The Part-Time Pay Penalty in a Segmented Labor Market

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Part-time work has recently received renewed attention in Spain as its Prime Minister, José Luís Rodríguez Zapatero, has offered to use part-time (PT) work as a measure to increase labor market flexibility in the midst of the current economic crisis. However, little is known in Spain on whether PT employment, which is mainly female employment, offers the opportunity for career progression or the risk of career stagnation in a country with a striking segmentation of its labor market. This paper uses a rich longitudinal Spanish data set to investigate the PT / full-time (FT) wage growth differential between prime-aged women strongly attached to the labor force, distinguishing by their type of contract: permanent or fixed-term. The empirical strategy adjusts for observed and unobserved heterogeneity and addresses differential measurement errors by PT status problems. The paper also exploits a legislative change to address sample selection into type of contract. We find evidence of a PT penalty in Spain and that this penalty is greater for workers with fixed-term contracts. After accounting for workers' observable and unobservable characteristics, 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 contract experience 3.9 log points lower hourly wage growth per year then their FT counterparts. These estimates are similar to the ones we would obtain if we were to control for employer characteristics. The reason for this is that the greater loss caused by the occupational downgrading and employer turnover of PT workers seems to be cancelled out by workers' upgrading to better paid industries (or, alternatively, coming from relatively lower paid industries to start with). Finally, we have found that while women with permanent contracts negatively self-select into PT employment, the opposite occurs for women with fixed-term contracts. We believe that the positive self-selection into PT fixed-term jobs is due to the dual nature of the Spanish labor market and the marginalization of fixed-term contracts. The paper concludes with some policy implications.

Key words: Fixed-term and permanent contract, wage growth, prime-aged women, fixed-effects estimator.
JEL classification: H42, H52, I20, J13, J21, J22, C23 C25, C33 C35 J16 J62 *CHECK!!*

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Introduction

Part-time work has recently received renewed attention in Spain as its Prime Minister, José Luís Rodríguez Zapatero, has included part-time (PT) work as one of its six key new measures to fight unemployment, which, at 16 %, is the highest in the EU-27. The underlying economic argument for using PT work is that an increase in PT work will add flexibility in a labor market with a high share of unemployment and stringent employment protection legislation (see, Jimeno Serrano and Ortega Massé, 2003 for a thorough description of the Spanish labor market). Supply and demand factors dovetail here, in that job shortages encourage workers who want full-time (FT) employment to accept PT jobs (Smith, Fagan and Rubery, 1998). Even though PT work may appear an interesting tool for policy makers to increase labor market flexibility in the midst of the current economic crisis, the usefulness of PT work in achieving economic and individuals' professional development is and has been a prominent and heated debate among academics, policy makers and practitioners in Europe and elsewhere (Bardasi and Gornick, 2000; Buddelmeyer *et al.*, 2005).

Within this literature, many researchers have increasingly become interested in analyzing the pay differential between PT female workers and their FT counterparts—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, 2008, among others. Finding out whether there is a PT penalty is important 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). Unfortunately, the PT/FT wage differential estimates are all over the board. 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; several identification problems within this literature are difficult to overcome. First, there is unobserved heterogeneity as PT and FT workers might differ in their tastes and expectations towards paid work. Second, most studies rely on survey data, which may lead to important measurement errors in key variables—such as hours, wages and PT status (OECD, 2002; Pissarides *et al.*, 2003; Naci Mocan and Erdal Tekin,2003; Buligescu *et al.*, 2008).[1] Third, there is the danger of reverse causation here: maybe it is low wages that 'cause' PT work, not PT work that 'causes' low wages (Aaronson and French, 2004; and Manning and Petrongolo, 2008, among others).

In addition, one of the major shortcomings of the European literature is the small sample size of individuals who work PT. This problem is particularly concerning when longitudinal data are available and fixed-effects 'within' estimators are used to address the unobserved heterogeneity problem by identifying the PT effect on wages through those workers who switch status. For instance, Connolly and Gregory (2008) report that in any year, fewer than 9% of those in FT work switch to PT work in the UK. For Spain, Buddelmeyer et al. (2005) find that in any year, 1.52% (1.79%) of those women in FT (PT) work switch to PT (FT) work. Perhaps, this explains why there has been so little attention in the literature on the differential effect of PT on wages for different population subgroups, such as 'insiders' and 'outsiders' in a segmented labor market.[2] Clearly the underlying forces behind PT work may differ drastically in the primary labor market, which consists of well paid and secure jobs with high-productivity growth, than in the secondary labor market, where jobs are poorly paid and of a precarious

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[1] There are some exceptions. For instance, Connolly and Gregory (2008) use an administrative employers' panel dataset, and Naci Mocan and Erdal Tekin (2003) use an employer-employee matched dataset.

[2] To our knowledge, Naci Mocan and Erdal Tekin (2003) analyze the nonprofit sector dimension, Mumford and Smith (2008) analyze the gender dimension, and O'Connel and Gash (2003), Hughes and Nolan (1997) and Tilly (1996) –CHECK!--focus on the segmentation issue.

nature. For instance, Tilly (1996) highlighted the coexistence in the US labor market of 'retention' PT jobs, which were used in the primary sector to retain valued and skilled employees, and PT jobs in the secondary market, which ended up being low pay and low security jobs.

The central point of this article is to examine the PT / FT wage growth differential between workers with permanent contracts and those with fixed-term contracts. Our focus is on adult women between 24 and 45 years old and strongly attached to the labor force. We control for workers' socio-demographic characteristics (including presence and age of children in the household), employer's characteristics, and workers' previous employment history. We account for worker heterogeneity by exploiting 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.[3] As our data comes for Social Security records, we use contractual monthly wages and hours to calculate the hourly wages. Despite the superiority of our data (compared to worker survey data), we believe that measurement error in contractual hours may still raise some concern if PT workers consistently work a greater number of hours in excess of contractual hours relative to their FT counterparts. To address this problem, 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 **(this represents about 5% of our sample. However, notice that for X% of our sample of switchers from FT to PT (or vice-versa) we observe them for subsequent years after their PT status switch)**. Assuming that differential measurement error by PT status is an individual-employment-status fixed effect, our approach circumvents the problem. We do not model selection into PT employment. Therefore, we do not strictly identify the causal impact on wages of working PT.[4] 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. Finally, we try to address selection into type of contract by exploiting a legislative change that took place in Spain in the late-1990s, and that increased workers' job protection for both types of workers—with the law change, workers who wanted to reduce their work week could do so without the risk of being fired. The law implied clear protection rules for *all* workers who exerted their rights of flexible (and reduced) working hours in 1999: it declared a layoff (or non-renovation of contract) invalid if the worker had previously asked for work-week reduction due to family or other family dependent responsibilities.[5]

Spain is a suitable case to investigate this issue because of the striking segmentation of the Spanish labor market (see for instance, Adam, 1996; Guell and Petrongolo, 2007; OTHERS). 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 the highest in Europe. In addition, an important dual labor market developed after the 1984 legislation changes, resulting in the economy with

[3] Although several papers have used longitudinal data to estimate the PT pay penalty (Blank, 1998; Hirsch, 2005; Booth and Wood, 2005; 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, 2008; OTHERS?).

[4] A different but related problem would be to model participation into employment. We do not model participation into employment because our dataset does not allow us to do so, as it is made of survivors (individuals not affiliated with the Social Security in 2006 are excluded from the sample even if they had a relationship with the Social Security in the past). This implies that the CSWH is not useful to analyze transition from employment to inactivity. We do not think this is of major concern since we are focusing our analysis on women who are strongly attached to the labor market (as in Connolly and Gregory, 2008).

[5] In addition, the law extended the right to reduce the weekly work load to parents of children aged 8 years old and younger.

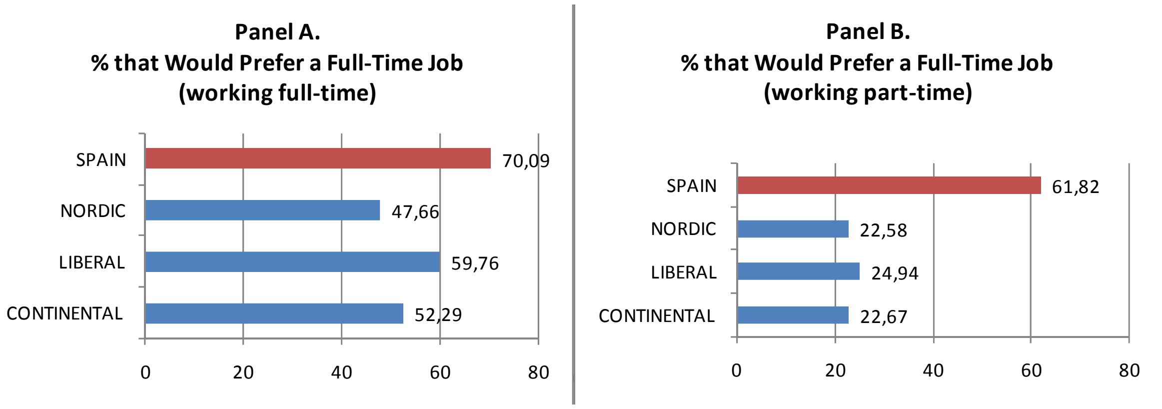
the highest rate of fixed-term contracts in Europe for the last two decades (over one third of all contracts are fixed-term contracts).

We find evidence of a PT penalty that is greater for workers with fixed-term contracts. After accounting for workers' observable and unobservable characteristics, 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 contract experience 3.9 log points lower hourly wage growth per year then their FT counterparts. These estimates are considerably large and concerning as their size ranges between one-and-a-half and twice the size of the estimated college premium on wage growth. We also find that these estimates are similar to the ones we would obtain if we were to control for employer characteristics. The reason for this is that the greater loss caused by the occupational downgrading and employer turnover that a switch to PT employment may imply seems to be cancelled out by workers' upgrading to better paid industries (or, alternatively, coming from relatively lower paid industries to start with). Finally, we have found that while women in permanent contracts negatively self-select into PT employment, the opposite occurs for women in fixed-term contracts. We believe that the positive self-selection into PT fixed-term jobs is due to the dual nature of the Spanish labor market and the marginalization of fixed-term contracts.

This paper is closer to Connolly and Gregory (hereafter, CG, 2008) in that it estimates the PT pay penalty through various specification of a human capital earnings equation using a long unbalanced panel and a fixed effects `within´ estimator approach. They find that the PT pay penalty in the UK can be fully explained through the following four channels: (1) Lower human capital accumulation during PT work; (2) Lower returns to PT experience; (3) Occupational downgrading; and (4) Change of employer. Methodologically, our work differs from CG study in that we estimate the PT pay penalty through various specifications of a human capital earnings *change* equation. In addition, given that our data contains information on children in the household, we are able to distinguish between the PT pay penalty and the `motherhood pay gap´ (C&G cannot distinguish between mothers and non-mothers). Finally, our analysis also distinguishes between insiders (those women with permanent contracts) and outsiders (those with fixed-term contracts).

To our knowledge two other studies have estimate the PT / FT wage differential in Spain using a cross-sectional approach with data from the European Community Household Panel Survey (Pissarides et al., 2003; and Pagán Rodríguez, 2007). Both studies find evidence of an unadjusted hourly wage penalty associated with being a female PT worker (of between 10% and 16%). In both papers, the PT penalty becomes a PT *premium* after adjusting for observable characteristics (and self-selection in the case of Pagán Rodríguez, 2007). 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. Given that Pagán Rodríguez does not correct for measurement error, and that neither of these studies corrects for unobserved quality differences between PT and FT workers, we find these earlier findings inconclusive, especially in the light of the anecdotal evidence suggesting that PT jobs in Spain are mainly involuntary in nature (see Figure 1 and Section III).

Figure 1. Full-time Job Preferences



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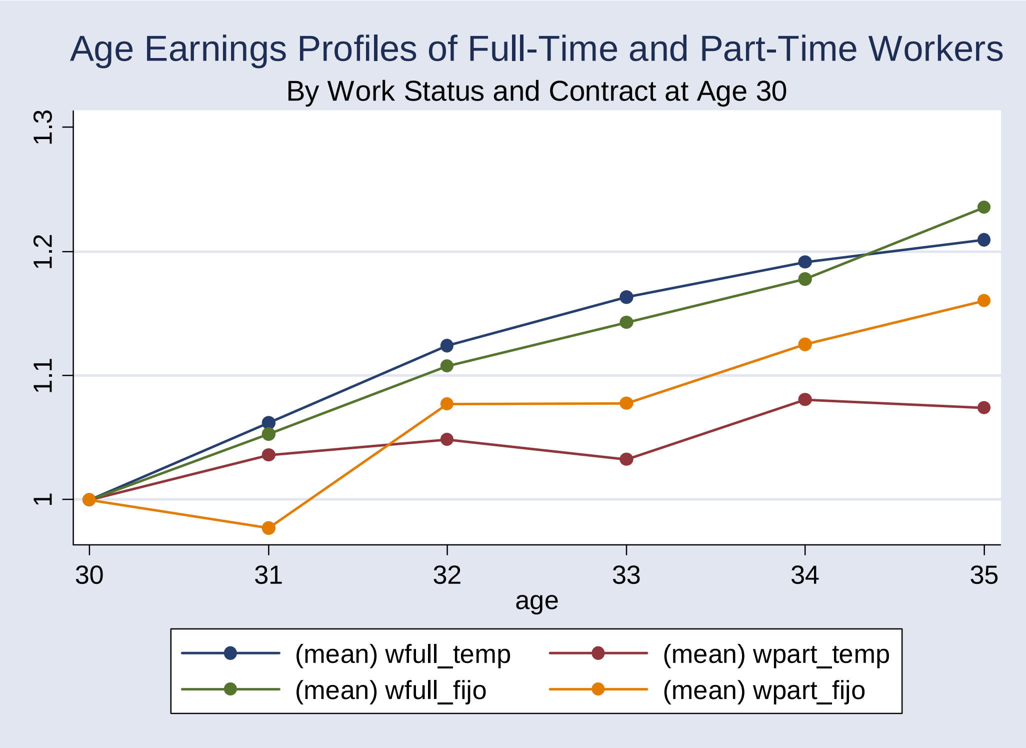
This paper is organized as follows. The next section presents an overview of the literature. Section III describes the institutional background. Section IV presents the data and the descriptive statistics. Section V explains the methodological approach. Section VI analyzes the results, and Section VII concludes with a discussion on policy implications. An additional appendix provides further information on the data and detailed empirical results.

I. Literature on PT Work and PT Earnings Penalty

The usefulness of PT work in achieving economic and individuals' professional development is and has been a prominent and heated debate among academics, political actors and practitioners in Europe and elsewhere (Bardasi and Gornick, 2000; Buddelmeyer *et al.*, 2005). Supporters of PT work argue that employers can use PT work to adjust hours worked to cyclical conditions, facilitating the adjustment of production and labor costs and drawing people who were previously unwilling or unable to work into the labor market (Euwals, 2001). In addition, PT work offers individuals the opportunity for career progression as workers can use part-time jobs as a stepping stone into full employment—moving first from non-employment into PT and then into FT work (Blank, 1998; McCall, 1997; Farber, 1999). Similarly, in countries where female employment rate is low, PT work may contribute to raise women's employment rates as reducing working-time barriers may facilitate labor market participation (Buddelmeyer *et al.*, 2005). Last but not least, PT work allows individuals to reconcile work and family, as many parents (mainly mothers) choose to work PT during the childrearing years. Good quality PT ought to facilitate transitions back to FT at a later stage of life (Fagan, 2004).

Figure 2. Age-Earnings Profiles for Women Working Full-Time and Part-Time by Type of Contract



Note: Earnings are hourly wages indexed to 1 at the age of 30. Women are aged 30 in 2001, aged 31 in 2002, etc. Full-time women were working full-time at the age of 30. Women with indefinite contracts had a permanent contract at the age of 30. The work status and the type of contract a woman has can change after the age of 30.

Plot PT pay penalty (raw regression controlling for years, education and province and PT dummy interacted with year) versus year
(see fig. 1 of Manning and Petrongolo, EJ 2008)

Deterrents of PT work, in contrast, argue that it may increase employers' fixed costs, such as recruitment and training costs (REF.) and that it may push part-time workers to become 'the new underclass' (Humphries and Rubery, 1995), as PT workers receive lower wages (Bassi, 1995; Gornick and Jacobs, 1996; OECD, 1994; Rubery, 1992; EBRI, 1993; Simpson, 1986) and benefits—in the form of reduced occupational benefits (Campling, 1987; ILO, 1989; OECD, 1994), or less public social welfare benefits (Euzeby, 1988; Maier, 1992)—, and have limited opportunities for advancement compared to similar workers in FT jobs (Rosenfeld, 1993; Tilly, 1990; Conolly and Gregory, 2008; and Manning and Petrolongo, 2008, among others). Critics of PT work denounce that, far from being stepping stones, PT jobs are the 'hidden brain drain' of women because women switch from 'better' FT jobs into lower-skilled occupations where PT opportunities are more readily available and they can find the flexibility in working the hours that they seek (the Equal Opportunities Commission, 2005).

Given the recent surge in PT employment in many industrialized countries, and the relative concentration of women in PT jobs, many researchers have analyzed the pay 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, 2006), The Netherlands (Hu and Tijdens, 2003); West Germany (Wolf, 2002), among others. Most studies find a negative unadjusted PT wage penalty, the magnitude of which differs substantially across the different countries. In some countries—such as, Australia (Rodgers, 2004), Belgium (Jepsen, 2001; Jepsen *et al.*, 2005), Norway (Hardoy and Schøne, 2006), and the UK (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 the other studies (Gallie et al., 1998; Gornich and Jacobs, 1996; Rubery, 1998; Rosenfeld and Kalleberg, 1990—COUNTRIES NOT AUTHORS), a wage gap remains and this unexplained part also shows considerable cross-country variation. Finally, in a third group of countries (Australia, Germany, Austria, Italy, Portugal, Greece, and Spain), a PT pay premium is found (Booth and Wood, 2008; Pissarides et al., 2003; and Pagán Rodríguez, 2007).

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 gradejobs 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, 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, Rosen (1976), Simpson (1986), Blank (1990), and Hotchkiss (1991), Ermisch and Wright (1993), Rodgers (2004), Pagán Rodríguez (2007), Manning and Petrongolo (2008), Mumford and Smith (2008), among others.[6]

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, 2005; and Connolly and Gregory, 2008 OTHERS?).[7] 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—for instance, Connolly and Gregory (2008) report that in any year, fewer than 9% of those in FT work switch to PT work in the UK. For Spain, Buddelmeyer et al. (2005) find that in any year, 1.52% (1.79%) of those women in FT (PT) work switch to PT (FT) work.[8] In addition, measurement errors of hours and wages, which are common in this literature (Altonji 1986; Bound, Brown, and Mathiowetz 2001), bias OLS estimates towards zero and magnify the attenuation bias in a fixed effects context (Aaronson and French, 2004; Manning and Petrongolo, 2008).

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[6] Although he uses data from 1995 through 2000, Pagán Rodríguez (2007) does not exploit the longitudinal characteristics of his data set and pools the data across the different waves.

[7] 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. .

[8] Using our sample, we find that, in any given year, 2.94% (1.57%) of women in FT (PT) work switch to PT (FT) work.

Given that most studies use worker' survey data, measurement errors of key variables is a frequent concern in this literature.[9] 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*., 2003; Naci Mocan and Erdal Tekin,2003; or Buligescu *et al*., 2008, among others). Most recently, Buligescu *et al*., 2008, 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 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*., 2003).

Another important identification 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, 1999; 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 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.

I. 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. 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 shy growth of PT employment (relative to other industrialized countries) is frequently explained by the fact that PT jobs seem to compete with fixed-term contracts in Spain, as employers clearly prefer the latter type to obtain flexible work arrangements (Toharia, 1996; and Guell and Petrongolo, 1998, among others).

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 (1994), and Jimeno and Toharia (1993). The concern was that the Spanish labor market would become a dual labor market with workers with fixed-term contracts

[9] See footnote 1.

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) for an analysis of the impact of the 1997 reform OTHERS?.

During the 1990s, labor market regulations introduced during the 1990s extended the coverage of statutory protection of PT workers to bring it closer to that of FT workers, in terms of redundancy, unfair dismissal, holidays, maternity leave and minimum notice. The major legislative change took place in Spain in 1999, with the establishment of clear protection rules for *all* workers who exerted their rights of flexible (and reduced) working hours: it declared a layoff (or non-renovation of contract) invalid if the worker had previously asked for work-week reduction due to family or other family dependent responsibilities.[10] This law clearly reduced workers' risks of reducing their work week for workers with permanent and fixed-term contracts—we claim that after this law change, workers who wanted to reduce their work week could do so without the risk of having to leave their job and move to a 'bad' job. In the results section, we use this policy change as a means to evaluate the robustness of our results on the effect of job protection on PT penalty.

Both types of work arrangements are disproportionately occupied by women, with 41% of contracts among women being fixed-term compared to 35% among men, and 15% of women working in PT jobs compared to 3% of men (European Community Household Panel Survey, 1994-1999). While women's role in home production may imply that women have stronger preferences than men for PT jobs, this does not necessarily imply different 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 (ECHPS), Pissarides *et al.* (2003) 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, whose effect on employment arrangements was allowed to differ across genders, single women in Spain are 10% more likely to be involuntary PT workers than single man. 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%).

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

[10] In 1995, the law first recognized the right to flexible work week for parents of children aged 6 years and younger or workers with dependents. However, this law was unclear on how it was going to be implemented and to what extent employers had to oblige on it. While it may have had some effect on workers with permanent contract, it was irrelevant for workers with fixed-term contracts. It was not until 1999 that clear protection rules for *all* workers who exerted their rights of flexible (and reduced) working hours were established. In addition, the 1999 law extended the right to reduce the weekly work load to parents of children aged 8 years old and younger.

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).[11] 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.*, 2003).[12] However, failure of correcting for unobserved heterogeneity and measurement problems raise caution before taken these esti mates at fa ce value (as ac knowledged by Pissarides *et al.*, 2003)

EXPLAIN WHY YOU THINK IT IS IMPO RTANT TO DO THE ANALYSIS SEPARATE FOR THE 4 GROUPS. MENTION IF OTHERS CONTROL FOR TYPE OF CONTRACT AND WHY. NEED to add a paragra ph on subgroup estimates and who controls for fix ed-term contract and why. Look if Nac ho (U. OLa vide) has look at the wage gap between <permanent and fixed-term contracts using Muestra….

FALTA: REF FERNANDEZ AND LA CUESTA

Other legislative changes of concern?

II. The Data and Descriptive Statistics (some of this text can go into a data appendix)

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 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, date of birth, country 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, 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).[14]

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[11] FALTA JIMENO TOHARIA, 1993. To identify participation into fixed-term versus permanent contract, 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.

[12] To identify participation into fixed-term versus permanent contract, 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).

[13] For a description of the CSWH and the sampling strategy, see Argimón and González, 2006.

[14] Reported earnings suffer from right censoring and we have eliminated from the analysis all individuals that had their earnings capped at lea st once during their working history (7.5% of total). We have computed the main results of the paper including observations with top coded income and we did not find any difference with the results we report here.

Although not reported in the CSWH, other variables such as working experience (in FT and PT work) and tenure can be easily calculated. As explained in the Appendix, these data comes matched to data from the 2006 Spanish Municipal Registry of Inhabitants (*Padrón Municipal de Habitantes*) which portrays information on the individual's education level, and number and date of birth of each of the members in the household. We believe that these variables are key into explaining women's selection into the type of contract and employment.

Following the most of the European and Canadian literature, we classify a worker working PT if he works 30 hours or less each week, and FT if he works 31 or more hours each week. The CSWH compares well with other datasets frequently used in studies of the Spanish labor market, such as the Spanish Labor Force Survey (LFS). Table 1 shows some descriptive statistics by age group comparing the CSWH and the LFS. In general, the distribution of individuals by the type of contract, permanent or fixed-term, and by FT status is very similar in the two datasets and across age groups. Considering the more aggregated results, we can see that two-thirds of the working population holds a permanent contract and a bit over one tenth of workers have a part-time job. We also find that the incidence of PT employment is very different across gender groups, being much higher for women than for men (20.8 % versus 5.3 %).

Table 1

Descriptive Statistics by Age and Sex
CSWH versus Labor Force Survey (LFS). Year=2006
(LFS distribution in parenthesis)

	Type of Contract**		FT- versus PT-time Status**	
	Permanent	**Fixed-term**	**Full-time***	**Part-time***
All ages	68.97 (66.74)	31.03 (33.26)	88.35 (87.89)	11.65 (12.11)
16-19	21.35 (22.13)	78.65 (77.87)	81.57 (74.17)	18.43 (25.83)
20-24	45.17 (37.62)	54.83 (62.38)	83.12 (80.55)	16.88 (19.45)
25-54	71.05 (69.31)	28.95 (30.69)	89.35 (88.86)	10.65 (11.14)
55 and older	81.01 (85.85)	18.99 (14.15)	85.71 (88.93)	14.29 (11.07)
Males	68.55 (66.04)	31.45 (33.96)	94.70 (95.75)	5.30 (4.25)
16-19	19.42	80.58	88.15	11.85
20-24	42.63	57.37	89.74	10.26
25-54	70.79	29.21	96.11	3.89
55 and older	79.85	20.15	89.50	10.50
Females	69.59 (65.36)	30.41 (34.64)	79.13 (75.97)	20.87 (24.03)
16-19	25.51	74.49	67.44	32.56
20-24	48.49	51.51	74.48	25.52
25-54	71.39	28.61	80.02	19.98
55 and older	83.72	16.28	76.83	23.17
Total				

*For all employed workers (self-employed and employees)
**Data weighted by days worked during calendar year (iweight=daysworked/365)

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.[15] 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 oldest women are 24 years old in 1985, and they are followed until age 45 in 2006. The potentially youngest women are born in 1978, and they are aged 28 in 2006.[16] 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 the number and age of children, which is unavailable in the CSWH but can be obtained from the Spanish Municipal Registry of Inhabitants. As explained in the Appendix, 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. 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 procedure as the one used by CG),. This sample selection results in an unbalanced panel of 582,974 observations on 76,025 women.

Compared to other data, the advantage of using the CSWH is threefold. 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. Of our sample of 75,062 women, we observe 16,469 switching from FT to PT (or vice-versa) at least once in our sample, providing us with 34,955 observations of switchers. 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 and occupation 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, as the information comes directly from the payroll records. Measurement error due to recall bias or self-reporting for these key 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.

There are drawbacks with the administrative data used. One of the biggest concerns with the data at hand is that we observe contractual hours worked (as in CG; and Buligescu *et al.*, 2008). Although these data are considered to be superior to workers' survey data (as we discussed in the Introduction and Section II), we have noticed that, in Spain, contractual hours consistently underreport actual worked hours for PT workers relative to FT workers leading to a differential measurement error by

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[15] If the worker held more than one job during the course of a year, 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. This approach is similar to the one used by the Spanish Statistics National Institute (INE) to define the worker's main job in the Labor Force Survey. About one fourth of the sample held more than one job in a given year, and the average number of jobs held in one year is 1.53.

[16] The reason our potentially smallest cohort is born in 1978 and aged 28 in 2006—instead of being born in 1982 and aged 24 in 2006—, is that we had to restrict the sample to women who were at least 18 years old in 1996 because that was the last time the variable education was updated nationwide. Because of this restriction we lose 13% of the sample.

PT status.[17] Using data from the Use of Time Survey, Table A5 in the Appendix provides evidence that PT workers consistently work a greater number of hours in excess of contractual hours relative to their FT counterparts, which would bias upwards the hourly wages of PT workers (relative to FT workers) leading to underestimating the PT wage penalty. To address this problem, we focus our attention on the wage change as opposed to wage levels, and drop from our sample of analysis the observations of wage change observed exactly when status changes.[18] Assuming that differential measurement error by PT status is an individual-employment-status fixed effect, our approach circumvents the problem, as shown in Section VI. An additional problem is that the CSWH does not keep track of those individuals who exit the Social Security Records. Therefore, the CSWH is not useful to analyze transitions from employment to inactivity.

Table 2 presents the sample sizes and descriptive statistics of the key covariates pooling the observations that are used in the longitudinal analysis. The main focus of the present study is to analyze how the change in hourly wage varies 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.[19] Among the sample under study, women between 24 and 45 years old strongly attached to the labor force, we find that those with permanent contracts represent a bit over half of the sample (52.6%). In addition, the percentage of women working in PT employment is similar across the two types of contracts, with PT employment representing close to 10% of jobs in each group.

[17] These 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.
[18] By doing this we drop 26,328 (5.6%) observations from 16,469 individuals. Notice, however, that we observe subsequent wage growth for most of these 16,469 individuals who have switched from or to PT employment.
[19] 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.

Table 2
Descriptive Statistics
Women Strongly Attached to the Labor Force, 1985-2006 CSWH
(24 to 45 years old)

	Permanent contract		**Fixed-term contract**	
	FT worker	**PT worker**	**FT worker**	**PT worker**
Change in log real hourly earnings	.036 (.212)	-.004 (.271)	.052 (.336)	.017 (.365)
Log of current hourly earnings in cents of €	6.868 (.416)	6.721 (.421)	6.707 (.414)	6.640 (.469)
Age 24 to 29 years old (percent)	36.17	31.78	59.49	37.71
Age 30 to 34 years old (percent)	27.75	35.13	21.79	30.24
Age 35 to 39 years old (percent)	20.10	19.61	10.80	18.15
Age 40 to 45 years old (percent)	15.98	13.47	7.91	13.90
Cohabiting (percent)	80.47	80.02	78.50	77.36
Without children (percent)	79.07	49.76	77.21	58.71
With children 0 to 2 years old (percent)	8.91	20.99	6.99	12.07
With children 3 years old (percent)	1.71	5.70	1.67	3.88
With children 4 to 6 years old (percent)	3.56	8.56	3.98	6.96
With children older than 6 years old (percent)	6.75	14.99	10.15	18.39
High-school dropout (percent)	34.69	41.87	34.93	46.50
High-school graduate (percent)	37.28	38.04	31.63	30.25
College graduate or above (percent)	28.03	20.09	33.34	23.25
Experience in PT employment (in years)	.113 (.954)	7.250 (4.730)	.270 (1.056)	4.157 (2.971)
Experience in FT employment (in years)	8.771 (5.289)	.171 (.879)	3.212 (2.605)	.033 (.357)
Unemployment (in years)				
Public servant (percent)	16.33	0.26	5.73	1.59
Firm tenure (in years)	6.573 (5.302)	4.270 (4.443)	1.415 (1.805)	1.528 (1.829)
Firm size—equal 1 if 200 employees or more	64	23	75	35
(median num. workers)	(2583.551)	(2521.763)	(1749.939)	(368.587)
White Collar (percent)	22.64	12.59	22.18	13.82
Change of employer (percent)	7.35	7.32	34.03	24.58
Change of occupation (-downgrading/+ upgrading)	.067 (.690)	.033 (.620)	.146 (1.405)	.028 (1.191)
Number of observations	319,061	11,237	129,825	8,409
Number of individuals	51,501	5,021	45,971	5,038

Note.- The numbers in parenthesis are standard deviations. All hourly wages are deflated by the gross domestic product (GDP) deflator (base year = 2006). 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

† Difference in the means between layoff and plant closing are significantly different at the 90% confidence level
†† Difference in the means between layoff and plant closing are significantly different at the 95% confidence level
††† Difference in the means between layoff and plant closing are significantly different at t he 99% confidence level

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 grows at a lower rate than FT workers. 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 Table 2. For instance, we observe that PT workers are older and more likely to have children of all ages than FT workers. In addition, women in PT jobs are also more likely to be high-school dropouts and less likely to be college graduates than women in FT jobs, and they have on average more experience in PT jobs and less in FT jobs than their FT counterparts. Looking at employer differences across the two groups, women in PT employment are concentrated in private sector, small firms and blue-collar occupations, and are less likely to upgrade occupations (relative to FT workers). These findings also suggest that PT workers may also segregate into low-paying firms and low-paying jobs. Overall the observed differences for PT versus FT workers hold across the two types of contract. However, some of these differences—like the (raw) PT penalty (measured both at levels and at changes), the age and education differences, and the employer size—are more pronounced for workers with fixed-term contracts than with permanent contracts. In contrast, other differences—like the likelihood of having children, the difference in experience and the likelihood of being a public servant—are more pronounced for workers with permanent contracts than those with fixed-term contracts. Finally, we find that women with PT fixed-term contracts are more likely to change employers than those with FT fixed-term contracts. No such difference is observed among women with permanent contracts. We also observe that women in PT jobs with permanent contracts have less tenure with their employer than those in FT permanent jobs (no such difference is observed among fixed-term contracts, which is of no surprise as these contracts cannot exceed three years and involve frequent turnover).

Table 2 also allows us to compare workers with fixed-term contracts versus those with permanent jobs. Similar to the PT status comparison, we find evidence of differences across the two groups. First, we find that workers with fixed-term contracts earn lower (raw) wages than those with permanent contracts. However, this difference is likely to be explained by the fact that different types of women are employed in the different types of contracts. For instance, Table 2 shows that women with fixed-term contracts are younger, less likely to be high-school graduates, but more likely to be college graduate, more likely to have children older than six years old, more likely to be working in large firms and to change employers than those with permanent jobs. Some of these differences, such as the PT penalty and the age difference, are more pronounced for women working FT than those working PT. In contrast, other differences, such as education, are more pronounced for those working PT than for those working FT. In addition, we also find clear differences across the two types of contract depending on whether they are FT or PT workers. On the one hand, we find that PT workers with fixed-term contracts are less likely to have children,[20] less likely to have experience (in both FT and PT employment), and more likely to be public servants than their counterparts with permanent contracts. In contrast, no such difference in the likelihood of having children by contract type appears for FT working women, and FT workers with fixed-term contract have more PT experience and less FT experience and are less likely to be public servants than their counterparts with permanent contracts.[21]

[20] This finding is consistent with results found in the literature that suggest that female workers in Spain prefer to wait and have a protected job before entering motherhood (Ahn and Mira, 2001; Gutierrez-Domenech, 2008; de la Rica and Iza, 2005; García Ferreira and Villanueva, 2007).

[21] Although our summary statistics do not suggest that workers with fixed-term contracts segregate into small firms or blue-collar occupations compared to workers with indefinite contracts (as in De la Rica, 2004), there are many reasons that can explain the different results. First, both samples differ drastically as De la Rica (2004) includes both males and female full-time workers during the mid-1990s and does not focuses on women during their childrearing years as we do. In addition, the definitions of the different categories differ across the two studies.

(NEED TO ADD : , IS THIS DIFFERENCE STATISTICALLY SIGNIFICANT?)

III. Methodology

Our objective is to exploit longitudinal data in Spain to analyze the direct consequences of PT employment on subsequent earnings growth and career trajectories. However, given 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.

For ease of the exposition, we 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 (this approach is similar to the one used by Hirsch, 2005; and CG) OTHERS??.[22] The form of the wage growth equation model is:

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

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} is a vector of individual and job characteristics for individual i at time t, with β the corresponding coefficient vector (including an intercept). X_{it} includes age dummies, marital status dummy, presence of children in the household dummies (aged 0 to 2 years old, 3 years old, 4 to 6 years old, and older than 6 years old), ethnicity dummy, three dummies for completed education (one for "high school drop-out"; one for "high-school graduate"; and one for "college graduate or above"), industry and occupation dummies at year *t-1*; experience and its square (in PT and FT jobs) at year *t-1*; previous tenure at year *t-1* (with breaks at 1, 2, 3, and 6 years), a change of occupation at year *t-1*, and change of employer dummy at year *t-1*. Because there has been much debate on whether variables that control for employer characteristics or change in occupation or employers ought to be included in the specification (see discussion in the next section), we present alternative specification to evaluate the robustness of the results and understand the underlying mechanism behind the (raw) PT penalty. All regressions use the Huber/White estimator of variance. PT_{it} is a binary variable equal to one if the worker's principal job is PT in year *t-1*; FT_{it} is a binary variable equal to one if the worker holds a permanent contract at time *t-1*. The error term includes both a random component μ_i with mean zero and constant variance, and a worker-specific fixed effect ϕ_i.

Using the longitudinal panel characteristics, we estimate the following fixed-effect equation (2):

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

Notice that, as in CG, the effect of PT work on earnings growth is identified through those who switch status, but (in contrast with these authors) we do not use the observation of the year the switch occurs—to avoid using a wage change that compares hourly wages in FT versus PT status (or vice-versa) and mitigating the differential measurement error in contractual hours by PTstatus.

[22] 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.

Table 4

The Part-time Penalty
Using Contractual versus Imputed Hours

	Dependent variable					
	Ln(real hourly wage)		One year change of Ln(real hourly wage)		One year change of Ln(real hourly wage)[ψ]	
	Cross-Section	**Fixed-Effects**	**Cross-Section**	**Fixed-Effects**	**Cross-Section**	**Fixed-Effects**
Computing hourly wages using CONTRACTUAL hours	-0.066***	+0.060***	-0.06***	-0.02***	-0.030***	-0.025***
Computing hourly wages using imputed EFFECTIVELY worked hours	-0.232***	-0.094***	-0.15***	-0.09***	-0.016**	-0.015***

*, **, *** significant at 10%, 5% and 1% levels. All regressions include year, age and education dummies. A negative number indicates a penalty for part-time workers.

[ψ] Excluding the year of switching: excluding observations for which work status changed from full to part-time or vice versa.

Although most of the literature focuses on hourly wage levels as the LHS variable, our paper analyzes the hourly wage growth. The reason for this is that we have identified a differential measurement error by PT status when using contractual hours. We have found evidence that PT workers consistently work a greater number of hours in excess of contractual hours relative to their FT counterparts, which (if uncorrected for) would bias upwards the hourly wages of PT workers (relative to FT workers) leading to underestimating the PT wage penalty. The first two columns of Table 4 show evidence of this. The top two columns of Table 4 show the PT penalty adjusted for age, education, and year dummies when the LHS variable is measured with contractual hours and using the OLS pooled specification (column 1) and the fixed-effect specification (column 2), respectively. While the OLS specification shows a 6.6 percentage-points penalty for women working PT compared to their FT counterparts, the penalty becomes a 6 percentage-points *premium* when correcting for unobserved heterogeneity using a fixed-effect specification (and similar to Pissarides et al., 2003; and Pagán Rodríguez, 2007). However, the bottom two columns show the same estimates when hourly wages have been computed using imputed reported hours instead of contractual hours—while imputed hours are not free of measurement error, they ought to *not* have the differential measurement error by PT status that we are concerned with contractual hours.[23] In such case, the OLS specification shows a 23.4 percentage-points PT penalty that becomes a 9 percentage-points penalty with the fixed-effects specification. These results clearly highlight that the levels specification when using contractual hours underestimates the PT wage penalty. In addition, the reduction of the PT penalty in both cases provides evidence of negative self-selection, indicating that those workers selecting into PT are less motivated workers than those who remain in FT employment. Note however, that since both estimates of hourly wages have a measurement error problem (a differential measurement error for those computed with contractual hours; and a general measurement error for those computed with imputed reported hours) it is likely that part of the observed reduction on the PT penalty is due to attenuation bias due to measurement error by estimating the fixed-effects estimator as opposed to the cross-section estimate.

To address this problem, 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 (last two columns of Table 4). Assuming that differential measurement error by PT status is an individual-employment-status fixed effect, our approach circumvents the problem. Under this assumption, estimates in the top two columns ought to be free of differential measurement error and superior to the estimates in the bottom two columns, as these have used imputed reported hours, which are known to generate more spurious correlation than contractual hours This time around, the cross-section OLS estimates show a larger PT penalty when the hourly wage change is calculated with the contractual hours (-3.3 percentage points) than the imputed hours (-1.6 percentage points). Again, this penalty decreases with the fixed-effects specification suggesting that there is negative self-selection into PT:[24]

Finally, although our specification does not account for selection by type of contract and by PT status, by controlling for number and age of children and education, and employer characteristics, we are *de facto* controlling for the same information that many researchers have used when and instrumental variables approach correction (see footnotes 11 and 12). In the case of selection into FT / PT employment, most researchers use family composition variables to identify participation into PT employment (Blank, 199 ?; Pissarides et al., 2003; OTHERS) arguing that these variables do not explain wages. Similarly, in the case of selection by type of contract, researchers use employer's characteristics, such as private versus public sector or firm

[23] Reported hours have been imputed by regressing contractual hours on effectively worked hours, age, work status and education dummies.

[24] The top estimates of the middle two columns continue to have the differential measurement error problem as observations in which the wage change is calculated with FT and PT hourly wages are included in the sample. As in the first two columns, the PT penalty is greater when using imputed hours than contractual hours.

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

IV. Results

Table 5 presents our pooled cross-sectional and fixed-effects estimates of the PT penalty 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. Looking first at Panel A, we see that the pooled cross-sectional estimates always report a smaller PT penalty than the longitudinal fixed-effects ones, suggesting that there may be positive sample selection into PT jobs in Spain. At first, this finding may seem surprising (given the anecdotal evidence that PT jobs tend to be involuntary jobs, in which workers are discriminated into). However, when we estimate the differential effects by contract type (Panel B), clearly the positive sample selection seems to be driven by those workers with fixed-term contracts. For PT workers with permanent contracts, comparison of columns 2 and 3 provide evidence of negative sample selection: Among workers with permanent contracts, those moving to PT employment are intrinsically different in taste and preferences about work than those in FT employment: Those switching to PT employment are less committed to their careers in the labor market, and would have had lower wage growth had they remained in FT work. For women with permanent contracts, comparing the second and third columns of Panel B shows that when we correct for unobserved heterogeneity, the PT penalty adjusted for worker characteristics falls from 3.8 to 2.9 log points. This reduction of more than one fifth of the penalty occurs because PT women with permanent contracts have less work oriented preferences and tastes than their FT counterparts.

Table 5

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

	Unadjusted	Pooled OLS (Basic controls)	Fixed-effects (Basic controls)	Pooled OLS (Basic controls + employer characteristics)	Fixed-effects (Basic controls + employer characteristics)	Pooled OLS (Basic controls + employer characteristics + industry)	Fixed-effects (Basic controls + employer characteristics + industry)	Pooled OLS (Basic controls + employer characteristics + change occupation or employer)	Fixed-effects (Basic controls + employer characteristics + change occupation or employer)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Without Contract Type									
	-0.036	-.032*** (.002)	-.038*** (.004)	-.032*** (.002)	-.037*** (.004)	-.029*** (.002)	-.044*** (.004)	-.027*** (.002)	-.036*** (.004)
Panel B. By Contract Type									
Fixed-term contract at time t	-0.035	-.027*** (.003)	-.039*** (.005)	-.027*** (.003)	-.037*** (.005)	-.024*** (.003)	-.046*** (.005)	-.023*** (.003)	-.040*** (.005)
Permanent contract at time t	-0.040	-.038***✓ (.003)	-.029***✓ (.005)	-.037***✓ (.003)	-.031*** (.005)	-.035***✓ (.004)	-.035***✓ (.005)	-.031***✓ (.003)	-.029***✓ (.005)
Sample size	468,532	468,532	468,532	468,532	468,532	468,532	468,532	468,532	468,532
(# individuals)	75,063	75,063	75,063	75,063	75,063	75,063	75,063	75,063	75,063

Hourly earnings have been deflated using 2006 deflator. ***, **, * 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 5% level.

The story is quite different for women with fixed-term contracts. First, the evidence shows that PT women are less well-educated, and have more children than those in FT jobs causing the unadjusted PT penalty (column 1 of Panel B) fall more than one fifth, from 3.5 to 2.7 log points, when controlling for worker characteristics (column 2 of Panel B).[25] However, once we control for unobserved heterogeneity using the fixed-effect specification (column 3 of Panel B), we find that the PT penalty rises to 3.9 log points, suggesting that there is positive selection into PT work for women with fixed-term contracts. Given the striking segmentation of the Spanish labor market, fixed-term contracts have become the jobs offered in the secondary labor market. These jobs are known to be unstable, low protected, poorly paid (see Segura et al., 1991; Bentolila and Dolado, 1994; Jimeno and Toharia, 1993), and segregated in low-paying firms and low-paying occupations (De la Rica, 2004). Given the marginalization and discrimination suffered among workers in fixed-term contracts in Spain (Pissaride et al., 2003), those who decide to work PT are those who know they can do so without risking to lose their job, that is, those who are more motivated to work and who can convince employers to keep them (or hire them) despite working PT. Seen from this perspective, evidence of positive self-selection among PT women with fixed-term contracts is no longer striking.

After accounting for workers' observable and unobservable characteristics, 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 contract experience 3.9 log points lower hourly wage growth per year then 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 contract and nearly doubles that of the college premium among women with fixed-term contract. 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 negative relationship between job protection and PT penalty.

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 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 again 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). 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. After controlling for employer characteristics, PT women wage growth

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

rises at an even lower rate than their FT counterparts (this occurs, independently of the fact that PT jobs may be more likely to be in low-paying jobs as observed in Table 2 and explained in Section IV).

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 the FT counterparts (as illustrated by the reduction in the cross-sectional estimates of the PT penalty—moving from columns 4 to 6), the PT 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.

Finally, columns 8 and 9 control for whether the worker has changed of employer or occupation. Again, inclusion of such covariates is not entirely clear-cut as a change of employer (and occupation) may be necessary when switching to PT work. Adding such controls leads to a decrease in the PT penalty for workers with both types of contracts, suggesting that there is occupational downgrading (this finding is consistent with evidence from other surveys, such as Montgomery and Cosgrove (1995), Stevens *et al.* (2004); Manning and Petrongolo (2008); CG, among others), and employer turnover. The decrease in the PT penalty is larger (17% reduction versus a 13% reduction) for women with permanent contracts than those with fixed-term contracts, leaving the PT penalty to 2.9 log points for the former to 4.0 log points for the latter.

Table 6

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

	Age, year, and province dummies	Plus family characteristics	Plus education dummies	Plus experience in FT or PT job at time *t*	Plus increase in experience FT or PT between *t-1* and *t*	Plus public vs. private sector	Plus employer size	Plus occupation	Plus industry	Plus tenure	Plus change of occupation	Plus change of employer
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Without Contract Type												
	-.025*** (.003)	-.024*** (.003)	-.024*** (.003)	-.043*** (.004)	-.038*** (.004)	-.036*** (.004)	-.036*** (.004)	-.037*** (.004)	-.044*** (.004)	-.044*** (.004)	-.041*** (.004)	-.036*** (.004)
Panel B. By Contract Type												
Fixed-term contract at time t	-.030*** (.004)	-.030*** (.004)	-.030*** (.004)	-.043*** (.005)	-.039*** (.005)	-.038*** (.005)	-.037*** (.005)	-.037*** (.005)	-.046*** (.005)	-.046*** (.005)	-.044*** (.005)	-.040*** (.005)
Permanent contract at time t	-.022*** (.004)	-.020***✓ (.009)	-.020***✓ (.004)	-.037*** (.005)	-.029***✓ (.005)	-.028***✓ (.005)	-.028***✓ (.005)	-.031*** (.005)	-.035***✓ (.005)	-.035***✓ (.005)	-.032***✓ (.005)	-.029***✓ (.005)
Sample size	468,532	468,532	468,532	468,532	468,532	468,532	468,532	468,532	468,532	468,532	468,532	468,532
(# individuals)	75,063	75,063	75,063	75,063	75,063	75,063	75,063	75,063	75,063	75,063	75,063	75,063

Hourly earnings have been deflated using 2006 deflator. ***, **, * 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.

Table 6 provides more detail of the fixed-effects estimates as we successively add additional covariates. Worth highlighting is the relevance of the following three variables in explaining the PT penalty: the experience in FT and PT jobs, and the change in experience within the year the wage growth is measured (the latter variable is a proxy for whether the worker has spent some time out of employment within that year). Clearly, past labor market history matter when explaining the PT penalty.[26] Moving from columns 3 to 4 increases the PT penalty, indicating that those women going into PT employment have accumulated some comparative advantage in PT jobs (as they have accumulated more PT experience). If we fail to control for such experience, the PT penalty appears smaller than it truly is. Finally, moving from columns 4 to 5 has the opposite effect. Since the switch to PT jobs is likely to bring some period of inactivity or unemployment, accounting for this lowers the PT penalty.

V. Conclusions

Using the 2006 wave of the Continuous Sample of Working Histories (hereafter CSWH), an unbalanced panel obtained from the Social Security records that covers employment history from 1985 to 2006, we explore the differences in the PT / FT wage growth for prime aged women strongly attached to the labor force, distinguishing by their type of contract. We control for worker's socio-demographic characteristics, worker's previous employment history, employer's characteristics, and whether the worker switches occupation or employer. Finally, the longitudinal analysis provides a method for controlling for unmeasured work-specific skills or preferences fixed across jobs.

Our estimates provide evidence of a differential impact of PT work on women's hourly wage growth by type of contract. While the penalty is larger for women with less job protection, that is, those with fixed-term contracts, the penalty for women with permanent contracts is far from negligible. After accounting for workers' observable and unobservable characteristics, 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 then their FT counterparts. These estimates are similar to the ones we would obtain if we were to control for employer characteristics. The reason for this is that the greater loss caused by the occupational downgrading and employer turnover that a switch to PT employment may imply seems to be cancelled out by workers' upgrading to better paid industries (or, alternatively, coming from relatively lower paid industries to start with). Finally, we have found that while women with permanent contracts negatively self-select into PT employment, the opposite occurs for women with fixed-term contracts. We believe that the positive self-selection into PT fixed-term jobs is due to the dual nature of the Spanish labor market and the marginalization of fixed-term contracts.

[26] Given that Blank (1998) and Buddelmeyer *et al*. (2005) have found that the past labor market history is important in determining the likelihood of working part-time, adding such covariates reduces our bias of potential endogeneity due to sample selection.

Table 7

The Effect of the 1999 Reform
One-Year Difference Model: The Part-Time Wage Growth Penalty
Women 24 to 45 years old

	Before 1999	After 1999
Fixed-term contract at time t	-.076***✓ (.013)	-.041*** (.005)
Permanent contract at time t	-.073*** (.020)	-.043*** (.005)
Sample size	440,813	440,813
(# individuals)	74,926	74,926

Hourly earnings have been deflated using 2006 deflator. ***, **, * indicate significance at the 1%, 5%, 10% level (two-sided test). All models control for all set of covariates. ✓ indicates that the difference between the after and before 1999 periods of the estimated effects by type of contract is significant at the 10% level. Observations for the difference between the year 2000 and 1999 have been excluded from the analysis.

Our findings suggest that PT employment marginalizes workers and that this marginalization is greater for workers with fixed-term contracts. Given the concentration of women in PT jobs (and with fixed-term contracts) in Spain, these results bring to light another dimension of gender discrimination in the Spanish labor market. Our results suggest the following two (MORE?) policy implications. First, in a country with important segmented labor markets, such as Spain, part-time workers are the least protected and the most penalized (we are not the first to evidence towards consistent with this finding, see, for instance, Pissarides et al., 2003; De la Rica and Ferrero, 2003). Instead of adding flexibility in the labor market by promoting a 'new underclass', more effort ought to be spent in engaging in a "real" reform of the Spanish labor market. Second, while PT work seems to be an option for reconciling work and family for women with permanent contracts, this does not seem to be the case for women with fixed-term contracts (as those who switched into PT employment are those with stronger work preferences). Given the vast reserve of potential labor force that represent inactive women in Spain, [27] and considering the challenges that the Spanish society has in reconciling work and family,[28] the current laws allowing for flexible working hours for mothers of young children are clearly not sufficient. Other policies (such as raising the availability of affordable good-quality childcare) ought to be used to help women turn to full-time jobs with more prospects. An alternative solution may be to reduce the negative future career consequences of a period spent in part-time work, for example by giving parents greater rights to change hours (including to work part-time but also to resume their full-time job—this was done in Spain with the Law of 1999.) Therefore we should see a decrease of the part-time wage penalty after the law. Table 7 illustrates the effect of the 1999 law. After the law, the PT penalty was reduced by half for both women with fixed-term contracts and permanent ones.[29] Unfortunately, while reducing the PT penalty, the law was not sufficient to eliminate such penalty.

[27] Buddelmeyer *et al.* (2005) estimate that 38% of females remained inactive in Spain between 1994 and 1999 (compared to an average of 26% in the EU).

[28] Sanchez-Mangas and Sanchez-Marcos (2008) highlight the following five stylized facts that illustrate the difficulties Spanish mothers have in reconciling employment and family. First, the Spanish employment rate for mothers is among the lowest in the OECD (Gutiérrez-Domenech, 2005). Second, Spanish maternity leave is, on average, nine weeks shorter than in most of the European countries (OECD, 2001). Third, the use of formal child-care arrangements for three-year-old children is much less frequent in Spain than in the average European country. For instance, in 2001 the proportion of children under the age of three in preschool was only 9 percent in Spain, in sharp contrast with the European average of 25 percent. Fourth, the 2004 Spanish Labor Population Survey indicates that 29 percent of women aged 45 and younger reported family responsibilities as their main reason for not participating in the labor market. Last, but not least, having one of the lowest fertility rate among the EU-15 countries is also indicative of difficulties of reconciling work and family in Spain (Eurostat, 2007).

[29] EXPLAIN WHY WE CANNOT EXPECT THE EFFECT GREATER FOR ONE GROUP VERSUS THE OTHER.

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