The Part-Time Pay Penalty in a Segmented Labor Market

Daniel Fernández Kranz
IE Business School

Núria Rodríguez-Planas[Ψ]
Universitat Autònoma de Barcelona,
IZA, FEDEA, and INSIDE-Spain

December 2010

Abstract

While much of the literature that investigates the part-time (PT) / full-time (FT) hourly wage differential and its causes focuses on average effects, very few studies analyze the heterogeneous effects of PT work across different subgroups, despite the policy relevance of understanding channels behind the (raw) PT penalty in different labor markets. This paper is the first to examine the implications of switching to PT work for women's subsequent earnings trajectories, distinguishing by their type of contract: permanent or fixed-term. Using a 21-year unbalanced Social Security records panel of over 76,000 prime-aged women strongly attached to the Spanish labor market, we find that PT work aggravates the segmentation of the labor market insofar there is a PT pay penalty and this penalty is larger and more persistent in the case of women with fixed-term contracts. This result is robust to using a 2SLS approach to address the endogeneity by type of contract. The paper discusses problems arising in empirical estimation, and how to address them. It concludes with policy implications relevant for Continental Europe and its dual structure of employment protection.

Key words: Fixed-term and permanent contract, hourly wage levels and growth, prime-aged women, fixed-effects estimator, differential measurement error of LHS variable, underlying channels.

JEL classification: J13, J16, J21, J22, J31, J62, C23

[Ψ]Financial support from the Spanish ministry of Education and Science (grant SEJ2006-712) and the Generalitat de Catalunya (grant SGR2005-712) is also gratefully acknowledged.

I. Introduction

In the light of the recent surge in PT employment in many industrialized countries, and the relative concentration of women in PT jobs (making the issue a major one in gender equality), many researchers have increasingly become interested in analyzing the hourly wage differential between PT female workers and their FT counterparts and its causes.[1] To disentangle the channels through which the (raw) PT pay penalty emerges is the first step for designing policies which aim at improving the conditions of PT workers (Manning and Petrongolo, 2008). Given the policy relevance of this line of research, it comes as a surprise the little attention there has been, thus far, on the differential effect of PT work on wages across different population subgroups, as the underlying forces behind the PT pay penalty may differ drastically in different labor markets leading to distinct policy recommendations.[2]

At the same time, there is a growing concern among academics, politicians and practitioners, that the path of partial reforms taken by many Continental European countries, such as France, Germany, Portugal, Italy, and Spain, over the last three decades of maintaining strong employment protection for regular jobs while attempting at establishing more flexible but marginal labor market segments has resulted in a dual labor market and has deepened the segmentation between 'insiders' (those with permanent contracts involving high level of employment protection, decent jobs and generous benefits) and 'outsiders' (those with fixed-term contracts leading to poor labor

[1] See Jones and Long, 1979; Blank, 1990; Ermisch and Wright, 1993; Montgomery and Cosgrove, 1995; Jepsen, 2001; Wolf, 2002; Hu and Tijdens, 2003; Rodgers, 2004; Jepsen *et al.*, 2005; Hardoy and Schøne, 2006; Manning and Petrongolo, 2008; and Connolly and Gregory, 2009, among others.

[2] A possible explanation for this is that most studies (especially in Europe) rely on relatively small sample sizes of individuals who work PT making difficult the heterogeneity analysis. We have identified the following exceptions: Mocan and Tekin, 2003, analyze the nonprofit sector dimension; O'Connell and Gash, 2003, focus on differences between skilled and unskilled workers; and Ferber and Waldfogel, 1998; Rodgers, 2004; Booth and Wood, 2008; Hirsch, 2005; O'Dorchai *et al.*, 2007; and Mumford and Smith, 2007, study the gender dimension (or focus on male workers).

market perspectives and low remuneration).[3] Clearly, analyzing the PT / FT hourly wage differential and understanding the underlying channels behind the (possible) PT penalties in these two segments of the labor market ought to be of most relevance for policy making in countries with a high share of unemployment and stringent employment protection legislation. This is the central point of this article.

Our paper is the first to examine the implications of switching to PT work for women's subsequent earnings trajectories from the dual labor market employment protection perspective, by analyzing the PT pay penalty and its cause across two groups of workers, those with and without a permanent contract. We focus on adult women between 24 and 45 years old and strongly attached to the Spanish labor market and use a rich longitudinal dataset obtained from the Social Security records that covers employment history from 1985 to 2006, and has only recently been available to researchers in Spain.[4]

Our paper brings to light that PT work aggravates the segmentation of the labor market insofar the detrimental effects of PT work are considerably bigger and more persistent for workers under a fixed-term contract compared to workers with a permanent one. More precisely, we find evidence of a PT penalty both in wage levels and in wage growth of a greater magnitude for workers with fixed-term contracts than those with a permanent one. After accounting for workers' observable and unobservable characteristics, we find that PT women with permanent contracts have wages that are, on average, 9 log points lower and grow 2.9 log points less per year than wages of FT counterparts. For women with fixed-term contracts, the PT pay

[3] See Bentolila and Dolado, 1994; Blanchard and Landier, 2002; Dolado, *et al.*, 2002; Cahuc and Kramarz, 2004; Beninger, 2005; Eichhorst, 2007; and Dolado, *et al.*, 2007, among others.

[4] Although several papers have used longitudinal data to estimate the PT pay penalty (Blank, 1998; Hirsch, 2005; and Booth and Wood, 2008, among others), very few have more than two decades of data allowing them to observe women extended labor market history (see for instance, Connolly and Gregory, 2009).

penalty is more than twice as large, 23 log points, and wages grow 3.9 log points less per year than wages of FT counterparts. To put the estimates of wage growth into context, their size ranges between one-and-a-half and twice the size of the estimated college premium on wage growth. Thanks to the richness of our dataset, our estimates control for workers' socio-demographic characteristics, employer's characteristics, workers' previous employment history, and workers' unobserved heterogeneity. In addition, our results are robust to using 2SLS to control for endogeneity by type of contract.

The paper also discusses problems arising in empirical estimation, and how to address them. In particular, one contribution of our paper is to uncover an empirical problem not discussed in the literature up to now: the differential measurement error of the LHS variable by PT status. We use an alternative dataset (the *Time Use Survey*), to compare contractual hours with actual hours worked and show that PT workers consistently work a greater number of hours in excess of contractual hours relative to their FT counterparts.[5] The result of this measurement error in contractual hours is to bias upwards the hourly wages of PT workers (relative to FT workers) leading to underestimating the PT wage penalty.[6] To address this problem, we follow two different strategies. First, we use imputed effective hours to obtain an estimate of the PT / FT wage differential in levels. Second, we focus our attention on the wage change as opposed to wage level, and drop from our sample of analysis the observations of wage change observed exactly when status changes.[7] Assuming that differential measurement error by PT status is an individual-employment-status fixed-effect, our

[5] The measurement error in contractual hours can be explained by employers having an incentive to underreport contractual hours to reduce total labor costs, and being able to act upon it in a much easier way for PT jobs (since they are less protected by the law and the unions) than for FT jobs.

[6] As our data comes for Social Security records, we use contractual monthly wages and hours to calculate the hourly wages.

[7] For most (96%) of our sample of individuals who switched to PT employment, we observe them several years in either status. Therefore individuals' attrition because of this restriction is practically negligible.

approach circumvents the problem of differential measurement effect and informs us on whether the PT status also implies a penalty in the subsequent growth of wages.

Spain is a suitable case to investigate this issue because of the striking segmentation of its labor market.[8] The Spanish unemployment rate has been extremely high (as much as one fifth of the labor force) for almost two decades (during the 1980s and 1990s), and it is currently, at 18%, the highest in Europe. In addition, an important dual labor market developed after legislation changes in 1984, resulting in the economy with the highest rate of fixed-term contracts in Europe for the last two decades (over one third of all contracts are fixed-term contracts). Finally, the issue is particularly timely as the Spanish Prime Minister, following other industrialized countries' practices, is proposing to promote the use of PT work to fight unemployment, arguing that it will add flexibility in the labor market.

This paper is closer to Connolly and Gregory, 2009, (hereafter, CG) in that it examines the implications of switching to PT work for women's subsequent earnings trajectories using a long unbalanced panel and a fixed-effects `within´ estimator approach. Methodologically, our work differs from CG study in the following three ways: First, we estimate the differential PT pay penalty by type of contract. Second, we are able to distinguish between the PT pay penalty and the `motherhood pay gap´, as our data contains information on children in the household (whereas CG cannot distinguish between mothers and non-mothers). Third, we identify and address a methodological issue regarding differential measurement error in the dependent variable. While our findings for the primary labor market are consistent with those found by Connolly and Gregory, 2008 and 2009, and Manning and Petrongolo, 2008, in the UK and Hirsch, 2005, in the US, our work brings to light that in addition to the

[8] See for instance, Bentolila and Saint-Paul, 1994; Adam, 1996; Amuedo-Dorantes, 2000; Galdón-Sánchez and Güell, 2003; and Güell and Petrongolo, 2007, among others.

conventional channels behind the PT penalty, workers from the secondary labor market suffer a further unexplained loss due to the PT status switch itself, in addition to experiencing negative returns to PT work.[9]

The paper is organized as follows. The next section presents an overview of the literature. Section III describes the Spanish economic and institutional background. Section IV presents the data and the descriptive statistics. Section V explains the methodological approach and analyzes the results. Section VI concludes with a discussion on policy implications.

II. Literature on PT Earnings Penalty

Many researchers have increasingly become interested in analyzing the hourly wage differential between PT female workers and their FT counterparts. While the earliest studies focused on the US (Jones and Long, 1979; Blank, 1990) and the UK (Ermisch and Wright, 1993), the more recent literature has evaluated the PT pay penalty in many industrialized countries, such as Australia (Rodgers, 2004), Belgium (Jepsen, 2001; and Jepsen et al., 2005), Norway (Hardoy and Schøne, 2004), The Netherlands (Hu and Tijdens, 2003); and West Germany (Wolf, 2002), among others. Most studies find a negative unadjusted PT wage gap (a PT pay penalty), the magnitude of which differs substantially across the different countries. In some studies—such as, Rodgers, 2004; Jepsen, 2001; Jepsen *et al.*, 2005; Hardoy and Schøne, 2004; Muñoz de Bustillo Llorente *et al.*, 2008; and Manning and Petrongolo, 2008—, the PT pay penalty vanishes or becomes small when controlling for differences in workers and job characteristics (especially education and occupation). In other studies (Gallie *et al.*, 1998; Gornich and Jacobs, 1996; Rosenfeld and Kalleberg, 1990), a wage gap remains

[9] In the primary labor market, we find that the PT penalty is fully explained by the change of employer, negligible returns to PT work experience, and job downgrading

and this unexplained part also shows considerable cross-country variation. Finally, in a third group of studies, a PT pay premium is found (Booth and Wood, 2008; Pissarides *et al.*, 2005; and Pagán Rodríguez, 2007).[10]

While some of the differences in the results are explained by countries' institutional and cultural differences, and the amount of information available on workers, jobs, and labor market characteristics in the different datasets used; several identification problems within this literature are difficult to overcome. Most of this literature compares the hourly wages of PT female workers with those of FT female workers after controlling for all observable characteristics, acknowledging that unobserved heterogeneity may still prevail, as women deciding to work PT may have different tastes and preferences about work than do women who work FT. As Hakim (1997) explains, while some women are committed to careers in the labor market, a second group of women are qualitatively different since they *give priority to their domestic roles and activities, do not invest in what economists term 'human capital' even if they acquire education qualifications, transfer quickly and permanently to part-time work as soon as a breadwinner husband permits it, choose undemanding jobs 'with no worries or responsibilities' when they do work, and are hence found concentrated in lower paid and lower grade jobs which offer convenient working hours with which they [are] satisfied*. (Hakim, 1997, p. 43). If there are unobserved quality differences between PT and FT workers, results from cross-sectional studies of the PT wage effect will reflect an omitted variable bias. Nevertheless, many of the studies on the PT wage effect have been estimated on cross-sectional samples—see, for example, Simpson, 1986; Blank, 1990; and Hotchkiss, 1991; Ermisch and Wright, 1993;

[10] A detailed discussion on the few studies that have analyzed the PT hourly wage differential in Spain can be found in the next section, which describes the Spanish economic and institutional background (Section III) and in the results section (Section V.1).

Rodgers, 2004; Pagán Rodríguez, 2007; Manning and Petrongolo, 2008; Mumford and Smith, 2008, among others.

One way to address the unobserved heterogeneity problem is to use panel data and to estimate a fixed-effects-'within' estimator, in which case, the effect of PT on wages is identified through those workers who switch status (see Booth and Wood, 2008; and Connolly and Gregory, 2009).[11] While having important advantages, longitudinal analysis is not without shortcomings. A frequent problem arises when there is a small sample size of switchers, especially due to the infrequent transitions between FT to PT work and vice-versa, questioning the external validity of the results. In addition, measurement errors of hours and wages, which are common in this literature (Altonji 1986; Bound *et al.*, 2001), bias OLS estimates towards zero and magnify the attenuation bias in a fixed-effects context (Aaronson and French, 2004; Manning and Petrongolo, 2008).

Given that most studies use worker' survey data, measurement errors of key variables is a frequent concern in this literature. For instance, the OECD, 2002, warns about the possibility of having measurement errors in the survey stemming from the fact that the interviewed persons provide direct information about their own wages, rather than their employers, as is the case with matched employer-employee data or social security records. Others have raised similar concerns (see for instance, Pissarides *et al*., 2005; Mocan and Tekin, 2003; or Buligescu *et al*., 2009). Most recently, Buligescu *et al*., 2009, find that reported actual working hours, which are usually observed only for one week, show considerable dispersion and are likely to induce spurious negative correlation between working hours and the calculated wage rate. They argue that it is better to use contractual hours as they do not tend to vary as

[11] Alternatively, Hirsch (2005) uses multiple short panels with two observations per worker (one year apart) to estimate the effect of switching between FT and PT status on wage changes.

much from week to week. Some efforts to reduce the effect of measurement error in reported hours worked (and consequently PT status) include instrumenting such variables with their lags. However, the results indicate that the instruments do not always seem to work as they are fairly similar to OLS estimates for some of the countries (Pissarides *et al.*, 2005).

Another important identification problem is the danger of reverse causation: maybe it is low wages that 'cause' PT work, not PT work that 'causes' low wages. This problem is usually addressed by using an instrumental variables strategy. However, for this technique to work well requires a variable that affects propensity to work PT but does not have a direct effect on earnings. Unfortunately, such a variable is extremely difficult to find. And albeit children and marital status are frequently used as variables affecting the decision to work PT but not the wages earned—see Ermisch and Wright, 1993; Blank, 1998; Manning and Petrongolo, 2008, among others—, it is well established in this literature that "*this is a very strong assumption that may not, in reality, be any better than the exogeneity assumption that this is supposed to replace*" (Manning and Petrongolo, page F33, *Economic Journal* 2008). Aaronson and French, 2004, are the only ones that we know of to use an alternative instrument for worked hours, the work disincentive of the Social Security system. They are able to isolate exogenous shifts into PT employment resulting from changes in Social Security rules for older males.

In our paper, we account for worker unobserved heterogeneity by exploiting a rich longitudinal dataset that covers employment history from 1985 to 2006, and has only recently been available to researchers in Spain. In addition, as our data comes for Social Security records, we use contractual monthly wages and hours to calculate the hourly wages, eliminating the problem of measurement error due to recall bias or non-response. We do not model selection into PT employment. Therefore, we do not strictly identify the causal impact on wages or wage growth of working PT. However, considering that longitudinal estimates more closely approximate average treatment effects among the treated than among random draws from the population (Hirsch, 2005), we believe that our estimates address some of the issues raised in this literature and bring new evidence on the situation of PT workers in segmented labor markets in general, and in Spain, more specifically.

III. Economic and Institutional Background

The two most common forms of flexible work arrangements (fixed-term contracts and PT work) have evolved quite differently in Spain over the last two decades. Both types of contracts were first regulated by law in 1984 with the objective of adding flexibility and promoting employment in a rigid labor market with stringent employment protection legislation and high levels of unemployment. While fixed-term employment soared, the growth in PT employment was modest, at most. As a result, since the early 1990s, fixed-term employment represents one third of the Spanish labor force (by far, the highest share among European countries), whereas the share of PT employment is below one tenth of the labor force (far from the EU average of 18%).

The surge of fixed-term contracts began to be questioned in the late-1980s when experts started to advise against the risk of segmentation with "good" (permanent) jobs and "bad" (fixed-term) jobs—Segura *et al.*, 1991; Bentolila and Dolado, 1992; Jimeno and Toharia, 1993; and Dolado et al., 2002. The concern was that the Spanish labor market would become a dual labor market with workers with fixed-term contracts holding unstable, low protected and poorly paid jobs, while workers with indefinite contracts enjoyed protection and presumably also higher wages. The reforms of 1994 and 1997 aimed to enhance the use of permanent contracts and reduce its cost. However, both reforms were quite unsuccessful at reducing the share of temporary contracts in the labor force—see Kugler *et al.*, 2002, and Dolado et al., 2002.

In Spain, women are over-represented in both types of work arrangements, part-time and fixed-term. For example, 41% of contracts among women in Spain are fixed-term compared to 35% among men, and 23% of women work in PT jobs compared to 4% of men (LFS, 2005). While women's role in home production may imply that women have stronger preferences than men for PT jobs, this does not necessarily imply gender differences for fixed-term contracts (as a permanent contract is at least as desirable as a temporary one, given that it would commit the firm rather than the worker to costly procedures in case of separation). Using data from the 1994 through 1999 waves of the European Community Household Panel Survey, Pissarides *et al.*, 2005, find evidence suggesting that the unequal allocation of genders across fixed-term contracts and PT work in Spain stems from employer discrimination as opposed to workers' comparative advantage. They find that, after controlling for comparative advantages by conditioning the likelihood of being in involuntary PT work on human capital and family characteristics, single women in Spain are 10% more likely to be involuntary PT workers than single men. Similarly, they find that fixed-term contracts are 4% more frequent among single women than single men in Spain, and that family ties reinforces this tendency, with married women with children being about 9% more likely than married men to hold a fixed-term contract. In addition, exploring workers' preferences, these authors do not find evidence that women are particularly happier (or less unhappy) than men on PT jobs or with fixed-term contracts, as they find that PT jobs (fixed-term contracts) in Spain tend to reduce both males' and females' overall job satisfaction by 16% (25%).[12]

The evidence on wage differences by type of contract or PT status has been scarce in Spain (mainly due to the lack of large databases containing individual information on wages until recently), and based on cross-sectional analysis. Given that wages are set by collective agreements and that these do not allow workers to be paid differently on type of contract, it seems reasonable to think that employers do not discriminate against workers by type of contract. Despite this fact, several empirical studies find that permanent workers earn around 10% more, for men, and about 5% more, for women, after controlling for observed heterogeneity in personal and job-related characteristics and for selection into type of contract (Jimeno and Toharia, 1993; Hernanz, 2002; and De la Rica, 2004**)**. Moreover, there is evidence that workers with fixed-term contracts segregate into low-paying firms and occupations (De la Rica, 2004). Turning to the evidence on PT / FT wage differential, the evidence on wage differences between PT and FT workers in Spain has found that there is an 'unexpected' (in the light of the anecdotal evidence and job satisfaction indicators) wage *premium* to working PT (Pagán Rodríguez, 2007), or no effect (Pissarides *et al.*, 2005, and Muñoz de Bustillo Llorente *et al.*, 2008). However, failure of correcting for unobserved heterogeneity and measurement problems raise caution before taking these estimates at face value—as acknowledged by Pissarides *et al.*, 2005.

IV. The Data and Descriptive Statistics

We use data from the 2006 wave of the Continuous Sample of Working Histories (hereafter CSWH), which is a 4% non-stratified random sample of the population

[12] While many studies from developed countries find a preference for part-time work among women (Booth and van Ours. 2008 ; Gregory and Connolly, 2008 ; Van Praag and Ferrer-i-Carbonell, 2004), no such effect is found in East Germany or France (Clark and Senik, 2006) or Honduras (López et al., 2009).

registered with the Social Security Administration in 2006.[13] The CSWH consists of nearly 1.1 million individuals and provides the complete labor market history of the selected individuals back to 1967. It provides information on: (1) socio-demographic characteristics of the worker (such as, sex, education, nationality, province of residence, number o children in the household and date of birth); (2) worker's job information (such as, the type of contract—fixed-term versus permanent contract—, the PT status, the occupation, and the dates the employment spell started and ended, and the monthly earnings); (3) employer's information (such as, industry—defined at the three-digits Spanish classification code or NACE—, public versus private sector—, the number of workers of the firm, and the location—at the province level). Although not reported in the CSWH, other variables such as working experience (in FT and PT work) and tenure can be easily calculated. These data can be matched to data from the 2006 Spanish Municipal Registry of Inhabitants, which portrays information on the individual's education level, and number and date of birth of each of the members in the household.

Following CG, we restrict our sample to women whose full labor market history to date can be observed. We focus our analysis on wage and salary workers, that is, we exclude from the analysis self-employed individuals.[14] We confine our selection to birth cohorts between 1961 and 1978, implying that women in our sample will be aged between 24 and 45 years. The reason for dropping women younger than 24 years old is that we want to eliminate part-time work by students. In addition, we confine our analysis to women living in households of five or fewer members (96.5% of the sample). The reason for restricting our attention to women 45 and younger living in households of five or fewer members is that we want to have accurate information on

[13] For a description of the CSWH and the sampling strategy, see Argimón and González, 2006.
[14] If the worker held more than one job, the analysis focuses on his main job, defined as the job in which the worker has a permanent contract—if he has one—, and in the case of multiple jobs with the same type of contract, the one for which the individual worked the largest number of days in a given year.

the number and age of children, which is unavailable in the CSWH but can be obtained from the Spanish Municipal Registry of Inhabitants.[15] Finally, because we want to confine the analysis to women with a strong attachment to the labor force, we further restrict our sample to women who record at least three years in wage and salary work after having worked at least one year FT (this is the same restriction as the one used by CG). This sample selection results in an unbalanced panel of 591,063 observations on 76,025 women, of which 16,469 (21.66%) are observed working PT at some point in time as shown in Table 1. The percentage of women who switch to PT at some point in time is higher if they are working with fixed-term contract (28.13%) than if they are working with permanent contract (18.68%). Individuals are in the dataset between 3 and 21 years, and for an average of 8 years.

Table 2 presents descriptive statistics of the key covariates for the year 2006. The main focus of the present study is to analyze how the hourly wage trajectories vary by FT status and by contract type (fixed-term versus permanent). The data are therefore divided in four groups, classified by FT status and type of contract.[16] Following most of the European literature, we classify a worker working PT if she works 30 hours or less each week, and FT if she works 31 or more hours each week. Among the sample under study, we find that those with permanent contracts represent about two thirds of the sample. In addition, the percentage of women working in PT employment doves around one tenth of the sample, with a slightly higher share among those women working with fixed-term contracts (11% versus 9%).

When comparing the variables for women working in PT versus FT jobs, Table 2 shows that PT workers have lower (raw) hourly wages and their (raw) hourly wage

[15] Information on family composition becomes noisy for older women and for women living in large households, but is considerably accurate relative to Census data for the sub-population of women under analysis (see Lacuesta and Fernandez-Kranz, 2009).

[16] Although one individual can appear under different categories in different waves of the panel, it should be noted that these four categories are mutually exclusive.

grows at a lower rate than FT workers.[17] However, this cannot be used as a reliable estimate of the pay penalty that a given woman would suffer if she changed from FT to PT status because women working PT are very different from those working FT, as found in the subsequent rows of this table. For instance, we observe that PT workers are less-educated, older and more likely to have children of all ages than FT workers. Looking at employer differences across the two groups, women in PT employment are concentrated in the private sector, smaller firms and blue-collar occupations (relative to FT workers). These findings suggest that PT workers may segregate into low-paying firms and low-paying jobs. Finally, the years of experience into FT and PT work highlight that there is high persistence into both FT / PT status—this result has also been found in other countries as found by Blank, 1998; Buddelmeyer *et al.*, 2005; and Connolly and Gregory, 2008, and 2009. Overall the observed differences for PT versus FT workers hold across the two types of contract.

Compared to other datasets, our data has several advantages. First, the CSWH is a very large sample, which is important because PT work and switching from FT to PT (and vice-versa) is a relatively infrequent event, and more so when we focus the analysis on women strongly attached to the labor market. Second, the CSWH provides the complete labor market history for those women registered in the Social Security Administration in 2006, for up to 21 years. The length of the panel gives the opportunity to trace women's earnings trajectories for the first half of the employment life-cycle in the case of older cohorts and for substantial periods even for younger cohorts. Third, it contains reliable information on monthly earnings, tenure, experience in FT and PT work, and change of employer, as the information comes directly from the payroll records. Measurement error due to recall bias or self-reporting for these key

[17] Our measure of pay is hourly earnings, calculated as gross yearly earnings excluding pay in respect to overtime hours, divided by total contractual hours, deflated by the 2006 price deflator.

variables is minimized with this data set. Similarly, non-response is not an issue. Fourth, the dataset has rich information on individual characteristics, including education, age, ethnicity, marital status, and number and age of children in the household.

V. Methodology and Results

Our objective is to exploit longitudinal data in Spain to analyze the direct consequences of PT employment on subsequent earnings, earnings growth, and career trajectories. Because of the striking segmentation of the Spanish labor market, we analyze the PT penalty by type of contract and explore the effectiveness of job protection into reducing the potential PT penalty.

V.1. PT log hourly wage differential

We begin our analysis by estimating the average effect of working PT on the hourly wage level. Table 3 presents our estimates using a variety of approaches. For ease of the exposition, we use a simple dummy variable approach to measure the log hourly wage differences associated with PT status, conditional on controls.[18] We begin by estimating the following equation using pooled OLS:

$$(1) \qquad LnW_{it} = X_{it}\beta + \theta PT_{it} + \phi_i + \mu_{it}$$

Here, LnW_{it} is the natural log of real hourly earnings of individual *i* at year *t*; X_{it} is a vector of individual and job characteristics for individual *i* at time *t*, with β the corresponding coefficient vector (including an intercept). Because there has been much debate on whether variables that control for employer characteristics or change in

[18] This approach is similar to the one used by Hirsch, 2005; Manning and Petrongolo, 2008, and CG, among others. Earnings function parameters differ between PT and FT status, but the gaps in the wage estimated using the dummy variable approach differ little from those based on separate equations by PT status, and evaluated at the means.

occupation or employers ought to be included in the specification (see discussion below), we present alternative specifications to evaluate the robustness of the results. PT_{it} is a binary variable equal to one if the worker's principal job is PT in year *t*. The error term includes both a random component μ_{it} with mean zero and constant variance, and a worker-specific fixed effect ϕ_i. All regressions use the Huber/White estimator of variance and allow for observations not being independent within cluster-individuals. Regression (1) is estimated for the whole sample (panel A), and separately for workers with fixed-term contract (panel B) and those with permanent contract (panel C).

Analyzing first the pooled OLS estimates for the whole sample (first row of panel A), the estimate headed "unadjusted" shows that the log hourly earnings of PT women are, on average, 11 log points less than the log hourly earnings of FT women. The subsequent columns estimate the average PT hourly wage differential adding additional controls. For instance, the second column shows that the PT penalty falls to 3 log points once we control for women socio-demographic characteristics. The inclusion of additional employer controls changes the sign of the PT penalty into a small premium (of up to 3 log points once all controls have been added). These results are in line with evidence from other (cross-sectional) studies from other countries that find that the "adjusted" PT / FT differential is very small (and it is mainly explained by workers' characteristics and occupational segregation).[19]

Nonetheless OLS estimates are based on a strong assumption that PT status is exogenous (conditional on the included covariates). Clearly this is not the case, as discussed earlier in Section II. To deal with unobserved heterogeneity, we proceed to estimate the following fixed-effects equation (2), with results shown in row 2 of panel A:

[19] See, for instance, results from Australia (Rodgers, 2004), Belgium (Jepsen, 2001; Jepsen *et al.*, 2005), Norway (Hardoy and Schøne, 2006), and the UK (Manning and Petrongolo, 2008), among others.

$$(2) \qquad LnW_{it} - \overline{LnW_i} = \left(X_{it} - \overline{X_i}\right)\beta' + \theta'\left(PT_{it} - \overline{PT_i}\right) + \mu_{it} - \overline{\mu_i}$$

We find that the fixed-effects estimates display a PT *premium* in Spain that ranges between 6 and 8.5 log points. Should we infer from these estimates that women working PT in Spain earn higher hourly earnings than those on FT work? Not necessarily. Certainly, these results are difficult to reconcile with the anecdotal evidence presented earlier (in Section III) suggesting that PT jobs in Spain are mainly involuntary in nature. In addition, estimates from Figure 1 show that almost two thirds of PT workers in Spain would prefer to have a FT job, in sharp contrast with what is found in other European countries.

To our knowledge, three other studies have estimated the PT / FT wage differential in Spain using a cross-sectional approach with data from the European Community Household Panel Survey (Pissarides *et al.*, 2005; and Pagán Rodríguez, 2007) and from the 2006 Survey on Income and Living Conditions Vida (Muñoz de Bustillo Llorente *et al.*, 2008). All three studies find evidence of an unadjusted hourly wage penalty associated with being a female PT worker (of between 10% and 16%), which becomes a PT *premium* after adjusting for observable characteristics (and self-selection in the case of Pagán Rodríguez, 2007) in the two studies that use the European Community Household Panel Survey.[20] However, the Pissarides *et al.*'s PT premium vanishes when potential measurement error in hours and PT status are instrumented with lagged values. The authors conclude that they are reluctant to believe their estimates as measurement error may still be affecting their IV estimates.[21]

Given that our data comes from Social Security records it ought to be less spurious than workers' survey data overcoming the measurement error problem found

[20] In the other study, the 'unadjusted' PT penalty vanishes after controlling for workers' and job characteristics.
[21] The other two studies do not correct for measurement error.

in earlier studies. Nonetheless, given our results thus far, we suspected that our measure of hours, that is, contractual hours, could be consistently underreporting actual worked hours for PT workers relative to FT workers, which would lead to a differential measurement error in contractual hours by PT status. An explanation for this is that employers have an incentive to underreport contractual hours to reduce their labor costs. Given that PT workers tend to be in more vulnerable situations than FT workers (Belous, 1989; Bardasi and Gornich, 2000; Connolly and Gregory 2008 and 2009; Manning and Petrongolo, 2008), and given the higher dispersion of hours worked among PT workers compared to FT workers in Spain (Muñoz de Bustillo LLorente *et al.*, 2008), underreporting of contractual hours, albeit unlawful, seems to be an easier and more common practice for PT contracts than FT ones. Using data from the *Time Use Survey*, Figure 2 provides evidence that PT workers consistently work a greater number of hours in excess of contractual hours relative to their FT counterparts, which biases upwards the hourly wages of PT workers (relative to FT workers) leading to underestimating the PT wage penalty.[22]

One way to address this problem is to use imputed effective hours to calculate the hourly wage as opposed to contractual hours.[23] Rows 3 and 4 of panel A of Table 3 show pooled OLS and fixed-effects estimates using as dependent variable hourly wages calculated with imputed effective hours. The fixed-effects estimates show that, on average, women working PT in Spain earn 19 log points *less* per hour than their FT counterparts (after controlling for women socio-demographic characteristics—column 2 row 4 of panel A). In addition, comparing rows 3 and 4 of panel A shows that the OLS estimates consistently overestimate the PT penalty relative to the fixed-effect estimates

[22] The effective-contractual hours' gap for PT workers is significantly different from the gap for FT workers at the 1% level across all age and education groups.

[23] Imputed hours come from a regression of effectively worked hours against contractual hours, age, education, two-digit industry dummies and occupation dummies using the Spanish Time Use Survey Dataset.

suggesting that women who move into PT are negatively self-selected, a common finding in this literature. These estimates highlight the weaknesses of using cross-sectional data for undertaking such type of analysis. A priori, our cross-sectional analysis seemed to offer sound results consistent with those found earlier in the literature. However, the availability of longitudinal data enables us to further investigate our findings and to uncover a new identification problem, not discussed (to our knowledge) in the literature until now.

The analysis thus far has analyzed the average hourly wage difference between women working PT and FT. However, the average effect may hide important differences across groups. In what follows, we study the PT hourly wage penalty by type of contract. The rationale being that the effect of PT on hourly wages and the channels through which it operates may well differ by the level of job protection the worker has, and whether he is in the primary labor market (with a permanent contract) or in the secondary labor market (with a fixed-term contract). For instance, low levels of unionization (Belous, 1989), and lower accumulation of skills and lower returns to skills (Connolly and Gregory, 2009; Manning and Petrongolo, 2008) are found both in PT jobs and 'bad' jobs. In addition, Bardasi and Gornich, 2000, have found evidence that this association is likely to be the strongest in countries where the size of the PT labor market is small, that is, where PT work is more likely to be in a 'marginalized' fringe of the labor market, such as in Spain.

Panel B and C of Table 3 replicate the analysis done in panel A but for two separate sub-samples. The heterogeneity analysis shows that the average effect of PT work on hourly wages differs by type of contract, bringing to light that the PT penalty is considerably larger for workers in the secondary labor market. Our preferred estimates (second column of rows 4) show that women with permanent contracts have, on average, 9 log points less hourly earnings than their FT counterparts. However, the PT penalty is more than twice as large (23 log points) for women with fixed-term contracts. In addition, examining the results from panels B and C shows that the negative sample selection that we are able to correct for when using fixed-effects is considerably larger for workers with permanent contracts. While the PT penalty for workers in the primary sector gets reduced by two thirds when moving from the OLS estimate to the fixed-effects one (from -27 to -9 log points), it only decreases by one third (from -32 to -23 log points) for workers in the secondary labor market. This finding may be explained by the fact that women with permanent contracts have job protection and are 'free' to move to PT work without 'too many' penalties. In contrast, for women with fixed-term contracts their move to PT may be 'less voluntary'. Finally, we find that the reduction of the differential measurement error bias is greater for women with fixed-term contract as one would expect if employers are more prone to under-report contractual hours among the most vulnerable workers.

While these results highlight the existence of a PT penalty in levels in Spain, and show that employment protection reduces it by half, they cannot provide much guidance on what explains the penalty as some noise remains in the LHS variable due to the fact that its denominator has been imputed (notice that the estimates do not vary much as we control for additional covariates).[24] In what follows, we propose to analyze how the change in log hourly wages differs by PT status and to explore how working PT affects the workers' earnings trajectories.

[24] As long as the noise is not related to PT status, it ought not to have an effect on our estimate of PT work.

V.2. PT log hourly wage growth differential

Assuming that differential measurement error by PT status is an individual-employment-status fixed effect, and dropping from our sample the wage observation the year in which the switch from FT to PT occurs, we estimate the effect of working PT on the change in log hourly wages free of differential measurement error. To do so, we estimate the equations (3) (OLS) and (4) (fixed-effects) below:

$$(3) \qquad \Delta LnW_{it} = X_{it-1}\beta + \theta PT_{it-1} + \gamma FT_{it-1} + \lambda\left(PT_{it-1} \times FT_{it-1}\right) + \phi_i + \mu_{it-1}$$

Here, ΔLnW_{it} is the change in the natural log of real hourly earnings of individual i between year $t-1$ and year t; X_{it-1} is a vector of individual and job characteristics previously described for individual i at time $t-1$, with β the corresponding coefficient vector (including an intercept). PT_{it-1} is a binary variable equal to one if the worker's principal job is PT in year $t-1$; FT_{it-1} is a binary variable equal to one if the worker holds a fixed-term contract at time $t-1$. The error term includes both a random component μ_{it} with mean zero and constant variance, and a worker-specific fixed effect ϕ_i. All regressions use the Huber/White estimator of variance and allow for observations not being independent within cluster-individuals.

$$(4)$$

$$\Delta LnW_{it} - \overline{\Delta LnW_i} = \left(X_{it-1} - \overline{X_i}\right)\beta' + \theta'\left(PT_{it-1} - \overline{PT_i}\right) + \gamma'\left(FT_{it-1} - \overline{FT_i}\right) + \lambda'\left(\left(PT_{it-1} \times FT_{i-1}\right) - \left(\overline{PT_i} \times \overline{FT_i}\right)\right) + \mu_{it-1} - \overline{\mu_i}$$

As in equation (2), in equation (4) we identify the effect of PT work through those who switch status. In contrast with estimates obtained with equations (1) and (2), in the regressions (3) and (4) we do not use the observation of the year the switch occurs. This implies that we loose those individuals for which we do not observe at least two consecutive periods in a given FT / PT status. If this lost were large, it could lead to a problem of sample selection. Fortunately, the number of individuals that we loose because we do not observe at least two consecutive periods in a given FT / PT status is very small as shown in Table 4 and ought not to be a concern in terms of selection bias as it represents less than 1.3% of the whole sample, and less than 4% of those who switch to PT work at some point in the sample—notice also that only half of these we loose to non-employment.

For ease of the exposition, equations (3) and (4) use a simple dummy variable approach to measure the change in log wage differences associated with PT status and type of contract, conditional on controls.[25] While it is true that our specifications do not account for selection by type of contract and by PT status, by controlling for number and age of children and education, on the one hand, and employer characteristics, on the other, we are *de facto* controlling for the same information that many researchers have controlled for when using an instrumental variable approach correction. In the case of selection into FT / PT employment, most researchers use family composition variables to identify participation into PT employment (Blank, 1998; Pissarides *et al.*, 2005) arguing that these variables do *not* explain wages.[26] Similarly, in the case of selection by type of contract, researchers use employer's characteristics, such as private versus public sector or firm size.[27] We find the assumption that these variables explain participation but not wage (or wage growth in our case) difficult to believe and, therefore, prefer using the information directly in the wage equation, acknowledging

[25] Earnings change function parameters differ between PT and FT status and type of contract, but the gaps in the wage change estimated using the dummy variable approach differ little from those based on separate equations by PT status and contract type, and evaluated at the means.

[26] To identify participation into PT work in Spain, Pagán Rodríguez, 2007, uses age, level of education, marital status, number of children 5 years old or younger, number of children between 6 and 12 years old, region and household income. He finds evidence of sample selection among women working PT (but not among those working FT).

[27] To identify participation into fixed-term versus permanent contract in Spain, Hernanz, 2002, uses gender, age, level of education, industry, public or private employer, firm size and region and working day duration (and occupation on the case of the estimation of the SES sample). De la Rica, 2007, uses age, tenure and education, controls for occupation (at one-digit) and the rate of fixed-term contracts by autonomous community. De la Rica, 2007, does not find evidence of selection into type of contract for females (while there is selection for males). Hernanz's estimates are not presented separately by sex, therefore we are unable to know whether her evidence of selection in the whole sample would hold when the analysis focuses on women.

that selection into the different types of jobs cannot be corrected, although unobserved heterogeneity is accounted for with the fixed-effects specification.

Table 5 presents our pooled OLS and fixed-effects estimates of the PT penalty on wage change using data from the CSWH, and controlling for different covariates. Panel A shows estimates for the whole sample, whereas Panel B shows the estimates for workers with fixed-term contracts and those with permanent contracts.

There are important differences between women with fixed-term contracts and those with permanent contracts. After accounting for workers' observable and unobservable characteristics (column 3 of Panel B), we find that PT women with permanent contracts experience on average 2.9 log points lower hourly wage growth per year than their FT counterparts, and that PT women with fixed-term contracts experience 3.9 log points lower hourly wage growth per year than their FT counterparts. How large are these estimates? We claim that these estimates are considerably large and concerning. For instance, compared to the effect of education on hourly wage growth, we find that having a college degree or more increases women's hourly wage growth by 2 log points per year compared to women without a high-school degree. Therefore, the size of the PT penalty is almost one-and-a-half that of the college premium among women with permanent contracts and nearly doubles that of the college premium among women with fixed-term contracts. Notice also that the PT penalty for women with fixed-term contracts is one fourth larger (and statistically significantly so) than for women with permanent contracts, suggesting that there is a negative relationship between job protection and PT penalty.

Also worth highlighting is the change in the estimates when moving from the unadjusted PT growth penalty (column 1 of Panel B) to the penalty once workers' characteristics are accounted for (column 2 of Panel B), especially for women with fixed-term contracts, as the estimate falls more than one fifth, from 3.5 to 2.7 log points.[28] In addition, we also observe that the PT growth penalty rises to 3.9 log points for women with fixed-term contracts, once we control for unobserved heterogeneity (column 3 of panel B), suggesting that there is "second-order" positive selection into PT work for women with fixed-term contracts (remember that the levels estimates showed the traditional "first-order" negative self-selection into PT jobs for women with both types of contracts).[29]

Columns 4 and 5 show the inclusion of employer characteristics—such as whether the employer is in the public or private sector, the size of the employer, and the occupation—, as additional controls. Whether to include or not such covariates in the specification has been the source of many academic discussions in this literature. The reason is that women who work PT may segregate into jobs or occupations with low-wage growth. As explained by Manning and Petrongolo (2008), "*if this is the case, controlling for such covariates will only, at best, provide an estimate of the PT penalty if women in PT employment are compared to those in FT employment in similar low-wage growth jobs or occupations. At the same time, an estimate that does not control for these characteristics may exaggerate the true PT penalty as part of the reason FT and PT women work in different jobs or occupations is the differences in the labor market experience they possess.*" Although controlling for employer characteristics has a small effect on the size of the PT penalty for both workers with fixed-term and permanent contracts, the story varies by type of contract. For workers with fixed-term contracts, controlling for employer characteristics (moving from columns 3 to 5 in panel B) reduces the PT penalty by 5% (the estimates falls from 3.9 to 3.7 log points).

[28] While a decrease is also observed for women with permanent contracts, the size of the decrease is smaller.

[29] For workers with permanent contracts, we observe the more common negative self-selection result as we move from the OLS estimate to the fixed-effect one.

In contrast, for workers with permanent contracts, the PT penalty *increases* by 7% (from 2.9 to 3.1 log points). The story for workers with fixed-term contracts is a story of PT workers downgrading into jobs or occupations with lower hourly wage growth. In contrast, for workers with permanent contracts those working PT were either already more concentrated in low-wage growth jobs (compared to their FT counterpart) before moving into PT, or when they switched to PT they moved to jobs with higher wage growth.

Columns 6 and 7 show the inclusion of industry as an additional control. Here, the story is the same for both types of contracts: the PT penalty is even larger if we control for industry. While, on average, women in PT employment are segregated in industries with low-wage growth compared to their FT counterparts (as illustrated by the reduction in the cross-sectional estimates of the PT penalty—moving from columns 4 to 6), the PT growth penalty *increases* when we move from columns 5 to 7, that is when we add an industry control to the specification that corrects for unobserved heterogeneity. Women switching to PT jobs either move to industries with higher wage growth than their FT counterparts, or they were already working in industries with low-wage growth. When controlling for industry, we find that the PT penalty increases by one fourth (more than one tenth),—from 3.7 to 4.6 log points (3.1 to 3.5 log points)—, for workers with fixed-term (permanent) contracts.

V.3. Earnings Trajectories and the cumulative PT penalty

Up to now, our analysis has focused on the average effect of PT work on hourly wages and wage growth. In this section we analyze how a switch from FT to PT work affects workers' earnings trajectories, i.e., we are interested in knowing whether there is a PT pay penalty not only the first year after switching to PT work but also thereafter.

As our results will confirm later, it is interesting for this analysis to distinguishing between two types of situations: whether the worker changed employer the year of switching to PT work or not.

Figure 3 shows the cumulative PT penalty by type of contract differentiating by whether the worker changes employer at the time of the PT switch or not (estimates of the key coefficients are shown in Tables 6.A and 6.B). While panel A of Figure 3 presents the cumulative PT penalty estimated with the specification that controls for workers' characteristics, panel B shows the estimates when we control for both workers and employers' characteristics.

Focusing first on panel A, we find that the return to PT experience is very different in the primary labor market than in the secondary one. For instance, for workers with permanent contracts, the return to PT experience gives a negative return during the first year and becomes flat thereafter. In contrast, for workers in the secondary market (those with fixed-term contract), we find that PT experience gives a negative return for at least the first four years. These results are in line with Hirsch, 2005, and CG who find that accumulated skills account for much of the PT wage disadvantage among workers in the US (the former) and the UK (the latter). Moreover, similar to CG, we find that the returns to PT work are lower in lower level jobs—CG find lower returns to PT work for workers in lower level occupations.

Another important insight emerges from panel A of Figure 3: For workers with permanent contracts, the PT penalty is mainly explained by the change of employer at the time of the switch to PT work. While no PT penalty is observed among those workers who remain with the same employer, the switch to PT work imposes an immediate earnings penalty of 10 log points if the worker changes employers.[30] Such penalty remains in evidence over at least four years. In addition, panel B shows that half of this penalty is accounted for employers' characteristics, providing evidence of job downgrading.

For workers in the secondary labor market, we also find that changing jobs with the switch to PT work is a further source of earnings penalty, over 10 log points, of which, one fourth are explained by employers' characteristics.[31] In addition, we find that for workers in the secondary labor market, there is an additional penalty of 9 log points at the time of the switch to PT that is not explained by employer switch, nor other observable characteristics. This is in addition to the further losses due to negative return to PT experience discussed earlier.

To sum up, for workers in the primary labor market, we find that the PT penalty is explained by the change of employer and job downgrading, as well as negligible returns to PT work experience during the first few years in PT work. Once these channels are taken into account, neither PT status nor the switch into PT is associated with a significant pay penalty directly. However, these three channels do give rise to non-negligible earnings losses, and it takes at least four years for these penalties to vanish. Perhaps not surprisingly, these results are not so different from those found in countries in which PT is well established, such as the UK. In contrast, for workers in the secondary labor market, the PT penalties are greater and long-lasting, raising serious concern for such workers in these types of contracts. We find that the switch to PT status in itself is associated with a 10 log points immediate drop in earnings that we

[30] These results are in line with those found by Manning and Petrongolo, 2008, for the UK, where they find that for those women who change hours status *without* changing employer there is a very small pay penalty of 0.2%.

[31] Note that the fact that job downgrading explains less of the PT penalty for workers with fixed-term contracts than for those with permanent contracts is consistent with the fact that jobs in the secondary labor market are already 'bad' jobs.

are unable to explain with workers' observable or unobservable characteristics nor employers' attributes. In addition to this unexplained PT penalty, we find evidence that experience in PT work is negative. Finally, the PT penalty is exacerbated by job downgrading and job change.

VI. Policy implications and directions for further research

The focus of this paper has been to study the linkage between the PT pay penalty and the type of contract. The main result of the paper is that PT work feeds into the labor market segmentation that is caused by a dual system of job protection insofar the negative wage effects of working PT are larger and more persistent for workers in the secondary market (with fixed-term contracts). Our estimates suggest that the leeway granted by job protection leads to a less favorable treatment of workers with weak rights, such as those with fixed-term contracts in PT jobs. This result must be seen in the context of current policy proposals of adding labor market flexibility through the use of PT work, especially in countries with rigid and dual market structures. Rather than dismissing the important role of PT work for labor market flexibility, we view our results as implying that PT work is a tough sell politically when labor markets are highly segmented (perhaps not surprisingly, PT work in Spain is mostly involuntary, as 60% of women working PT say they would prefer a FT job). In this regards, an important topic for future research is the study of transition patterns for PT workers, especially the transitions from PT to FT and from fixed-term to permanent contracts by work status.

Finally, our results bring to light another dimension of gender and family pay gaps in segmented labor markets. Given the relative concentration of mothers in PT work, they suggests that Spain is still far from enabling the conciliation of work and family through the reduction of regular work schedule.

REFERENCES

Aaronson, D., and E. French. 2004. “The Effect of Part-Time Work on Wages: Evidence from the Social Security Rules.” *Journal of Labor Economics*, Vol. 22, No. 2 (April): 329-352.

Adam, P., 1996. “Mothers in an Insider–Outsider Economy: the Puzzle of Spain.” *Journal of Population Economics* 9, 301– 323.

Altonji, J. 1986. “Intertemporal Substitution in Labor Supply: Evidence from Microdata.” *Journal of Political Economy* 94 (June): S176-S215.

Amuedo-Dorantes, C. 2000. “Work Transitions Into and Out of Involuntary Employment in a Segmented Market: Evidence from Spain,” *Industrial Labor Relations Review,* 2000, 53(2): 309-325.

Argimón, I., and C. González. 2006. “La Muestra Continua de Vidas Laborales de la Seguridad Social.” *Boletín Económico*, Banco de España, (May): 39-53.

Bardasi E., and J. C. Gornick. 2000. “Women and Part-Time Employment: Workers’ Choices and Wage Penalties in Fve Industrialized Countries.” Institute for Social and Economic Research, *ISER Discussion Paper*, No. 11.

Belous R. 1989. “The Contingent Employment: The Growth of Temporary, Part-Time, and Subcontracted Workforce.” Washington D.C.: Nacional Planning Association.

Beninger. D. 2005. “Emploi et social en France: Description et evaluation.” ZEW Documentation 03-05, Mannheim, ZEW.

Bentolila, S. and J. Dolado. 1994. “Labour Flexibility and Wages: Lessons from Spain.” *Economic Policy* 18: 53-99.

Bentolila S. J. J. Dolado, and J.F. Jimeno. 2008. “Two-Tier Employment Protection Reforms: The Spanish Experience.” *CESifo DICE Report 4/2008.*

Blanchard O. and A. Landier. 2002. “The Perverse Effects of Partial Labor Market Reform: Fixed Duration Contracts in France.” NBER Working Paper 8219, Cambridge, NBER.

Blank, R. 1990. “Are Part-Time Jobs Lousy Jobs?” In *A Future of Lousy Jobs?* edited by Gary Burtless, pp. 123-155. Washington, DC: The Brookings Institute.

Blank, R. 1998. “Labor Market Dynamics and Part-Time Work”, in Solomon W. Polacheck (ed.), *Research in Labor Economics, Volume 17,* JAI Press, Stamford, Connecticut.

Bentolila S., and G. Saint Paul. 1992. “The Macroeconomic Impact of Flexible Labor Contracts, with an Application to Spain.(with Gilles Saint-Paul).” European Economic Review 36, 1992.

Booth and van Ours. 2008. “Job Satisfaction and Family Happiness: The Part-time Work Puzzle.” *Economic Journal* 118(526): F77-F99.

Booth A., and M. Woods. 2008. “Back-to-Front Down Under? Part-Time/ Full-Time Wage Differentials in Australia.” *Industrial Relations*, Vol. 47, No. 1.

Bound, John, C. Brown, and N. Mathiowetz. 2001. “Measurement Error in Survey Data.” In *Handbook of econometrics*, vol. 5, ed. James Heckman and Ed Leamer, 3705–3843. Amsterdam: North-Holland.

Buddelmeyer H., G. Mourre, and M. Ward-Warmedinger. 2005. “Part-Time Work in EU Countries. Labour Market Mobility, Entry and Exit.” European Central Bank Working Paper n. 460.

Buligescu, B., de Crombrugghe, D., Mentesoglu, G., and Montizaan, R. 2009. “Panel Estimates of the Wage Penalty for Maternal Leave.” *Oxford Economic Papers*, 61: 35-55.

Cahuc P. and F. Kramarz. 2004. “De la Précarité à la Mobilité: Vers une Sécurité Sociale Professionnelle.” Rapport au Ministre d’Etat, Ministre de l’Economie, des Finances et de l’Industrie et au Ministre de l’Emploi, du Travail et de la Cohésion Sociale, Paris.

Clark, A.E., and C. Senik. 2006. “The (Unexpected) Structure of ‘Rents’ in the French and British Labour Markets.” *Journal of Socio-Economics* 35(2): 180–96.

Connolly, S. and M. Gregory. 2008. “Moving Down: Women’s Part-Time Work and Occupational Change in Britain, 1991–2001.” *Economic Journal*, vol. 118: F52–F76.

Connolly, S. and M. Gregory. 2009. “The Part-Time Pay Penalty: Earnings Trajectories of British Women.” *Oxford Economic Papers*, vol. 61 no. S1 : 76-97.

De la Rica, S. 2004. "Wage Gaps between Workers with Indefinite and Fixed-Term Contracts: The Impact of Firm and Occupational Segregation." *Moneda y Crédito* 219: 43-69.

Dolado, J., C. García-Serrano and J.F. Jimeno. 2002. “Drawing Lessons from the Boom of Temporary Jobs in Spain,” *Economic Journal*, 112(480): F270-295.

Dolado, J.; M. Jansen, and J.F. Jimeno. 2007. "A Positive Analysis of Targeted Employment Protection Legislation," *The B.E. Journal of Macroeconomics*: Vol. 7 : Iss. 1 (Topics), Article 14.

Employee Benefits Research Institute. 1993. “Part-Time Work: Characteristics of the Part-Time Work Force: Analysis of the March 1992 Current Population Survey.” Working Paper P-55.

Eichhorst, W. 2007. “The Gradual Transformation of Continental European Labor Markets: France and Germany Compared.” IZA Discussion Paper 2675.

Ermisch, J., and R. Wright. 1993. “Wage offers and full-time and part-time employment by British women.” *Journal of Human Resources*, vol 28 (Winter), 111-33.

Ferber M., and J. Waldfogel, 1998. "The Long-Term Consequences of Non-Standard Work", *Monthly Labor Review*, Vol. 121: 3 - 12.

Fernández-Kranz, D., and A. Lacuesta. 2009. "Mothers' Quest for Job Protection: Building the Nest or Breaking the Glass Ceiling? Evidence Using Spanish Longitudinal Data." IE Business School Working Paper.

Gallie D., M. White, Y. Cheng and M. Tomlinson. 1998. "Restructuring the Employment Relationship." Oxford University Press, 1998.

Gornick, J., and J. Jacobs. 1996. "A Cross-National Analysis of The Wages of Part-Time Workers: Evidence from the United States, the United Kingdom, Canada, and Australia." *Work, Employment and Society*, 10/1: 1-27.

Gregory, M., and S. Connolly. 2008. "The Price of Reconciliation: Part-time Work, Families and Women's Satisfaction." *Economic Journal*, Vol. 118 (february), pp. F1-F7.

Galdón-Sánchez, and M. Güell. 2003. "Dismissal Conflicts and Unemployment." European Economic Review, 47 (2): 127-139.

Güell, M. and B. Petrongolo. 2007. "How Binding are Legal Limits?: Transitions from Temporary to Permanent Work in Spain", *Labour Economics* 14, 153-183.

Gutiérrez-Doménech M. 2005. "Employment Transitions After Motherhood in Spain", *Labour* Special Issue, 19(0), 123-148.

Hakim, C. 1997. "A Sociological Perspective on Part-Time Work," in Hans-Peter Blossfeld, and Catherine Hakim (eds.), *In Between Equalization and Marginalization:Women Working Part-Time in Europe and The United States of America*. Oxford: Oxford University Press, 22-70.

Hardoy I. and P. Schøne, 2004. "The Part-Time Wage Gap: How Large Is it Really?" *British Journal of Industrial Relations* 44:2 June 2006, pp. 263–282.

Hernanz, V. 2002. "El Trabajo Temporal y la Segmentación: Un Estudio de las Transiciones Laborales." Unpublished PhD Dissertation, Universidad de Alcalá de Henares, Madrid.

Hirsch, B. 2005. "Why Do Part-Time Workers Earn Less? The Role of Worker and Job Skills." *Industrial and Labor Relations Review* 58(July):525–51.

Hotchkiss, J. 1991. "The Definition of Part-Time Employment: A Switching Regression Model with Unknown Sample Selection." *International Economic Review*, Vol. 32, No. 4 (November): 899–917.

Hu, Y. and K. Tijdens, K. 2003. "Choices for Part-Time Jobs and the Impacts on the Wage Differentials. A Comparative Study for Great Britain and the Netherlands", An Integrated Research Infrastructure in the Socio-Economic Sciences at CEPS/INSTEAD, *IRISS Working Paper*, No. 2003-05, 36p.

Jepsen M., 2001. "Some Evidence on the Price for Working Part-Time", PhD in Economics,Université Libre de Bruxelles.

Jepsen, M., O'Dorchai, S., Plasman, R. and Rycx, F. 2005. "The Wage Penalty Induced by Part-Time Work: The Case of Belgium", *Brussels Economic Review*, Vol. 48, No. 1/2, pp. 73-94.

Jimeno, J.F. and L. Toharia. 1993. "The Effects of Fixed-Term Employment on Wages: Theory and Evidence from Spain," *Investigaciones Económicas*, 17(3): 475-494.

Jimeno, J.F., and A.C. Ortega. 2003. "Veinticinco Años de Mercado de Trabajo en España." *Economía Industrial*, num. 349-350.

Jones, E., and J. Long. 1979. "Part-Week Work and Human Capital Investment by Married Women." *Journal of Human Resources*, vol. 14 (Autumn), pp. 563–78.

Kugler A., J.F. Jimeno, V. Hernanz. 2005. "Employment Consequences of Restrictive Permanent Contracts: Evidence from Spanish Labor Market Reforms.", IZA working papers 657.

López Bóo F., L. Madrigal, and C. Pagés. 2009. "Part-Time Work, Gender and Job Satisfaction: Evidence from a Developing Country." Inter-American Development Bank, research department working paper.

Manning, A. and B. Petrongolo. 2008. "The Part-Time Pay Penalty for Women in Britain." *The Economic Journal*, 118: F28–51.

Mocan, H. N., and E. Tekin. 2003. "Nonprofit Sector and Part-Time Work: An Analysis of Employer-Employee Matched Data of Child Care Workers." *Review of Economics and Statistics*, Vol. 85, No. 1 (*February*): pp. 38–50.

Mumford, K. and P. Smith. 2007. "The Gender Earnings Gap in Britain: Including the Workplace." Manchester School, 75: 653–72.

Muñoz de Bustillo Llorente R., E. Fernández Macías, and J.I. Antón Pérez. 2008. "El Trabajo a Tiempo Parcial en España en el Contexto de la Unión Europea." Ministerio de Trabajo e Immigración. Colección Informes y Estudies, Serie Empleo, n. 36.

OECD. 2002. "Women at Work: Who Are They and How Are They Faring?" *Employment Outlook*, 63-125.

O'Connell P., and V. Gash. 2003. "The Effects of Working Time, Segmentation and Labour Market Mobility on Wages and Pensions in Ireland." *British Journal of Industrial Relations*, 41:1 (March): 71-95.

O'Dorchai, S., R. Plasman, and F. Rycx. 2007. "The Part-Time Wage Penalty in European Countries: How Large Is It for Men?" IZA Discusión Paper, 2591.

Pagán Rodríguez R. 2007. "Diferencias Salariales entre el Empleo a Tiempo Completo y el Parcial." *Revista de Economia Aplicada*, vol. XV, 43.

Pissarides, C., P. Garibaldi, C. Olivetti, B. Petrongolo, and E. Wasmer. 2005. "Women in the labor force: how well is Europe doing?" In: Boeri, Tito and Del Boca, Daniela and Pissarides, Christopher, (eds.) *Women at work: an economic perspective*. Oxford University Press, Oxford, UK, pp. 9-120. ISBN 0199281882

Rodgers, J. 2004. "Hourly Wages of Full-Time and Part-Time Employees in Australia." *Australian Journal of Labour Economics* 7(June):231–54.

Segura, J. F. Durán, L. Toharia and S. Bentolila. 1991. "Análisis de la Contratación Temporal en Espanta." Centro de Publicaciones, Ministerio de Trabajo y Seguridad Social

Simpson, W. 1986. "Analysis of Part-Time Pay in Canada." *Canadian Journal of Economics* 19(November):798–807.

Van Praag, B.M.S., and A. Ferrer-i-Carbonell. 2004. *Happiness Quantified*. Oxford, United Kingdom: Oxford University Press.

Wolf, E. 2002. "Lower Wage Rates for Fewer Hours? A Simultaneous Wage-Hours Model for Germany." *Labour Economics*, 9: 643–63.

Table 1
Sample Sizes
Women Strongly Attached to the Labor Force, 1985-2006 CSWH
(24 to 45 years old)
(In parenthesis, as a % of the total number of individuals in each category)

	Whole sample		Permanent contract at time t-1		Fixed-term contract at time t-1	
Number of individuals	76,025		54,726		50,015	
Of which only work FT	59,556	(78.34%)	44,504	(81.13%)	35,947	(71.87%)
Of which switch to PT	16,469	(21.66%)	10,222	(18.68%)	14,068	(28.13%)
Of which return to FT	8,153	(49.51%)	4,968	(48.60%)	7,549	(53.34%)

Table 2
Descriptive Statistics
Women Strongly Attached to the Labor Force, 2006 CSWH
(24 to 45 years old)

	Permanent contract		**Fixed-term contract**	
	FT worker	**PT worker**	**FT worker**	**PT worker**
Change in log real hourly	.034	.000†	.035	.012†
earnings	(.165)	(.252)	(.290)	(.337)
Log of current hourly	6.883	6.729†	6.788	6.646†
earnings in cents of €	(.408)	(.389)	(.403)	(.412)
Age 24 to 29 years old	.331	.227†	.380	.272†
(percent)	(.470)	(.419)	(.485)	(.445)
Age 30 to 34 years old	.381	.423†	.360	.378†
(percent)	(.485)	(.494)	(.480)	(.485)
Age 35 to 39 years old	.160	.221†	.150	.197†
(percent)	(.367)	(.415)	(.357)	(.397)
Age 40 to 45 years old	.127	.127	.107	.151†
(percent)	(.333)	(.334)	(.310)	(.358)
Cohabiting (percent)	.763	.800†	.760	.765†
	(.424)	(.399)	(.426)	(.423)
Without children (percent)	.642	.390†	.661	.474†
	(.479)	(.487)	(.473)	(.499)
With children 0 to 2 years	.146	.249†	.106	.155†
old (percent)	(.353)	(.432)	(.308)	(.362)
With children 3 years old	.026	.073†	.026	.058†
(percent)	(.162)	(.261)	(.160)	(.253)
With children 4 to 6 years	.051	.110†	.055	.095†
old (percent)	(.221)	(.313)	(.228)	(.294)
With children older than 6	.132	.175†	.149	.215†
years old (percent)	(.339)	(.380)	(.356)	(.411)
High-school dropout	.307	.422†	.362	.468†
(percent)	(.461)	(.494)	(.480)	(.499)
High-school graduate	.398	.389†	.295	.303†
(percent)	(.489)	(.487)	(.456)	(.459)
College graduate or above	.294	.187†	.342	.228†
(percent)	(.455)	(.390)	(.474)	(.419)
Experience in PT	.295	7.867†	.347	4.599†
employment (in years)	(1.553)	(4.519)	(1.256)	(2.952)
Experience in FT	8.133	.180†	4.476	.049†
employment (in years)	(4.742)	(.899)	(3.003)	(.511)
Public servant (percent)	.044	.002†	.119	.020†
	(.205)	(.053)	(.324)	(.142)
Firm tenure (in years)	5.089	4.523†	1.805	1.507†
	(4.532)	(4.278)	(2.021	(1.726)
Firm size (number of	545.165	514.845†	725.393	394.063†
workers)	(1729.016)	(2043.409)	(1925.107)	(1410.323)
White Collar (percent)	.231	.120†	.261	.137†
	(.421)	(.325)	(.439)	(.344)
Number of individuals	32,343	3,110	15,637	1,832

Note.- The numbers in parenthesis are standard deviations. All hourly wages are deflated by the gross domestic product (GDP) deflator (base year = 2006). † PT mean significantly different from FT mean at the 90% confidence level.

Table 3
Estimation of the Part-Time Pay Penalty, Different methodologies
Dependent variable: Ln(real hourly wage)

	Unadjusted	Basic controls	Basic controls + employer characteristics	Basic controls + employer characteristics + industry	Basic controls + employer characteristics + occupation	Basic controls + employer characteristics + change occupation or employer
Panel A: Whole sample (number of observations : 591,063)						
1. Pooled OLS	-.109*** (.003)	-.033*** (.003)	-.025*** (.003)	+.005* (.003)	+.027*** (.002)	+.028*** (.002)
2. Fixed-effects	+.062*** (.001)	+.070*** (.001)	+.075*** (.001)	+.079*** (.001)	+.085*** (.001)	+.085*** (.001)
Using imputed effective hours to estimate LHS variable						
3. Pooled OLS	-.376*** (.003)	-.298*** (.003)	-.290*** (.003)	-.259*** (.003)	-.236*** (.003)	-.234*** (.003)
4. Fixed-effects	-.195*** (.001)	-.187*** (.001)	-.182*** (.001)	-.176*** (.001)	-.171*** (.001)	-.171*** (.001)
Panel B: With fixed-term contracts (number of observations: 194,218)						
1. Pooled OLS	-.057*** (.004)	-.023*** (.004)	-.005 (.004)	+.016*** (.003)	+.031*** (.003)	+.032*** (.003)
2. Fixed-effects	+.049*** (.002)	+.055*** (.002)	+.062*** (.002)	+.068*** (.002)	+.069*** (.002)	+.070*** (.002)
3. FE-2SLS	+.038*** (.003)	+.069*** (.003)	+.072*** (.003)	+.074*** (.003)	+.082*** (.003)	+.081*** (.003)
Using imputed effective hours to estimate LHS variable						
4. Pooled OLS	-.352*** (.004)	-.319*** (.004)	-.300*** (.004)	-.277*** (.004)	-.262*** (.004)	-.261*** (.004)
5. Fixed-effects	-.237*** (.003)	-.230*** (.003)	-.223*** (.003)	-.216*** (.003)	-.215*** (.003)	-.213*** (.003)
6. FE-2SLS	-.316*** (.004)	-.285*** (.004)	-.281*** (.004)	-.277*** (.004)	-.268*** (.004)	-.268*** (.004)
Panel C: With permanent contracts (number of observations: 396,845)						
1. Pooled OLS	-.122*** (.005)	-.037*** (.004)	-.033*** (.004)	+.005 (.004)	+.034*** (.004)	+.036*** (.004)
2. Fixed-effects	+.111*** (.002)	+.119*** (.002)	+.120*** (.002)	+.121*** (.002)	+.127*** (.002)	+.128*** (.002)
3. FE-2SLS	+.089*** (.003)	+.079*** (.003)	+.084*** (.003)	+.092*** (.003)	+.094*** (.003)	+.094*** (.003)
Using imputed effective hours to estimate LHS variable						
3. Pooled OLS	-.360*** (.005)	-.273*** (.005)	-.269*** (.005)	-.229*** (.004)	-.200*** (.004)	-.197*** (.004)
4. Fixed-effects	-.095*** (.003)	-.087*** (.003)	-.086*** (.003)	-.085*** (.003)	-.079*** (.002)	-.077*** (.002)
6. FE-2SLS	-.080*** (.003)	-.091*** (.003)	-.085*** (.003)	-.078*** (.003)	-.076*** (.003)	-.076*** (.003)

*** Significant at 1% level. Imputed hours come from a regression of effectively worked hours against contractual hours, age, education, two-digit industry and occupation using the Spanish Time Use Survey Dataset. A negative number indicates a penalty for part-time workers. Each set of regressions has the following controls: UNADJUSTED – year and province dummies; WORKERS CONTROLS – age, education, nationality, province of residence, experience and quadratic of experience, tenure, with children less than 3 and bigger than 6 dummies, and immigrant status; EMPLOYER CHARACTERISTICS – number of workers, public sector dummy; INDUSTRY & OCCUPATION – two-digits industry dummies and ten occupation categories dummies; CHANGE OF EMPLOYER – a dummy indicating if the individual's employer at year t is different from t-1.

Figure 1
Full-time Job Preferences
2005 Social Survey Programme (ISSP)—Work Orientations Module

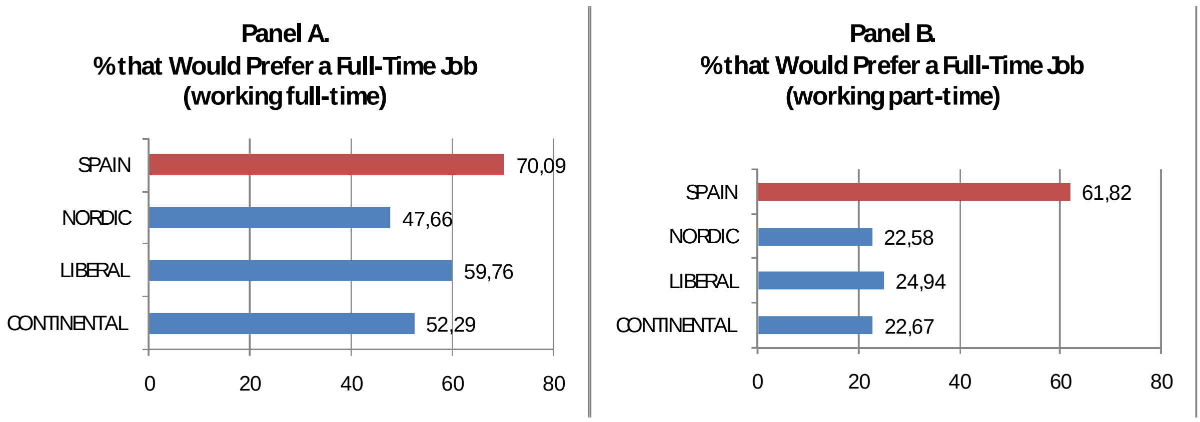
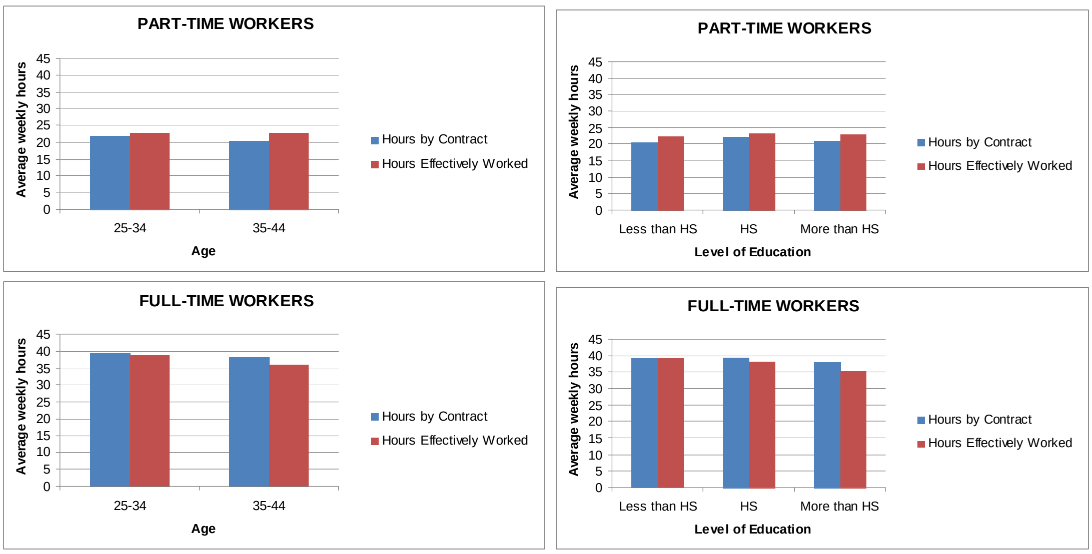


Figure 2
Contractual and Effective Hours for PT and FT Workers
2003 Time Use Survey



Note: The effective-contractual hours gap for PT workers is significantly different from the gap for FT workers at the 1% level across all age and education groups. The effective-contractual hours gap is always positive for PT workers and negative for FT workers and the difference between the two groups of workers grows with age and the level of education: is -1.34 hours when age is between 25-34, -4.83 hours at ages 35 to 45, -1.85 hours for individuals with less than high school completed, -2.31 hours for those with a high school degree and -5.15 hours for college graduates.

Table 4

Individual Attrition in Wage Change
(In parenthesis, as a % of the total number of individuals in each category)

	Whole sample	Permanent contract at time t-1	Fixed-term contract at time t-1	Not working at time t-1
Number of individuals*	962 (1,27%)	91 (0.17%)	537 (1.07%)	334
Of which only work in FT	324 (0.54%)	0 (0.00%)	0 (0.00%)	324
Of which switch to PT	638 (3.87%)	91 (1.83%)	537 (3.82%)	10
And go to non-employment	305	43	276	6
And return to FT within one period	333	48	261	4

*Number of individuals we drop in the hourly wage change specification because we do not observe them for at least two consecutive years in a given employment status

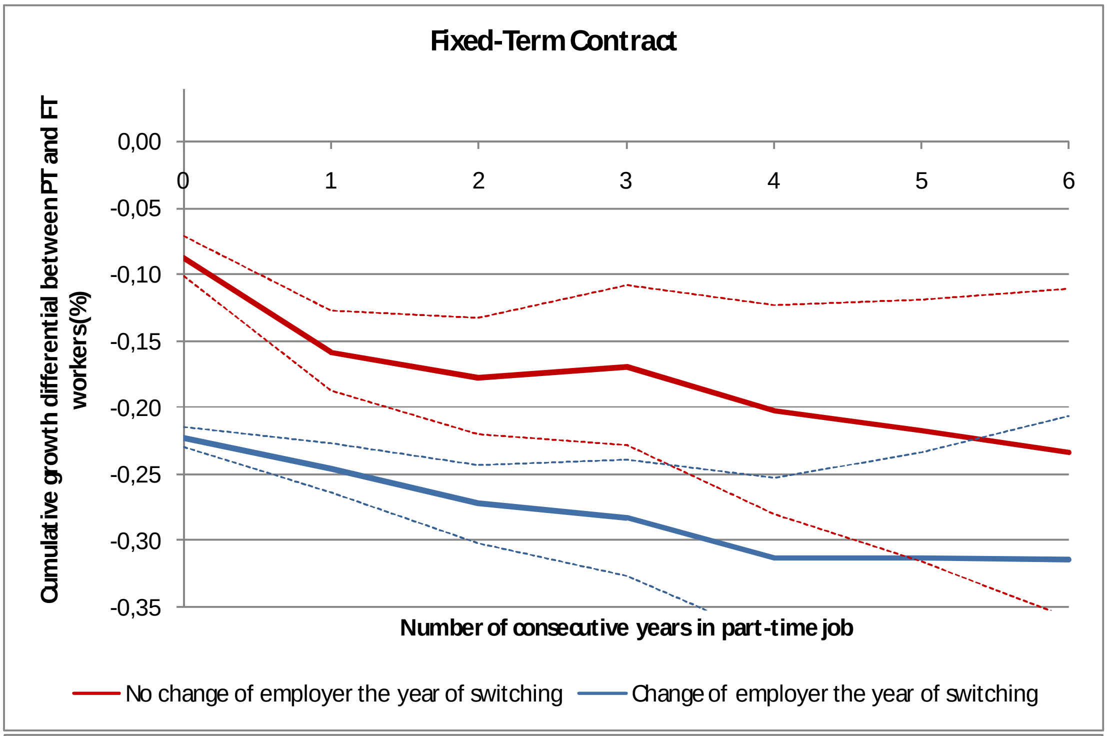
Table 5

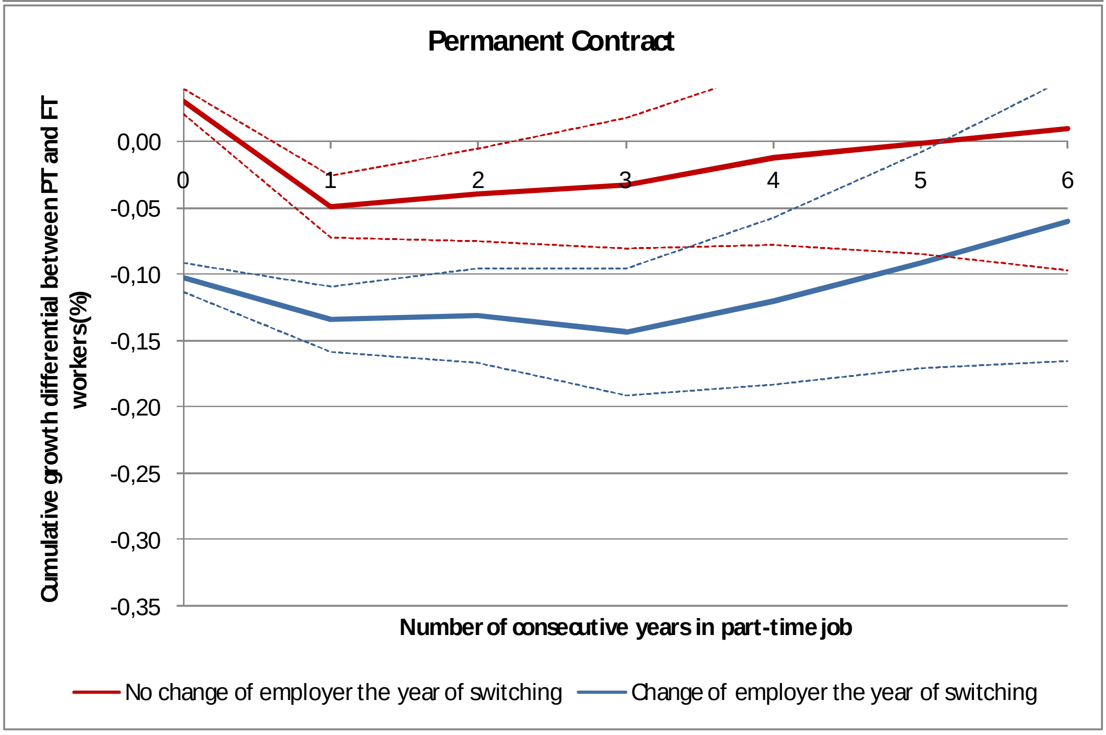
The Part-time Wage Growth Penalty
Women 24 to 45 years old

	Unadjusted	Pooled OLS (Worker controls)	Fixed-effects (Worker controls)	FE-2SLS (Worker controls)	Pooled OLS (Worker controls + employer characteristics)	Fixed-effects (Worker controls + employer characteristics)	FE-2SLS (Worker controls + employer characteristics)	Pooled OLS (Worker controls + employer characteristics + industry)	Fixed-effects (Worker controls + employer characteristics+ industry)	FE-2SLS (Worker controls + employer characteristics+ industry)
	(1)	(2)	(3)		(4)	(5)		(6)	(7)	
Panel A. Without Contract Type										
	-.033*** (.001)	-.032*** (.002)	-.038*** (.004)		-.032*** (.002)	-.037*** (.004)		-.029*** (.002)	-.044*** (.004)	
Panel B. By Contract Type										
Fixed-term contract at time t	-.032*** (.002)	-.027*** (.003)	-.039*** (.005)	-.045*** (.008)	-.027*** (.003)	-.037*** (.005)	-.043*** (.008)	-.024*** (.003)	-.046*** (.005)	-.041*** (.008)
Permanent contract at time t	-.035*** (.002)	-.038***✓ (.003)	-.029***✓ (.005)	-.021***✓ (.008)	-.037***✓ (.003)	-.031*** (.005)	-.022***✓ (.008)	-.035***✓ (.004)	-.035***✓ (.005)	-.020***✓ (.008)
Sample size	468,532	468,532	468,532	386,455	468,532	468,532	386,455	468,532	468,532	386,455
(# individuals)	75,063	75,063	75,063	74,775	75,063	75,063	74,775	75,063	75,063	74,775

Hourly earnings have been deflated using 2006 deflator and calculated with contractual hours. ***, **, * indicate significance at the 1%, 5%, 10% level (two-sided test). ✓ indicates that the difference of the estimated effects by type of contract is significant at the 10% level. UNADJUSTED: regressions control for year and province dummies. WORKER CONTROLS: part-time status, the number of consecutive years in part-time work, the type of contract at t-1, age, immigrant status, year, province, education, level of experience in part-time and full-time jobs, the change in the level of experience, tenure, number of children, with children less than 3 and bigger than 6 dummies, and cohabiting status; EMPLOYER CHARACTERISTICS: industry, occupation, number of workers and public sector.

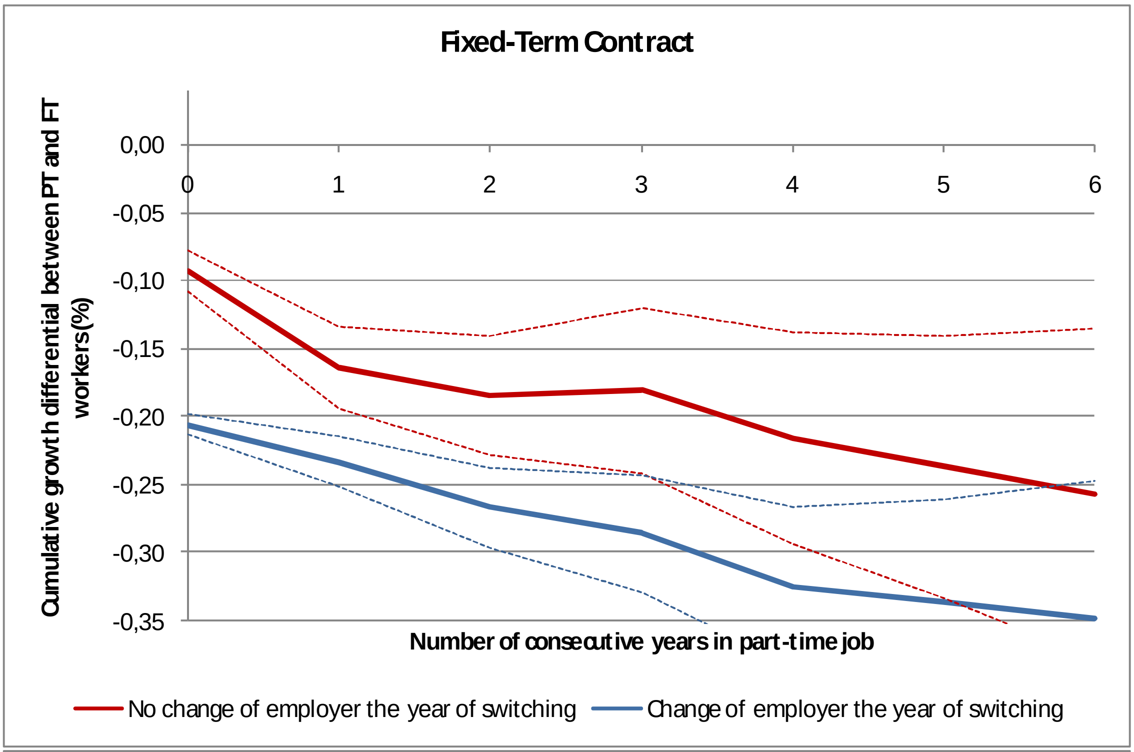
Figure 3. The Cumulative Part-Time Penalty by Years in Part-Time Work
PANEL A. Controlling for worker characteristics

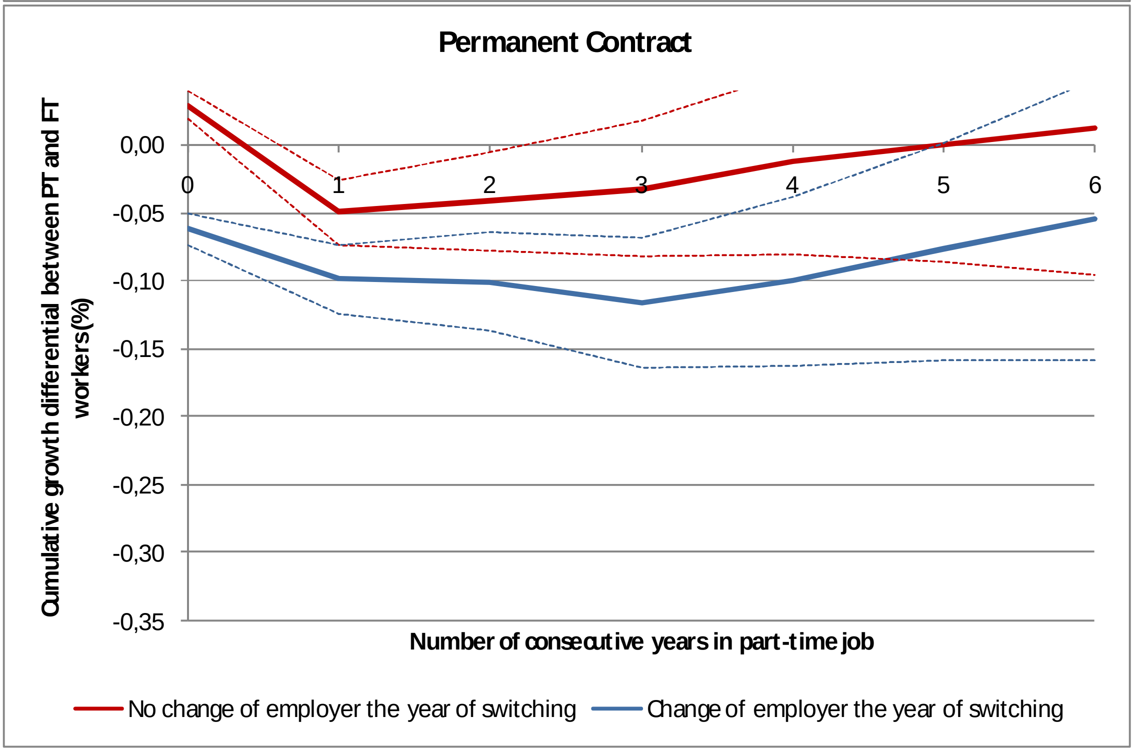




Note: Women 24 to 45 years old strongly attached to the labor market. Results come from a first-difference specification with individual fixed effects, where the dependent variable is the one-year change in real hourly wages and controls are: part-time status, the number of consecutive years in part-time work, the type of contract at t-1, age, year, province, education, level of experience in part-time and full-time jobs, the change in the level of experience, tenure, number of children, with children less than 3 and bigger than 6 dummies, cohabiting status, and immigrant status. Dashed lines represent the 5% confidence intervals of the part-time effect. The value for the first year in part-time job (switchers) comes from a regression where hours of work are effectively worked hours imputed using the Spanish Time Use Survey.

Figure 3. The Cumulative Part-Time Penalty by Years in Part-Time Work
PANEL B. Controlling for worker, employer and job characteristics, and change of occupation and employer





Note: Women 24 to 45 years old strongly attached to the labor market. Results come from a first-difference specification with individual fixed effects, where the dependent variable is the one-year change in real hourly wages and controls are: worker characteristics + firm and job characteristics + change of occupation and change of employer. Dashed lines represent the 5% confidence intervals of the part-time effect. The value for the first year in part-time job (switchers) comes from a regression where hours of work are effectively worked hours imputed using the Spanish Time Use Survey.

Table 6. A

The Part-time Wage Growth Penalty for Workers with Fixed-Term Contract at Time *t-1*, by Experience in Part-time Work
Women 24 to 45 years old

	Fixed-effects (Worker controls)		**Fixed-effects (Worker controls + employer characteristics + change occupation or employer)**	
	Marginal effects	**Cumulative effects**	**Marginal effects**	**Cumulative effects**
Number of consecutive years in part-time work if no change of employer the year of the switching				
At least 1 year	-.071***	-.071***	-.071***	-.071***
	(.007)	(.007)	(.007)	(.007)
At least 2 years	-.020**	-.090***	-.022**	-.092***
	(.010)	(.014)	(.010)	(.014)
At least 3 years	.008	-.082***	.004	-.088***
	(.014)	(.023)	(.014)	(.022)
At least 4 years	-.035*	-.115***	-.038**	-.123***
	(.019)	(.032)	(.018)	(.032)
At least 5 years	-.018	-.131***	-.023	-.144***
	(.020)	(.042)	(.020)	(.041)
At least 6 years	-.018	-.147***	-.023	-.164***
	(.020)	(.055)	(.020)	(.053)
Number of consecutive years in part-time work if change of employer the year of the switching				
At least 1 year	-.023***	-.023***	-.028***	-.028***
	(.005)	(.005)	(.005)	(.005)
At least 2 years	-.028***	-.050***	-.033***	-.061***
	(.008)	(.011)	(.008)	(.011)
At least 3 years	-.011	-.061***	-.021*	-.080***
	(.011)	(.018)	(.011)	(.018)
At least 4 years	-.032**	-.091***	-.042***	-.119***
	(.016)	(.027)	(.016)	(.026)
At least 5 years	-.000	-.091***	-.013	-.131***
	(.019)	(.036)	(.018)	(.035)
At least 6 years	-.000	-.092*	-.013	-.143***
	(.019)	(.051)	(.018)	(.047)
Sample size	138,234	138,234	138,234	138,234
(# individuals)	48,217	48,217	48,217	48,217

Hourly earnings have been deflated using 2006 deflator and estimated with contractual hours. ***, **, * indicate significance at the 1%, 5%, 10% level (two-sided test). Women 24 to 45 years old strongly attached to the labor market. Results come from a first-difference specification with individual fixed effects, where the dependent variable is the one-year change in real hourly wages and controls are: WORKER CONTROLS: part-time status, the number of consecutive years in part-time work, the type of contract at t-1, age, year, province, education, level of experience in part-time and full-time jobs, the change in the level of experience, tenure, number of children, with children less than 3 and bigger than 6 dummies, cohabiting status, and immigrant status; EMPLOYER CHARACTERISTICS: industry, occupation, number of workers and public sector.

Table 6. B

The Part-time Wage Growth Penalty for Workers with Permanent Contract at time *t-1*, by Experience in Part-time Work
Women 24 to 45 years old

	Fixed-effects (Worker controls)		Fixed-effects (Worker controls + employer characteristics + change occupation or employer)	
	Marginal effects	**Cumulative effects**	**Marginal effects**	**Cumulative effects**
Number of consecutive years in part-time work if no change of employer the year of the switching				
At least 1 year	-.079***	-.079***	-.079***	-.079***
	(.006)	(.006)	(.006)	(.006)
At least 2 years	.010	-.070***	.009	-.071***
	(.009)	(.013)	(.009)	(.012)
At least 3 years	.008	-.062***	.009	-.062***
	(.011)	(.020)	(.010)	(.020)
At least 4 years	.020	-.042	.021	-.042
	(.014)	(.029)	(.013)	(.028)
At least 5 years	.011	-.031	.013	-.029
	(.014)	(.038)	(.014)	(.038)
At least 6 years	.011	-.020	.013	-.017
	(.014)	(.050)	(.014)	(.049)
Number of consecutive years in part-time work if change of employer the year of the switching				
At least 1 year	-.031***	-.031***	-.037***	-.037***
	(.007)	(.007)	(.006)	(.006)
At least 2 years	.002	-.029***	-.001	-.039***
	(.008)	(.012)	(.008)	(.012)
At least 3 years	-.011	-.041**	-.015	-.054***
	(.009)	(.018)	(.009)	(.018)
At least 4 years	.024**	-.017	.017	-.038
	(.013)	(.026)	(.012)	(.026)
At least 5 years	.030**	.012	.023*	-.015
	(.014)	(.036)	(.014)	(.034)
At least 6 years	.030**	.043	.023*	.007
	(.014)	(.049)	(.014)	(.047)
Sample size	330,298	330,298	330,298	330,298
(# individuals)	54,093	54,093	54,093	54,093

Hourly earnings have been deflated using 2006 deflator and estimated with contractual hours. ***, **, * indicate significance at the 1%, 5%, 10% level (two-sided test). Women 24 to 45 years old strongly attached to the labor market. Results come from a first-difference specification with individual fixed effects, where the dependent variable is the one-year change in real hourly wages and controls are: WORKER CONTROLS: part-time status, the number of consecutive years in part-time work, the type of contract at t-1, age, year, province, education, level of experience in part-time and full-time jobs, the change in the level of experience, tenure, number of children, with children less than 3 and bigger than 6 dummies, cohabiting status, and immigrant status; EMPLOYER CHARACTERISTICS: industry, occupation, number of workers and public sector.