.

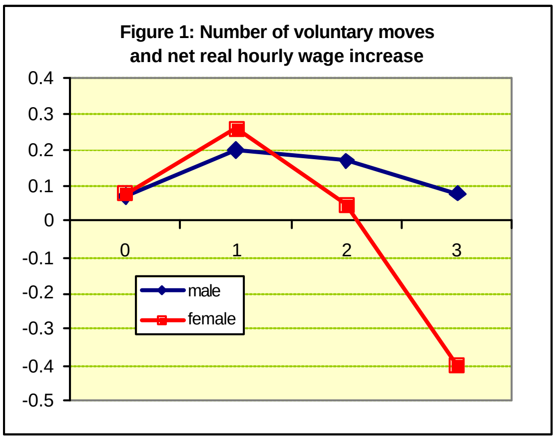

Figure 1: Number of voluntary moves and net real hourly wage increase

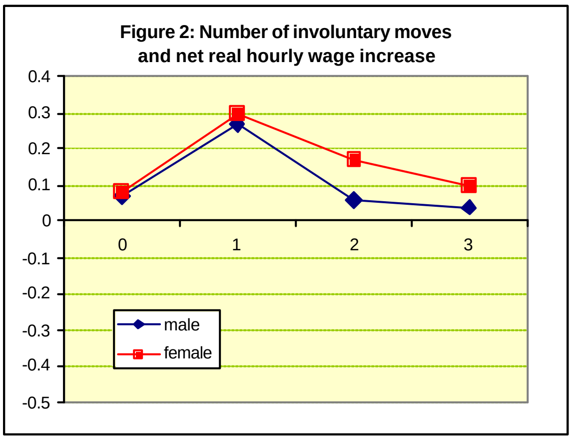

Figure 2: Number of involuntary moves and net real hourly wage increase

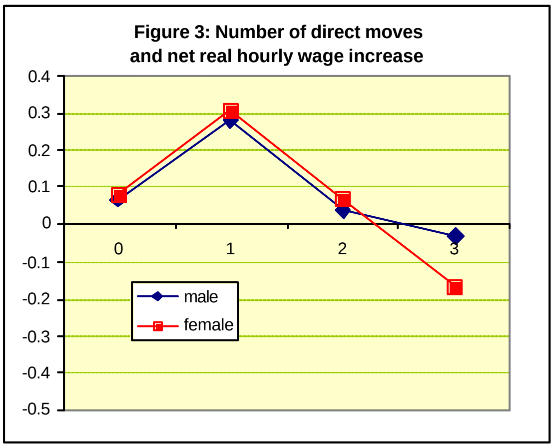

Figure 3: Number of direct moves and net real hourly wage increase

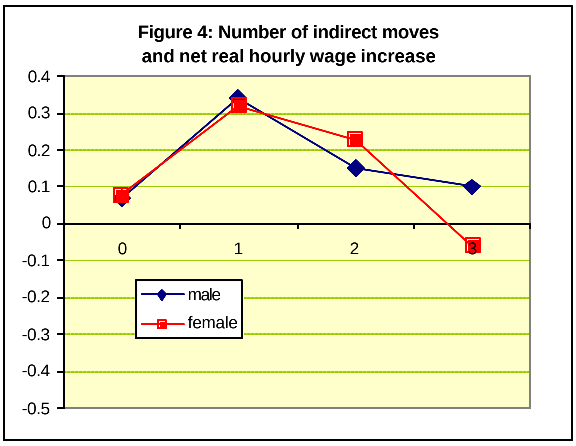

Figure 4: Number of indirect moves and net real hourly wage increase

Source: ECHP (1995-2001), Eurostat.

This has a very relevant impact in our research: should we analyse all the movements we register regardless their order, we would be giving a misleading figure or result of the overall impact of job mobility. That is why we have decided to analyse only the first movement we observe, and not all of them, keeping in mind that the effect we measure is the most optimistic scenario, and that in the case of multiple movers the situation would be different. Unfortunately the sample sizes of multiple movers are very small, so that we have not been able to reproduce the analysis of first moves for second moves and third moves. Nevertheless, Figures 1 to 4 show very clearly that the impact of job mobility decreases when movements are more frequent.

5. The empirical strategy

Why using propensity score matching?

From a practical the point of view, Job mobility is a typical problem of endogeneity in the main explanatory variable, together with unobserved heterogeneity. Both problems may be overcome with the use of Differences in Differences Propensity Score Matching.

Let us start with endogeneity. Job mobility may generate wage mobility precisely because it is driven by the desire of wage growth. Moreover, there are given features that may influence job mobility and wage mobility at the same time, namely, productivity or motivation. One of the more standard ways of dealing with endogeneity is using instrumental variables. The underlying identification strategy in the IV approach in our case is to find a variable which determines job mobility but does not influence the wage growth. The instrumental variable affects the observed outcome indirectly through the decision to move across jobs and hence causal effects can be identified through a variation in this instrumental variable (Caliendo and Hujer (2005)). The main problem with instrumental variables is that it is very difficult to find a good instrument. Needless to say, and given the different theoretical frameworks that contribute to explain the job mobility - wage mobility relationship, it is frankly difficult to find such a variable, particularly in the context of young people[7].

One alternative is Propensity Score Matching. We will deal with job moves as if they were different "treatments" workers receive in order to maximize the outflow of income during their employment careers. If workers moved randomly across jobs, we would get an unbiased estimate of the average effect on the treatment (movement) if we compared movers with stayers. But workers do not move randomly and we have to simulate random assignment to different types of move. In order to simulate such an experiment we divide the sample in different groups according to the type of mobility across jobs they have experienced and try to find another group in the population which is very similar to this group in an (as exhaustive as possible)

[7] Some pieces of work related to samples of adults use home ownership as an instrumental variable for job mobility, but in the case of youths (and, mode particularly, in the case of Spain) this variable does not register enough variability.

number of (observable) variables. We compare individuals who are equal to each other except for the fact that the type of job mobility they have experienced (if any) is different. One way of achieving this is matching.

In short, matching involves pairing together individuals from various treatment groups who are similar in terms of their observable characteristics. When selection into treatment is exclusively based on these observable characteristics, matching on them yields unbiased estimates of the average treatment effects. Consequently, if each individual moving across jobs can be matched with an individual with the same matching variables who does not move across jobs, the impact of job mobility on job movers can be measured. One of the weak points of this technique is that it is a matter of prior assumption as to whether the appropriate matching variables have been chosen. If not, the counterfactual effect will not be correctly measured.

Matching methods were developed in the context of biostatistics (Rosenbaum and Rubin 1983) and have been adopted by Economics who use them heavily in the context of labour market policy evaluation. They have spread to study many other issues, and there are countless papers in the literature applying this technique to the most varied problems. One shortcoming of PSM compared to instrumental variables is matching only relies on observable characteristics (Caliendo and Hujer, 2005) whereas IV account for selection on unobservables. But this is overcome in our case because we are actually using difference-in-differences propensity score matching. We will explain this nuance later in this section.

The evaluation problem in a multi treatment context

This paper compares the outcome of three options at a time: direct/voluntary moves versus indirect/involuntary moves. In terms of an evaluation problem it would be labelled a "multiple treatment problem". This type of problems have received less attention than the usual single treatment case, but it is not much more complicated than that. Pioneering works in multiple treatment are Lechner (2001) and Imbens (2000) who came up with the same way to deal with this problem. Larsson (2003) is an application of Lechner (2001). In what follows we just use Lechner (2001) and Larsson (2003) to present the multiple evaluation problem:

Consider participation in (M+1) mutually exclusive treatment, denoted by an assignment indicator $T \in \{0, 1, \ldots, m\}$. We will call "treatment" to each type of movement across jobs. Therefore, the "0" category indicates the lack of movement across jobs, 1 denotes direct moves across jobs/voluntary moves and 2 denotes moves across unemployment/involuntary moves. The set of variables that may define the probability of each treatment are called *covariates* and are designated by X. The outcomes of the treatments (the increase in wages from $t = 0$ to $t = 1$) are denoted by $\{Y^0, Y^1, \ldots, Y^M\}$. For any mover only wages before and after moving are observable. For stayers only wages in $t = 0$ and $t = 1$ in the same job are observable. We do not know neither what the increase in wages could have been for stayers had they moved nor what the increase in wages could have been for movers had they stayed with their initial employers. This means that for $m = 1$ (voluntary or direct moves) only Y^1 is observed. That is why the remaining M outcomes are called counterfactuals. The number of observations in the population is N, such that $N = \sum_{m=0}^{M} N^m$

where N^m is the number of participants in treatment m. With this we want to stress that the possibilities of treatment/job movement observed are exhaustive. Every individual in the sample participates in some sort of (non) job mobility.

The evaluation problem consist on defining the effect of treatment m compared to the treatment l, for all combinations of $m, l \in \{0, 1, \ldots, M\}$, m ? l[8]. We want to compute the so called *average treatment on the treated (ATT)* in evaluation literature. The ATT may be presented as follows:

(1) $?_0^{ml} = E(Y^m - Y^l \mid T=m) = E(Y^m \mid T = m) - E(Y^l \mid T=m)$,

$?_0^{ml}$ in equation (1) denotes the expected average treatment effect on the treatment m relative to treatment l for participants in treatment m (sample size N^m). In the binary case, where $m = 1$ and $l = 0$, this is normally called the "treatment-on-the-treated" effect. It translates in our particular problem to the effect of job mobility on the wage

[8] That is why we compare the wage growth for direct job movers against job keepers and against indirect job movers, and the wage growth for voluntary job movers against job keepers and against involuntary job movers.

growth of job movers minus wage growth for movers if they had not moved across jobs (the counterfactual). As Larsson (2003) notes, average treatment effect is not symmetric in the sense that $?_0^{ml}$? - $?_0^{lm}$. This is due to the fact that participants in treatments *m* and *l* differ in a non-random way.

Most of all the literature on evaluation tries to overcome the problem stemming from the fact that one cannot observe the counterfactual $E(Y^l|T=m)$ for *m ? l*, since it is impossible to observe the same individual in several states at the same time. This means that the true causal effect of a treatment *m* relative to treatment *l* can never be identified. However, the average causal effects defined by equation (1) can be identified under a given assumption, called the conditional independence assumption (CIA).

The CIA consists on assuming that the selection between the groups of participants in treatment *m* (say, job movers) and treatment *l* (say, job stayers) are captured by a vector of observable characteristics, *X*. In order to accept the CIA the researcher needs a fairly rich data set where he may claim that is controlling for all the factors that influence each type of treatment. We think this is our case, given the exhaustive nature of the vector of variables characterising jobs in the European Community Household Panel. We are able to control not only for observed objective characteristics (age, initial wage, type of employer, industry, occupation status measured by International Socio Economic Index (ISEI), level of education, experience, tenure in current job, type of contract, training and health care paid by the employer, specific training previous to current job, previous unemployment, initial overeducation, being in first job, being looking for another job) but also subjective ones (namely, satisfaction with wage, with type of job, with employment stability, with working hours, with working times, working conditions/environment, distance to job, leisure, economic situation, and main activity). In total, our X vector consists on 27 characteristics (see table 1 for a simple descriptive of mean values).

A formalization of the CIA assumption in the multiple is available in Larsson (2003), as well as the identification problem. We omit them here for the sake of brevity. We will just mention that the identification of the average treatment effect on the treated, $?_0^{ml,}$, requires the estimation of the conditional choice probability of treatment *l* given

either treatment m or l (what we call propensity score). The ATT will is no more than the expected outcome of each type of movement conditional to the propensity score.
For a more detailed description of the identification of both $?_0^{ml}$ the reader should see Imbens (2000) and Lechner (2001). The estimation procedure is described at Lechner (2001) and Larsson (2003).

Matching and Differences in Differences

The Conditional Independence Assumption (CIA) is very strong. It is very difficult to assert that by controlling for a set of observables we are actually explaining all the factors behind the decision to move across jobs or the determinants of movements across jobs. Moreover, it is difficult to explain moves across jobs in such a perfect way that we do get rid of the risk of having individuals making decisions on the basis of future rewards. However, by combining matching with differences in Differences (DID) there is scope for an unobserved determinant of participation as long as it lies on separable individual and/or time specific components of the error term (Blundell and Costa Dias, 2002). Since DID effectively controls for the other components of the outcomes under non treatment, only the temporary individual-specific shock requires additional control. The main matching hypothesis is now stated in terms of the before-after estimation instead of levels. It means that controls have evolved from a pre to a post-programme period in the same way as treatments would have done had they not been treated.
What we do is performing the matching on X, but our Y is not the level of the outcome variable (wages) but the relative difference between the outcome variable after the period when job mobility has taken place and before that period. For a more formalised explanation, see Blundell and Costa Dias (2002).

Matching algorithms and common support

We perform the analysis twice, using two algorithms as a consistency check: nearest neighbour with replacement and calliper and radius (For a survey of the main algorithms, see Caliendo and Kopeining, 2005). Both are easily comparable and differ in terms of the variance and the bias in the estimate of the average treatment on the treated they may incur in. In the nearest neighbour matching every control is matched with the 5 treated individuals that are closest in terms of propensity score. Therefore it allows for a minimum bias in the estimation of the ATT. In our case we do the algorithm "with replacement", which means that an untreated individual can be used more than once as a match.

The problem with this algorithm is that, if the closest neighbour is far away we may be making bad matches. Therefore we have imposed a calliper: we only consider matches in a distance lower than 0.01. With this system bad matches are avoided and the quality of the match improves. But, at the same time, if only few matches are performed inside the calliper, the variance of the estimates increases. Our choice for the calliper has been relatively generous, 0.01, which is in several occasions not binding (meaning that in most occasions the restriction does not leave matches out).

Our second algorithm is a variant of calliper matching called *radius matching*. It does not only use the nearest neighbour within each calliper but all of the comparison units available within the calliper. There is a trade-off in terms of bias and efficiency (variance) in the estimation of ATT with both algorithms. Since radius matching uses more matches than nearest neighbour, the estimate of the ATT has a lower variance here, but since it uses more matches, and not only the very best ones, the bias is likely to be higher than in the nearest neighbour.

We only work on the common support area and define it by a trimming procedure which is meant to restrict the common support region to those values of P (the propensity score or probability of moving across jobs) that have positive density within both the distributions of P across treated and controls. In defining a trimming of 5%, we do not only exclude those points for which the estimated density of the propensity score is exactly zero, but also an additional 5 percent of the remaining P points for which the estimated density is positive but very low are excluded. By excluding from the common support those individuals with the 5 percent of lowest values of P we are constraining the common support and the number of available matches (increasing variance) but allowing for better matches (reducing bias).

6. Main results

We display the main results of the paper in Tables 2A and Table 2B and tables 3.A and 3.B. Tables 2.A. and 2.B. show the average treatment on the treated for every pair of alternatives in voluntary versus involuntary movements and direct versus direct (via unemployment) both for men and women. Tables 3A and 3B show several indicators about the process of matching: sample sizes and some relevant figures about the quality of the matching.

We pay most our attention to Tables 2A and 2B. What does the ATT mean? Let us look at the first pair of alternatives in Table 2.A.: voluntary job moves versus stability. The average treatment on the treated for men is 0.088 and significant. This mans that the expected wage increase for those young men who move voluntarily across jobs is 8.8% higher than the one those who do not move could expect if they moved. In other words, it says that, after controlling for all the observable characteristics that influence voluntary job mobility, and when comparing equal with equal, we obtain a wage increase premium of 8.8% for those who move voluntarily across jobs than for those who stay with the same employer.

This difference, as well as the differences observed in all other pair of alternatives, is considerably smaller than the one observed for any two groups before the matching is performed. This shows that the figures displayed in table 1 are very much biased due to the fact that we are not comparing equal with equal, but individuals with given features that make them more mobile, for some reason or another.

Let us now comment all the other pair of movements in table 2.A. for men, the only relevant way to accelerate the rhythm of wage growth at the beginning of the working career seems to be moving voluntarily across jobs. Moving involuntarily, which seemed to have even a positive effect on wage growth in Figure 2, is not significant any longer compared to not moving at all. Interestingly, the difference in growth between moving voluntarily and involuntarily, although in favour of voluntary moves, is not large enough to be significant. As for women, we register more significant differences across types of movers. To begin with, women take more advantage of voluntary moves than men do. And, accordingly, not moving is significantly worse than moving voluntarily. Finally, only one of the algorithms (nearest neighbour (5)) shows that not moving is worse than moving involuntarily, while involuntary moves are not significantly better than staying with the same employer. The only clear difference is the stronger positive effect of voluntary moves for women than for men.

What we find in general in the case of voluntarily in movement is that, despite the initial apparent differences, moving voluntarily across jobs really pays, whereas, fortunately, it does not seem to be any scarring effect of involuntary moves, which is quite a good news, meaning that the end of temporary contracts and dismissals are not affecting negatively (at least significantly) to the wage path of youths, and therefore initial failures and bad moves at the labour trajectory do not imply scarring effects. Notice that this result is not so positive for adults (Arranz *et al*, 2005, García Pérez and Rebollo Sanz, 2005).

As regards job mobility directly and via unemployment we notice interesting differences across genders (Table 3.B.). For men, direct job moves are significantly better in terms of wage growth than staying with the same employer, and, strikingly, moving across jobs throughout unemployment implies a positive rewarding, even higher than moving directly across jobs. This would mean not only that unemployment does not have any scarring effect for Spanish youths in terms of wages, but also that it is somehow used as an investment. Consequently, not moving is worse for men than moving via unemployment and it is not significantly worse than moving directly across jobs.

This positive effect of both direct and indirect movements on wages holds true amongst women as well. Direct moves are more rewarding for women than for men and the difference between direct and indirect moves is hardly noticeable for women, whereas in the case of men it seems that there is a clear advantage of indirect moves compared to direct ones. Another difference feature of women and men is that, in the case of men, non movers perform significantly worse than indirect movers, which is no longer the case amongst women. At the same time, female stayers perform significantly worse than direct female movers, whereas male stayers are not significantly worse paid than direct movers. Therefore, both direct and indirect moves are rewarding for both men and women, but direct movements they are significantly better for women than for men and indirect movements are still more profitable for men than for women. This would confirm that unemployment is more of an investment and less scarring for men than for women, and direct moves are more rewarding for women than for men.

Anyway, this rosy picture of the very positive effect of job moves needs to be put in proper terms: as we have observed in Figures 1 to 4, the effect of the first movement is much stronger and positive regardless the type of move than the effect of the following movements, to the extent that the impact of job mobility may turn negative if this is too frequent. The results we have reported are only obtained for the very first job movement in our observation window, and we already notice that the initially very positive effect, once equals have been matched, gets reduced to nothing. Therefore, what can we expect from subsequent movements, whatever the type? Probably, a negative effect.

7. Conclusions

The overall result of the present piece of work shows a quite optimistic picture of wage dynamics amongst Spanish youths. Given the high temporality rates in Spain during all the nineties, Spanish youths bear with the highest rates of job turnover across the European Union, are also some of the most affected by unemployment and involuntary job interruptions. This should make their wages very vulnerable towards movements. We have tested this hypothesis here and have observed that the vulnerability of young Spaniards is less tragic than might be imagined.

We have observed positive impact of both direct and indirect, voluntary and involuntary movements. The empirical strategy has confirmed that the positive effect was only significant in some cases, for voluntary and direct moves, but at least non scarring effects of indirect and involuntary moves have been obtained. Nevertheless, we need to remember that we are only observing one move amongst the several an individual may experience during the observation window. The initial descriptive analysis showed that the positive impact of job mobility fades rapidly with the number of moves. Therefore, should we be able to disentangle, by using the same empirical strategy, the effects of the second and subsequent job moves on wages, the outcome we would expect would be negative. Further research with a larger observation window or larger samples (which might be available in other data sets) should be developed to shed more light into the effect of multiple movements.

What type of theoretical framework do our results support? Once unobserved heterogeneity and endogeneity are controlled for, there is still room for a positive impact of job mobility per se on wage growth. Therefore, the initial mover-stayer model would be rejected. But with the technique used here we are unable to disentangle which is really the mechanism that explains wage increase when young workers move across jobs. Is it that productive workers are poached by other firms? Or that individuals improve the knowledge about their productivity and labour market conditions and move towards better matches? Moreover, the more puzzling results are the ones related to the non scarring effects of involuntary and indirect job moves. In a very dynamic youth labour market where 20% of young workers in average move across jobs every year job mobility is so spread that it affects to all sorts of workers and not only to the weakest/less productive. This may also explain the lack of scarring effect of unemployment and involuntary moves.

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Table 1.A. **Mean values of explanatory variables in the propensity score matching estimations**

	men	women
Age	24,78	25,14
Years of post-compulsory education	2,43	3,42
Initially in first job	0,32	0,39
Weekly working hours	42,82	38,98
Initially looking for a job	0,13	0,16
Initial hourly wage	4,96	4,61
Tenure (years)	2,68	2,82
ISEI (Internacional socio-economic index)	35,77	40,49
Public sector	0,10	0,14
Overqualified	0,61	0,67
Supervised	0,84	0,83
Previous specific training	0,42	0,57
Temporary contract	0,44	0,42
Employer pays health care	0,39	0,36
Employer pays training	0,20	0,23
Devotes time to taking care of a child	0,07	0,13
Low satisfaction with wage	0,27	0,28
Low satisfaction with job stability	0,20	0,22
Low satisfaction with type of job	0,11	0,12
Low satisfaction with working hours	0,18	0,19
Low satisfaction with working times	0,13	0,16
Low satisfaction with employment conditions	0,14	0,09
Low satisfaction with commuting time	0,17	0,14
Low satisfaction with main activity	0,12	0,10
Low satisfaction with financial situation	0,24	0,25
Low satisfaction with housing situation	0,07	0,07
Low satisfaction with amount of leisure time	0,22	0,26
Number of cases	2867	2064

Table 2.A
Results for the propensity score matching. Voluntary and involuntary mobility compared to staying with the same employer.
MEN

Measure	Method	ATT	St. Error	Method	ATT	St. Error
Voluntary job mob - no job mob	NN(5) caliper (0.01)	**0.088**	0.039	Radius caliper (0.01)	**0.084**	0.042
No job mob - voluntary	NN(5) caliper (0.01)	0.012	0.037	Radius caliper (0.01)	0.016	0.036
Involuntary - no job mob	NN(5) caliper (0.01)	0.036	0.042	Radius caliper (0.01)	0.042	0.034
No job mob-involuntary	NN(5) caliper (0.01)	-0.022	0.028	Radius caliper (0.01)	-0.028	0.030
Voluntary-involuntary	NN(5) caliper (0.01)	0.014	0.060	Radius caliper (0.01)	0.019	0.055
Involuntary - voluntary	NN(5) caliper (0.01)	-0.097	0.064	Radius caliper (0.01)	-0.071	0.075

WOMEN

Measure	Method	ATT	St. Error	Method	ATT	St. Error
Voluntary job mob - no job mob	NN(5) caliper (0.01)	**0.122**	0.066	Radius caliper (0.01)	**0.122**	0.070
No job mob - voluntary	NN(5) caliper (0.01)	**-0.156**	0.081	Radius caliper (0.01)	**-0.165**	0.062
Involuntary - no job mob	NN(5) caliper (0.01)	0.062	0.048	Radius caliper (0.01)	0.077	0.056
No job mob-involuntary	NN(5) caliper (0.01)	**-0.069**	0.027	Radius caliper (0.01)	-0.062	0.041
Voluntary-involuntary	NN(5) caliper (0.01)	0.059	0.096	Radius caliper (0.01)	0.056	0.084
Involuntary - voluntary	NN(5) caliper (0.01)	-0.030	0.089	Radius caliper (0.01)	-0.035	0.109

Source: ECHP (1995-2001) Eurostat.

Table 2.B
Results for the propensity score matching. Direct and indirect (via unemployment) mobility compared to Staying with the same employer.
MEN

Measure	Method	ATT	St. Error	Method	ATT	St. Error
Direct job mob - no job mob	NN(5) caliper (0.01)	**0,088**	0,045	Radius caliper (0.01)	**0,093**	0,036
No job move - direct	NN(5) caliper (0.01)	-0,009	0,032	Radius caliper (0.01)	-0,014	0,030
Indirect - no job mob	NN(5) caliper (0.01)	**0,105**	0,055	Radius caliper (0.01)	**0,107**	0,048
No job move -indirect	NN(5) caliper (0.01)	**-0,098**	0,039	Radius caliper (0.01)	**-0,105**	0,039
Direct-indirect	NN(5) caliper (0.01)	-0,002	0,065	Radius caliper (0.01)	-0,016	0,063
Indirect – direct	NN(5) caliper (0.01)	-0,032	0,070	Radius caliper (0.01)	-0,032	0,062

WOMEN

Measure	Method	ATT	St. Error	Method	ATT	St. Error
Direct job mob - no job mob	NN(5) caliper (0.01)	**0,104**	0,055	Radius caliper (0.01)	**0,107**	0,053
No job mob – direct	NN(5) caliper (0.01)	**-0,204**	0,095	Radius caliper (0.01)	**-0,185**	0,082
Indirect - no job mob	NN(5) caliper (0.01)	**0,116**	0,059	Radius caliper (0.01)	**0,105**	0,048
NO JOB –indirect move	NN(5) caliper (0.01)	-0,072	0,046	Radius caliper (0.01)	-0,060	0,042
Direct-indirect move	NN(5) caliper (0.01)	0,104	0,090	Radius caliper (0.01)	0,068	0,086
Indirect – direct move	NN(5) caliper (0.01)	-0,055	0,084	Radius caliper (0.01)	-0,075	0,082

Source: ECHP (1995-2001) Eurostat.

Table 3.A Quality indicators for the propensity score matching.
Voluntary and Involuntary mobility compared to Staying with the same employer.

MEN	**A**	**B**	**C**	**D**	**E**	**F**	**G**	**H**	**I**	**J**
voluntary job mob - no job mob										
NN(5) calliper (0.01)	2,736	2,588	24	244	2488	23.60	4.37	0.123	0.015	0.817
radius calliper (0.01)	2736	2588	24	248	2488	23.60	3.78	0.123	0.013	0.009
no job mob - voluntary										
NN(5) calliper (0.01)	2,736	2,588	124	2488	248	23.60	7.14	0.123	0.056	0
radius calliper (0.01)	2,736	2,588	124	2,488	248	23.60	6.87	0.123	0.052	0
involuntary - no job mob										
NN(5) calliper (0.01)	2,742	2,600	18	254	2488	16.62	3.15	0.131	0.01	0.979
radius calliper (0.01)	2742	2600	18	254	2488	16.62	2.81	0.131	0.008	0.421
stayer -involuntary										
NN(5) calliper (0.01)	2742	2600	124	2488	254	16.62	6.60	0.131	0.045	0
radius calliper (0.01)	2742	2600	124	2488	254	16.62	6.07	0.131	0.039	0
voluntary-involuntary										
NN(5) calliper (0.01)	502	462	19	248	254	9.27	4.66	0.095	0.022	0.980
radius calliper (0.01)	502	462	19	248	254	9.27	4.51	0.095	0.02	0.990
involuntary - voluntary										
NN(5) calliper (0.01)	502	462	21	254	248	9.27	6.30	0.095	0.028	0.907
radius calliper (0.01)	502	462	21	254	248	9.27	5.29	0.095	0.031	0.837

WOMEN	**A**	**B**	**C**	**D**	**E**	**F**	**G**	**H**	**I**	**J**
voluntary job mob – Stayer										
NN(5) calliper (0.01)	1,891	1,779	24	118	1773	26.78	5.93	0.168	0.023	0.977
radius calliper (0.01)	1891	1779	24	118	1773	26.78	5.03	0.168	0.025	0
No job mob - voluntary										
NN(5) calliper (0.01)	1891	1779	88	1773	118	26.78	18.23	0.168	0.156	0
radius calliper (0.01)	1,891	1,779	88	1773	118	26.78	19.54	0.168	0.157	0
involuntary – Stayer										
NN(5) calliper (0.01)	1,980	1868	24	207	1773	15.05	2.99	0.123	0.018	0.857
radius calliper (0.01)	1980	1868	24	207	1773	15.05	4.90	0.123	0.022	0.001
Stayer -involuntary										
NN(5) calliper (0.01)	1,980	1,868	88	1773	207	15.05	8.88	0.123	0.062	0
radius calliper (0.01)	1,980	1,868	88	1773	207	15.05	9.00	0.123	0.056	0
voluntary-involuntary										
NN(5) calliper (0.01)	325	288	13	118	207	10.35	8.67	0.111	0.061	0.674
radius calliper (0.01)	325	288	13	118	207	10.35	8.95	0.111	0.068	0.464
involuntary - voluntary										
NN(5) calliper (0.01)	325	288	24	207	118	10.35	11.10	0.111	0.076	0.311
radius calliper (0.01)	325	288	24	207	108	10.35	11.03	0.111	0.076	0.301

A. Sample size; B: common support area; C: treated falling outside the common support (calliper 1% trimming 5%) D: number of treated; E: number of controls:

F and G: median absolute standardised bias before and after matching, median taken over all the 27 regressors. Following Rosenbaum and Rubin (1985) for a given covariate X the standardized difference before matching is the difference of the sample means in the full treated and non treated samples as a percentage of the square root of the average of the sample variances in the full treated and non treated groups. The standardised differences after matching is the differences of the sample means in the matched treated and matched non treated samples as a percentage of the square root of the average of the sample variances in the fool treated and non treated groups. For a precise definition, see Appendix C in Sianesi (2004).

H: Pseudo R2 from probit estimation on the conditional mobility probability. It is an indicator of how well the regressors X explain the participation probability.

I: R2 from a probit of D on X on the matched samples, to be compared with H

J: P-value of the likelihood ratio test after matching. The joint significance of the regressors is rejected in several occasions.

Source: ECHP (1995-2001) Eurostat.

Table 3.B Quality indicators for the propensity score matching.
Voluntary and Involuntary mobility compared to Staying with the same employer.

MEN	A	B	C	D	E	F	G	H	I	J
direct move - no move										
NN(5) calliper (0.01)	2620	2488	15	277	2,343	19.65	3.38	0.11	0.012	0.897
radius calliper (0.01)	2620	2488	15	277	2343	19.65	2.76	0.11	0.008	0.518
no move - direct										
NN(5) calliper (0.01)	2620	2488	117	2343	277	19.65	6.63	0.11	0.041	0
radius calliper (0.01)	2620	2488	117	2343	277	19.65	5.74	0.11	0.035	0
indirect - no move										
NN(5) calliper (0.01)	2590	2435	38	247	2343	21.64	3.29	0.185	0.015	0.894
radius calliper (0.01)	2590	2435	38	247	2343	21.64	3.17	0.185	0.012	0.04
NO JOB -INdirect										
NN(5) calliper (0.01)	2590	2435	117	2343	247	21.64	5.62	0.185	0.078	0
radius calliper (0.01)	2590	2435	117	2343	247	21.64	5.46	0.185	0.073	0
direct-INdirect										
NN(5) calliper (0.01)	524	493	18	277	247	7.31	5.01	0.06	0.03	0.083
radius calliper (0.01)	524	493	18	277	247	7.31	4.74	0.06	0.024	0.952
INdirect - direct										
NN(5) calliper (0.01)	2524	493	13	247	277	7.31	3.53	0.06	0.017	0.997
radius calliper (0.01)	524	493	13	247	277	7.31	3.94	0.06	0.017	0.996

WOMEN	A	B	C	D	E	F	G	H	I	J
direct move - no move										
NN(5) calliper (0.01)	1862	1765	12	156	1706	20.17	6.12	0.142	0.026	0.766
radius calliper (0.01)	1862	1765	12	156	1706	20.17	5.41	0.142	0.019	0.014
no move - direct										
NN(5) calliper (0.01)	1862	1765	85	1706	156	20.17	17.82	0.142	0.096	0
radius calliper (0.01)	1862	1765	85	1,706	156	20.17	15.77	0.142	0.08	0
indirect - no move										
NN(5) calliper (0.01)	1908	1794	29	202	1706	19.02	3.04	0.155	0.028	0.38
radius calliper (0.01)	1908	1794	29	202	1706	19.02	2.77	0.155	0.02	0.007
NO JOB -INdirect										
NN(5) calliper (0.01)	1908	1794	85	202	1706	19.02	7.17	0.155	0.072	0
radius calliper (0.01)	1908	1794	85	1706	202	19.02	7.16	0.155	0.07	0
direct-INdirect										
NN(5) calliper (0.01)	358	328	15	202	156	9.06	3.86	0.069	0.026	0.996
radius calliper (0.01)	358	328	15	156	202	9.06	3.98	0.069	0.025	0.996
INdirect - direct										
NN(5) calliper (0.01)	358	328	15	202	156	9.06	5.35	0.069	0.039	0.912
radius calliper (0.01)	358	328	15	202	156	9.06	5.01	0.069	0.038	0.92

A. Sample size; B: common support area; C: treated falling outside the common support (calliper 1% trimming 5%) D: number of treated; E: number of controls:

F and G: median absolute standardised bias before and after matching, median taken over all the 27 regressors. Following Rosenbaum and Rubin (1985) for a given covariate X the standardized difference before matching is the difference of the sample means in the full treated and non treated samples as a percentage of the square root of the average of the sample variances in the full treated and non treated groups. The standardised differences after matching is the differences of the sample means in the matched treated and matched non treated samples as a percentage of the square root of the average of the sample variances in the fool treated and non treated groups. For a precise definition, see Sianesi (2004).

H: Pseudo R2 from probit estimation on the conditional mobility probability. It is an indicator of how well the regressors X explain the participation probability.

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J: P-value of the likelihood ratio test after matching. The joint significance of the regressors is rejected in several occasions.

Source: ECHP (1995-2001) Eurostat.