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

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Abstract

The beginning of a person's career is very often defined by a period of intensive job mobility which can influence the path of wage progression and may have an important impact on individual future income. In this study we aim to measure the impact of different types of job moves on the subsequent hourly wage and to identify the factors that make a transition across successful jobs or, on the contrary, which circumstances may have a scarring effect on subsequent earning potential. For that purpose we use the Spanish section of the European Community Household Panel and work on a sample of young people. 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 on both direct and voluntary moves but also a non-scarring effect on 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) young people are supposed to acquire positive wage gains from (voluntary) mobility. 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 youth is found to have the highest rate of job turnover across the European Union, and young people are severely affected by unemployment and involuntary job interruptions. This may have made them very vulnerable towards movements. Moreover, it has often been argued that an excessive job turnover at the beginning of a person's career may seriously damage labour market outcomes in the mid or long term.

This piece of work is aimed at studying the rewards afforded 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 significant positive effects from both direct and voluntary moves for both young men and young women. We do not find any scarring effects from involuntary unemployment. Nevertheless, our initial descriptive analysis shows as job moves become frequent they no longer generate positive results. In this case our very optimistic results should be treated with caution 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 database is presented in Section 3; Section 4 is devoted to some basic descriptive analyses 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 drawn much attention in the empirical literature in recent years. In most cases, research has focused on the scarring effect of spells of unemployment on subsequent jobs and careers, comparing the effects of both redundancy and voluntary termination. 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. For the sake of concreteness we will, in this study, essentially focus on the first type of contribution.

In this section, we intend to offer a broad picture of what the main issues are in the link between job mobility and wage mobility from a theoretical point of view in the study. 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 to 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 their tenure rises in the firm, creates a higher earning potential. This reduces the 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 current 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 accompanied by a spell of unemployment, and there are several hypotheses (rooted in human capital theory) that point to a wage loss by displaced employees, which is often labelled in the related literature as the "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 spells of unemployment 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, unsurprisingly, involuntary.

Job matching models would predict a positive effect of job mobility on wages since workers 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 robust increases in productivity with tenure. In this case, more stable matches will be a signal of productivity and should be paid accordingly (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. These conclusions can also be applied to the training approach considered by

[1] However there are other possibilities. Antel (1991) shows that spells of unemployment amongst voluntary movers provide 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.

Mortensen (1988) and to the job-shopping theory (Stigler, 1962) according to which younger workers are more likely to try a variety of jobs in order to acquire knowledge of the labour market and their individual 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 an off-the-job search is higher than an on-the job search, 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. However the worker will continue searching on- the-job after acceptance[2]. 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 is related to job matching and search theories which also expect higher wage increases from movers than from stayers as a result of occupational mobility across jobs and employers (Sicherman and Galor, 1990). Other views (contract models and models where employers poach 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 these 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 an isolated 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 employer-employee matches. They complement each other, given that neither job search nor human capital alternatives alone may explain the link between job and wage mobility. Empirical evidence is vital to distinguish which approach better fits reality.

[2] Jobs offering more than the high reservation wage will be accepted and job search will cease.

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 flip side of the coin), which causes bias in the OLS estimators in a typical Mincerian wage equation. There are several strategies for overcoming this problem. One of the most widespread is the instrumental variables approach as an alternative to the simultaneous resolution of equation systems such as 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 to tenure (the latter). The same idea was used subsequently 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 simultaneously considers job turnover processes and wage growth in the study of the 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 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 wage 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). Lillard's proposal is to simultaneously account for job mobility and wage mobility through a multilevel estimation with three levels of sources of unobserved heterogeneity: the individual level; 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 that this wanderlust results in a lower productivity, then we should accept that any checks on unobserved heterogeneity amongst individuals which lend significance to the variables reporting mobility should be cancelled. If, on the contrary, control of unobserved heterogeneity does not cancel explanatory power of mobility, we should accept a causal link between both variables. Either way, even if we accept that there is a causal link between them, the rationale behind this relationship, that is, the causal mechanism, is a third issue to be tackled. This is beyond the scope of this paper[3].

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 substitute for a before and after estimator.

Empirical evidence on job mobility very often focuses on youth, since it is during the early stages of their working lives that young people experience more intensive job and wage mobility. The availability of data-sets on early careers has also contributed to this. The seminal pieces of evidence (Bartel and Borjas (1978, 1981), Mincer (1986)), point to higher mobility returns amongst youths. Later on Antel

[3] 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-exclusive and it is difficult to disentangle which of the explanations carries more weight. Generally researchers deploy information about the three possible explanatory factors, for instance: tenure in previous jobs is often used as a substitute for specific human capital accumulated in former jobs; satisfaction and wages in former jobs may be used as a substitute for initial quality of the previous job match, and job search intentions should play the same role as regards job search strategies.

(1991), Topel and Ward (1992), Light and McGarry (1998) and Perticara (2002) focus only on youths, whereas Le Grand and Tåhlin (2002) also comprise young adults aged 26 to 35, an age at which both job and wage mobility are very relevant.

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). However sometimes this distinction cannot be made because attention is then driven towards spells of unemployment after dismissals, such as Arulampalam (2001), Gregory and Roberts (2001) and Lefranc (2003), who study the scarring effect of unemployment, and Antel (1991), who discovers the positive effects of unemployment on the basis that young unemployed people search more intensively for jobs than those who register job interruptions but no unemployment.

Wage and job mobility have been explored in a large number 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 British Household Panel Survey and German Socio Economic Panel. Exceptionally some 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 to deal with both the endogeneity and the unobserved heterogeneity, namely difference-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 database: the European Community Household Panel*

In order to gain evidence on both determinants of and rewards to job mobility, the database used here will be the European Community Household Panel (hereinafter, ECHP). This survey gathers information on several socio-economic aspects in the European Union and labour market related issues is one of the most important fields to be considered. This database, 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.

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 time of the interview from the distance between the date of the interview and the beginning of the relation with the employer and the hourly net wage which 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[4]. 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 also report on whether s/he experienced unemployment before accessing the current job and why s/he left the previous one. This information on job movers will result in more complete variables, which combine information about movement across jobs, willingness in job mobility and the presence of spells of unemployment between jobs. This will allow us to distinguish

[4] For instance, if two interviews are in subsequent years, a job move is detected when the individual is employed on 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.

between voluntary and involuntary, direct and indirect moves detected since 1995[5] until the date of the current interview. We are aware that our way of computing job mobility may 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 who 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 job at the beginning of the observation period so that we could observe the impact of the first job move. However, 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 (ISEI). Women are as well more often employed in the public sector and have specific training previous to current jobs than their male counterparts. However, 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 the frequency of job mobility and the size of wage growth amongst the different types of young movers in Spain. Table 1 shows the main year in year transition rates from employment, split into 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 the movers are involuntary movers, namely, those who, once in their subsequent job, report they were obliged to leave the prior job by the employer,

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

because a temporary contract had ended or for family reasons[6]. On the other hand, those who move voluntarily are those who have been offered a better job or for other reasons[7].

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	80	9	11	78	12	10
women	83	6	11	80	9	11
Wage increase (%)				Wage increase (%)		
men	4	27	20	5	23	26
women	5	22	23	5	21	23
Wage increase after the first movement (%)				Wage increase after the first movement (%)		
male	7	30	27	7	30	34
female	7	30	28	8	33	32

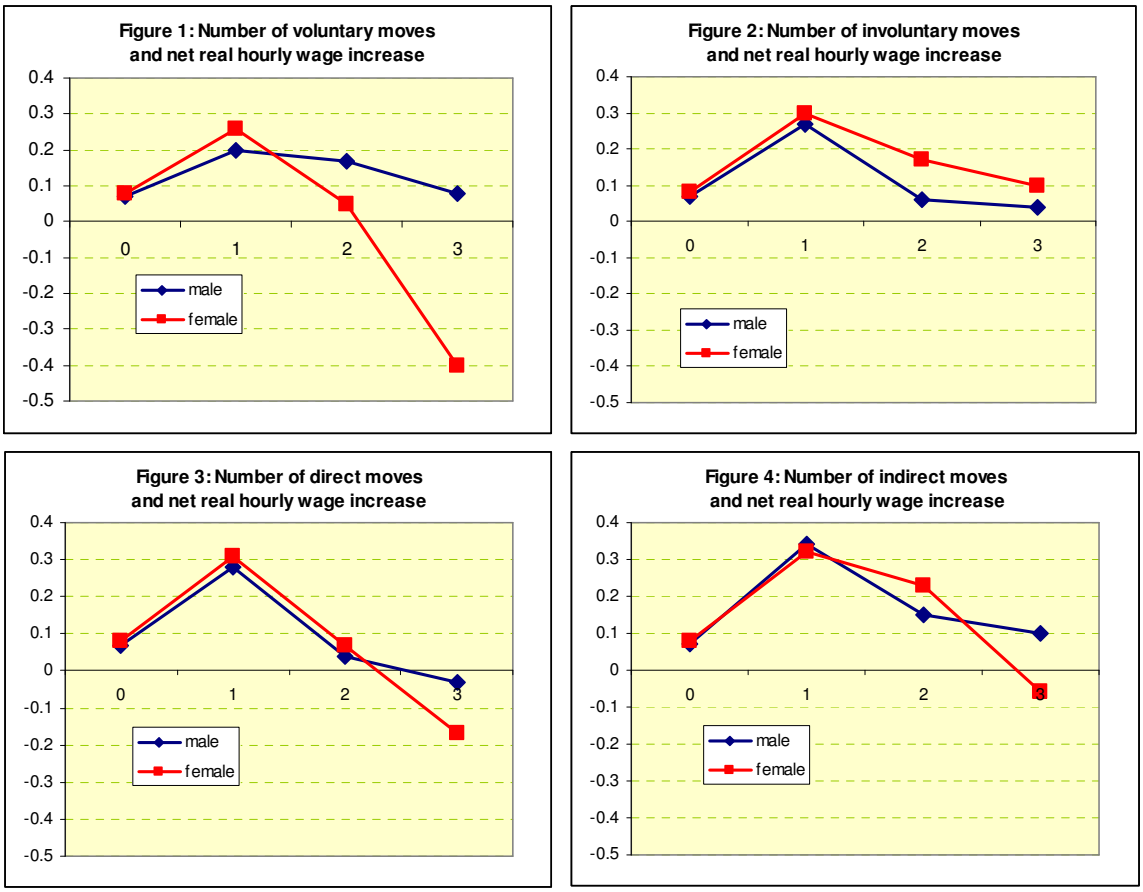
Source: ECHP (1995-2001)

For those who willingly leave their job, net real hourly wage increase during the period is, on average, 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 experience a wage loss to those who invest in information during their period of unemployment and therefore end up with a better job than the one they lost or quit. These straightforward comparisons may be biased as long as they are not comparing equivalent individuals but the average wage growth of individuals which may be quite different.

[6] 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 that family reasons are not a voluntary option.

[7] This choice is also arguable, but the alternative 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 alternative employment. The outcome of this type of situation is 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.

Moreover, we have observed that 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 decreases the more movements we observe. This holds true for both voluntary and involuntary, direct and indirect movements, as Figures 1 to 4 show.



Source: ECHP (1995-2001), Eurostat.

This has a very relevant impact on our research: should we analyse all the movements we register regardless of 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 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 use propensity score matching?

From a practical 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 difference-in-differences propensity score matching.

Let us start with endogeneity. Job mobility may generate wage mobility precisely because it is driven by the desire for 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 (IV). The underlying identification strategy in the IV approach in our case is to find a variable which determines job mobility but does not influence 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 IV is that it is very difficult to find a good instrument. Needless to say, given the different theoretical frameworks that contribute to explain the job mobility - wage mobility relationship, it proves difficult to find such a variable, particularly in the context of young people[8].

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 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 into different groups according to the type of mobility across jobs they have experienced. We then try to find another group in the

[8] 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, more particularly, in the case of Spain) this variable does not register enough variability.

population which is very similar to this group in an as exhaustive 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 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 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 economists who use them a great deal in the context of labour market policy evaluation. They have been used 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 that 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 problem has received less attention than the usual single treatment case, but it is not much more complicated than that. Pioneering works on multiple treatment are Lechner (2001) and Imbens (2000) who came up with the same way of dealing with this problem. Larsson (2003)

is an application of the technique presented at Lechner (2001). In what follows we 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 use the term "treatment" for 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 is 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 neither know 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^l 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 consists of defining the effect of treatment m compared to the treatment l, for all combinations of $m, l \in \{0, 1, \ldots, M\}$, $m \neq l$[9]. We want to compute the so called *average treatment on the treated (ATT)* in the evaluation literature. The ATT may be presented as follows:

$$(1) \qquad \theta_0^{ml} = E(Y^m - Y^l \mid T=m) = E(Y^m \mid T = m) - E(Y^l \mid T=m),$$

θ_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"

[9] 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.

effect. It translates in our particular problem to the effect of job mobility on the wage 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 $\theta_0^{ml} \neq - \theta_0^{lm}$. 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 $m \neq l$, since it is impossible to observe the same individual in several states at the same time. This means that the true causal effect of 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 works on the assumption that the selection between the groups of participants in treatment *m* (say, job movers) and treatment *l* (say, job stayers) is captured by a vector of observable characteristics, *X*. In order to accept the CIA the researcher needs quite a rich data set to enable him to claim to be controlling for all the factors influencing each type of treatment. We think this is the case, given the exhaustive nature of the vector of variables characterising jobs in the ECHP. We are able to monitor not only the observed objective characteristics (age, initial wage, type of employer, industry, occupation status measured by 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 over-education, being in a first job, 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 of 27 characteristics (see table 1 for a display of mean values).

A formalization of the CIA assumption in the multiple treatment case is available in Larsson (2003), as well as the identification problem. This is omitted here for the sake of brevity. However, we will just mention that the identification of the average treatment effect on the treated, $\theta_0^{ml,}$ requires an estimation of the conditional choice

probability of treatment l given either treatment m or l (what we call propensity score). The ATT 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 θ_0^{ml} the reader should see Imbens (2000) and Lechner (2001). The estimation procedure is described at Lechner (2001) and Larsson (2003).

Matching and Difference-in-Differences

The CIA is a very strong assumption. It is very difficult to assert that by monitoring 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 so as to facilitate getting rid of the risk of having individuals making decisions on the basis of future rewards. However, by combining matching with difference-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 perform 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 but differ

in terms of the variance and the bias in the estimate of the average treatment on the treated they may result 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 on several occasions not binding (meaning that on 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. At the same time since it uses more matches (and not only the very best ones) the bias is likely to be higher than in the nearest neighbour algorithm.

We only work in the area of common support 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 distributions of P across the treated and controls. In defining a trimming of 5%, we not only exclude those points for which the estimated density of the propensity score is exactly zero, but an additional 5 percent of the remaining P points, for which the estimated density is positive but very low, is 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 ATT for every pair of alternatives in voluntary versus involuntary movements and direct versus indirect (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 particular 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 means that the expected wage increase for those young men who move voluntarily across jobs is 8.8% higher than those who do not move could expect if they moved. In other words, it says that, after monitoring 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 as opposed to 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 who have given features that for some reason or another make them more mobile.

Let us now comment on all the other pairs of movements in table 2.A. For men, the only relevant way to accelerate the rhythm of wage growth at the beginning of their career seems to be moving voluntarily across jobs. Moving involuntarily, which appeared to have 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 voluntary movement is that, despite the initial apparent differences, moving voluntarily across jobs really pays whereas, fortunately, involuntary moves do not seem to have any scarring effect. This is good news, and means that the end of temporary contracts and dismissals are not negatively affecting (at least significantly) the wage path of young people. Therefore initial failures and bad moves at the labour trajectory do not imply scarring effects. Notice that this result is not so positive for older people (Arranz *et al*, 2005, García Pérez and Rebollo Sanz, 2005).

As regards job mobility directly and via unemployment we observe 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 appears to yield more positive rewards than moving directly across jobs. This would mean not only that unemployment does not have any scarring effect for young people in Spain 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 also holds true amongst women. 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 indirect moves have a clear advantage compared to direct ones. Another different feature between 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 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.

Nonetheless this optimistic 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 more positive regardless of 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 young people. Given the high temporality rates in Spain during the nineties, young Spaniards register the highest rates of job turnover across the European Union and are also some of the most affected by unemployment and involuntary job interruptions. This makes their wages very vulnerable towards movements. We have tested this hypothesis here and have observed that the vulnerability of young Spaniards is less disastrous 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, ie. for voluntary and direct moves; nevertheless non scarring effects of indirect and involuntary moves have been obtained. However, we need to remember that we are only observing one move amongst 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 the effects of the second and subsequent job moves on wages by using the same empirical strategy, we would expect a negative outcome. 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 on 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 unravel which is the real mechanism that explains wage increase when young workers move across jobs. Is it that productive workers are poached by other firms? Or that individuals have more knowledge of their productivity and labour market conditions, enabling them to 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 a high proportion of young workers move across jobs at least once every year, mobility is so spread that it affects all sorts of workers, not only 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

Source: ECHP (1995-2001) Eurostat.

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 full 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.
Direct and indirect 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 full 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.
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.