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JOB MOBILITY AND WAGE GROWTH AT THE BEGINNING OF THE PROFESSIONAL CAREER IN SPAIN*

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The beginning of professional careers is often characterised by intensive job mobility, which may influence wage progression. In this study, we aim to measure the immediate impact of different types of job moves on subsequent hourly wages. We use the Spanish section of the European Community Household Panel and work on a sample of young adults. Propensity Score Matching and difference-in-differences are combined to disentangle the impact of long-term and short-term, direct and via unemployment, voluntary and involuntary and one-time and multiple job mobility on subsequent wages during the period 1995-2001. We observe a positive impact of both direct and voluntary moves and a non-scarring effect of involuntary moves, both via unemployment and multiple job moves, but long-term interruptions do have a negative impact on wages.

Key words: job mobility, wage mobility, propensity score matching.

JEL classification: J31, J63.

The impact of job mobility on wages is a very important and controversial issue in Labour Economics. In most of the theoretical approaches (human capital, job search and job-matching as well as career mobility models), young people are assumed to acquire positive wage gains from (voluntary) mobility. Nevertheless, in Spain they often move neither voluntarily nor directly between jobs. Moreover, high turnover rates often mean repeated job separations. This should erode wage gains from mobility in the Spanish youth labour market.

This paper aims to study the rewards for different types of job mobility at the beginning of the professional career in Spain in order to find out whether inten-

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sive job mobility erodes wage progression. To that end, a sub-sample of young people (under 29 in 1994) was drawn from the European Community Household Panel (ECHP hereinafter) and the subjects were observed from 1995 to 2001. In order to control for endogeneity in the movements between jobs and unobserved heterogeneity, the selected empirical strategy will consist of difference-in-differences propensity score matching (DID-PSM) with multiple outcomes.

Results show that, after observed and unobserved heterogeneity are controlled for and when observationally equivalent 'stayers' and 'movers' are compared, returns on recent mobility vary depending on the type of movement. Within the first year after the interruption, direct and voluntary moves are rewarding, and involuntary moves and those via unemployment do not necessarily slow wage growth down. Multiple movements (observed within a single year) do not seem to result in lower wage increases. Those needing more than one year to find a new job seem, instead, to be negatively affected by the interruption. Nevertheless, the latter two sub-samples are small and caution should be taken when deriving conclusions from them.

The contents of the paper are organised as follows. Section 2 surveys the main theoretical and empirical literature on the link between job mobility and wage dynamics. The database and the sample are presented in Section 3. Section 4 is devoted to the main methodological problems regarding job mobility and wage mobility and to explain our empirical strategy. Section 5 shows the main results from the difference-in-differences propensity score matching and, lastly, some conclusions are drawn from these results in Section 6.

1. Job mobility and wage mobility, theory and evidence

The connection between job mobility and wage mobility has attracted much attention in theoretical and empirical literature. Different theoretical approaches have highlighted two main methodological problems tackled in empirical work: the role of unobserved factors and endogeneity in job mobility. Two arguments support the unobserved factors problem: unobserved productivity and unobserved transferability and/or deterioration of human capital. The first argument [Blumen *et al*. (1955)] hypothesises inherent (in)stability amongst workers, correlated with productivity and, hence, with wages. Later, contract models [Lazear (1986)] forecasted wage increases as a result of mobility, since firms poach the best employees. Mobility indicates unobserved differences in productivity, which explains wage growth differentials between movers and stayers.

As for transferability and deterioration of skills, human capital models point out the investment in employer-specific human capital, part of which is not transferable [Becker (1962); Parsons (1972); Hashimoto (1981)]. Through on-the-job and formal training, workers accumulate firm-specific skills, which result in higher earnings, reducing relative gains from job mobility. Therefore, if firm-specific skills influence earnings, experienced movers risk incurring losses if they move between jobs.

The approach explained above is based on the assumption that job mobility is voluntary. However, several hypotheses rooted in human capital theory indicate wage losses ("wage scar") amongst displaced employees. Unemployment erodes human capital and skills, decreases productivity and, subsequently, re-employment wages. If employers take past unemployment experience as an indication of productivity [Vishwanath (1989); Pissarides (1992)], they may offer lower wages to previously unemployed workers[1].

A second crucial empirical problem in the job mobility – wage mobility literature is endogeneity: mobility may be a way to maximise lifetime income, as job matching, occupational mobility and job search models will assume.

Job matching models predict a positive effect of job mobility on wages since workers quit jobs to seek better matches [Jovanovic (1979a)]; if they succeed in their search, wages will be higher in the new jobs. Stable matches will be an indication of productivity [Jovanovic (1979b)]. The model predicts steeper experience-wage profiles for movers, but not necessarily higher wages in the end. The job-shopping theory [Stigler (1962)] and the training approach in Mortensen (1988) make similar predictions. Sicherman and Galor (1990) also expect higher wage increases for movers than for stayers as a result of occupational mobility between jobs and employers, provided skills are transferable across occupations.

In a similar vein, job search models also suggest that voluntary mobility will generate positive wage gains [Burdett (1978)]. Job mobility results in higher wages when the worker moves directly between jobs but may result in lower wages if she becomes unemployed and her reservation wage subsequently decreases.

Internal labour markets and segmentation hypotheses [Doeringer and Piore (1971); Edwards *et al*. (1975)] also pay attention to endogeneity but in a different way: mobility from the external/secondary labour market into the internal/primary labour market or between primary segment jobs is expected to result in wage increases. Otherwise, wage losses are also plausible.

Last but not least, when movements between jobs 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 become 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. Moreover, they are observationally equivalent. Empirical evidence is vital to detecting causality in the link between job mobility and wage mobility. Still, the underlying argument to this causality is very difficult to disentangle.

There are several strategies in the empirical literature for dealing with endogeneity, structural models being but one of them [Flinn (1986); Antel (1991)]. A heavily used alternative is instrumental variables (IV). Examples of papers using IV are Althonji and Shakotko (1987) and Topel (1991), both reassessed by Altonji and Williams (2005), followed by Light and McGarry (1998), Topel (2001) and

(1) However, there are other possibilities. Antel (1991) shows that spells of unemployment amongst voluntary movers make job searches more intensive and allow for better matches as a result of the search process. The alternative hypothesis is that unemployed workers' search intensity may be weaker because links to the labour market tend to fade while people are unemployed.

Lefranc (2003), amongst many others. More sophisticated strategies have been developed by Lillard (1999) and Abowd and Kang (2002). Lillard models job turnover and job duration in continuous time jointly with the wage time series for that job, while Abowd and Kang (2002) resume and revise the results of Altonji and Shakotko (1987), Topel (1991) and Lilliard (1999) in a new simultaneous estimation of wages and tenure.

As for the second key methodological issue, unobserved heterogeneity, it has been frequently treated via fixed effects estimations [Light and McGarry (1998); Arulampalam (2001); Gregory and Roberts (2001) and Munasinghe and Sigman (2004)].

The present article focuses on Spanish youth and distinguishes several types of job mobility: voluntary versus involuntary, direct versus via unemployment and one-time versus multiple movements. We also distinguish between short-term (short interruptions within a year) movements and long-term (longer interruptions, within two years) movements between jobs. Moreover, we detect repeated mobility within a year[2]. Since job and wage mobility are more intensive during the early stages of working life [Bartel and Borjas (1978, 1981)], this strand of the literature is often based on samples of young workers [Antel (1991); Topel and Ward (1992); Light and McGarry (1998)], as is the case in the present work.

We contribute to the literature with an empirical strategy which attempts to deal with both endogeneity and unobserved heterogeneity, namely, difference-in-differences propensity score matching. Gash and McGinnity (2007) and Ham, Li and Reagan (2006) use this technique to study wage growth as well. The former compare wages, wage growth and labour market outcomes of temporary workers relative to a matched sample of permanent workers with similar characteristics in Germany and France. The latter measure the effect of internal job migration on subsequent wages of young men in the U.S.. We also contribute to our knowledge of the Spanish labour market by adding several nuances to previous literature [García-Pérez and Rebollo-Sanz (2005) and Arranz *et al.* (2005)], such as focusing on youths and allowing for a more exhaustive typology of movements between jobs.

2. The ECHP, a first look at the sample

The ECHP gathers information on several socio-economic aspects in the European Union, including labour market issues. It is longitudinal in nature and the information about job characteristics is quite rich. These two features make it very relevant for our study. The information on job characteristics allows us to compute (both gross and net) hourly wage and tenure with the employer. The former is computed from the current monthly wage and the length of the working week.

(2) The wage effects of cumulative mobility have not received as much attention as the distinction between direct mobility and mobility via unemployment or voluntary and involuntary mobility. Keith and McWilliams (1995) and Stevens (1997) are good examples of papers on cumulative mobility.

Tenure in the current job is derived from the starting date of the relationship with the current employer.

There is no explicit question in the survey about recent changes across employers. Job mobility is computed when the time between two subsequent interviews in employment (usually, around one year) is longer than the tenure in the current job reported in the second interview. For completeness, we have used the calendar information regarding monthly status during year t-1 to detect job interruptions between two subsequent interviews. We distinguish between "short-term interruptions" (within a year) and "long-term interruptions" (within a two-year span). Job interruptions and their impact on wage growth are only observable among youths reporting at least two (either consecutive or two-year distant) wages. This condition is not randomly distributed and it calls for a correction of the selection bias, as explained in Section 3.

Every employed person will also report whether she experienced unemployment before accessing the current job and why she left the previous one. The first question will allow us to distinguish between direct movements and moves via unemployment detected from 1995 until the date of the current interview[3]. The second question enables us to distinguish between voluntary and involuntary movements[4]. Lastly, the use of information on monthly labour market status between each pair of subsequent interviews allows us to distinguish between one-time and multiple (when there are two or more job interruptions between the two interviews) interruptions.

In order to observe wage increases, we consider workers who were under 29 years old in 1994 and who registered at least two positive subsequent[5] wages during the period 1995-2001. Otherwise, wage increases would not be observed. Studies on youth in Mediterranean countries usually admit youths to be up to 29 or even 34 years old. Allowing for up to 29 year-olds in the initial sample is, in our case, crucial for young university graduates to be well represented in the sample. Unfortunately, sample sizes are not large enough to allow for a separate analysis of men and women. Gender is, though, a control variable in the PSM equation.

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(3) We start the analysis in wave 2 (data corresponding to 1995) because wave 1 (1994) has no information on the type of contract, which is a very important variable in defining the probability of a job interruption (our propensity score equation).

(4) Voluntary job mobility is detected when the interviewee reports having left her previous job because she found a better job, because she got married, or because she had to look after her own children, resume her education, do military service or even retire. Family reasons are a very rare cause of job mobility amongst Spanish youths. Involuntary job moves refer to interruptions obliged by the employer, the end of the contract, the sale or closure of the firm, the need to look after others (not children), one's own illness or following a partner who moved. In the question about reasons for leaving the last job, "other reasons" is a residual category which only accounts for 5% of the sample. The corresponding observations have been excluded from the analysis since we were not able to distinguish whether they had left their jobs voluntarily or involuntarily.

(5) When the observation window is two years long, the requirement is to register non-missing wages in *t* and *t+2*.

Let us now report the incidence of job mobility and the size of wage growth amongst the different types of young movers versus stayers in Spain. Table 1 shows the main transition rates from employment, split into four types of job interruptions. During the period of observation (1995-2001), around 30% of all observations in employment experienced at least one interruption. Most job interruptions last less than one year. When we focus on these short-term interruptions, we see that more than half of them were involuntary, nearly half of them required a spell of unemployment and multiple interruptions within a year were quite infrequent. Wages in wave t (before the potential interruption) were higher amongst stayers than among all types of movers.

As regards wage growth, Table 1 shows that certain types of movers experience significantly higher wage increases than stayers[6]: this is the case of workers experiencing short-term interruptions, voluntary movers, direct movers and those that experienced a one-time interruption. No significant wage increases compared to stayers were detected in those who registered a long interruption, involuntary movers, those who experienced unemployment and those who registered more than one job interruption between the two interviews[7]. As a result, wages in t+1 (t+2 for long-term interruptions) are more similar for successful movers compared to stayers, and the wages of those who experience some sort of negative mobility lag behind.

In our empirical strategy, we have compared stayers and movers by matching stayers and movers with a similar propensity to experience a job separation and return to paid employment. This propensity has been estimated via a multinomial logit model for each type of job move on the basis of a set of explanatory variables related to personal, job and regional characteristics. Table 2 shows the mean values for those variables for stayers and all types of movers. It shows that stayers are more experienced than movers and have been working with the current employer for longer. They are, consequently, a bit older than movers. Long-term movers, involuntary movers, those moving via unemployment and those with multiple interruptions hold tertiary education degrees less often. Movers more often look for a job before the interruption, and are initially worse paid. Their status in their current job (measured by the ISEI, International Socio-economic Index) is a bit lower and usually below the average for their qualification and gender.

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(6) This may be seen in our target variable, the difference between the log of the wages at the beginning and at the end of the observation window (diff in diffs). For instance, simple t-tests for equity of means in the distribution of wage increase (diffs in logs) show that short-term movers, register a significantly higher relative wage increase (0.172) than stayers (0.126), whereas wage growth among long-term movers (0.133) does not significantly differ from stayers', i.e., the difference between the two means is not significantly different from zero.

(7) Wage growth among this type of job movers is computed as the relative difference between the hourly wage in the interview in wave *t* and the hourly wage in the interview in t+1 provided there has been more than one job interruption between t and t+1 regardless of the actual wages in the intermediate spells. Although we are aware that this strategy entails missing important information, we are not able to compute hourly wages for intermediate employment spells between the two interviews. Moreover, in order to generate really comparable outcomes for one-time movers, stayers and multiple movers, the time gap between the two observations of wages must be the same (namely, the distance between two consecutive interviews, i.e., around a year).

Table 1: Mobility rates for different types of job mobility.
Proportions are taken from a sample of employees who register the relevant wages in order to compute wage increases

	Freq (%)	Wage in t (logs)	Wage Increase (diff)	Wage Increase (d-in-d)	Wage Increase (%)	Freq (N)
Two-year observation window						
Stayers	69.59	6.506	0.126	–	13.46	3368
Short-term movers	22.81	6.373	0.172	0.0456	18.76	1104
Long-term movers	7.60	6.291	0.133	0.0062	14.17	368
One-year observation window						
Stayers	71.9	6.527	0.062	–	6.44	5040
Voluntary movers	11.09	6.418	0.123	0.0609	13.12	773
Involuntary movers	17	6.388	0.068	0.0059	7.07	1184
Direct movers	14.14	6.444	0.104	0.0414	10.94	986
Moving through unemployment	13.95	6.355	0.076	0.0136	7.90	971
One-time interruptions	23.82	6.404	0.091	0.0287	9.54	1834
Multiple interruptions	4.28	6.336	0.074	0.0115	7.67	123

Table 1: Mobility rates for different types of job mobility.
Proportions are taken from a sample of employees who register the relevant wages in order to compute wage increases
(continuation)

Differences in average wage increase compared to stayers: are they statistically significant?

	Wage Increase (diff)	Ho: diff == 0		Ha: diff < 0	Ha: diff != 0	Ha: diff > 0
Two-year observation window		t	D.f.	*Pr(T < t)*	*Pr(|T| > |t|)*	*Pr(T > t)*
Stayers	0.126					
Short-term movers	0.172	4.522	4472	1.0000	0.0000	0.0000
Long-term movers	0.133	0.474	3736	0.6823	0.6354	0.3177
One-year observation window						
Stayers	0.062					
Voluntary movers	0.123	5.957	5811	1.0000	0.0000	0.0000
Involuntary movers	0.068	0.678	6222	0.1495	0.4980	0.6269
Direct movers	0.104	4.506	6024	1.0000	0.0000	0.0000
Moving through unemployment	0.076	1.442	6009	0.925	0.751	0.687
One-time interruptions	0.091	3.854	6872	0.9999	0.0001	0.0001
Multiple interruptions	0.074	0.486	5161	0.075	0.249	0.313

Source: ECHP (1995-2001), Eurostat.

Table 2: MOVERS' AND STAYERS' PROFILES ACCORDING TO THE VARIABLES USED IN THE PSM

	Two-year observation window			One-year observation window						
					willingness to move		direct or via unemployment		one-time versus multiple movers	
Variable	stayers	short-term	long-term	stayers	volunt	Invol	Direct	via unempl	One-time movers	Multiple movers
Female	0.385	0.346	0.340	0.386	0.318	0.395	0.330	0.401	0.363	0.390
Potential experience in the labour market (years)	8.054	7.650	6.204	8.159	7.382	7.370	7.482	7.266	7.352	7.715
Tertiary education	0.323	0.263	0.166	0.332	0.298	0.242	0.288	0.239	0.270	0.171
Upper secondary education	0.264	0.237	0.277	0.260	0.254	0.237	0.244	0.243	0.246	0.203
Holds her first job	0.332	0.122	0.266	0.325	0.155	0.144	0.166	0.130	0.154	0.057
Working week	41.56	41.70	41.49	41.50	41.59	41.04	41.40	41.11	41.27	41.11
Looking for a different job in t	0.100	0.208	0.212	0.094	0.225	0.182	0.192	0.207	0.194	0.276
Initial wage (logs)	6.506	6.373	6.291	6.527	6.418	6.388	6.444	6.355	6.404	6.336
Tenure (years)	3.771	1.191	0.73	3.693	1.363	0.988	1.342	0.931	1.183	0.421
ISEI (occupational status)	38.76	35.81	34.05	38.90	36.73	35.10	37.03	34.44	36.00	31.95
Difference with average ISEI for the same qualification	-0.453	-2.210	-2.623	-0.446	-1.873	-2.702	-1.425	-3.339	-2.229	-4.553
Public sector	0.182	0.097	0.101	0.172	0.071	0.126	0.083	0.126	0.104	0.106
Overqualified	0.633	0.658	0.625	0.634	0.642	0.645	0.621	0.667	0.647	0.602
Supervised, does not supervise others	0.773	0.850	0.886	0.777	0.834	0.873	0.844	0.872	0.858	0.862
Previous specific training	0.522	0.466	0.402	0.513	0.481	0.419	0.459	0.427	0.451	0.333

Table 2: MOVERS' AND STAYERS' PROFILES ACCORDING TO THE VARIABLES USED IN THE PSM (continuation)

	Two-year observation window			One-year observation window						
					willingness to move		direct or via unemployment		one-time versus multiple movers	
Variable	stayers	short-term	long-term	stayers	volunt	Invol	Direct	via unempl	One-time movers	Multiple movers
Temporary contract	0.372	0.662	0.715	0.359	0.568	0.720	0.628	0.692	0.654	0.748
Training paid by the employer	0.287	0.178	0.120	0.272	0.176	0.158	0.171	0.159	0.168	0.122
Looks after children	0.141	0.135	0.092	0.145	0.131	0.134	0.136	0.130	0.131	0.154
Comes from unemployment	0.494	0.598	0.628	0.474	0.455	0.661	0.407	0.756	0.571	0.715
Studies and works	0.083	0.084	0.103	1.076	1.082	1.075	1.082	1.073	0.081	0.033
Regional unemployment rates (for age and gender)	21.27	22.59	26.29	20.15	20.85	22.31	20.57	22.92	21.71	22.12
Regional employment rates (for age and gender)	57.27	54.39	47.57	58.40	55.74	53.99	56.14	53.21	54.73	54.01
Married	0.435	0.341	0.266	0.433	0.326	0.317	0.339	0.302	0.318	0.358
Number of cases	3368	1104	368	5040	773	1184	986	971	1834	123
Agriculture	0.025	0.080	0.068	0.024	0.050	0.081	0.046	0.093	0.061	0.195
Manufacturing+mining	0.230	0.221	0.223	0.231	0.202	0.238	0.235	0.212	0.227	0.171
Utilities construction	0.105	0.162	0.158	0.112	0.163	0.162	0.172	0.152	0.158	0.236
Sales hotels	0.215	0.232	0.269	0.220	0.263	0.209	0.251	0.210	0.236	0.154
Transport	0.051	0.057	0.038	0.053	0.060	0.048	0.048	0.058	0.055	0.024
Finance property	0.109	0.079	0.076	0.105	0.101	0.072	0.091	0.075	0.085	0.065

Table 2: MOVERS' AND STAYERS' PROFILES ACCORDING TO THE VARIABLES USED IN THE PSM (continuation)

	Two-year observation window			One-year observation window						
					willingness to move		direct or via unemployment		one-time versus multiple movers	
Variable	stayers	short-term	long-term	stayers	volunt	Invol	Direct	via unempl	One-time movers	Multiple movers
Other industry	0.056	0.056	0.038	0.054	0.063	0.047	0.047	0.061	0.056	0.024
Not satisfied with one's job in terms of wages	0.266	0.324	0.318	0.259	0.345	0.298	0.316	0.317	0.311	0.398
Not satisfied with one's job in terms of job security	0.153	0.322	0.380	0.143	0.266	0.370	0.287	0.372	0.321	0.447
Not satisfied with one's job in terms of type of work	0.091	0.149	0.158	0.089	0.147	0.139	0.145	0.140	0.141	0.163
Not satisfied with one's job in terms of working hours	0.163	0.213	0.253	0.166	0.202	0.181	0.194	0.184	0.188	0.211
Not satisfied with one's job in terms of working times	0.145	0.156	0.196	0.138	0.145	0.149	0.140	0.156	0.147	0.163
Not satisfied with one's job in terms of working conditions	0.124	0.140	0.149	0.118	0.145	0.131	0.137	0.136	0.135	0.154
Not satisfied with one's job in terms of distance to job	0.156	0.184	0.207	0.152	0.163	0.170	0.159	0.175	0.167	0.171
Not satisfied with the job or main activity	0.090	0.161	0.198	0.090	0.164	0.139	0.149	0.149	0.146	0.195
Not satisfied with the financial situation	0.226	0.289	0.326	0.214	0.316	0.273	0.281	0.299	0.286	0.350

Table 2: MOVERS' AND STAYERS' PROFILES ACCORDING TO THE VARIABLES USED IN THE PSM (continuation)

	Two-year observation window			One-year observation window						
					willingness to move		direct or via unemployment		one-time versus multiple movers	
Variable	stayers	short-term	long-term	stayers	volunt	Invol	Direct	via unempl	One-time movers	Multiple movers
Not satisfied with housing situation	0.053	0.081	0.073	0.053	0.082	0.073	0.081	0.071	0.075	0.098
Not satisfied with the amount of leisure time	0.271	0.292	0.228	0.253	0.286	0.254	0.276	0.257	0.270	0.220
Likelihood of remaining in the sample in t+1 (or t+2)	0.530	0.369	0.292	0.523	0.377	0.332	0.366	0.333	0.353	0.306
Number of cases	3368	1104	368	5040	773	1184	986	971	1834	123

Source: ECHP (1995-2001), Eurostat.

They work in the public sector much less often than stayers and, as expected, they hold temporary contracts much more often than stayers do. They are more often supervised (and do not supervise anybody). Sometimes their jobs do not require prior specific training, and they are paid for their training less often than stayers. They experienced unemployment before the current job more often than stayers, and they live in regions with higher unemployment rates and lower employment rates. The proportion of married youths is lower amongst movers. Movers are more often employed in construction and less often in finance and real estate and public services. Negligible differences are found between stayers and movers as regards overqualification, working hours, full-time or part-time status, the incidence of formal education while in employment and devoting time to child care.

Apart from objective employment and personal characteristics we also control for subjective information, namely, satisfaction with several job characteristics (wages, job security, type of work, working hours, working times, working conditions and distance to job) and other aspects of life (main activity, financial situation, housing situation and leisure time). Movers are, on average, more dissatisfied with their jobs than stayers. Voluntary movers and "multiple movers" (those who move more than once within a single year) tend to be more dissatisfied with wages and their financial situation**.** Those experiencing unemployment, long-term interruptions, involuntary moves and multiple interruptions are more dissatisfied not only than stayers but also more than those registering direct, voluntary, short-term and one-time interruptions. Differences in satisfaction as regards activity and financial situation are similar to the job-related ones. Finally, no significant differences in satisfaction with regard to leisure time and housing situation are found.

3. Methodological issues and empirical strategy adopted

3.1. Why Difference-in-differences propensity score matching?

Difference-in-differences propensity score matching may overcome both endogeneity in the main explanatory variable (i.e., job mobility) and unobserved heterogeneity. First of all, it is an alternative to IV[8], since it compares individuals with a similar propensity to move between jobs as a way to control for endogeneity. Furthermore, it has common features with before-after estimators, which are a common strategy for tackling unobserved heterogeneity[9].

(8) The main problem with IV is that it is very difficult to find a good instrument, particularly in the context of young people. While some authors use home ownership as an instrumental variable for job mobility in samples of adults, in the case of youths (and, more particularly, in the case of Spanish youths) this variable does not register enough variability.

(9) If control of unobserved heterogeneity does not cancel the explanatory power of mobility, we should accept a causal link between the two variables. However, the rationale behind this relationship, that is, the causal mechanism, is beyond the scope of this paper. The causal mechanism in the job mobility-wage mobility relationship may come from either job search, job matching and/or specific human capital considerations. The three arguments are not self-exclusive and it is difficult to determine which of the explanations carries more weight. Generally, researchers use 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

In the PSM strategy, we take different kinds of transitions between jobs as different "treatments" for workers to maximise their lifetime income. If workers moved randomly between jobs, a direct comparison of wage growth between movers and stayers would yield unbiased estimates of the average effect on the "treatment" (movement). However, workers do not move randomly and random assignment to different types of movements must be simulated. PSM compares pairs of workers who are similar to each other in an exhaustive number of (observable) characteristics and differ in the type of "treatment" (movement) they register. If selection into treatment were exclusively based on these observable characteristics, matching would yield unbiased estimates of the average treatment effects. This is quite unlikely since selection into treatment may also be grounded on unobservable characteristics [Caliendo and Hujer (2005)]. The inclusion of control for unobservables is possible by combining matching with difference-in-differences [Heckman *et al.* (1997) and Heckman *et al.* (1998)], which is preferable to common/standard propensity score matching [Smith and Todd (2005)].

3.2. The evaluation problem in a multi-treatment context

This paper compares the outcome of several types of movement between jobs. In the evaluation literature, this is labelled as "multiple treatment". Pioneering papers on multiple treatment are Lechner (2001) and Imbens (2000). Larsson (2003) implements the technique presented in 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 treatments, denoted by an assignment indicator $T \in \{0, 1, \ldots, M\}$. We will use the term "treatment" for each type of movement between jobs. The set of variables that may define the probability of each treatment (covariates) is designated by X. The outcomes of the treatments (the increase in wages from t = 0 to t = 1) are denoted by $\{Y^0, Y^1, \ldots, Y^M\}$. We 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: for m = 1 (short-term, voluntary, direct or "one-time" moves), only Y^1 is observed, so the remaining M outcomes are called counterfactuals.

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

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

θ_0^{ml} in equation (1) denotes the expected average treatment effect on treatment m relative to treatment l for participants in treatment m. It may be decomposed in the following way:

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 job search strategies.

$$\theta_0^{ml} = E(Y^m - Y^l \mid X, T=m) = \\ = E(Y^m \mid X, T=m) - E(Y^l \mid X, T=l) - [E(Y^l \mid X, T=m) - E(Y^l \mid X, T=l)] \quad [2]$$

The latter right-hand-side term is the bias conditional on X, which is assumed to be zero. The technique consists of replacing the unobserved outcomes of the participants in treatment m had they received treatment l with the outcomes of the participants in treament l with the same X characteristics [Blundell and Costa Dias (2000)].

As Larsson (2003) notes, the 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. The average causal effects defined by equation [1] can be identified under a given assumption, called the Conditional Independence Assumption (CIA): the selection between the groups of participants in treatment m and treatment l is captured by a vector of observable characteristics, X. In order to accept the CIA, the researcher needs quite a rich data set to claim to be controlling for all the factors influencing both the propensity to receive a given treatment and its outcome. The vector of variables characterising jobs and satisfaction in the ECHP is very exhaustive. Our X vector consists of 34 characteristics[10] (see Table 2 for a display of mean values of all of them for all stayers and types of movers). However, as we will see later, it might not be exhaustive enough.

A formalisation of the CIA assumption in the multiple treatment case is available in Larsson (2003), and the identification problem and the estimation procedure are described in Lechner (2001) and Larsson (2003). This procedure entails estimating the ATT effect for every pair of treatments twice: first, using treatment m as the treated group, and treatment l as the control group, and then, the other way around. As we always compare three possible situations (namely, stayers, "type A" movers and "type B" movers), we compute six ATT effects in each of the four typologies of job separation considered.

3.3. 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 determinants of movements between jobs. Heckman *et al*. (1998) proposed combining matching with difference-in-differences (DID). In doing so, we may control for selection into the treatment group caused by unobserved variables [Aassve *et al.* (2006)]. The main matching hypothesis is now stated in terms of the before-after evolution instead of levels [Blundell and Costa Dias (2000)]. It means that controls have evolved from a pre- to a post-treatment period in the same way as treatments would have done had they not been treated. We perform the matching on the vector of variables X. Instead of comparing Y (logarithm of waves) for movers and

(10) As a consistency check, the same estimations were obtained with a less exhaustive set of control variables in the propensity score equation, keeping only objective aspects of the job as covariates (23 variables). Results for this second specification are available in Davia (2009).

stayers in t+1, we now compare the mean change in wages from one period t to another, t+1, following the difference-in-differences approach:

$$DID = E(Y_{\mathrm{m}}^{t+1} - Y_{\mathrm{m}}^{t}) - E(Y_1^{t+1} - Y_1^{t}) = E(\Delta_m) - E(\Delta_t) \qquad [3]$$

As Aassve *et al*. (2006) explain, under the assumption that unobserved heterogeneity is time-fixed, its effect will be netted out by taking the first difference [Heckman *et al*. (1998)]. The DID-PSM estimator is argued to be more robust since it eliminates temporarily invariant sources of bias [e.g., Dehejia and Wahba (1998, 1999) and Smith and Todd (2005)]. The effect of the treatment can now be estimated over the common support in the context of longitudinal data as follows:

$$DID - PSM = E_{p(X|T=m)}\{[E(\Delta_m \mid T = m, p(X)) - E(\Delta_t \mid T = l, p(X))] \mid T = m\} \qquad [4]$$

where p(X) is the propensity to participate in each treatment (the propensity score), obtained from a multinomial choice model, given the set of characteristics X used to perform the matching. Under the matching assumptions, the conditional independence remains valid when controlling for P(X) instead of X.

Expression [4] may be written in a more formal way [Blundell and Costa Dias (2000)] as:

$$\theta_{MMDID}^{ml} = \sum_{m \in T}\left[\left(Y_1^m - Y_0^m\right) - \sum_{l \in T} w_{ml}\left(Y_1^l - Y_0^l\right)\right] w_m \qquad [5]$$

where MMDID denotes "method of matching with difference-in-differences". w_{ml} is the weight placed on an observation l comparison for an individual receiving treatment m. This weight is the distance between those treated with treatment m and l based on a kernel (Epanechnikov) function on the propensity scores of observations in treatment m and l. Finally, w_m accounts for the re-weighting that reconstructs the outcome distribution for the treated sample. It reconstructs the outcome of the treated sample by re-weighting with kernel weights [Blundell and Costa Dias (2002)][11].

The matching procedure based on the PSM implies that all variables have to be balanced between treated and control units. This is the so-called balancing property. Following Smith and Todd (2005), we use a t-test for equality of means for each covariate, X, before and after matching. Accepting equality of means between treated individuals in group m and matched control units in group l means that unit controls are not different from those that are treated in group m except for the treatment status. The balancing property is satisfied in most of our estimates. The complete set of results is not displayed here since it would require 24 tables (4 typologies of job interruptions times 3 possible "treatments" times 2 ATT effects per outcome (θ_0^{ml} and θ_0^{lm})). Instead, we display the median absolute

(11) Re-weighting is necessary if the group of the treated individuals is larger than the group of controls due to oversampling of treated individuals. This is often the case here since, in the multi-treatment context, we compute two ATT effects per outcome (θ_0^{ml} and θ_0^{lm}). The procedure implies taking first treated m as "treated" and those under treatment l as "controls", and switching the roles afterwards.

standardised bias over all the regressors in Table 2 for the 24 combinations of treatments and outcomes. Columns F and G in Table 2 display the bias before and after the matching[12], when it becomes considerably smaller. An additional indicator of the quality of the balancing is the comparison between columns H and I in Table 2. They register the pseudo R^2 of a probit on the conditional probability of treatment, before and after the matching[13], and in all our specifications it becomes considerably reduced after the matching. After testing several specifications of the propensity score matching equation, the specification finally chosen for this article is the one that reduces both the bias and the explanatory power of the propensity score equation most (for an alternative specification and the relevant ATT, see Davia (2009).

The main problem in the estimation of standard errors of the ATT effect is that the estimated variance of the ATT effect should also include the variance due to the estimation of the propensity score. The common solution to this problem, bootstrapping, is implemented in the module developed by Leuven and Sianesi (2003) for STATA.

3.4. Sample selection bias

Our sample consists of youths with at least two consecutive observations of wages. For two-year long observation windows, there can be up to two waves between two comparable observations. This is quite a restrictive selection that must be taken into account in our empirical strategy[14]. In order to do so, we have computed the probability of reporting non-missing wages for all respondents in two subsequent interviews (or two waves later for potential long-term movers). This likelihood has been estimated via a binomial probit model of a pool of all young respondents for waves 2 to 7 (or 6 for potential long-term movers). The explanatory variables may be classified into two groups: those that influence the likelihood of being interviewed in the next wave and those that influence the likelihood

(12) The standardised bias is a measure of the distance in marginal distributions of the X – variables in the propensity score matching equation. For a given covariate X, the standardised 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 simple variances in the full treated and non-treated groups. Table 2 displays the overall (across all covariates) bias before and after matching, which becomes smaller after matching in all cases.

(13) The propensity score is estimated before and after the matching. The first one is necessary to perform the matching on the propensity score, whereas the second one is used as a quality check of the matching. This second estimation of the propensity score uses a sample of participants and matched non-participants only. Then, the pseudo-R^2 of the propensity score equations are compared: the pseudo-R^2 indicates how well the regressors X explain the participation probability. But if only treated and matched non-participants are taken in the second propensity score equation, there should be no systematic differences in the distribution of covariates. Therefore, the pseudo-R^2 of the propensity score equation should be (and is) fairly low after matching.

(14) I thank one of the referees for pointing out this issue in a prior version of the paper.

of being employed in the next interview. The first group consists of a set of household and personal variables. Household variables include whether the household splits in any of the following interviews, whether it has recently moved to the house, whether it will move in any of the following interviews and whether the members of the household have recently migrated or will migrate during the following interviews. Personal variables include living alone, not being present at home in the following interview, leaving the survey prematurely and the year of entry into the survey. Variables assumed to affect the likelihood of being employed in the subsequent interview are gender, educational attainment, age, status as head of the household, holding an open-ended contract, working in the public sector and potential experience in the labour market (grouped into three dummies). Results are reported in Table 1, together with the mean values of the variables used in the model.

Table 1: SAMPLE SELECTION: PROBABILITY OF BEING EMPLOYED IN TWO CONSECUTIVE INTERVIEWS (RESTRICTED SAMPLE) OR IN T+2 (INITIAL SAMPLE)

	Initial sample		Restricted sample		
	Coef.	z	Coef.	z	Mean values
Male	0.203	11.09	0.201	10.80	0.509
Upper secondary	-0.176	-7.47	-0.188	-7.83	0.327
Tertiary	-0.024	-1.10	-0.036	-1.59	0.442
Age in the first interview	0.032	12.76	0.031	12.40	21.768
The HH will NOT split during the observation window	-0.36	-5.73	-0.166	-6.83	0.741
The interviewee is the head of the HH	0.259	11.59	0.270	11.87	0.232
The interviewee lives alone	-0.157	-2.67	-0.184	-3.07	0.019
Open-ended contract	1.146	54.45	1.157	53.90	0.185
Public sector worker	0.661	19.90	0.645	19.04	0.062
The HH moved to that address less than 3 years before the first interview	0.093	2.88	0.098	2,95	0.099
The HH will move during the observation window	0.022	0.94	-0.033	-1.41	0.304
The HH has recently migrated or will migrate during the observation window	-0.154	-3.81	-0.212	-5.08	0.049
The HH will split in the following interview	1.560	22.82	2.680	25.77	0.025

Table 1: SAMPLE SELECTION: PROBABILITY OF BEING EMPLOYED IN TWO CONSECUTIVE INTERVIEWS (RESTRICTED SAMPLE) OR IN T+2 (INITIAL SAMPLE) (continuation)

	Initial sample		Restricted sample		
	Coef.	z	Coef.	z	Mean values
The interviewee will not be present in the following interview	-1.155	-23.98	-2.253	-24.34	0.105
The interviewee will abandon the survey before wave 8 (2001)					
From one to four years of LM	-0.114	-4.38	-0.026	-0.98	0.172
experience in the first interview	-0.151	-5.99	-0.145	-5.66	0.726
Five or more years of LM experience in the first interview	0.137	4.12	0.133	3.93	0.094
Entered the survey in wave 3 (1996)	-0.167	-4.72	-0.163	-4.53	0.077
Entered the survey in wave 4 (1997)	-0.247	-6.67	-0.250	-6.62	0.080
Entered the survey in wave 5 (1998)	-0.332	-6.28	-0.354	-6.53	0.037
Entered the survey in wave 6 (1999)	-0.525	-8.93	-0.515	-8.65	0.031
Entered the survey in wave 7 (2000)	-0.953	-11.13	-0.926	-10.72	0.021
Intercept	-1.501	-19.98	-1.481	-19.35	
Total number of cases	33001		33001		
Log likelihood	-14055.3		-13578.2		
Pseudo R2	0.2451		0.2583		
LR chi2(22)	9126.6		9458.04		
Prob > chi2	0.000		0.000		
Proportion of interviewees in initial sample					0.252
Proportion of interviewees in restricted sample					0.243

Source: ECHP (1995-2001), Eurostat.

Table 2: QUALITY INDICATORS FOR THE PROPENSITY SCORE MATCHING

Long-term and short-term moves across jobs	A	B	C	D	E	F	G	H	I	J
Short-term moves-stayer	4.308	4.091	53	1.070	3.238	12.98	0.95	0.179	0.109	0.000
Stayer-Short-term moves	4.308	4.094	161	3.238	1.070	12.98	4.01	0.179	0.109	0.000
Long-term moves-stayer	3.591	3.413	17	353	3.238	20.40	4.07	0.23	0.093	0.000
Stayer-Long-term moves	3.591	3.413	161	3.238	353	20.40	7.76	0.23	0.093	0.000
Short-Long-term moves	1.423	1.353	53	1.070	353	9.21	3.52	0.082	0.034	0.163
Long-Short-term moves	1.423	1.353	17	353	1.070	9.21	2.07	0.082	0.034	0.163

Willingness to move: voluntary versus involuntary movements	A	B	C	D	E	F	G	H	I	J
Voluntary move-stayer	5.582	5.304	37	755	4.827	13.09	1.80	0.135	0.04	0.000
Stayer-voluntary	5.582	5.304	241	4.827	755	13.09	4.35	0.135	0.04	0.000
Involuntary-Stayer	5.966	5.669	56	1.139	4.827	15.29	1.23	0.204	0,.18	0.000
Stayer -involuntary	5.966	5.669	241	4.827	1.139	15.29	3.61	0.204	0.118	0.000
Voluntary-involuntary	1.894	1.801	37	1.139	755	7.14	1.98	0.092	0.051	0.000
Involuntary-voluntary	1.894	1.800	57	1.139	755	7.135	2.062	0.085	0.057	0.000

Table 2: Quality indicators for the propensity score matching (continuation)

Direct moves versus moves through unemployment	A	B	C	D	E	F	G	H	I	J
Direct move-no move	5.778	5.490	47	951	4.827	12.02	1.43	0.137	0.053	0.000
No move-direct	5.778	5.490	241	4.827	951	12.02	3.40	0.137	0.053	0.000
Indirect-no move	5.770	5.482	47	943	4.827	17.52	1.23	0.23	0.129	0.000
No job move-indirect	5.770	5.482	241	4.827	943	17.52	4.82	0.23	0.129	0.000
Direct- indirect move	1.894	1.797	47	951	943	5.99	2.82	0.133	0.088	0.000
Indirect-direct move	1.894	1.800	47	943	951	5.99	1.30	0.133	0.088	0.000
One-time versus multiple movements	**A**	**B**	**C**	**D**	**E**	**F**	**G**	**H**	**I**	**J**
One-time move-stayer	6.607	6.277	89	1.780	4.827	14.05	0.91	0.164	0.097	0.000
Stayer-one-time move	6.607	6.277	241	4.827	1.780	14.05	3.43	0.164	0.097	0.000
Multiple move-Stayer	4.941	4.690	10	114	4.827	22.41	11.55	0.302	0.132	0.000
Stayer-multiple move	4.941	4.691	240	4.827	114	22.41	12.86	0.302	0.132	0.000
One-time-multiple move	1.894	1.800	89	1.780	114	13.17	5.84	0.12	0.023	0.999
Multiple-one-time move	1.894	1.800	5	114	1.780	13.17	6.15	0.12	0.023	0.999

A. Sample size; B: common support area; C: treated falling outside the common support (bandwidth 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 34 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 R^2 from probit estimation on the conditional mobility probability. It is an indicator of how well the regressors X explain the participation probability.
I: R^2 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 on few occasions.

Source: ECHP (1995-2001), Eurostat.

The estimated probability has been included in the set of regressors X of the multinomial logit used to define the propensity score[15]. Therefore, when matching similar treated individuals/groups/samples (in treatment m) and controls (in treatment l), we try to match observations with a similar propensity to belong to the sample. Table 2 shows that, in all cases, stayers register a higher probability of reporting two subsequent waves than movers. The distance in this likelihood is greater between stayers and long-term movers, involuntary movers, those moving via unemployment and multiple movers than between stayers and short-term, voluntary, direct and "one-time" movers. These differences confirm the need to control for sample selection; otherwise, we would omit an important variable in vector X.

3.5. Matching algorithms and common support

The matching algorithm, Kernel matching, creates a kernel-weighted average over multiple units in the comparison group (for a survey of the main algorithms, see Caliendo and Kopeining (2006)). Together with local linear matching, it is one of the algorithms recommended by Heckman to be used with DID matching. Additionally, it is not affected by the problems of nearest neighbour algorithm as regards the use of bootstrapping to compute standard errors of the ATT effect [Abadie and Imbens (2006)][16]. The general expression for Kernel matching is as follows:

$$w_{ml} = \frac{G\left(\frac{P(X)_m - P(X)_l}{a_n}\right)}{\sum_{l \in T} G\left(\frac{P(X)_m - P(X)_l}{a_n}\right)} \qquad [6]$$

where w_{ml} is the distance between those treated with treatment m and l denoted in [5], G(.) is a kernel (Epanechnikov) function, $P(X)_m$ and $P(X)_l$ are the propensity scores of observations in treatment m and l (controls), and a_n is a bandwidth parameter. After several tests, we chose a 0.05 bandwidth parameter[17]. 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 (X) (the propensity score) that have positive density within both distributions of (X) across those that are/have been treated and the controls. The trimming is set at 5%[18].

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(15) I am very grateful to Marco Caliendo for his suggestion about how to tackle sample selection.

(16) Abadie and Imbens (2006) are quite critical of the use of bootstrapping in the nearest neighbour algorithm. They argue that, due to the extreme non-smoothness of nearest neighbour matching, the standard conditions for the bootstrap are not satisfied, leading the bootstrap variance to diverge from the actual variance.

(17) We think it is a good compromise between a small variance and an unbiased estimate of the true density function: high bandwidth values yield a smoother estimated density function, therefore leading to a better fit and a decreasing variance. On the other hand, underlying features may be smoothed away by a large bandwidth but this increase biases the ATT effect estimate [Caliendo and Kopeining (2006)].

(18) In defining a trimming of 5%, we exclude not only 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. By excluding those individuals with the lowest 5 percent of values of P from the common support, we are constraining the common support and the number of available matches (increasing variance), but allowing for better matches (reducing bias).

4. Main results

The main results of the DID – PSM are displayed in Table 3: the observed outcome to be compared to the ATT, the bias and the standard deviation, together with a 95% confidence interval for the estimated ATT. Table 3 displays the result of a matching with the complete set of 34 covariates (see Table 2 for a description of them).

As mentioned in Section 3, in a multi-treatment context, PSM must be done in two directions, comparing each group against each of the others. This is because, in the multi-treatment case, the average treatment effect is not symmetric in the sense that $\theta_0^{ml} \neq - \theta_0^{lm}$[19]. We therefore estimate both θ_0^{ml} and θ_0^{lm} for each pair of "treatments." The difference between the observed differentials in wage growth across movers and stayers and the estimated ATT proves that the matching analysis contributes to adjusting the biased observed effect. If the ATT estimates were non-significantly different from the observed effect, job mobility and the type of job mobility itself should not be taken as endogenous, but the analysis shows that the empirical strategy used here (DID-PSM) has contributed to a better understanding of the relation between job mobility and wage mobility.

The first outcomes displayed in Table 3 refer to the effect of short-term versus long-term movements. Differences in wage growth for both short and long-term movers against stayers were significantly different from zero in the unmatched sample: short-term movers' wage growth was higher than stayers', long-term movers' wage growth was slightly higher than stayers' and short-term movers' wage growth was higher than long-term movers'. In the matched samples, differences in wage growth are negative and significant when we compare long-term movers against both stayers and short-term movers. This means that long-term movers register relative wage losses (lower wage growth) compared to the other groups. Once endogeneity and unobserved heterogeneity are controlled for, the corresponding advantage for short-term movers is reduced to one-third of that initially observed, and the relatively tiny advantage for long-term movers turns into a significant disadvantage. Anyway, we should qualify our results because of the small sample of long-term movers (see Table 2), which, compared to that of stayers, makes a quite unbalanced distribution of treated and control samples[20]. These

(19) As mentioned in Section 3, this asymmetry is due to the fact that m and l differ in a non-random fashion [Lechner (2001)]. This sample selection bias affects the validity of some of the results. The asymmetries among the ATTs are more pronounced the larger the imbalance in the sample sizes of the groups compared (i.e., when comparing multiple movers and involuntary movers with stayers). Anyway, when this asymmetry is more pronounced, the ATT are not significantly different from zero. The observed differences between the means of the target variable (wages increases measured in logs) between such types of movers and stayers were not significantly different from zero. In such cases, matching confirms the lack of an important effect of job mobility even when observed and (time-constant) unobserved heterogeneity are controlled for. Moreover, results across specifications are very consistent in most of the combinations of treated groups and controls.

(20) Davia (2009) shows some differences in the ATT of multiple movers versus stayers and the stayers-involuntary movers comparisons. Since only the ATT for the stayer-long-term mover is significantly different from zero, it is the one where differences across specifications deserve more attention. In this comparison the ratio controls-treated is 10:1. In such cases, since the

Table 3: RESULTS FROM DID PSM. ATTS EXPRESSED AS DIFFERENCES IN RATES OF WAGE GROWTH

Long-term and short-term moves across jobs	Observed	ATT estimate	Bias	Std. Err.[1]	95% Conf. Interval	
Short-term moves-stayer	0.0476	0.0135	-0.0003	0.0129	-0.0117	0.0388
Stayer-Short-term moves	-0.0476	-0.0259	-0.0010	0.0166	-0.0585	0.0067
Long-term moves-stayer	0.0076	-0.0483	0.0041	0.0204	-0.0884	-0.0083
Stayer-Long-term moves	-0.0076	0.0587	-0.0015	0.0271	0.0054	0.1119
Short- Long-term moves	0.0400	0.0616	-0.0013	0.0229	0.0166	0.1065
Long- Short-term moves	-0.0400	-0.0634	0.0005	0.0213	-0.1053	-0.0214
Voluntary versus involuntary movements	**Observed**	**ATT estimate**	**Bias**	**Std. Err.[1]**	**95% Conf. Interval**	
Voluntary job mob-stayer	0.0612	0.0439	0.0004	0.0123	0.0196	0.0681
Stayer-voluntary	-0.0612	-0.0434	0.0011	0.0147	-0.0722	-0.0146
Involuntary-Stayer	0.0091	-0.0103	0.0005	0.0113	-0.0325	0.0120
Stayer-involuntary	-0.0091	0.0031	0.0001	0.0139	-0.0243	0.0304
Voluntary-involuntary	0.0522	0.0480	0.0020	0.0172	0.0143	0.0818
Involuntary-voluntary	-0.0522	-0.0543	-0.0007	0.0170	-0.0876	-0.0210
Direct moves versus moves through unemployment	**Observed**	**ATT estimate**	**Bias**	**Std. Err.[1]**	**95% Conf. Interval**	
Direct move-stayer	0.0439	0.0293	0.0006	0.0111	0.0076	0.0511
Stayer-direct move	-0.0439	-0.0302	-0.0004	0.0124	-0.0546	-0.0059
Indirect move-Stayer	0.0157	-0.0124	0.0012	0.0132	-0.0383	0.0134
Stayer-indirect move	-0.0157	-0.0093	0.0001	0.0177	-0.0441	0.0255
Direct- indirect move	0.0283	0.0402	0.0000	0.0179	0.0050	0.0753
Indirect-direct move	-0.0283	-0.0500	-0.0001	0.0156	-0.0805	-0.0194
One-time versus multiple movements	**Observed**	**ATT estimate**	**Bias**	**Std. Err.[1]**	**95% Conf. Interval**	
One-time movement-stayer	0.0308	0.0094	0.0004	0.0095	-0.0093	0.0280
Stayer-one-time movement	-0.0308	-0.0177	0.0008	0.0116	-0.0404	0.0050
Multiple movement-Stayer	0.0147	0.0032	-0.0064	0.0346	-0.0648	0.0713
Stayer-multiple movement	-0.0147	-0.0104	0.0025	0.0403	-0.0896	0.0689
One-time-multiple movement	0.0161	0.0172	-0.0011	0.0301	-0.0419	0.0763
Multiple-one-time movement	-0.0161	-0.0142	-0.0044	0.0307	-0.0744	0.0461

1. The standard error has been computed via bootstrapping (500 replications).

Source: ECHP (1995-2001), Eurostat.

results are consistent with Arranz *et al*. (2005), which shows that job interruptions have a negative impact on wage dynamics that is stronger the longer the interruption is (particularly if it entails a spell of unemployment).

The results of estimated wage differential effects of voluntary versus involuntary mobility and non-mobility confirm the expected results in the literature: moving voluntarily improves wage growth significantly more than staying in the same job, and a parallel (but negative) result is obtained for stayers versus voluntary movers. Those moving unwillingly do not perform significantly worse than stayers, and the result is confirmed when comparing stayers to involuntary movers. Finally, voluntary movers perform significantly better than involuntary movers, and vice versa. Comparing equivalent workers of different groups instead of the mean values of the outcome variable across groups slightly reduces the effect for voluntary interruptions and, in the case of involuntary movers, the correction is not strong enough to make the estimated wage increase differential become significant. Results are consistent with those of Arranz *et al*. (2005), where wage gains from voluntary transitions and non-scarring effects of involuntary transitions are found compared to stayers in a sample of adult (18-58 year olds) males.

The third outcome studied refers to wage growth of direct job-to-job transitions compared to movers who experience a spell of unemployment between jobs. Here the results are somewhat less significant than in the previous outcome, but it may still be observed that direct (job-to-job) moves are more rewarding than staying with the same employer. Movers via (short-termed) unemployment and stayers do not differ in terms of subsequent wage growth, and moving directly from one job to the next is better than experiencing a spell of unemployment in between. Those who move directly between jobs experience a slightly more pronounced wage increase than stayers and a considerably higher one than those who move through unemployment, even if it is short-lived. Compared to what was seen in the first stage of the descriptive analysis, the effect of job-to-job transitions is considerably reduced (nearly halved) once matching is performed, while the relative advantage of going directly from one job to the next compared to spending time unemployed increases (is more pronounced) in the matched sample. The wage premia for direct job-to-job transitions is also observed in García-Pérez and Rebollo-Sanz (2005)[21]; they also find a relative disadvantage for movers through unemployment compared with both job-to-job movers and stayers. This penalty for transitions through unemployment is somewhat larger for youths than for adults when comparing stayers but similar comparing job-to-job movers.

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matching is performed on the propensity score, the algorithm may match certain movers with different stayers in different specifications of the propensity score equation. And if the control group composition varies slightly across specifications, the ATT will accordingly vary across specifications. This is why we need to qualify our results for these particular groups and take advantage of the results stemming from other papers based on different methodology or samples.

(21) They label job-to-job transitions as voluntary and transitions through unemployment as involuntary. This is due to the fact that they work with calendar variables in the ECHP and they are not provided with the precise reasons for moving in every single interruption they find. Using spells of unemployment as a proxy for willingness to move is an extended practice, so we may compare our “(in)direct” interruptions with García-Pérez and Rebollo-Sanz’s “(in)voluntary” interruptions.

The final set of results in Table 3 deals with one-time versus multiple movements. Initially, those who move just once experienced a significantly higher improvement in their wages compared to stayers. When similar observations are compared in the common support area, these differences are wiped out, and those who move only once do not register better results any longer[22]. Multiple movers do not perform significantly worse than either stayers or one-time movers. Results in this particular case need to be qualified due to the strong imbalance between the number of multiple movers and stayers in the common support. This imbalance has two effects: the computed ATT is sensitive to specifications of the PS equation (although never significantly different from zero), and a smaller reduction in overall bias (see Table 2) is achieved after the matching compared to other types of mover-stayer comparisons.

To sum up, results indicate positive effects of voluntary and direct moves, non-scarring effects of movements via unemployment and involuntary movements, and no significant impact of multiple versus one-time movements within a single year. When the observation window is widened up to two years and longer interruptions are observed as well, the expected negative impact on wage dynamics arises. However, the latter two groups are very small and results must be interpreted with caution.

5. Conclusions

The present article provides a comprehensive picture of the effects of wage mobility on wages amongst young Spanish people. Given the high temporality rates in Spain during the 1990's, young Spaniards registered the highest job turnover rates in the European Union and were also some of the most affected by unemployment and involuntary job interruptions. This made their wages very vulnerable to movements. However, we find that the wage vulnerability of young Spaniards was not very strong in the short term. Our results should be qualified, since our sample is far from being universal. The results of the estimation process are focused on the "common support" workers, affecting the external validity of the sample.

We have observed a positive impact of direct and voluntary movements and no scarring effects of moves via unemployment and involuntary moves compared to remaining with the same employer. Moreover, multiple interruptions within a year do not seem to be scarring, either. When we widen the observation window to two years to allow for long-term interruptions, we do observe a negative effect of long-term interruptions on wage dynamics, though. Nevertheless, the results of the latter two types of job interruptions need to be taken with caution. Our estimates refer to the first wage observed after the movement has taken place, but

(22) Bear in mind that one-time movers include all types of movers as long as they register only a single transition between jobs throughout the year; they may therefore be either voluntary or involuntary, and either direct or via unemployment. This explains the non-significant estimate, since it is the result of combining both rewarding and non-rewarding short-term transitions.

after some time at the new job, wage differentials between movers and stayers may have vanishised. Youths may also overcome initially scarring transitions via rewards to tenure. It could also happen that returns on mobility decrease with time if movers repeatedly move between jobs during a longer period. The future research agenda therefore includes the widening of the observation window, looking either at wages two years (or later) after the new job is found, or at cumulative movements within a wider observation window.

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 direct and voluntary job mobility on wage growth. Therefore, the models that support the hypothesis of a mere unobserved heterogeneity problem would be rejected. Besides, job matching models and job search models hypothesising positive rewards for voluntary mobility are supported. However, with the technique used here and the information available, we are unable to disentangle the true mechanism behind wage increases when young workers move between jobs. Are productive workers poached by other firms? Do movers have more knowledge of their productivity and opportunities in the labour market? Do movers take advantage of the transferability of skills and human capital? The non-scarring effect of moves via unemployment and involuntary movements may be interpreted as follows: in a very dynamic youth labour market such as the Spanish one, mobility is so widespread that it affects all sorts of workers, not only the weakest/least productive. This may also explain the lack of a scarring effect of unemployment and involuntary moves when looking at short-term interruptions.

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RESUMEN

El inicio de la vida laboral es un período de intensa movilidad que puede influir en las trayectorias salariales. Pretendemos aquí medir el impacto de distintos tipos de movilidad laboral sobre el salario hora en España. Para ello estudiamos una muestra de jóvenes españoles extraída del Panel de Hogares de la Unión Europea. Combinamos *Propensity Score Matching* y Diferencias en Diferencias para explorar el impacto de la movilidad a largo y corto plazo, directa y a través del paro, voluntaria e involuntaria y "única" frente a "múltiple" sobre los salarios subsiguientes. Observamos un impacto positivo de la movilidad directa y la voluntaria, y no detectamos un efecto estigmatizador de la movilidad involuntaria, la "múltiple" y la que se produce a través de un episodio (breve) de desempleo. Sin embargo, las interrupciones de más de un año tienen un impacto negativo en los salarios posteriores.

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

Clasificación JEL: J31, J63.