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The Determinants of Urban (Un)employment Duration: Evidence from Barcelona

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The Determinants of Urban (Un)employment Duration: Evidence from Barcelona*

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Abstract

This paper analyses the likelihood of leaving and joining employment in an urban area. Estimates show that individual, firm, regulatory and macroeconomic factors affect urban (un)employment duration in different degrees. Also, national and urban (un)employment seem to share a common baseline hazard and similar macroeconomic and regulatory drivers. Individual characteristics are the only source of difference we can identify between national and urban (un)employment duration.

Keywords: Duration Models, Urban (Un)employment.
JEL Classification: J64, R23.

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1 Introduction

Urban (un)employment is an important policy concern. The main reason for this is the size of the urban labour markets: a substantial share of the population, and hence of the (un)employed, lives in cities. The population of the city of Barcelona, for example, represents roughly one third of the population of the Catalan region and five per cent of the Spanish population. These figures are slightly higher if the population of the unemployed is considered. In terms of employment, it suffices to say that the GDP of the province of Barcelona (most of which is produced in the metropolitan area of the city) is nearly 15% of the Spanish GDP.

This has motivated an extensive literature on urban labour markets (Crampton 1999a, Zenou 2009). However, very few studies have examined the duration of (un)employment in a European urban area. Previous research has focused on the probability of leaving unemployment (Alperovich 1993, Fu *et al.* 1993, Dendir 2006), paying particular attention to the effects of the spatial mismatch originated by poor job accessibility (Holzer *et al.* 1994, Rogers 1997, Thomas 1998, Détang-Dessendre and Gaigné 2009, Andersson *et al.* 2011) and/or residential segregation (McGregor 1977, Dawkins *et al.* 2005, Gobillon *et al.* 2011).[1] In contrast, little is known about the determinants of urban employment duration. What is the mean or median spell duration and whether there are age, gender, and educational differences, for example, are questions that have not been previously addressed. Moreover, the extant evidence is largely limited to US cities (Holzer *et al.* 1994, Rogers 1997, Dawkins *et al.* 2005, Andersson *et al.* 2011) and urban areas of developing countries (Fu *et al.* 1993, Dendir 2006). However, the European labour markets have such distinctive features as long (un)employment durations and generous employment benefits, which are likely to result in differences in the determinants of urban (un)employment duration.[2] Whether or not this is the case is still an open question, because none of the studies reporting results from hazard models in European cities (Thomas 1998, Détang-Dessendre and Gaigné 2009, Gobillon *et al.* 2011) account for these features.

This paper analyses the impact of individual, firm, regulatory and macroeconomic factors on the likelihoods of living and finding employment in the city of Barcelona between 1979 and 1994. We find that the main determinants of employment duration are an individual's education, macroeconomic conditions, employers' characteristics and the legal reform of 1992, whereas the main determinants of the unemployment duration include employees' and employers' characteristics, the unemployment rate and the legal reforms of 1980, 1984 and 1992. We also find differences in the baseline hazards of employment and unemployment. This means

[1] This is an issue that we cannot address here due to the lack of appropriate data (see Section 4). In any case, results reported by Matas *et al.* (2010) suggest that the effects of the spatial mismatch in the city of Barcelona during our period of analysis (1979 to 1994) are likely to be very weak.

[2] Note that, in this respect, Spain is often seen as an extreme case (Blanchard and Jimeno 1995).

that the function that approximates the pattern of (un)employment duration dependence – i.e. how the hazard rate varies with (un)employment survival times – is not the same.

Our results are in line with those typically found in national studies.[3] However, there are also discrepancies, notably in the determinants of employment duration and the effects of the legal reforms. Unfortunately, we cannot properly assess the statistical significance of these differences because none of these studies uses our data sources, model specification and period of analysis. In this respect, García-Perez (1997) is the study that most resembles ours. Thus, we provide an approximation of how different Barcelona's (un)employment duration is from the typical Spanish pattern by modifying our sample and model specification to replicate his estimates. In this comparative analysis individual characteristics stand as the main source of difference between national and urban (un)employment duration (see also Gobillon *et al.* 2011).

Therefore, our results point to the need for economic policies that take into account the specificities of urban labour markets. Note, however, that this applies not only to policies at the national or regional level, but also to similar policies across different cities. To illustrate, policies based on the finding that previous job tenure increases the chances of leaving unemployment in US cities (see e.g. Andersson *et al.* 2011) may not work in a European city, for our estimates show that this variable is not a statistically significant determinant of the duration of unemployment in Barcelona.

The rest of the paper is set out as follows. We review the literature in Section 2. We examine the institutional setting in Section 3, both in terms of the legal framework and Barcelona's labour market. In Section 4 we describe the sample and the (un)employment spells. In Section 5 we present the econometric model and analyse the empirical results. And we summarise the main conclusions of this study in Section 6.

2 Related literature

In this paper we seek to empirically analyse the determinants of urban (un)employment in Barcelona. Therefore, the literature on this issue can be defined by three basic questions. First, what does economic theory say about the determinants of urban (un)employment duration? Second, what is the empirical evidence on urban (un)employment duration? And third, what is the empirical evidence on Spanish (un)employment duration?

From this literature review, we can advance the following conclusions. First, job search theory provides the main analytical framework for investigating urban (un)employment duration.

[3]There are a variety of studies on the duration of Spanish unemployment —see e.g. Ahn and Ugidos-Olazabal (1995), Bover *et al.* (2002), Jenkins and García-Serrano (2004) and Alba-Ramírez *et al.* (2007). In contrast, studies on the duration of both employment and unemployment seem to be limited to García-Fontes and Hopenhayn (1996), García-Perez (1997) and García-Perez and Muñoz-Bullón (2005).

However, this framework must be complemented with elements of urban economic theory that account for local labour market conditions. Second, previous empirical studies restrict their analysis to unemployment, although they do not always use hazard models and mostly seek to test the spatial mismatch hypothesis. Evidence in this respect is mixed: whereas residential segregation (and minority discrimination) effects are usually significant, job accessibility measures have, if any, a limited impact. Third, covariates commonly used in the analysis of (un)employment duration in Spain include gender, age, education, local (regional) labour market conditions, legal changes, and the regional and/or national business cycle. Unemployment studies usually also include unemployment benefits, whereas size and sector are the employer characteristics typically considered in the fewer studies analysing Spanish employment durations.

2.1 Urban (un)employment duration: Theoretical foundations

Two strands of theoretical literature seem to be relevant to our study. First, the analysis of (un)employment duration has a long tradition in labour economics (Kiefer 1988). In particular, many empirical studies derive their econometric specifications from a model of search behaviour (Eckstein and van den Berg 2007). Second, a large body of research shows that labour market outcomes may differ depending on local labour market conditions (Crampton 1999a, Zenou 2000). Notice, however, that it is essentially the same principles guiding job search theory that allow these differences to be derived within and across urban environments (Zenou 2009).

Consequently, the empirical analysis of urban (un)employment durations is typically based on reduced-form models of job search —see e.g. Holzer *et al.* (1994), Rogers (1997) and Détang-Dessendre and Gaigné (2009); see also Eckstein and van den Berg (2007) for a critical assessment. In particular, one of the main concerns in urban search models is the labour market frictions caused by the spatial mismatch between employees and employers. Thus, a commonly tested hypothesis is that poor job accessibility and/or residential segregation results in poor labour market outcomes (Gobillon *et al.* 2007).

Time spent commuting makes workers potentially less productive (e.g. because they are more tired) and/or may involve a wage premium to compensate for the additional travel costs (Glaeser and Maré 2001). Thus, employers may prefer to hire workers that have easy access to their jobs. This would explain, for example, the higher unemployment rates of certain groups of individuals (e.g. low-skilled minorities) living far from their potential jobs and/or using slower means of transport. By the same token, workers that have difficulties accessing their jobs will be unemployed for longer.

However, these poor labour market outcomes may also be the result of firms redlining workers (Zenou 2002). That is, employers may be less willing to make a job offer to certain workers not (only) because of their distant location, but because they live in a bad neighborhood. As Gobillon *et al.* (2011: 1082) point out, "the motivation can hinge upon the stigma or prejudice associated with the residential location of candidates (*sheer discrimination*), or because they consider that, on average, workers from stigmatized areas have bad work habits or are more likely to be criminals (*statistical discrimination*)". Whatever the reason, these neighborhood effects reduce the likelihood of receiving a job offer and, hence of, leaving unemployment.

Perhaps surprisingly, analogous predictions cannot be found in the literature with respect to employment durations. In fact, this issue is barely mentioned in urban labour economics. In particular, we have not found any reference to the possible existence of spatial mismatch effects on the duration of employment.

2.2 Urban (un)employment duration: Evidence

The lack of urban studies on both employment and unemployment duration is worth noting. To be precise, some studies have analysed urban unemployment duration (although did not all use hazard models), but to our knowledge no previous study has investigated the determinants of urban employment duration. Also, unemployment duration studies dwell almost exclusively on the effects of the spatial mismatch.[4]

Holzer *et al.* (1994) seem to have been the first to test the implications of the spatial mismatch hypothesis on urban unemployment duration. They use a simultaneous equations framework with a log-linear specification for unemployment duration and data from the 1981-1982 National Longitudinal Survey of Youth Cohort. Their main results are that job accessibility (miles traveled while searching for a job) shows a negative coefficient and job decentralisation shows a positive coefficient for blacks (regardless of whether they live in the city center or in the suburbs). Thus, their estimates seem to support the spatial mismatch hypothesis. However, the effects are barely significant.

Thomas (1998) argues that this may be attributed to the use of a poor proxy for job accessibility (see also Andersson *et al.* 2011) and proposes using the individual's willingness to commute. He uses data from the 1987-1988 U.K. Survey of Incomes In and Out of Work to estimate a log-log discrete time model with a piecewise-constant baseline hazard conditional to the probability that the unobserved farthest distance an individual is willing to commute

[4]However, some studies examining urban unemployment durations do not address this issue. For example, Alperovich (1993) estimates the relation between city size and the length of unemployment spells (long term job seekers out of total population) in a cross-section of non-Arab Israelis cities, Fu *et al.* (1993) estimate the distribution of unemployment spells in Shanghai using a Gaussian kernel estimator, and Dendir (2006) estimates a lognormal accelarated failure time model using data from households in seven major urban centers in Ethiopia.

lies in a certain range of miles (estimated using a grouped dependent variable model). He finds that non-whites have a significantly lower propensity to commute. However, their hazard rate is smaller only if their willingness to commute lies in the four-to-nine mile range. He thus concludes that these results "do not constitute direct evidence on the validity of spatial mismatch for the U.K. experience".

Rogers (1997) also provides weak supportive evidence from a competing risks analysis for the municipalities of the Pittsburgh Metropolitan Area. Using data covering the period 1980:6 to 1986:3 on males aged between 18 and 55 from the Pennsylvania Comprehensive Wage and Benefits History data set, she finds that better job access (measured by the distribution of employment in other locations relative to commuting times at peak from that location) is positively related to unemployment duration when the exit from unemployment is to a new job. However, this relation is not statistically significant if the weighting of the job access measure is quadratic. Also, job access is generally not statistically significant for recalls.

Détang-Dessendre and Gaigné (2009) propose yet another measure of accessibility that weights the proximity to jobs for which the individual is qualified with the number of workers living within a radius of 30/45/60 minutes from the location (a proxy for competition in the labour market). This proxy and its square both affect unemployment duration in a piecewise constant hazard specification with unobserved heterogeneity estimated using 1998-2002 data from a stratified survey performed by the French National Institute of Economic Statistics (*Formation, Qualification Professionnelle*) on 16 to 60 year olds. However, the significance of these accessibility measures depends on the inclusion of local economic conditions among the covariates. Also, job accessibility increases the likelihood of leaving unemployment in small/medium municipalities; in large cities, on the other hand, the effect is not statistically significant.

Andersson *et al.* (2011) also explore individual-specific job accessibility measures that account for both how many skill-specific jobs are close by and how many other potential candidates are within a certain radius. Moreover, to mitigate the selection bias into more accessible residential locations, they restrict the analysis to job seekers aged 20 to 64 who involuntarily lost their low paid jobs between 2000 and 2005 in six Midwestern MSA's (extracted from employer-employee matched confidential microdata of the Longitudinal Employer-Household Dynamics dataset). Results from an ordered logistic model for the quarterly job search length provide evidence of spatial mismatch. However, the positive impact of a large pool of competitors is generally larger than the negative impact of a better access to jobs.

Dawkins *et al.* (2005) extend the analysis of the spatial mismatch hypothesis by also considering neighbourhood peer effects.[5] To this end, they estimate accelerated failure time models

[5]See also McGregor (1977) for an early analysis using log-linear models and UK data from the city of Pasley.

with Weibull parametrisation using a sample of household heads and their spouses who were unemployed at some point during the period 1990 to 1992 (Sensitive Data Files from the Panel Study of Income Dynamics). Results show that there are no significant differences between blacks and whites residing in MSAs once neighbourhood characteristics are controlled for. Also, better job accessibility (share of total MSA jobs located in each worker's zip code of residence) has a negative impact on unemployment duration, although the coefficient for black workers doubles that of white workers.

Gobillon *et al.* (2011) essentially address the same issues but using a novel three-step method. First, a proportional hazard model is estimated. This model has an unspecified municipality-specific baseline hazard, individual covariates and independent competing risks (in this case, finding a job and dropping out of the labour force). Second, municipality effects are recovered under the assumption that the municipality-specific baseline hazard is the product of municipality fixed effects and a general baseline hazard function. Third, the municipality effects are regressed on proxies for the alternative spatial mismatch mechanisms one considers. Data from job applicants to the National Agency for Employment (ANPE) who lived in the Île-de-France region during the first semester of 1996 show that residential segregation and, to a much lesser extent, job accessibility account for nearly three quarters of the spatial disparities across municipalities in unemployment durations. Also, individual and local effects reinforce each other: longer durations are due not only to adverse individual characteristics (e.g., being African) but to residential sorting in municipalities with adverse characteristics.

2.3 Spanish (un)employment duration: Modelling and evidence

The high rates of unemployment in Spain during the 1980s and 1990s prompted a number of studies on unemployment duration. In particular, two features of the Spanish labour markets that attracted considerable attention were the system of benefits and the length of the (un)employment spells (Blanchard and Jimeno 1995). In contrast, only a few studies considered the duration of both employment and unemployment. However, space limitations preclude us from examining all them. For the sake of comparability, we restrict the analysis to those studies using data and models that are analogous to ours.

Our main statistical source is an administrative dataset constructed from Social Security records that contains the complete work history of individuals (see Section 4 for details). Other studies using this source include García-Fontes and Hopenhayn (1996), García-Perez (1997), García-Perez and Muñoz-Bullón (2005) and Alba-Ramírez *et al.* (2007). In principle, a major limitation of this dataset is the lack of information on unemployment benefits. This is why some unemployment duration studies extract the data from the monthly benefit payrolls of the *Sistema Integral de Prestaciones* (SIPRE) —see e.g. Jenkins and García-Serrano (2004). However, this limitation may be overcome by matching the Social Security records with appropriate information from the SIPRE (García-Perez 1997, Alba-Ramírez *et al.* 2007).[6]

Regardless of the data source used, however, all these studies consider essentially the same individual determinants (gender, age and education). They also have in common the inclusion of controls for macroeconomic factors (unemployment rates and levels, GDP growth and levels, etc.) and the institutional setting (dummies for regions and legal reforms, regional unemployment rates and levels, etc.) among the covariates. In contrast, what they usually lack is information about employers. Recent versions of the Social Security records include an identifier of public firms (García-Perez and Muñoz-Bullón 2005) and/or firm size information (Alba-Ramírez *et al.* 2007). However, this was not available in the earlier records (García-Perez 1997). Fortunately, we managed to obtain firm size and sector of activity from the 1985 Input–Output Table of Catalonia and the Trade Union Census of 1991.

As for the modelling strategy, our basic specification is a discrete-time duration model with a logistic hazard function (Jenkins 1995). We also consider both parametric and non-parametric specifications of the baseline hazard and allow for unobserved heterogeneity (Heckman and Singer 1984, Meyer 1990, Cameron and Trivedi 2005). In essence, this is the model used by most previous studies on (un)employment duration in Spain. Having said that, some differences are worth noting.

First, the specification of the baseline hazard varies across the studies considered. While some studies use a set of time-period dummies (Ahn-Ugidos 1995, Bover *et al.* 2002, Jenkins and García-Serrano 2004; see also Alba-Ramírez *et al.* 2007 for a piecewise constant specification), others use a polynomial in the duration of the spell (García-Perez 1997, García-Perez and Muñoz-Bullón 2005), and one leaves it unspecified (García-Fontes and Hopenhayn 1996). Second, individuals' unobserved heterogeneity is not considered by Ahn-Ugidos (1995), García-Fontes and Hopenhayn (1996) and García-Perez (1997). In contrast, Bover *et al.* (2002), Jenkins and García-Serrano (2004), García-Perez and Muñoz-Bullón (2005) and Alba-Ramírez *et al.* (2007) control for unobserved heterogeneity. However, only Alba-Ramírez *et al.* (2007) and, to a certain extent, García-Perez and Muñoz-Bullón (2005) find substantial differences in the estimates when controlling for unobserved heterogeneity. Third, among the unemployment

[6] Yet another set of studies use (waves of) survey data, either from the Labour Force Survey (the EPA, e.g. Bover *et al.* 2002) or the Survey of Conditions of Life and Work (the ECTV, e.g. Ahn-Ugidos 1995). "However the ECVT, like the EPA, did not have information about benefit levels. Moreover, it was a cross-section survey and unemployment spell data were collected by retrospective recall questions" (Jenkins and García-Serrano *et al.* 2004: 240). On the other hand, these surveys included household information (ECTV) and covered all workers (EPA). Lastly, notice that none of these sources enable the effects of the mismatch hypothesis on the duration of (un)employment to be tested; see, in contrast, results in Matas *et al.* (2010) on the probability of females being employed using census data (individual characteristics being the main covariates considered).

studies, Ahn-Ugidos (1995) and Alba-Ramírez *et al.* (2007) explore competing risks models (see also García-Perez and Muñoz-Bullón 2005). The former considers exits to employment versus exits of the labour force, whereas the latter considers recalls by the same employer versus jobs in a different firm.

Despite these differences, there are some common findings. On the one hand, the duration of unemployment in Spain is procyclical, shows seasonal, regional and sectorial patterns and was positively affected by the 1984 reform. Also, gender, age and education stand as the main individual determinants. Conditional on the duration of the unemployment spell, more educated males have a higher probability of leaving unemployment while older people are more likely to stay unemployed. In addition, unemployment benefits and tenure at previous jobs both have a negative impact on the probability of leaving unemployment. On the other hand, the duration of employment in Spain is (weakly) countercyclical and was positively affected by the 1984 reform. Also, gender, age and education stand as the main individual determinants of Spanish employment duration. *Ceteris paribus*, older educated men are less likely to be fired.

3 Institutional Setting

3.1 Changes in the Spanish regulatory framework

Spanish labour market regulations have been considerably modified since democracy was established in 1978. Notably, the Workers Statute of 1980 and the reform of 1984 brought about major restructurings in the legal framework. There have been other legal changes, but they are either too specific to deserve discussion here (e.g., in the empirical analysis we consider the effects of the Law 22/1992, but this basically modified the system of unemployment benefits) or beyond the scope of our observation period (e.g., the profound reforms carried out in 1994 and 1997). It is also important to bear in mind that our goal in briefly describing these reforms is to gain insights for the empirical analysis. Thus, we are mostly interested in their impact on the conditions of entry (i.e., types of contract) and exit (i.e., firing costs) in the labour market.

3.1.1 The Workers Statute of 1980

"Under Franco, (...) only full–time and permanent jobs could be created, dismissal procedures were very cumbersome, collective firings had to be approved by the government and severance pay was very high" (Bentolila and Blanchard, 1990: 254). Stemming from this background, the Workers Statute of 1980 (*Estatuto de los Trabajadores*, ET hereafter) sought to apply two guiding principles to the Spanish labour market. First, collective bargaining was to be considered an alternative institutional mechanism to regulation. This meant that agreements between employers' associations and trade unions (*convenios colectivos*) were efficient *erga* *omnes*. Second, there had to be a direct correspondence between the duration of the job and the type of employment contract. Accordingly, the ET only acknowledged permanent (i.e. indefinite) and temporary (i.e. fixed–term) contracts. As for the firing conditions, the ET distinguished between individual dismissals subject to the contractual obligations and collective layoffs resulting from an employer–employee agreement with prior (or direct) administrative authorisation.

Unfortunately, the Spanish unemployment rate showed an upward trend from the late 1970s that the ET could not break. In fact, this legal framework was soon to be seen as too rigid in terms of the firing and hiring conditions. It was argued that it hampered the creation of employment and discouraged long–term contracts. In short, the ET did not succeed and, as a result, in 1984 the government launched new regulations intended to introduce more flexibility in the system (roughly three quarters of the contracts were permanent at that time). The rate of unemployment in those days was above 20% and this was explicitly mentioned in the preface of the new legal framework as the main argument for updating the ET barely four years after its enactment.

3.1.2 The 1984 reform

The 1984–reform increased the scope of temporary contracts. The so–called "measures to foster employment" (*medidas de fomento al empleo*) launched fourteen new fixed–term contracts, including temporary and part–time contracts as well as contracts associated with training programs. However, the reform also allowed for indefinite and limited duration contracts.

Indefinite contracts actually corresponded to the permanent contracts defined by the ET and rapidly lost importance. In contrast, limited–duration contracts soon became the most popular, to the extent that by the end of the 1980s nearly two thirds of the contracts were of this kind (García–Perea and Gomez 1993). In particular, limited–duration contracts could be used to "carry out a job or provide a service" (e.g., the construction of a building) and to adjust a firm's turnover to the seasonal evolution of economic activity (thus becoming temporary contracts to cope with peaks of production).

The range of non–indefinite contracts meant greater flexibility for entering the labour market. However, the successive use of temporary contracts in the same employer–employee relationship was limited to three years. Beyond this time the contracts became permanent. If firms did not wish to make the contract permanent, they could not hire another person for that job and had to wait a year before recalling the worker. The aim was, whenever possible, for temporary contracts to eventually be transformed into permanent ones.

In addition, the reform of 1984 involved a reduction of firing costs. In accordance with its preference for permanent contracts, the ET did not provide for any severance pay for temporary workers. The reform of 1984 modified this, although the severance pay of the indefinite–duration contracts remained comparatively high. As a result, employees were usually hired every 3 or 6 months and, in many cases, this practice extended beyond the 3-year limit through legal tricks, holding structures and other devices that distorted the *bona fide* sense of the law. All in all, practically one out of three contracts in Spain were non-permanent in the decade 1984-1994 (García–Perea and Gomez 1993).

3.2 Barcelona's labour market

Urban labour markets all over the world share a number of distinctive features (Zenou 2009). Commuting and the spatial mismatch, for example, cause certain socio–economic problems (e.g., unemployment and discrimination) that are less stringent outside metropolitan areas. Similarly, the relative importance of urban areas in the spatial distribution of the population means that the number of unemployed and the number of job offers are usually above the average for the region or the country. There is also evidence that urbanisation economies impinge on the rates and the duration of unemployment, that most cities define the boundaries of a local labour market area, and that the incidence of unemployment varies between inner and outer areas of the cities (Crampton 1999a, Gobillon *et al.* 2007).

However, there are also important differences between cities. In particular, the high rates of unemployment in European cities seem to be related to the tertiarisation of economic activity, which acts in practice as a mismatch mechanism (Crampton 1999b). Barcelona is a good example of this deindustrialisation process, since by the early 1990s about 70% of the jobs in the city were related to traditional services and, increasingly, new emerging activities (Rojo 1999). Delocation or decentralisation is another important trend in European cities (Symes 1995), especially those in southern Europe (Cheshire 1995). In this respect, Trullén *et al.* (1989) shows that the importance of industrial concerns in Barcelona continuously declined in the period 1970 to 1985. In contrast, the Metropolitan Area of Barcelona followed the opposite trend. However, possibly because of the tertiarisation process, employment in the city of Barcelona was still higher than in the metropolitan area (around two thirds).

The effects of these changes on Barcelona's labour market have not been fully investigated.[7] For example, do commuting, tertiarisation and delocation make Barcelona's labour market

[7] An important exception is the work of Matas *et al.* (2010), who find evidence of spatial mismatch in the metropolitan area of Barcelona (and Madrid). Notice, however, that their probit estimates were obtained using 2001 census data and a wide definition of the metropolitan area. Moreover, reported statistics show that poor job accessibility and residential segregation are particularly severe in recent years (e.g. the percentage of jobs in the central city was ten percentage points larger in 1981) and in the suburban areas of the metropolitan area (i.e. those far from the city centre). Therefore, the effects of the spatial mismatch in the city of Barcelona should be substantially lower in the 1980s and early 1990s.

different and, if so, in which way? In order to address this question, in Table 1 we report the evolution of the population, the unemployed and the economically active population of Spain, Catalonia and Barcelona, whereas in Table 2 we present the frequency distribution of the different types of contract signed in Spain and in the province of Barcelona.

[**Insert Table 1 around here**]

The figures in Table 1 show that the labour markets of Barcelona and Catalonia behave in a similar way and, in turn, their general trends are similar to those of Spain. However, there also some differences: for example, the rate of activity, which is notably higher in Barcelona and Catalonia than in Spain (because of the higher female rate of activity). Also, the unemployment rate during most of the 1980s was higher in Barcelona than in Catalonia and even higher than in Spain. On the other hand, since the late 1980s Barcelona, and to an even greater extent Catalonia, had an unemployment rate that was lower than in Spain. This seems to be mostly due to the decreasing trend of the unemployed population in both Barcelona and Catalonia.

[**Insert Table 2 around here**]

In addition, temporality appears to be high in Barcelona, particularly with the decline of economic activity in the early 1990s. In fact, the figures in Table 2 reveal that the use of temporary contracts is actually more frequent in Barcelona than in Spain. On the other hand, the percentage of limited–duration contracts in the whole country is higher than in Barcelona, specially those that were used for a specific job or service. There are no important differences, however, in the number of indefinite contracts.

From these descriptive statistics it is tempting to infer some sort of relation between the low unemployment rates in Barcelona and the extensive use of temporary contracts. However, such an inference would be flawed because it clearly lacks statistical rigour. Still, these statistics do indicate that Barcelona's labour market has particularities worth considering.

4 Descriptive analysis

4.1 The sample

Our data set was assembled in 1995, but the sample of individuals we analyse was extracted from the records for 1989. In particular, we obtained information on their work history (i.e., from their first job to their (un)employment status at the end of 1994) from the Social Security reports used by the INEM to compute insurance and assistance unemployment benefits. The data included basic features of the contracts signed by the individual (beginning and end dates, cause of severance, job category and an identifier of the employee) as well as personal characteristics (date of birth and gender). Moreover, for each unemployment period that followed a contract we know whether s/he enjoyed insurance and/or assistance benefits.[8] Lastly, the employee identifier provided a provincial postal code and, by cross-referencing information from the 1985 Input–Output Table of Catalonia and the Trade Union Census of 1991, the number of employees in 1984 and 1991 and the sector of activity (SIC three–digit code, CNAE–74).

More precisely, in 1989 the individuals analysed in this study contacted one of the fourteen offices of the National Employment Institute (INEM) in the city of Barcelona (in those days there were 52 offices in the province of Barcelona) with the aim of registering an employment contract. From this population, we selected 1041 subjects whose National Identity Card ended in 25. Thus, the sampling scheme is analogous to the inflow sample with right censoring discussed e.g. in Cameron and Trivedi (2005). In particular, note that all histories are right-censored at the end of 1994. However, not all the individuals were active at that time. In fact, during the period of analysis some individuals left the labour market. A few did so permanently (9 contracts had death and 1 had retirement as the cause of severance), but most were only temporarily out of the market because of the military service (this affected 59 spells of unemployment).

Deaths did not require any special treatment for our analyses, for these observations are simply not censored but fully observed. In contrast, we needed to address the cases involving retirement and military service. Thus, we dropped those (un)employment spells in which the age of the individual at exit was 55 years or more to avoid distortions associated with the end of the working life. Also, we subtracted from the duration of the subsequent unemployment spell the duration of the military service (24 months until 1984, 12 months between 1985 and 1991, and 9 months between 1992 and 1994).

In sum, the resulting dataset contains individual characteristics and the labour market history of a sample of employees and the basic features of some of their employers. The use of a single cohort may underestimate the weight of long term contracts and overestimate the initial steps of the professional careers (Jenkins and García-Serrano 2004). But the sampling procedure largely guarantees that the selected group of individuals provides a representative snapshot of Barcelona's labour market. Also, the period of analysis covers a large business cycle of the Spanish economy: the recession of the early 1980s, the recovery of the late 1980s, and the downswing of the early 1990s.

[8]We found a few cases of benefits related to temporary disability. Rather than distinguish them as a different category, we decided to include them as assistance benefits.

4.2 The (un)employment spells

Time spent unemployed, denoted hereafter by t, was originally measured in days. However, since contracts are typically stated in months, (un)employment durations were transformed to one-month-long periods. Moreover, we removed durations less than or equal than a month. These probably correspond to transitions among states rather than changes of state (García-Pérez 1997, Alba-Ramírez *et al.* 2007), although we also find cases of highly temporary work such as weekend jobs (e.g. in the leisure sector) and on-duty workers (e.g. doctors and nurses). Thus, a duration of two in our data set, for example, corresponds to a spell of more than two months but less than three. Lastly, we dropped all spells starting before 31^{st} December 1978, because including the pre-constitutional period would involve a completely different institutional setting (see Section 2).

It is important to notice that we consider the different spells of (un)employment a person may have as different observations in the data set. This means that we do not explicitly address the existence of multiple spells and treat the different spells of an individual as different single-spell individuals. Bearing in mind this assumption, we go on to provide descriptive analyses of the spells of (un)employment. First we report the mean and median duration for groups of individuals (Table 3.A) and firms (Table 3.B). Then we report the unconditional survivor functions, distinguishing between those estimated from the spells that occurred prior to the 1984 reform (which largely correspond to those under the 1980 reform) and those from the spells that occurred after the 1984 reform. Thus, these descriptive analyses give an idea of the sample of individuals and firms under study. They also provide insights into the duration of (un)employment in Barcelona and the effects of the major legal reforms during our observational period.

[**Insert Table 3.A around here**]

Table 3.A shows that the typical individual in our sample is a young male of little education who has been (un)employed for less than a year. Also, there are no substantial differences between employed and unemployed individuals (as expected from the sampling scheme we used). Similarly, Table 3.B shows that the typical firm in our sample is a large concern located in the province of Barcelona that operates in the services sector. We also find similarities between the sub-samples of hiring firms, i.e. those contracting during the employment spell and after the unemployment spell. However, rather than being a result derived from the sampling scheme, this is an expected result in an urban labour market where the population of (mature) firms is almost constant.

[**Insert Table 3.B around here**]

Table 3.A also shows that the spells are, on average, longer for females than males. That is, women in our sample on average have longer contracts than men, but also spend more time unemployed. Moreover, people in their thirties and early forties on average enjoy longer contracts than young people, who in turn have longer contracts than people aged between 45 and 55. This is not the case for the unemployment spells, however, in which a negative relation between duration and age seems to emerge. Lastly, more educated people tend to have longer/shorter periods of employment/unemployment.

Table 3.B also shows that the duration of employment spells differs across hiring firms. Large firms in construction and agriculture, for example, on average use shorter contracts than small and medium-size firms in the industry and the services. It is also interesting to note the longer contracts of the firms located in the province of Barcelona with respect to those located outside (on average). In contrast, the duration of the unemployment spells associated with the post-unemployment hiring firms are more homogeneous. Durations barely differ across size and location groups, and only services (longer spells) and agriculture (shorter spells) stand out as different in the discrimination by sectors.

[**Insert Graph1 around here**]

We report the unconditional survivor functions in Graph 1. These estimates of the probability of having completed spell durations of different lengths have the expected profile: they decrease with the time spent in (un)employment. However, the decline is more pronounced in the unemployment spells, particularly for those that were shorter and occurred prior to the 1984 reform. This means that for spells of the same duration and conditional on being (un)employed up to that month the probability of continuing unemployed was greater than that of continuing employed. It should also be pointed out that the changes in the trend around 36 and 48 months probably correspond to indefinite contracts (employment) and people who decide to leave the labour market (unemployment).

The effects of the 1984 reform are apparent in the unemployment chart (García-Fontes and Hopenhayn 1996, Bover *et al.* 2002), but the employment spells do not provide such a clear picture. We thus test whether the survivor functions before and after the 1984 reform were statistically the same using lifetable estimates. The log-rank test rejects the null hypothesis of equality of the survivor functions in both cases: the χ^2 test for the employment spells was 45.86 and the value for the unemployment spells was 1017.38, both statistically significant at the 5% level.

Therefore, the reform seems to have succeeded in its goal of tackling unemployment by reducing the likelihood of remaining unemployed (regardless of the length of time the individual had been unemployed).[9] However, the shape of the employment survivor function looks the

[9] It may argued that this result is driven by the number of long-term unemployed. However, dropping the

same before and after the 1984 reform. So what is the origin of the statistical difference found by the long rank test? As pointed out above, the 1984 reform critically affected employment decisions by expanding the menu of temporary contracts with low firing costs. This is reflected in the width of the steps of the employment survivor function after the reform of 1984 (see also García-Perez and Muñoz-Bullón 2005), which are indicative of the use of repeated short contracts (typically of 3 or 6 months) and probably cause the test to reject the null hypothesis.

5 Empirical results

We require a duration model that accounts for the inflow sampling scheme, the right censoring in the duration variable and the discrete nature of the spells. We use the discrete-time model with a logistic hazard function proposed by Jenkins (1995). Thus, if we denote by T_{is} the number of months individual i has been (un)employed in spell s, the conditional hazard h_{ist} given covariates can be written as

$$h_{ist} = h\left(t, W_{is}(t)\right) = Pr\{T_{is} = t | T_{is} \geq t, W_{is}(t)\} = F\left(\alpha_t + W_{is}(t)\beta_t\right),$$

where α_t is the baseline hazard, $F()$ is the logistic c.d.f. and $W_{is}(t) = [X_{is}, Z_{is}(t)]$ includes fixed, X_{is}, and time-varying, $Z_{is}(t)$, covariates.

We use a set of explanatory variables that is largely consistent with those used in previous Spanish studies.[10] This means that basic individuals' characteristics include gender, age and education, but we also control for the duration of the previous (un)employment spell and the receipt of unemployment benefits (indicator and duration of insurance benefits, and indicator of assistance benefits). As for the employers' characteristics, when available they include location, the number of employees and the sector of activity. In addition, changes in the regulatory framework take the form of dummies for the periods of validity of the major reforms (the ET and the 1984 reform) and for the change in the unemployment benefits program since 1992. Lastly, we use regional gross added value rates and provincial unemployment level and rates to control for macroeconomic factors.

We report the estimates of this model in Tables 4 and 5. In particular, the results were obtained controlling for the main causes of duration dependence: "true" state dependence and unobserved heterogeneity (Kiefer 1988, Cameron and Trivedi 2005). Prior to the analysis of these results, however, next we discuss in detail the model selection procedures.

[Insert Table 4 around here]

spells longer than three years did not greatly affect the shape of the figures reported in Graph 1 and, more importantly, the log-rank test still rejected the null hypothesis.

[10]See Section 2. See also the Appendix for details on the definitions and data sources we employ.

[Insert Table 5 around here]

5.1 Model selection

We start our analysis with a specification that does not address duration dependence. In particular, $W_{is}(t)$ initially contains individual and macroeconomic factors. Firm and regulatory factors are subsequently included. We report estimates of these specifications in the first three columns of Tables 4 and 5, respectively. Since most studies essentially consider individual, labour and/or macroeconomic determinants of (un)employment, in this way we can assess the bias caused by the omission of firm and regulatory variables. Notice that we use the same individual, regulatory and macroeconomic factors to explain both the employment and the unemployment duration. However, employers' characteristics and individual labour factors differ because they refer to the hiring firm (i.e. the firm in which the individual worked during the employment spell and the firm that hired him/her after the unemployment spell) and the previous spell (i.e. the previous unemployment/employment spell duration and the previous/current unemployment benefits are determinants of the current employment/unemployment spell).[11]

Next we introduced state dependence into the model, either parametrically (by specifying α_t and β_t as polynomials in $ln(t)$) or non-parametrically (by including a set of time-period dummies to specify α_t and multiplying them by certain covariates to specify β_t).

The degree of the polynomials in the parametric specification was determined in the following way. We started with a degree one polynomial for the hazard baseline using the specification that includes all the determinants available. We then included additional terms of the polynomial in $ln(t)$ as long as they were statistically significant (at the 5 per cent level we use throughout) and reduced the value of the Akaike Information Criterion. We proceed in this way up to the median value of the corresponding spell. In our best specification, α_t contained six terms in the employment model and five terms in the unemployment model, which, as reported in Table 3, are indeed the sample median values.

We then used this specification of the baseline hazard to determine the polynomial in β_t. We started with a degree one in the individual, regulatory and macroeconomic factors (excluding quarterly dummies) and subsequently added a degree two for those variables whose coefficients in $ln(t)$ were statistically significant but dropped those terms in $ln(t)$ that were not statistically significant. We found that these specifications, whose estimates are reported in the fourth column of Tables 4 and 5, produced the lowest AIC values of all the alternative parametric

[11]In the employment model we set to zero both the unemployment spell duration and the unemployment benefits prior to the first contract we observe. Since we have the work history of the individuals, this seems a plausible imputation. In the unemployment model we drop the last unemployment spell because we do not have information about the post-spell employers.

functions we explored.[12]

We proceeded in an analogous way to non-parametrically specify the state dependence. Thus, we initially estimated a model with a set of month-dummies and all the available individual, firm, regulatory and macroeconomic factors. In particular, we considered three specifications with $T = 12, 24$ and 36 month-dummies, which approximately identify 55 (65), 75 (83) and 86 (91) per cent of the (un)employment spells. We found that the specification with $T = 36$ and $T = 12$ month-dummies produced the lowest AIC values in the employment and unemployment model, respectively.[13]

Next we estimated a model with all the available determinants, $T = 12, 24$ and 36 month-dummies, and cross-products of the individual, regulatory and macroeconomic factors (excluding quarterly dummies) with the month-dummies. We then dropped those groups of $12, 24$ and 36 cross-products that were not jointly significant and reestimated the model. We found that in the employment model none of these specifications yielded lower AIC values than the one using only thirty-six month-dummies (i.e. without cross products and dummies starting at $T = 2$), but in the unemployment model the specification using twelve dummies and the significant cross-products yielded lower AIC values than any of the other non-parametric specifications. These specifications that yielded the lowest AIC values are reported in the fifth column of Tables 4 and 5.[14]

In sum, the best-fit specifications of the model with state dependence (according to the AIC) are the following. In the employment model, covariates include thirty-six month-dummies and all the available individual, firm, regulatory and macroeconomic factors as covariates; in the unemployment model, covariates include all the available determinants, a degree five polynomial in $ln(t)$ and cross products of $ln(t)$ and $ln(t)^2$ with the dummies of gender, lower-middle age, high education, insurance benefits and the 1992 reform as well as the days of insurance benefits received and the growth rate of Gross Added Value.

We use these specifications to address the individual unobserved heterogeneity. That is, we

[12] We also estimated the model with the number of terms found significant in $ln(t)$. We found that all the terms were statistically significant and the AIC decreased with each additional term. We also explored a degree three polynomial in β_t for the variables whose terms in the degree one and/or two polynomials were statistically significant. However, we did not obtain substantially better results.

[13] There were no exits from employment at $T = 1$, so in the employment model we either constructed the dummies from $T = 2$ or did not include the dummy for $T = 1$. We found that the first option produced better results in terms of AIC values. We also explored a constant duration for the first two or three periods of employment and a piece-wise specification based on the months-intervals reported in Table 3. However, these approaches resulted in much worse AIC values.

[14] We also explored using cross-products of the month-dummies and the variables found relevant to specify β_t, as well as a piece-wise constant hazard based on the months-intervals reported in Table 3. None of these alternatives yielded a better fit.

use the best-fit specifications of the model with state dependence to construct a model with different intercepts (u_i) for the hazard function:

$$h_{ist} = F\left(\alpha_t + W_{is}(t)\beta_t + u_i\right).$$

The unobserved heterogeneity can be controlled using a parametric or a non-parametric specification, the difference being the use of a continuous or a discrete distribution to characterise the random intercepts. In the parametric case, it is typically assumed that u_i is a Gamma- or Normally-distributed (independently of the covariates) random variable (Meyer 1990, Jenkins 1995). In the non-parametric case, it is assumed that the individual heterogeneity follows a discrete distribution (i.e, there are for example two different types of individuals in the sample), so that the likelihood function is a weighted sum of the contributions of each type of individual (Heckman and Singer 1984).

We report estimates of the employment and unemployment specifications with normally distributed frailty in column six of Tables 4 and 5, respectively. We faced convergence problems in all the other cases, i.e., when assuming the alternative Gamma-distributed or non-parametric frailty.[15] However, it is worth noting that all the specifications yielded statistically significant likelihood ratios regarding the frailty variable when convergence was achieved. Also, the signs and statistical significance of the coefficients were essentially the same across the alternative specifications of the unobserved heterogeneity.

5.2 Estimates

We initially focus on the first three columns of Tables 4 and 5. These correspond to specifications that do not allow for duration dependence and use as covariates individual characteristics and macroeconomic conditions (first column), then add employers' characteristics (second column) and finally add regulatory factors (third column). In this vein we seek to empirically asses the extent to which omitting firm and regulatory factors may bias the coefficient estimates of the individual and macroeconomic factors.

We find that including firm and regulatory factors has an impact on such employee' characteristics as gender, education and insurance benefits. In fact, these variables often undergo a sizable change not only in the magnitude of the coefficient estimates but also in their statistical significance. Moreover, these effects are apparent in both the employment and the unemployment model. In contrast, macroeconomic covariates remain practically unaltered in the employment model and, perhaps with the exception of the unemployment rate, also in the unemployment model. It seems, therefore, that although an omitted variables bias may exist,

[15] Coefficients and standard errors were also not stable when using a "complementary log-log" model (see e.g. Cameron and Trivedi 2005) with either Gamma-distributed or non-parametric frailty.

this essentially affects individual characteristics not macroeconomic factors. Also, much of this bias arises from the omission of firm characteristics, as reflected in the values of the AIC.

Next we consider the specifications that allow for duration dependence, either parametrically (column 4 in Tables 4 and 5) or non-parametrically (column 5 in Tables 4 and 5). Whether one approach or the other is used does not make a great deal of difference, since there is a high correspondence in terms of coefficient estimates (once we take into account the cross-products of variables) and statistical significance. In fact, this correspondence largely extends to the specification that includes all the available determinants but does not allow for duration dependence (column 3 in Tables 4 and 5). However, there are substantial differences in some estimates. Notably, we would misleadingly conclude that the legal reform of 1980, and possibly that of 1984, had a positive impact on the probability of ending a contract. We would also miss the differential role of small firms and some sectorial effects in the likelihood of leaving unemployment. Ultimately, these differences result in a poorer fit of the specification without duration dependence.

Lastly, we consider the specifications that control for unobserved heterogeneity and state dependence. We again make a comparative analysis, in this case between the results we obtained with and without different intercepts for the hazard function (columns 6 versus 4 and 5, respectively, in Tables 4 and 5). Differences are as expected, for the specifications without unobserved heterogeneity tend to over-/under-estimate the degree of negative/positive state dependence. Also, the coefficients in the non-frailty model tend to be smaller in absolute values than in the frailty model (see e.g. Cameron and Trivedi 2005: 617-618). However, in line with previous studies (Bover et al. 2002, Jenkins and García-Serrano 2004), the value and significance of the parameter estimates obtained controlling for unobserved heterogeneity are not substantially different from those obtained without controlling for unobserved heterogeneity.

The fit in Tables 4 and 5 is best when unobserved heterogeneity and state dependence are taken into account. Thus, we use the signs of the statistically significant coefficients in this specification to derive our main findings. First, more educated people tend to be hired for longer periods and may leave unemployment earlier. Second, the duration of unemployment benefits and the receipt of assistance benefits harm your chances of leaving unemployment. Third, contracts are longer in the upswings of the business cycle and shorter in the downswings. Fourth, higher/lower unemployment rates result in shorter/longer contracts and unemployment periods. Fifth, there is seasonality in the duration of employment, for contracts are generally shorter in the third and second quarters than in the first and second. Sixth, contracts are likely to be shorter if individuals work for a large firm or in a firm located outside the province of Barcelona (compared to the contract you would have if you had been hired by smaller, Barcelona-located firms). Lastly, all the major reforms of the 1980s and early 1990s had an impact on the labour market. However, whereas the 1992 reform reduced the duration of employment, the reforms of 1980 and 1984 increased the likelihood of leaving unemployment (in fact, the 1992 reform also did this, but mostly for long-term unemployed individuals).

However, the lack of significance of certain variables is also worth noting. First, gender, age and labour factors (previous unemployment spell and unemployment benefits) do not seem to affect the probability of leaving current employment. This means that there are no statistical differences in the likelihood of being fired between men and women, and between people of different ages. Also, the duration of the previous unemployment spell and the receipt and duration of unemployment benefits do not seem to make any difference when it comes to a current contract being terminated. Second, gender and employer's characteristics do not seem to affect the probability of leaving unemployment. This means that whether you are a man or woman and the characteristics of the firm that is hiring you make no difference when it comes to exiting unemployment.

5.3 Comparative analyses with the Spanish pattern

Our dataset may be regarded as the result of a sampling scheme that was statistically representative for certain urban areas. That is, a sample analogous to ours could be obtained from a random sample of Spanish workers that is geographically stratified to be representative of the city of Barcelona (see e.g. Détang-Dessendre and Gaigné 2009 for French data). However, to our knowledge such a sample cannot be obtained from the extant statistical sources, which tend to provide random samples from the whole country that may or may not be representative of specific urban areas. Consequently, we take the study by García–Perez (1997) as a benchmark for comparing our results with those obtained using (representative) samples of (un)employment durations in Spain. The similarities with our study include the period of analysis, the data sources and the use of discrete-time hazard models. The differences essentially stem from the construction of the sample and the vector of explanatory variables.

In order to assess the extent to which the differences between our estimates and those reported by García–Perez (1997) may be due to sampling differences it is interesting to compare the descriptive statistics reported in Section 4 with those reported in Tables 1 and 2 by García–Perez (1997). First, the employment spells are slightly longer in our sample (the median in Spain is 5), whereas unemployment spells are much shorter (the median in Spain is 11). Second, there are more males in our sample (6 percentage points more in the employment spells and almost 10 more in the unemployment spells). Third, we have fewer high- and low-educated people (differences around $4-5$ percentage points in each category), but a similar number of upper-middle educated people. Fourth, there are very few differences in the distribution of people by age, most notably fewer youths in the sample of unemployment spells.

All in all, it seems that our initial sample is not greatly different from that analysed by García–Perez (1997). However, we had to make some further changes to facilitate comparisons between model coefficient estimates. First, we did not impose our correction for military service on the duration of unemployment. Second, we did not consider the first unemployment duration of young people between 16 and 29 years (a spell that García–Perez (1997) did not observe in his sample). Third, we censored durations of unemployment larger than three and a half years to be consistent with his assumption that they are actually drop offs.

In addition, we modified the set of explanatory variables to closely follow his specification: *i*) employer characteristics and assistance benefits were not included; *ii*) we used the Spanish GDP (Source: INE, 2000 constant prices) rather than the Gross Added Value of Catalonia; *iii*) we used Catalonia's unemployment level and rate (Source: EPA) rather than Barcelona's unemployment level and rate; *iii*) we included cross-products of education and age dummies with the 1984-reform dummy; *iv*) we included a dummy to distinguish previous employment periods shorter than three years. Lastly, we used his specification of state dependence and did not control for unobserved heterogeneity.

We report estimates of this specification of the employment and unemployment model using the modified sample in the last columns of Tables 4 and 5, respectively. We find that some of the determinants of (un)employment duration in Barcelona are indeed different from those of Spain. In particular, the effects of personal characteristics such as gender, age, the duration of the previous unemployment spell and the receipt of insurance benefits are hardly relevant in the employment model. In the unemployment model, however, this is less clear. Interestingly, the results obtained using all the determinants available and controlling for state dependence and unobserved heterogeneity suggest that these differences cannot be attributed to the omission of relevant explanatory factors. Rather, they arise as a genuine effect of the local labour market (see also Gobillon *et al.* 2011).[16]

On the other hand, we find substantial similarities in the macroeconomic and regulatory factors. Thus, the business cycle and the 1984 reform seem to have had a similar effect on the urban and national labour markets considered. It is also interesting to note that both studies use analogous polynomial approximations to the shape of the baseline hazard, which suggests that urban and national (un)employment share a common pattern of state dependence. This means that, conditional on the covariates, the probability of leaving (un)employment at any point during the spell is essentially the same in both geographical aggregations. What differs is how this conditional probability changes when the value of some determinants of the

[16] We speculate that the higher rate of activity and use of temporary contracts (see Section 3.2) may lie behind these differences. However, the flexibility and demand of skilled workers that characterise urban labour markets are other factors worth considering (Zenou 2000, Glaeser and Maré 2001).

(un)employment change.

6 Conclusion

Urban labour markets have distinctive features (commuting, spatial mismatch, etc.) that have been extensively investigated. However, evidence on the determinants of urban (un)employment duration is scarce. This paper aims to fill this gap in the literature by analysing a European case study: Barcelona. We use data from a random sample of labour force participants and model the probability of leaving (un)employment as a discrete-time process to show that employers' and individuals' characteristics, changes in the legal setting and macroeconomic indicators, all affect the probabilities of leaving and joining unemployment. In particular, we find that the main determinants of employment duration are an individual's education, macroeconomic conditions, firm characteristics and the 1992 reform. As for the determinants of unemployment duration, they include both employee and employer characteristics, the unemployment rate and the legal reforms.

Comparative analyses indicate that although national and urban (un)employment may share a common baseline hazard, the duration of (un)employment in an urban area like Barcelona differs from the national pattern. Also, such differences are more important in the duration of employment and seem to originate from personal characteristics, because macroeconomic and regulatory drivers are very similar. This points at the risk of rubber-stamping policies derived from studies that employ representative samples at the national level (unless of course they explicitly take into account the spatial heterogeneity of the labour market). It also shows that the study of urban (un)employment durations may provide useful insights into the design of economic policies. For example, our estimates indicate that changes in the legal framework may be particularly important in increasing the likelihood of leaving unemployment in urban areas.

However, further research is needed if specific policy implications are to be derived. Issues that for the sake of simplicity have not been addressed here include alternative exits from (un)employment and the existence of multiple spells. It would also be interesting to compare results from other urban areas in Spain (e.g. Madrid) and in Europe (London, Paris, etc.). These extensions of the present work may help to better understand the determinants of urban (un)employment durations in Europe.

7 Appendix: Definition of variables and data sources

- Employees' characteristics
  - Gender: A dummy variable that takes value 1 for males and 0 for females.
  - Age: On the basis of the difference between the year in which the (un)employment episode starts and the date of birth, we define three categories: Young (aged between 16 and 29, the residual category), Lower-Middle Age (aged between 30 and 44) and Upper-Middledle Age (aged between 45 and 55).
  - Education: We use job category levels as a proxy (García-Pérez 1997, Alba-Ramírez *et al.* 2007). In particular, we define education in terms of four dummy variables: High Education (which takes value 1 for engineers and graduates, technical engineers and other skilled workers, and chief and department heads), Upper-Middle Education (other semi-skilled workers, skilled workers and auxiliary workers), Lower-Middle-Education (semi-skilled and skilled labourers) and Low Education (semi-skilled labourers, unskilled labourers and 16 to 18 years old workers), which is the residual category.

  Labour factors
  - Previous (un)employment spell. Duration in months of the previous (un)employment spell.
  - Insurance Benefits. A dummy variable that takes value 1 if the individual enjoyed insurance benefits during the unemployment spell and for how long (number of days).
  - Assistance Benefits. A dummy variable that takes value 1 if the individual enjoyed assistance benefits during the unemployment spell.

  (Source: Social Security and INEM)

- Macroeconomic Indicators
  - Gross Added Value. Growth rate of the yearly Gross Added Value of Catalonia at 2000 constant prices (in thousands of euros) in the year in which the (un)employment episode starts and with respect to the previous year.

  (Source: BMORES DATABASE, Ministerio de Economía y Hacienda).

  - Unemployment. Quarterly unemployment level of the province of Barcelona in the year-quarter in which the (un)employment episode starts and growth rate of this unemployment level with respect to the same quarter of the previous year.

  (Source: Own calculations from EPA, Institut Nacional de Estadística)

  - Seasonality. Quarterly dummy variables that take value 1 if the (un)employment episode started in the second, third and fourth quarter of the year, the first quarter being the residual category.

(Source: Own calculations)

- Employer characteristics

  - Size: Dummies for firms of different sizes, measured by the (rounded up) average number of employees in 1985 and 1991. In particular, Small Size firms are those with fewer than 10 employees, Lower-Medium-Size firms are those with 10 to 19 employees, Upper-Medium-Size firms are those with 20 to 49 employees, and Large firms are those with more than 50 employees (the residual category).
  - Sector. We grouped the sampling mode of the three–digit SIC codes (CNAE–1974) for 1985 and 1991 into four sectors: Agriculture (SIC codes below 100), Industry (SIC codes between 100 and 500), Construction (SIC codes between 500 and 600, the residual category) and Services (SIC codes above 600).

(Source: 1985 Catalonia Input–Output Table and 1991 employers census of *Comisiones Obreras*)

  - Location. A dummy variable that takes value 1 for those concerns located in the province of Barcelona and 0 otherwise.

(Source: Social Security and INEM)

- Regulatory factors

  - Reform of 1980. A dummy variable that takes value 1 if the (un)employment episode started after October 8 1980 but before August 2 1984.
  - Reform of 1984. A dummy variable that takes value 1 if the (un)employment episode started after August 2 1984.
  - Reform of 1992. A dummy variable that takes value 1 if the (un)employment episode started after April 8 1992. We also included cross-products of this dummy with the dummies of unemployment benefits.

(Source: Boletín Oficial del Estado)

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Graph 1: **Estimated Survivor Functions.**

Table 1: Population over 16, Unemployment Population and Economically Active Population: Province of Barcelona, Catalonia and Spain (1980-1994)

	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994
Population															
Barcelona	3.34	3.38	3.41	3.44	3.48	3.51	3.54	3.59	3.62	3.66	3.69	3.72	3.76	3.79	3.82
Catalonia	4.31	4.35	4.40	4.44	4.49	4.52	4.56	4.62	4.66	4.71	4.78	4.83	4.88	4.92	4.97
Spain	26.80	27.16	27.52	27.88	27.24	28.63	28.95	29.36	29.84	30.21	30.45	30.73	31.03	31.31	31.59
Unemployed															
Barcelona	0.25	0.32	0.40	0.42	0.44	0.45	0.44	0.47	0.40	0.31	0.27	0.27	0.27	0.45	0.47
Catalonia	0.28	0.36	0.45	0.47	0.49	0.51	0.49	0.52	0.43	0.34	0.31	0.32	0.33	0.53	0.56
Spain	1.54	1.91	2.18	2.38	2.78	2.99	2.92	2.96	2.90	2.54	2.45	2.56	2.88	3.65	3.84
Active															
Barcelona	1.76	1.77	1.78	1.78	1.79	1.77	1.83	1.98	1.93	1.96	1.96	2.02	1.99	2.07	2.08
Catalonia	2.27	2.29	2.32	2.31	2.32	2.30	2.37	2.54	2.50	2.53	2.56	2.63	2.61	2.67	2.71
Spain	13.42	13.50	13.68	13.88	13.92	14.01	14.20	14.88	15.22	15.33	15.49	15.68	15.75	15.97	16.11

Source: Survey of the Active Population (EPA), National Institute for Employment

Note: Data (in millions) refer to the third quarter of the corresponding year

Table 2: Types of labour contracts
(% of total, Spain and province of Barcelona)

	1989		1990	1991	1992		1993	1994
	Spain	Barcelona	Barcelona	Barcelona	Spain	Barcelona	Barcelona	Barcelona
To foster employment	42.7	54.7	53.6	49.1	35.3	45.0	38.6	37.0
Temporary	20.8	28.2	28.2	25.2	16.6	21.4	16.8	7.0
Part-time	6.7	8.6	9.7	10.6	10.5	12.0	16.2	18
Training	6.3	10.9	9.9	8.7	2.7	4.6	2.1	4.0
Practice	4.2	5.7	4.8	4.1	2.1	2.5	1.5	1.0
Others	4.8	1.3	1.0	0.5	3.42	4.5	2.0	7.0
Ordinary contracts	57.3	45.3	46.4	44.7	64.7	55.0	61.4	63.0
Indefinite	4.52	5.8	6.3	4.3	4.82	5.9	5.2	6.0
Limited duration	51.6	39.5	40.1	49.9	57.4	49.1	56.2	57.0
Others	1.2	-	-	5.2	2.5	4.1	1.5	-

Source: García–Perea and Gomez (1993) for Spanish data and INEM Barcelona (*Memoria 1993* and *Memoria 1994*) for data for the province of Barcelona

Table 3.A: Descriptive Statistics (by Groups of Individuals)

Variables	Observations	Percentage (by Variable)	Mean Duration	Median Duration
Employment				
Less than 3 months	897	26.11	2.49	2
3 to 6 months	862	25.09	4.99	5
6 to 12 months	761	22.15	8.42	7
12 to 24 months	503	14.64	16.79	16
24 to 36 months	211	6.14	28.55	28
More than 36 months	201	5.85	49.00	43
Gender				
Female	1,129	33.87	11.43	6
Male	2,306	67.13	10.56	6
Age				
Young	2,214	64.45	10.78	6
Lower-Middledle-Age	919	26.75	11.41	7
Upper-Middledle-Age	302	8.79	9.61	6
Education				
Low-Education	1,421	41.37	9.30	6
Lower-Middledle-Education	1,166	33.94	10.31	6
Upper-Middledle-Education	444	12.93	12.85	7
High-Education	241	7.02	14.66	8
Uncensored Spells	3,435	100	10.85	6
Unemployment				
Less than 3 months	938	36.20	2.39	2
3 to 6 months	651	25.13	4.83	5
6 to 12 months	589	22.73	8.91	9
12 to 24 months	217	8.38	17.64	17
24 to 36 months	63	2.43	30.01	30
More than 36 months	77	2.97	58.71	54
Gender				
Female	779	30.07	9.20	5
Male	1,812	69.93	7.59	5
Age				
Young	1,658	63.99	8.35	5
Lower-Middledle-Age	697	26.90	7.96	5
Upper-Middledle-Age	235	9.07	6.53	4
Education				
Low-Education	1,133	43.73	8.39	5
Lower-Middledle-Education	863	33.31	6.94	4
Upper-Middledle-Education	306	11.81	8.64	5
High-Education	145	5.60	7.31	4
Uncensored Spells	2,591	100	8.07	5

Table 3.B: Descriptive Statistics (by Groups of Firms)

Variables	Observations (Uncensored Employment Spells)	Percentage (by Variable)	Mean Duration	Median Duration
Size	2,349			
Small	538	22.90	11.50	7
Lower-Middledle-Size	310	13.20	10.68	6.5
Upper-Middledle-Size	488	20.77	11.11	7
Large	1,013	43.12	10.07	6
Sector	2,603			
Agriculture	4	0.15	8	4.5
Industry	562	21.59	11.79	7
Construction	483	18.56	7.82	6
Services	1,554	59.70	11.00	6
Location	3,435			
Province of Barcelona	3,135	91.27	11.01	6
Others	300	8.73	9.85	6

Variables	Observations (Uncensored Unemployment Spells)	Percentage (by Variable)	Mean Duration	Median Duration
Size	1,710			
Small	400	23.39	7.79	5
Lower-Middledle-Size	232	13.57	7.61	5
Upper-Middledle-Size	347	20.29	7.87	4
Large	731	42.75	7.62	4
Sector	1,924			
Agriculture	4	0.21	4.75	2.5
Industry	443	23.02	7.38	4
Construction	365	18.97	7.14	5
Services	1,112	57.80	8.10	5
Location	2,590			
Province of Barcelona	2,371	91.54	8.05	5
Others	219	8.46	8.32	6

Note: There are 481 censored employment spells (12.28% of the 3,916 employment spells experienced by 1,014 individuals in 2,729 firms) and 347 censored unemployment spells (11.81% of the 2,938 unemployment spells experienced by 920 individuals exiting from unemployment to 2,188 firms).

Table 4: Determinants of Employment Duration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender	0.1502***	0.0653	0.0578	-0.5213***	0.0126	0.0117	-0.1425
	(0.0397)	(0.0486)	(0.0488)	(0.2268)	(0.0496)	(0.0640)	(0.0939)
Lower-Middle Age	-0.1098***	-0.1230***	-0.1144***	-0.0721	-0.0698	-0.0954	0.1832
	(0.0416)	(0.0524)	(0.0528)	(0.0537)	(0.0538)	(0.0662)	(0.1262)
Upper-Middledle Age	0.1364***	0.0479	0.0637	-1.8147***	0.0052	-0.0301	-0.0363
	(0.0655)	(0.0864)	(0.0868)	(0.5379)	(0.0893)	(0.1114)	(0.2272)
High Education	-0.7235***	-0.8260***	-0.8384***	-0.6984***	-0.6715***	-0.7419***	-0.6100***
	(0.0720)	(0.0874)	(0.0882)	(0.0887)	(0.0894)	(0.1061)	(0.1755)
Upper-Middle Education	-0.4329***	-0.5194***	-0.5192***	-0.4366***	-0.4274***	-0.4400***	-0.3398***
	(0.0563)	(0.0688)	(0.0693)	(0.0698)	(0.0704)	(0.0810)	(0.0554)
Lower-Middle Education	-0.0601	-0.1317***	-0.1280***	-0.7481***	-0.1236***	-0.1262***	-0.0259
	(0.0413)	(0.0514)	(0.0515)	(0.2410)	(0.0521)	(0.0612)	(0.0396)
Duration Previous Unemployment Spell	-0.0012	-0.0003	-0.0004	0.0001	0.0006	0.0011	-0.0011
	(0.0012)	(0.0014)	(0.0014)	(0.0015)	(0.0016)	(0.0019)	(0.0013)
Insurance Benefits	0.1169***	0.1442*	0.1173	-1.0706***	0.0431	-0.0098	-0.0173
	(0.0580)	(0.0739)	(0.0773)	(0.3839)	(0.0796)	(0.0831)	(0.0385)
Duration Insurance Benefits	-0.0003	-0.0005	-0.0005	0.0014	-0.0002	0.0001	
	(0.0002)	(0.0003)	(0.0003)	(0.0013)	(0.0003)	(0.0003)	
Assistance Benefits	0.2427***	0.2928***	0.2642***	0.2313***	0.1904*	0.1810	
	(0.0851)	(0.1010)	(0.1156)	(0.1146)	(0.1154)	(0.1267)	
GAV Growth Rate[a]	0.0222***	0.0202	0.0299***	-0.0287	0.0338***	0.0334***	-0.1346
	(0.0102)	(0.0131)	(0.0139)	(0.0467)	(0.0150)	(0.0164)	(0.0850)
Unemployment Rate[b]	0.0546***	0.0655***	0.0584***	-0.0203	0.0398***	0.0500***	0.0321***
	(0.0043)	(0.0059)	(0.0065)	(0.0288)	(0.0069)	(0.0078)	(0.0052)
Unemployment Growth Rate[b]	0.0027***	0.0022	0.0077***	0.0047*	0.0048*	0.0032	0.0003
	(0.0013)	(0.0017)	(0.0025)	(0.0027)	(0.0027)	(0.0030)	(0.0013)
2^{nd} Quarter	0.2309***	0.2148***	0.2292***	0.1808***	0.1523***	0.1495***	0.1388***
	(0.0487)	(0.0603)	(0.0603)	(0.0613)	(0.0615)	(0.0689)	(0.0482)
3^{rd} Quarter	0.3873***	0.4015***	0.4061***	0.3462***	0.3131***	0.3052***	0.2868***
	(0.0510)	(0.0624)	(0.0625)	(0.0632)	(0.0634)	(0.0706)	(0.0506)
4^{th} Quarter	0.1590***	0.1576***	0.1510***	0.1325***	0.1152*	0.1009	0.1423***
	(0.0510)	(0.0636)	(0.0639)	(0.0640)	(0.0646)	(0.0713)	(0.0493)
Small Firm		-0.1867***	-0.1862***	-0.2092***	-0.2187***	-0.2230***	
		(0.0583)	(0.0587)	(0.0593)	(0.0593)	(0.0683)	
Lower-Medium-Size Firm		-0.0207	-0.0199	-0.0605	-0.0617	-0.0634	
		(0.0700)	(0.0702)	(0.0712)	(0.0717)	(0.0828)	
Upper-Medium-Size Firm		-0.1046*	-0.1105*	-0.1316***	-0.1350***	-0.1125	
		(0.0597)	(0.0599)	(0.0606)	(0.0605)	(0.0708)	
Agriculture		0.0130	-0.1208	-0.1453	-0.1970	-0.4254	
		(0.7380)	(0.7445)	(0.7567)	(0.7797)	(0.8136)	
Industry		-0.6135***	-0.6192***	-0.4953***	-0.4543***	-0.4519***	
		(0.0736)	(0.0744)	(0.0760)	(0.0758)	(0.0923)	
Services		-0.4167***	-0.4111***	-0.3361***	-0.3071***	-0.3212***	
		(0.0641)	(0.0646)	(0.0667)	(0.0668)	(0.0836)	
Located in Barcelona		-2.4908***	-2.5913***	-2.7348***	-2.5595***	-2.9451***	
		(0.9939)	(0.9638)	(1.3949)	(1.0185)	(1.0819)	
1980 Reform			0.4542***	0.2464	0.2180	-0.0171	
			(0.2092)	(0.2146)	(0.2222)	(0.2430)	
1984 Reform			0.4142*	0.1230	0.0452	-0.1914	0.4238***
			(0.2161)	(0.2163)	(0.2199)	(0.2368)	(0.1644)
1992 Reform			-0.3573***	0.2708	-0.4099***	-0.4586***	
			(0.1137)	(0.4115)	(0.1123)	(0.1182)	
1992 Reform × Insurance Benefits			0.1748	0.1938	0.1615	0.2210	
			(0.1442)	(0.1479)	(0.1462)	(0.1574)	
1992 Reform × Assistance Benefits			0.2001	0.1777	0.2337	0.1793	
			(0.2332)	(0.2384)	(0.2332)	(0.2577)	

Table 4 (Cont): Determinants of Employment Duration

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
$ln(t)$				13.9664***			10.3676***
				(1.1410)			(0.4039)
$ln(t)^2$				-17.4566***			-8.6336***
				(2.0686)			(0.6788)
$ln(t)^3$				10.0224***			2.9587***
				(1.7838)			(0.4480)
$ln(t)^4$				-3.0912***			-0.4162***
				(0.7423)			(0.1257)
$ln(t)^5$				0.4921***			0.0144
				(0.1475)			(0.0126)
$ln(t)^6$				-0.0315***			
				(0.0112)			
Gender$\times ln(t)$				0.4690***			0.1032***
				(0.2296)			(0.0463)
Gender$\times ln(t)^2$				-0.0826			
				(0.0518)			
Lower-Middle Age $\times ln(t)$				1.7616***			0.1835***
				(0.5394)			(0.0705)
Lower-Middle Age$\times ln(t)^2$				-0.3574***			
				(0.1250)			
Lower-Middle Education $\times ln(t)$				0.6136***			
				(0.2427)			
Lower-Middle Education $\times ln(t)^2$				-0.1229***			
				(0.0548)			
Insurance Benefits$\times ln(t)$				0.9928***			
				(0.3782)			
Insurance Benefits$\times ln(t)^2$				-0.1785***			
				(0.0848)			
Duration Insurance Benefits$\times ln(t)$				-0.0011			
				(0.0013)			
Duration Insurance Benefits$\times ln(t)^2$				0.0001			
				(0.0003)			
1992 Reform $\times ln(t)$				-0.4313			
				(0.4964)			
1992 Reform $\times ln(t)^2$				0.0259			
				(0.1360)			
GAV Growth Rate^a $\times$ $ln(t)$				0.0985***			0.1052***
				(0.0442)			(0.0435)
GAV Growth Rate$\times ln(t)^2$				-0.0288***			
				(0.0097)			
Unemployment Rate$\times ln(t)$				0.0421			
				(0.0300)			
Unemployment Rate$\times ln(t)^2$				-0.0039			
				(0.0071)			
High Education $\times ln(t)$							0.0253
							(0.0793)
1984 Reform $\times ln(t)$							-0.1230*
							(0.0736)
1984 Reform $\times$Lower-Middle Age							-0.0970
							(0.1327)
1984 Reform $\times$Upper-Middledle Age							-0.0719
							(0.1994)
AIC	24958.68	16983.13	16977.42	15921.64	15463.44	15407.38	24244.25

Note: Robust standard errors in brackets. ***, ** and * denote 1%, 5% and 10% significance, respectively. Columns *(1)*, *(2)* and *(3)* do not include state dependence variables. Column *(4)* includes a parametric function in $ln(t)$ to allow for state dependence, whereas column *(5)* uses a non-parametric approach (36 unreported month-dummy variables). Column *(6)* controls for Normally-distributed unobserved heterogeneity using the same specification as that of Column *(5)*. Column *(7)* aims to replicate the specification used by García Perez (1997) for Spain. The variable with the upper index ^{a} refers in this case to Spain rather than Catalonia, whereas variables with the upper index ^{b} refer to Catalonia rather than Barcelona.

Table 5: Determinants of Unemployment Duration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gender	0.3382***	0.1504***	0.1927***	0.3211	0.2311***	0.4094	0.3064***
	(0.0466)	(0.0696)	(0.0701)	(0.2476)	(0.0734)	(0.2604)	(0.0483)
Lower-Middle Age	-0.0153	0.1406*	0.0881	0.1107	0.5706***	0.1394	0.0653
	(0.0500)	(0.0779)	(0.0784)	(0.2734)	(0.2184)	(0.2825)	(0.0712)
Upper-Middledle Age	0.0988	0.2852***	0.2447*	0.3031***	0.3118***	0.3062***	0.5298***
	(0.0783)	(0.1261)	(0.1256)	(0.1308)	(0.1314)	(0.1542)	(0.2358)
High Education	0.1702*	0.1398	0.1007	0.8000*	0.1070	0.7868*	0.5956
	(0.0965)	(0.1467)	(0.1483)	(0.4551)	(0.1555)	(0.4640)	(0.3830)
Upper-Middle Education	-0.1218*	-0.0023	-0.0364	-0.0305	-0.0405	0.0115	-0.3067
	(0.0684)	(0.1060)	(0.1064)	(0.1102)	(0.1103)	(0.1244)	(0.2239)
Lower-Middle Education	0.1509***	0.1869***	0.1393***	0.1487***	0.1559***	0.1789***	0.5662***
	(0.0483)	(0.0710)	(0.0707)	(0.0735)	(0.0733)	(0.0834)	(0.1631)
Duration Previous Employment Spell	-0.0022*	-0.0007	0.0003	0.0010	0.0000	0.0016	-0.0074***
	(0.0012)	(0.0018)	(0.0018)	(0.0020)	(0.0018)	(0.0019)	(0.0023)
Insurance Benefits	0.3599***	0.0882	0.0437	-0.4338	0.5187***	-0.4931	-0.3133***
	(0.0624)	(0.0908)	(0.0921)	(0.3689)	(0.1171)	(0.3488)	(0.0564)
Duration Insurance Benefits	-0.0021***	-0.0017***	-0.0014***	-0.0076***	-0.0009***	-0.0070***	
	(0.0002)	(0.0003)	(0.0003)	(0.0031)	(0.0004)	(0.0019)	
Assistance Benefits	-0.6745***	-0.3690***	-0.4325***	-0.6362***	-0.6251***	-0.7346***	
	(0.0678)	(0.1012)	(0.1084)	(0.1146)	(0.1131)	(0.1269)	
GAV Growth Rate[a]	0.0795***	0.0659***	0.0788***	0.0026	0.0855***	0.0002	0.1649***
	(0.0113)	(0.0147)	(0.0179)	(0.0534)	(0.0193)	(0.0520)	(0.0498)
Unemployment Rate[b]	0.0366***	-0.0109*	-0.0286***	-0.0355***	-0.0362***	-0.0415***	0.0130***
	(0.0045)	(0.0064)	(0.0087)	(0.0091)	(0.0092)	(0.0101)	(0.0059)
Unemployment Growth Rate[b]	-0.0033***	-0.0136***	0.0025	0.0068	0.0057	0.0068	-0.0089***
	(0.0017)	(0.0023)	(0.0040)	(0.0042)	(0.0042)	(0.0045)	(0.0017)
2^{nd} Quarter	-0.0755	-0.0377	0.0164	0.0547	0.0385	0.0495	-0.0617
	(0.0635)	(0.0934)	(0.0948)	(0.0983)	(0.0987)	(0.1071)	(0.0658)
3^{rd} Quarter	-0.2165***	-0.1247	-0.1086	-0.1364	-0.1575*	-0.1530	-0.1227***
	(0.0600)	(0.0862)	(0.0873)	(0.0902)	(0.0899)	(0.0992)	(0.0618)
4^{th} Quarter	-0.0244	-0.0274	-0.0459	-0.0643	-0.0815	-0.0388	-0.0371
	(0.0612)	(0.0881)	(0.0893)	(0.0926)	(0.0926)	(0.1020)	(0.0628)
Small Firm		-0.0614	-0.1163	-0.1570*	-0.1571*	-0.1729*	
		(0.0797)	(0.0805)	(0.0835)	(0.0835)	(0.0942)	
Lower-Medium-Size Firm		-0.0016	-0.0462	-0.1033	-0.1080	-0.1038	
		(0.0959)	(0.0967)	(0.1012)	(0.1011)	(0.1135)	
Upper-Medium-Size Firm		-0.1048	-0.0759	-0.1034	-0.0906	-0.1011	
		(0.0826)	(0.0836)	(0.0874)	(0.0876)	(0.0978)	
Agriculture		1.0044	1.3015	2.5769***	1.9920***	2.6869	
		(1.4208)	(1.4220)	(0.9015)	(0.9028)	(1.9033)	
Industry		0.0363	0.1199	0.1752*	0.1445	0.1999*	
		(0.0994)	(0.1004)	(0.1046)	(0.1034)	(0.1177)	
Services		-0.1103	-0.0141	-0.0186	-0.0230	-0.0339	
		(0.0856)	(0.0884)	(0.0919)	(0.0914)	(0.1053)	
Located in Barcelona		0.0018	-0.1012	-0.1571	-0.0941	0.1670	
		(1.0352)	(1.0361)	(1.0366)	(1.0345)	(1.1458)	
1980 Reform			1.0450***	1.4579***	1.1979***	1.5941***	
			(0.2425)	(0.2899)	(0.2878)	(0.2981)	
1984 Reform			1.6815***	2.2186***	1.8064***	2.5059***	0.4309***
			(0.2723)	(0.3218)	(0.3427)	(0.3328)	(0.0956)
1992 Reform			0.5104***	0.1205	2.8988***	0.1336	
			(0.1946)	(0.5014)	(0.9821)	(0.5357)	
1992 Reform × Insurance Benefits			-0.1010	-0.0994	-0.0603	-0.1903	
			(0.2218)	(0.2375)	(0.2506)	(0.2618)	
1992 Reform × Assistance Benefits			0.0474	-0.0957	-0.0782	0.0065	
			(0.2891)	(0.3037)	(0.3108)	(0.3302)	

Table 5 (cont.): Determinants of Unemployment Duration

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*
$ln(t)$				8.9019***		9.2062***	15.3973***
				(0.8170)		(0.8253)	(0.5672)
$ln(t)^2$				-11.6151***		-11.9473***	-16.6344***
				(1.1460)		(1.1574)	(0.8732)
$ln(t)^3$				6.3781***		6.6151***	7.6765***
				(0.7407)		(0.7434)	(0.5627)
$ln(t)^4$				-1.5890***		-1.6535***	-1.5831***
				(0.2045)		(0.2031)	(0.1566)
$ln(t)^5$				0.1457***		0.1516***	0.1169***
				(0.0199)		(0.0196)	(0.0156)
Gender$\times ln(t)$				-0.3108		-0.3959	
				(0.2834)		(0.2888)	
Gender$\times ln(t)^2$				0.1353*		0.1544***	
				(0.0729)		(0.0731)	
Lower-Middle Age $\times ln(t)$				-0.2594		-0.3163	
				(0.3213)		(0.3323)	
Lower-Middle Age$\times ln(t)^2$				0.1227		0.1319	
				(0.0867)		(0.0908)	
High Education $\times ln(t)$				-0.4508		-0.3097	-0.1532
				(0.5591)		(0.5442)	(0.1099)
High Education $\times ln(t)^2$				0.0111		-0.0194	
				(0.1502)		(0.1395)	
Insurance Benefits$\times ln(t)$				0.7661*		0.7995***	
				(0.3916)		(0.3859)	
Insurance Benefits$\times ln(t)^2$				-0.1529		-0.1566	
				(0.1002)		(0.1009)	
Duration Insurance Benefits$\times ln(t)$				0.0025		0.0016	
				(0.0025)		(0.0016)	
Duration Insurance Benefits$\times ln(t)^2$				-0.0001		0.0001	
				(0.0005)		(0.0003)	
1992 Reform $\times ln(t)$				-0.5267		-0.6097	
				(0.7717)		(0.8209)	
1992 Reform $\times ln(t)^2$				0.5273*		0.5949***	
				(0.2848)		(0.3023)	
GAV Growth Rate$\times ln(t)$				0.0626		0.0583	
				(0.0640)		(0.0606)	
GAV Growth Rate$\times ln(t)^2$				-0.0029		0.0016	
				(0.0181)		(0.0171)	
Lower-Middle Education $\times ln(t)$							-0.0999*
							(0.0538)
Lower-Middle Age $\times$ Insurance Benefits							-0.0539
							(0.1024)
Upper-Middledle Age $\times$Insurance Benefits							0.1368
							(0.1555)
1984 Reform $\times$ High Education							-0.1879
							(0.3296)
1984 Reform $\times$ Upper-Middle Education							0.2389
							(0.2364)
1984 Reform $\times$ Lower-Middle Education							-0.2976***
							(0.1402)
1984 Reform $\times$Upper-Middledle Age							-0.6323***
							(0.2405)
Previous Employment Spell Below 3 Years							-0.0603
							(0.1385)
AIC	16429.53	7515.67	7396.42	6911.99	6913.01	6899.78	15795.03

Note: Robust standard errors in brackets. ***, ** and * denote 1%, 5% and 10% significance, respectively. Columns *(1)*, *(2)* and *(3)* do not include state dependence variables. Column *(4)* includes a parametric function in $ln(t)$ to allow for state dependence, whereas column *(5)* uses a non-parametric approach (12 unreported month-dummy variables and cross products with dummies of lower-middle age, days of insurance benefits received and the reforms of 1980, 1984 and 1992). Column *(6)* controls for Normally-distributed unobserved heterogeneity using the same specification as that of Column *(4)*. Column *(7)* aims to replicate the specification used by García Perez (1997) for Spain. The variable with the upper index ^{a} refers in this case to Spain rather than Catalonia, whereas variables with the upper index ^{b} refer to Catalonia rather than Barcelona.