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María Cervini Plá
Xavier Ramos
José Ignacio Silva

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María Cervini Plá
Universitat Autònoma de Barcelona

Xavier Ramos
Universitat Autònoma de Barcelona and IZA

José Ignacio Silva
Universitat de Girona

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

Wage Effects of Non-Wage Labour Costs*

We study short- and long-term wage effects of two important elements of non-wage labour costs: firing costs and payroll taxes. We exploit a reform that introduced substantial reduction in these two provisions for unemployed workers aged less than thirty and over forty five years. Theoretical insights are gained with a matching model with heterogeneous workers, which predicts an ambiguous effect on wages: firing costs are expected to increase wages, because they increase the bargaining power of workers, while the effect of payroll taxes is negative. Difference-in-differences estimates, which account for the endogeneity of the treatment status, suggest that decreased firing costs and payroll taxes have a positive overall short-term effect on wages – and also on unemployment. We find larger effects for older than for younger workers and for men than for women. Calibration and simulation of the model shows that about fifty percent of the predicted cumulative increase on wages takes place during the first year of the reform. Our simulations also show that the increase in wages is mostly due to the reduction in payroll taxes, while the overall effect of firing costs is nil because direct and indirect effects of firing costs offset each other.

JEL Classification: C23, D31, J31

Keywords: dismissal costs, payroll tax, evaluation of labour market reforms, difference-in-difference, matching model, Spain

Corresponding author:

Xavier Ramos
Departament d'Economia Aplicada
Universitat Autònoma de Barcelona
Edifici B
08193 Bellaterra
Spain
E-mail: xavi.ramos@uab.es

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1 Introduction

In the last decades, several European countries have reduced employment protection and payroll taxes to improve the performance of the labour market (see Kugler (2007) for employment protection legislation (EPL) reforms and Carone, Nicodme, and Schmidt (2007) for recent changes in payroll taxes).[1] However, the estimation and evaluation of the causal effects of the changes has proved difficult, since most changes have been gradual (i.e. not sharp) and accross the board (i.e. applied to everyone).

In 1997, Spain drastically reduced dismissal costs and payroll taxes for young and old workers only, which provides a unique natural setting to examine the effects of non-wage labour costs. Severance payments for unfair dismissals were reduced 20%, while payroll taxes decreased between 40 and 60%, depending on the targeted group. These sharp changes, which applied only to some age groups, provide a unique opportunity to examine the causal effects of firing costs and payroll taxes on employment and wages.

There is an increasing amount of empirical evidence, which points that stringent employment protection regulations reduce employment flows (Autor, Donohue, and Schwab (2004, 2006); Kugler and Pica (2003, 2008)). However, evidence on wage effects is very scarce and not very conclusive. Leonardi and Pica (2007) analyse an increase in firing costs implemented in Italy for small firms and find that more stringent employment protection has no effect on entry wages, but has a negative impact on subsequent wages, while van der Wiel (2010) finds positive wage effects of extending employer's term of notice in the Netherlands.

The incidence of payroll taxes also gathers mixed evidence. Generally speaking, when employees percieve a close link between employers' contributions and their benefits, payroll taxes are likely to be fully shifted from firms to employees, with no disemployment effects. However, with a loose link between taxes and benefits, payroll taxes are usually not fully passed on to employees and employment decreases.[2] Small changes have also been found easier to pass on to employees than large changes (Gruber (1997)).

[1]For instance, in the late 1980s France relaxed employment protection provisions to facilitate employment for certain types of workers, and Germany has recently (in 2004) exempted small firms (from 5 to 10 employees) from EPL. Payroll taxes decreased in the EU-27 from 7.5% to 7.3% of GDP between 1995 and 2005, and the Nordic countries have been reducing payroll taxes selectively for some regions since the mid 1980s.

[2]This may be the case for pay-as-you-go social security systems, such as the Spanish one, with weak linkages between pensions and other benefits, on the one hand, and contributions, on the other.

Our analysis focusses on the wage effects of firing costs and payroll taxes, though we also examine their implications for employment. To do so, we extent the matching model with heterogeneous workers put forth by Dolado, Jansen, and Jimeno (2007) in two important ways to accomodate the salient features of the Reform. We introduce an exogenous job retirement probability to account for the important age difference of the targeted groups, and we also consider the joint effect of payroll taxes and firing costs.

We provide short- and long-term evidence, which yield consistent results. Short term estimates come from a microeconometric analysis of panel individual administrative records, while longer-term evidence results from calibrating the model and simulating the reform.

We exploit the variation of firing costs and payroll taxes across age groups (young, prime-age, and older) and over time (before and after 1997), and identify the effects of the reform using a difference-in-differences estimator, i.e. we compare wages of younger and older individuals with those of prime-age individuals, before and after the reform.

Our main findings suggest that decreased firing costs and payroll taxes have a positive effect on wages and employment. Simulations show that about 50 percent of the predicted cumulative wage increase takes place during the first year of the reform. This partial transmission of the adjustment in non-wage labour costs occurs because the reform applied only to newly signed permanent contracts, which is the effect captured by our short-term estimates, whereas simulations show the effect that would obtain if all employed workers in the treatment groups were affected by the reform. Simulations also show that the increase in wages is mostly due to the reduction in the payroll tax.

The experience of Spain should also provide direct evidence on the effects other countries might expect from a decision to promote (permanent) employment by reducing non-wage labour costs. Since firing costs and payroll taxes account for a large proportion of overall non-wage labour costs in many countries, they are likely to be used in the future to boost employment, as they have been extensively used in the past. Our results suggest that a substantial cut in non-wage labour costs has an important and substantial effect.

Our paper contributes to the small but growing literature that uses large policy changes within a country over time or across groups to evaluate their labour market effects. Our analysis makes several advances over previous studies. Unlike many previous studies we present a taylor-made model that fits the salient features of the policy changes. On the empirical side, we provide new evidence on the wage effects of non-wage labour costs, and additional estimates on the employment effects. The data we use is a unique longitudinal data set which contains information on individual job histories from social security records and basic individual information from the census. Thus, we can work with all relevant job spells instead of quarterly data, as provided for instance by the Labour Force Survey. We use information on previous unemployment spells to overcome the sample selection problem we face when estimating the causal effects on wages, which results from those getting new employment not being a random sample.

The rest of the paper is structured as follows. Next we briefly describe the main changes brought about by the 1997 Spanish labour market reform, while Section 3 accommodates the salient features of the reform into a matching model with heterogenous workers. Section 4 explains our identification strategy and section 5 presents the data. Our main estimation results are reported in Section 6. Finally, section 7, summarises the main findings of the paper.

2 Institutional background

Employment protection legislation and especially firing costs have undergone substantial changes in the last twenty five years in Spain. In the early 1990s, nearly one third of overall employment in Spain was temporary –twice the European average–, and nearly all new hires signed temporary contracts (Guell and Petrongolo (2007)), which entailed lower severance payments than permanent contracts when separation took place earlier than agreed or nil when the termination date was observed, and whose termination could not be appealed. Such a rapid increase in temporary employment, brought about by a liberalisation in the use of temporary contracts that took place in 1984, led to a dual labour market (insider-outsider) and segmentation problems between unstable low-paying jobs and stable high-paying jobs (Dolado, García-Serrano, and Jimeno (2002)).

In order to increase the share of permanent employment, and after a first unsuccessful reform in 1994,[3] the 1997 reform substantially lowered firing costs for unfair dismissals

[3] The new regulations introduced with the 1994 reform restricted the use of temporary contracts to seasonal jobs and tried to reduce dismissal costs for permanent contracts by relaxing the conditions for 'fair' dismissals of workers under permanent contracts. In particular, the definition of 'fair' dismissal was widened by including additional 'economic reasons' for dismissals. However, as Dolado, García-Serrano, and Jimeno (2002) point out, in practice, not much changed: employers continued to hire workers under temporary contracts for all type of jobs —and not only for seasonal jobs—, and judges did not change

and payroll taxes to newly signed permanent contracts, when the worker belonged to certain population groups. In particular, severance payments for unfair dismissals were cut by about 25% and payroll taxes fell between 40 and 90% for new permanent contracts of workers younger than 30 years old, over 45 years old, the long-term unemployed, women under-represented in their occupations, and disabled workers. We only exploit the differential treatment by age group, since the long-term unemployed and women under-represented in their occupations may be self-selected, and disabled workers are a very distinct group which deserves a separate analysis. In particular, we study newly signed permanent contracts from unemployment. Conversions of temporary to permanent contracts after the second quarter of 1997 were also promoted with reductions in dismissal costs and payroll taxes for some population groups —see Appendix Table 9. However, since the reductions were very similar across age groups, identification of the effects becomes less clear-cut and therefore we will not use this group either. Table 1 shows the principal changes in key provisions introduced by the 1997 reform for the younger and older workers. Severance payment for targeted groups were reduced from 45 to 33 days' wages per year of seniority and the maximum time period was reduced by half, from 24 to 12 months. Reductions in payroll tax differ by age group; they fall by 60 and 40% for older and younger unemployed individuals, respectively for a period of 24 months. After the first 24 months, a lower payroll tax reduction of 50% is extended indefinitely only for individuals over 45 years of age.

Social security contribution rebates decreased slightly for newly signed contracts in 1999 and these changes were eventually extended in 2001.[4] These further changes in provisions, though minor, will condition our sample period to one year before and after the reform, i.e. 1996 and 1998 (see Section 4).

3 A theoretical framework

In order to analyze the wage effects of the 1997 reform, this section uses the matching model with heterogeneous workers put forth by Dolado, Jansen, and Jimeno (2007) with

their behaviour when appraising dismissals, despite the new regulations, i.e. dismissals under 'economic reasons' continued to be granted mainly when there was agreement between employers and workers, so labour courts continued to rule most dismissals as unfair.

[4]In particular, payroll taxes were reduced 35% in the first year and 25% in the second year for newly hired young unemployed workers under permanent contract, while reductions for older unemployed workers were 45% for the first year and 40% for the second one. Dismissal costs, however, did not change in 1999.

Table 1: **Principal Changes in Dismissal Cost and Payroll Tax due to the Labour Market Reform of 1997 which permit identification for Unemployed Workers**

		Dismissal cost under existing permanent contracts (pre-reform)	Dismissal cost under new permanent contracts (post-reform)	Payroll tax reductions for newly hired workers under permanent contracts after 1997
Treated groups	Young (<30 years)	45 days' wages per year of seniority with a maximum of 42 months' wages	33 days' wages per year of seniority with a maximum of 24 months' wages	40% of employer contribution for 24 months
	Older (>45 years)	45 days' wages per year of seniority with a maximum of 42 months' wages	33 days' wages per year of seniority with a maximum of 24 months' wages	60% of employer contribution for 24 months, 50% thereafter
Control group	Middle-aged (30-45 years)	45 days' wages per year of seniority with a maximum of 42 months' wages	45 days' wages per year of seniority with a maximum of 42 months' wages	None

two extensions. First, we illustrate the joint effects of payroll taxes and firing costs on wages. Second, since there are important differences in age between the two targeted groups of workers (less than 30 years and more than 45 years old, respectively), we introduce an exogenous job retirement probability in the model.

This labour market consists of a measure 1 of risk-neutral, infinitely-lived workers and a continuum of risk-neutral, infinitely-lived firms. Workers and firms discount future payoffs at a common rate δ and capital markets are perfect. In addition, time is discrete.

There are three type of workers, young (y), middle-age (m) and old (o) workers who can be either unemployed, employed or retired from the labour market. Every quarter, a proportion ϕ^j of each type of employed worker, $j = y, m, o$, leaves the labour market, replaced by and identical number of new workers who flow into the unemployment pool. Thus, the composition of the workers age population does not change among groups. We assume that this retirement happens upon reaching a certain age or when physical conditions do not allow to work any longer (by illness or accident).

There is a time-consuming and costly process of meeting unemployed workers and job vacancies. As in den Haan, Ramey, and Watson (2000), we assume that the meeting function takes the following form

$$M(u_t, v_t) = \frac{u_t v_t}{(u_t^{\varphi} + v_t^{\varphi})^{1/\varphi}}, \qquad \varphi > 0, \tag{1}$$

where u_t denotes the unemployment rate and v_t are vacancies. This constant-return-to-scale matching function ensures that ratios $M(u_t, v_t)/u_t$ and $M(u_t, v_t)/v_t$ lie between 0 and 1. Due to the CRS assumption they only depend on the vacancy-unemployment ratio θ_t. The former represents the probability at which unemployed workers meet jobs, $f(\theta_t) = M(1, 1/\theta_t)$. Similarly, the latter denotes the probability at which vacancies meet workers, $q(\theta_t) = M(\theta_t, 1)$. Each period, there is a proportion $\lambda_t^j = u_t^j/u_t$ of each type of workers looking for jobs.

Firms have a production technology that uses only labour. Each firm consists of only one type of job which is either filled or vacant. Before a position is filled, the firm has to open a job vacancy with cost c per period. A firm's output depends on aggregate worker's productivity A_t^j and a match-specific term z_t. We assume there is a productivity gap between each type of worker. The match-specific productivity term z_t is assumed to be independent and identically distributed across firms and time, with a cumulative distribution function $G(z)$ and support $[0, \bar{z}]$.

Firms may voluntarily terminate employment relationships, for which they may incur in a firing cost. In particular, firms lose γ^j when a match with a worker is destroyed by the firm. This cost is assumed to be fully wasted and not a transfer, reflecting firing restrictions imposed by the government. The second policy parameter is the wage payroll tax to be paid by the firm, τ^j.

The equations characterizing the value of vacancies, V_t, and filled positions, $J_t^j(z_t)$ are,[5]

[5] For exposition reasons, we omit writing the aggregate state variables $\{A_t, \theta_t\}$ as arguments of these value functions.

$$
\begin{aligned}
V_t &= -c + \lambda_t^y \delta \left[q(\theta_t) \int_{\tilde{z}_{t+1}^{y_H}}^{\bar{z}} J_{t+1}^y(z) dG(z) + [1 - q(\theta_t)(1 - G(\tilde{z}_{t+1}^{y_H}))] V_{t+1} \right] \\
&+ \lambda_t^m \delta \left[q(\theta_t) \int_{\tilde{z}_{t+1}^{m_H}}^{\bar{z}} J_{t+1}^m(z) dG(z) + [1 - q(\theta_t)(1 - G(\tilde{z}_{t+1}^{m_H}))] V_{t+1} \right] \\
&+ (1 - \lambda_t^y - \lambda_t^m) \delta \left[q(\theta_t) \int_{\tilde{z}_{t+1}^{o_H}}^{\bar{z}} J_{t+1}^o(z) dG(z) + [1 - q(\theta_t)(1 - G(\tilde{z}_{t+1}^{o_H}))] V_{t+1} \right], \quad (2) \\
J_t^j(z_t) &= A_t^j z_t - (1 + \tau^j) w_t^j(z_t) + \delta(1 - \phi^j) \left[\int_{\tilde{z}_{t+1}^{j_D}}^{\bar{z}} J_{t+1}^j(z) dG(z) + G(\tilde{z}_{t+1}^{j_D}) \left(V_{t+1} - \gamma^j \right) \right] \\
&\quad + \delta \phi^j V_{t+1}, \quad (3)
\end{aligned}
$$

where $\tilde{z}_{t+1}^{jH}$ and $\tilde{z}_{t+1}^{jD}$, $j = \{y, m, o\}$, are match-specific productivity thresholds, defined such that nonprofitable matches (i.e., with negative surplus) are severed. These thresholds or reservation productivities must satisfy the following conditions:

$$
\begin{aligned}
J_t^j(\tilde{z}_t^{j_H}) - V_t &= 0, \quad (4) \\
J_t^j(\tilde{z}_t^{j_D}) - V_t + \gamma^j &= 0. \quad (5)
\end{aligned}
$$

Expression (4) defines the the reservation productivity associated to the hiring process of unemployed workers who meet a vacant job. Note that in this case the firm is not entailed to γ in the absence of agreement since the job has not been created yet. In turn, (5) defines the reservation productivity for job destruction of existing positions. In this case, firing costs γ become operational.

It follows that each type of worker separate with probabilities,

$$
s_t^j = \phi^j + (1 - \phi^j) G(\tilde{z}_t^{jD}). \quad (6)
$$

On the workers' side, each type of unemployed worker gets b^j units of the consumption good each period, which could be understood as the value of leisure, home production, or unemployment benefit. Those who are employed earn a wage w_t^j. The values of the different statuses - unemployed, U_t^j and employed, $W_t^j(z_t)$ - are given by the following expressions:

$$
\begin{aligned}
U_t^j &= b^j + \delta \left[f(\theta_t) \int_{\tilde{z}_{t+1}^{jH}}^{\bar{z}} W_{t+1}^j(z) dG(z) + [1 - f(\theta_t)(1 - G(\tilde{z}_{t+1}^{jH}))] U_{t+1} \right], && (7)\\
W_t^j(z_t) &= w_t^j(z_t) + \delta \left[(1 - \phi^j) \left(\int_{\tilde{z}_{t+1}^{jD}}^{\bar{z}} W_{t+1}^j(z) dG(z) + G(\tilde{z}_{t+1}^{jD}) U_{t+1}^j \right) + \phi^j R_{t+1}^j \right]. && (8)
\end{aligned}
$$

For simplicity, we assume that workers retired only from employment and receive the same income as unemployed workers ($U_t^j = R_t^j$).

To close the model, we need first to incorporate two additional assumptions. One is the free entry condition for vacancies: firms will open vacancies until the expected value of doing so becomes zero. Therefore, in equilibrium we must have

$$
V_t = 0. \tag{9}
$$

The other assumption is that wages are set through Nash bargaining. The Nash solution is the wage that maximizes the weighted product of the worker's and firm's net return from the job match. The first-order conditions for each type of employees yield the following condition,

$$
(1 - \beta)(1 + \tau^j)(W_t^j(z_t) - U_t^j) = \beta(J_t^j(z_t) - V_t + \gamma^j). \tag{10}
$$

Defining the total surplus for each type of job as $S_t^j(z_t) = (1 + \tau^j)(W_t^j(z_t) - U_t^j) + (J_t^j(z_t) - V_t + \gamma^j)$, and using (2)-(10), the equilibrium wages are

$$
w_t^j(z_t) = (1-\beta)b^j + \frac{\beta}{(1+\tau^j)} \left[\beta A_t^j z_t + [1 - \delta(1 - \phi^j)]\gamma^j + \delta f(\theta_t)(1 - \beta) \int_{\tilde{z}_{t+1}^{jH}}^{\bar{z}} S_{t+1}^j(z) dG(z) \right], \tag{11}
$$

where

$$
\begin{aligned}
S_t^j(z_t) &= A_t^j z_t - (1 + \tau^j)(1 - \beta)b^j + [1 - \delta(1 - \phi^j)]\gamma^j - \delta f(\theta_t)\beta \int_{\tilde{z}_{t+1}^{jH}}^{\bar{z}} S_{t+1}^j(z) dG(z) \\
&\quad + (1 - \phi^j)\delta \int_{\tilde{z}_{t+1}^{jD}}^{\bar{z}} S_{t+1}^j(z) dG(z) && (12)
\end{aligned}
$$

It is immediate to see that direct effects of firing costs γ and pay roll taxes τ go in opposite direction, so that the overall effect of the 1997 reform on permanent wages is entirely an empirical question. Payroll taxes decrease wages because they reduce the net share of the match product obtained by the worker as well as the match surplus in next periods. In contrast, job firing costs increase wages because they increase the workers 'implicit' bargaining power. Notice that the higher the labour market exit rate ϕ, the higher the effect of firing costs on wages. Thus, the job retirement probability introduces an extra amplification mechanism on wages in response to non-wage labour cost adjustments.

To fully characterize the dynamics of this economy, we need to define the law of motion for unemployment and the mass of employed workers (u_t^j and n_t^j). These evolve according to the following difference equations:

$$
\begin{aligned}
n_t^j &= n_{t-1}^j + f(\theta_{t-1})\lambda_t^j(1 - G(\tilde{z}_t^{jH}))u_{t-1}^j - s_t^j n_{t-1}^j && (13)\\
n_t &= n_t^y + n_t^m + n_t^o && (14)\\
u_t^j &= u_{t-1}^j + s_t^j n_{t-1}^j - f(\theta_{t-1})\lambda_t^j(1 - G(\tilde{z}_t^{jH}))u_{t-1}^j, && (15)\\
u_t &= u_t^y + u_t^m + u_t^o, && (16)\\
1 &= u_t + n_t, && (17)
\end{aligned}
$$

4 Identification strategy

In order to identify the impact of dismissal costs and payroll taxes on wages, we compare the change in mean wages of young and older employees holding a permanent contract in the current spell and who were unemployed in the previous spell before and after the 1997 reform, with the change in mean wages of middle age workers who got a permanent job from unemployment. That is, we exploit the variation over time and across age groups and use a difference-in-differences estimator. The identifying assumption requires that the difference between wages of treatment and control groups would not change in the absence of the reform. More formally,

$$E\{\tilde{w}_{pre}^T\} - E\{\tilde{w}_{pre}^C\} = E\{\tilde{w}_{post}^T\} - E\{\tilde{w}_{post}^C\}$$

where $\tilde{w}$ is the counterfactual wage in absence of the reform, superscript $j = T, C$ indicates treatment or control group and subscripts *pre* and *post* refer to pre- and post-reform periods.

In the empirical analysis, we identify the average effect of the reform on wages as:

$$\beta_{DID} = (E\{w^T_{post}\} - E\{w^T_{pre}\}) - (E\{w^C_{post}\} - E\{w^C_{pre}\}) \tag{18}$$

where w is actual wages. The identification strategy is illustrated in Figure 1, which plots average wages for men and women by age group relative to the second quarter of 1997, for the years before and after the reform, i.e. 1995 to 1999. Figure 1 shows a marked change in the growth rate of average wages of the treatment groups, after the reform. That is, after the second quarter of 1997 average wages of younger and older workers increase much faster than those of the control group, and the increase is larger for men and for the older age group.

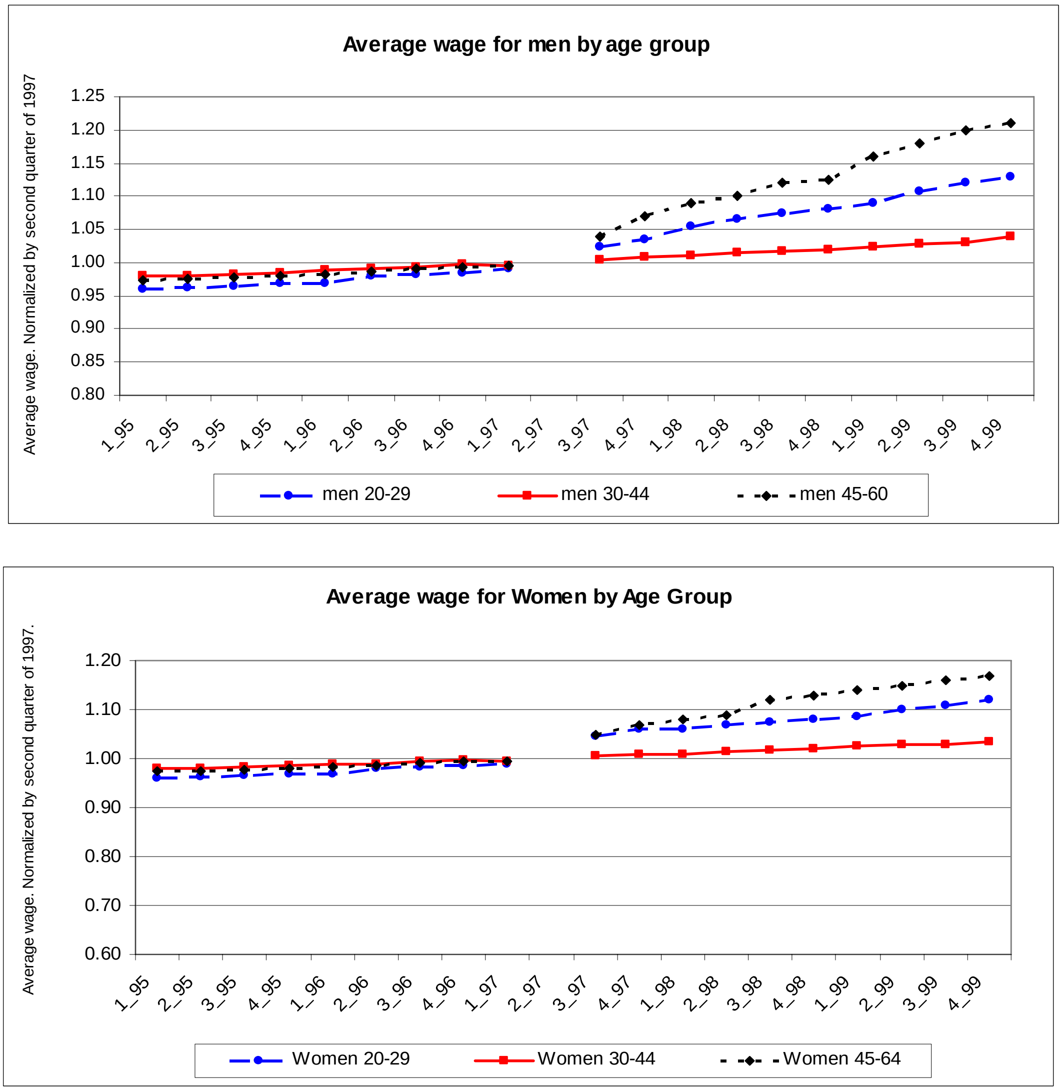
As treatment and control groups consist of individuals who make a transition from unemployment to permanent employment, they are likely not to be a random sample since some individual characteristics may determine the probability of entering permanent employment from unemployment. We take account of this sample selection problem with a two-step Heckman type correction, and identify the first step (i.e. the probability of making a transition to permanent employment from unemployment) with two variables that characterize the unemployment history of the individual: number of unemployment spells prior to the transition and unemployment duration over all spells.

We estimate the effect of the reform on wages with the following wage equation:

$$W_{it} = \alpha_0 + \alpha_1 D_t + \alpha_2 D_i + \alpha_{31} time + \alpha_{32} D_i \times time + \beta^{'} D_i \times D_t + X^{'}\gamma + \delta\lambda + \theta_i + \epsilon_{it} \tag{19}$$

where W_{it} is the log of gross monthly wage, D_i is a vector of dummies for treated groups (i.e. workers who make a transition to permanent employment from unemployment and are aged less than 30 or older than 45 years) and D_t is a vector of dummies that identify the post-reform years. The variable *time* is a time-trend, so α_{31} captures the evolution of wages over time, the impact of macro shocks affecting wages in both treated and control groups, while α_{32} allows for different age-group specific slopes. The vector X includes time-varying covariates such as education, occupation. The coefficients of interest in this regression are the βs, which represent the treatment effects; that is, capture the effects of the reform on wages in the years after the reform. Finally, λ is the selection coefficient,

Figure 1: **Wage trend for treated and control groups in our sample**



Source: MCVL, own calculations.

which derives from the following first stage linear probability model:

$$Pr[e_{it}=1|X_{it}]=\Lambda[\mu_o+\mu_1 D_t+\mu_2 D_i+\mu_{31}time+\mu_{32}D_i\times time+\theta' D_i\times D_t+X'\gamma+Z'\eta] \quad (20)$$

where $e_{it}=1$ if individual i transits from unemployment to permanent employment and $e_{it}=0$ otherwise. The vector Z includes the two variables that help identify this first step regression, that is, the number of unemployment spells prior to the transition and unemployment duration over all spells.

Our strategy assumes that employers do not substitute workers not affected by the reform for targeted workers. However, if the change in provisions brought about by the reform is perceived as beneficial by employers, they will tend to substitute non-targeted workers (our control group) for targeted workers (our treatment group) who are otherwise deemed similar. To see whether the assmumption holds, Table 2 presents pre- and post-reform employment probabilities for individuals with ages adjacent to the relevant age thresholds, i.e. 30 and 45 years. If employers substituted workers, pre- and post-reform employment probabilities for control group workers would fall. Table 2 shows that employment probabilities for these workers do not change significantly, which suggests that the possible substitution of workers is not likely to affect our results. To further check whether substitution is a problem we estimate the effects on employment of the reform with the sample restricted to the narrower defined age treatment and control groups. Results of these regressions are shown in Table 10 in the Appendix. If substitution took place then we would find larger effects in the restricted sample. Estimates of Table 10 and Table 8 show that this is not the case: the effects of the reform on employment probabilities estimated with the restricted sample are quite similar to (and usually slightly smaller than) the effects obtained with the whole sample.[6]

Table 2: **Pre- and Post-reform employment probabilities for a restricted sample**

	Men		**Women**	
Age	Pre-reform	Post-reform	Pre-reform	Post-reform
27	50.2%	56.0%	45.2%	49.1%
28	53.1%	59.0%	46.7%	50.1%
29	55.0%	62.0%	48.7%	52.1%
30	60.3%	60.5%	51.2%	52.3%
31	62.8%	63.0%	55.9%	55.7%
32	64.0%	63.9%	56.1%	57.2%
42	70.1%	70.7%	65.1%	66.1%
43	71.3%	71.8%	66.3%	66.8%
44	71.2%	71.1%	67.4%	68.0%
45	73.5%	76.3%	67.9%	70.3%
46	73.0%	76.9%	69.3%	74.2%
47	74.1%	79.1%	70.1%	73.6%

[6]The only exception is the unemployment to permanent employment transition probability of older women.

5 Data and methodological decisions

We employ a unique administrative dataset with Social Security records called Countinuous Sample of Job Histories (Muestra Continua de Vidas Laborales, MCVL) for the year 2005, which consist of a random sample of 4% of all affiliated workers, working or not, and pensioners from the Social Security archives. This dataset contains detailed job-related information on the complete job history of of 1,142,118 individuals, which include labour market status and type of contract for each and every job spell. The MCVL is very rich and detailed as regards job histories, but lacks information on basic individual characteristics. To this end, we match the MCVL and municipal information (padrones) and recover individual information on sex, education and age.

Our sample selection is as follows. First, we study men and women aged between 21-60 to select out the two ends of the labour career. Second, we only use job spells posterior to 1993, since prior to that year information on type of contract is not reliable. Third, we drop incomplete or incorrect registers. Forth, we consider workers who are in the "Regimen General", which includes 90 per cent of all workers; i.e. we exclude the self-employed, workers in Agriculture, Fishing and other minor special cases.[7] To avoid capturing the effects of the 1999 reform, we compare the year prior to the reform (1996) with the year after the reform (1998). Sensibility checks are performed with slightly wider time windows (i.e.1995-1996 and 1998-1999), but results do not change substantially (see Appendix Table 11).

The wage measure is the log of gross monthly wage or salary, deflated by the consumer price index.

Tables 3 and 4 provide summary statistics by relevant age groups of our sample for men and women separately. Descriptive statistics are presented for the period before and after the 1997 Reform. The last three rows suggest that the probability of getting a permanent contract or to make a transition from temporary to permanent employment might have increased after the reform and especially so for the youngest age group. In the Appendix A.1 we investigate whether such figures can be taken at face value or, on the contrary, are rather misleading.

The matched MCVL has important advantages over other data sets which have been

[7] This is common practice in the few studies that use the MCVL (e.g. García-Pérez and Rebollo-Sanz (2009)) and is also the choice of Kugler, Jimeno, and Hernanz (2002) when studying the employment effects of the reform using the Spanish Labour Force Survey.

Table 3: Descriptive Statistics by Age Group, Pre- and Post-Reform for **Men**

Variable	Age<30		Age 30-45		Age>45	
	Pre	Post	Pre	Post	Pre	Post
Wages	1017.33	1138.80	1308.47	1397.11	1399.34	1548.18
Log wages	6.925	7.048	7.177	7.242	7.240	7.345
Age	25.15	24.96	36.57	36.15	51.94	52.25
% Incomplete Primary Education	12.80	16.07	21.59	25.84	45.18	50.12
% Primary Education	43.47	45.68	35.50	39.42	28.24	28.12
% Secondary and Technical Education	37.19	32.89	35.55	28.65	20.47	17.09
% University	6.54	5.37	7.36	5.99	6.01	4.68
% with Permanent Contract	43.89	53.28	75.54	76.69	82.07	83,81
% with Transitory Contract	56.11	46.67	24.46	23.28	18.93	16.19
Unemployment spells	4.54	4.66	3.24	3.67	2.35	2.75
Unemployment duration	1017.63	897.64	1123.21	1075.13	1409.93	1228.51
N	26,443	70,394	49,950	102,144	26,973	50,867

Table 4: Descriptive Statistics by Age Group, Pre- and Post-Reform for **Women**

Variable	Age<30		Age 30-45		Age>45	
	Pre	Post	Pre	Post	Pre	Post
Wages	987.55	1045.78	1305.56	1399.76	1377.76	1401.76
Log wages	6.90	6.95	7.17	7.24	7.23	7.25
Age	24.94	24.93	36.28	35.97	51.76	51.70
% Incomplete Primary Education	7.98	8.22	19.3	18.10	49.52	48.85
% Primary Education	33.57	33.98	35.15	35.62	28.22	31.06
% Secondary and Technical Education	47.80	47.04	35.66	36.06	17.16	15.55
% University	10.65	10.76	9.89	10.21	5.1	4.55
% with Permanent Contract	40.38	41.81	67.35	70.49	70.79	73,52
% with Transitory Contract	59.62	58.19	32.65	29.51	29.21	26.48
Unemployment spells	4.90	4.97	3.83	4.08	2.90	3.32
Unemployment duration	1071.63	942.17	1224.94	1163.55	1517.24	1362.98
N	30,886	118,854	34,969	103,510	10,940	28,602

employed in previous studies. For instance, as compared with the Spanish Labour Force Survey (Encuesta de Población Activa, EPA), used by Kugler, Jimeno, and Hernanz (2002) to examine the effects of the reform on employment, the MCVL contains information on wages for each job spell, which allows us to examine the effects on wages, for first time. Secondly, the MCVL provides information on each and every single job spell and not only at the time of the interview, as typically occurs with other large and representative surveys such as the European Community Household Panel (ECHP), the EU Survey on Income and Living Conditions (EU-SILC), or the Labour Force Surveys, which eliminates the possibility of aggregation bias. The time-span of the MCVL, however, is not long enough as to cover more than one economic cycle, and thus cycle effects cannot be taken account of in the empirical analysis.

6 Wage Effects of the 1997 Reform

As pointed out in the Introduction, we present two sets of complementary evidence on the effects of the 1997 reform. We first present short-term microeconometric estimates (Section 6.1) and then longer-term evidence which results from calibrating and simulating the model of Section 3 (Section 6.2). Simulations will also allow us to calculate the separate effect of dismissal costs and payroll taxes.

The distinction between short-term and long-term effects is the following: While the difference-in-difference estimates only capture the first year effects of the 1997 reform, the simulation of our theoretical model predicts the cumulative impact when all workers belonging to the treatment population groups are directly affected by the reform. That is, simulated long-term effects assume that the new levels of firing costs and payroll taxes apply to all employed workers in the treatment groups, and not only to newly signed permanent contracts.

6.1 Short-term microeconometric estimates

Table 5 reports the estimates of interest of the wage equation (19) in the upper panel and of the selection equation (20) in the lower panel, for men and women separately. The effect of the reform on wages is captured by the coefficients β on the interaction $(D_i \times D_t)$, which is positive and statistically significant for the two treatment groups and both genders. This means that the reduction in dismissal costs and payroll taxes results in a sizeable wage increase for the two treated groups as compared to the control group. The increase is larger for the older group than for the younger one and smaller for women than for men. More precisely, we find a 4.6% wage increase for young unemployed men transiting to permanent contract; the increase for women of the same age is lower (2.5%). For the older unemployed workers doing the same transition, wages increase an 8% and 6% for men and women, respectively.

Table 5: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Wage equation				
Age<30	-0.328	-32.01	-0.251	-18.98
Age>45	0.063	5.11	0.047	2.42
(Age<30)*(Post 1997)	0.046	3.37	0.025	2.54
(Age>45)*(Post 1997)	0.078	4.63	0.061	2.57
Selection coeff (λ)	23.655	5.27	1.173	18.5
Selection equation				
Age<30	-0.080	-11.14	-0.127	-13.89
Age>45	-0.034	-3.99	0.041	2.95
(Age<30)*Reform	0.042	4.53	0.014	3.25
(Age>45)*Reform	0.053	13.62	0.021	8.39
Unemployment spells	-0.015	-31.01	-0.009	-17.65
Duration	-0.0001	-24.54	-0.0001	-30.70

Notes: Control group are men and women aged 30 to 45 years.

Controls have the expected sign. For instance, age dummies (D_i) show a monotonic and positive relationship between age and wages. Full estimates of the wage and selection regressions are shown in Appendix Table 12.

Selection into the relevant transition from unemployment to permanent employment is indeed not random, but positive, i.e. unobservables are positively correlated with both doing the transition and wages, as indicated by the positive and statistically significant δ. The two coefficients of the variables that identify selection into the relevant transition η are negative and statistically significant. That is, a larger number of unemployment spells or longer overal time in unemployment reduces the probability of signing a permanent contract from unemployment.

6.2 Long-term effects: Calibration and simulated results of the model

In this section we quantify the cumulative impact in relative wages when all workers of each target group are assumed to be directly affected by the reform. To this end, we first calibrate the model presented in section 3 at quarterly frequencies just before the 1997 labour market reform. Then we departure from the initial setup by reproducing the observed reduction in firing costs and payroll tax in the targeted age-groups during the 1997 reform. Finally, we analyze the simulated long-term post reform effects on the level of wages of each target group with respect to the non targeted group of workers (m). Thus, the simulated results complement the short-run estimated effects presented in section 6.1 by predicting the long-term impact on wages.

6.2.1 Benchmark calibration: Before the reform

Our benchmark parametrization must match the following targets in the steady state, which are summarized in the upper part of Table 6. The first three targets consist of the average unemployment rates for workers with less than 30 years old, $u^y = 34.7\%$, between 30 and 45 years old, $u^m = 18.0\%$, and more than 45 years old, $u^o = 12.3\%$. We also target the wage differential among these groups. Thus, $w^y/w^m = 0.777$ and $w^o/w^m = 1.070$.

With respect to the calibration of our parameters, we set the discount factor $\delta = 0.99$, which implies a reasonable quarterly interest rate of nearly 1 percent. The workers' bargaining power β is set to 0.5. Petrongolo and Pissarides (2001) identify an elasticity of unemployment with respect to the matching function in the range 0.5-0.7. We take 0.5 as reference and thus set the matching parameter φ at 0.552.

In Spain the standard age of retirement is 65, although retirement at earlier ages is common due to disability. In Spain, the average age for those workers older than 45 years was around 54 in 1996. In turn, Blanco (2000) estimates an exit probability of 0.04 from employment due to job retirement and disability for workers near this age. Thus, we set $\phi^o = 0.040$. Moreover, using data from the Spanish National Institute of Statistics (INE), we calculate a quarterly exit rate from employment of 1.8% for young workers and 1.2% for middle age employees due to illness or accident. So, the labour market exit probability for these two age-groups are $\phi^y = 0.018$ and $\phi^m = 0.012$, respectively. Following Benito and Hernando (2008) we set the payroll tax at 0.27 for all groups. Thus,

Table 6: **Benchmark Calibration. Spain, 1996**

		Value	Source
Targets:			
Unemployment rate (< than 30 years old)	u^y	0.347	[A]
Unemployment rate between 30 and 45 years old	u^m	0.180	[A]
Unemployment rate > than 45 years old	u^o	0.123	[A]
Wage gap for young workers	$\frac{w^y}{w^m}$	0.777	[A]
Wage gap for old workers	$\frac{w^0}{w^m}$	1.070	[A]
Parameters:			
Aggregate labour productivity > than 45 years old	A^o	1.000	Normalized
Aggregate labour productivity between 30 and 45	A^m	0.90	[C]
Aggregate labour productivity < than 30 years old	A^y	0.695	[C]
Mean of log z	μ	0.000	Normalized
Standard deviation of log z	σ_z	0.20	[D]
Discount rate	δ	0.990	[A]
Exogenous retirement probability > than 45 years old	ϕ^o	0.040	[B]
Exogenous retirement probability between 30 and 45	ϕ^m	0.012	[A]
Exogenous retirement probability < than 30 years old	ϕ^y	0.018	[A]
Employment opportunity cost < than 30 years old	b^y	0.549	[C]
Employment opportunity cost between 30 and 45	b^m	0.691	[C]
Employment opportunity cost > than 45 years old	b^o	0.228	[C]
Employers payroll tax	τ^j	0.270	[B]
Cost of vacancy	c	0.030	[C]
Parameter of the Matching function	φ	0.552	[D]
Worker's bargaining power	β	0.50	[D]
Firing tax parameter < than 30 years old	γ^y	$0.26w^y$	[A,B]
Firing tax parameter between 30 and 45	γ^m	$0.87w^m$	[A,B]
Firing tax parameter > than 45 years old	γ^o	$1.78w^o$	[A,B]

Note: [A] Own calculation based on original data; [B] Other studies;
[C] Obtained from model to match the targets; [D] Own assumption

$\tau^y = \tau^m = \tau^o = 0.27$.

Next we turn to the firing costs γ^j. We first estimate the total severance payments (SP^j) for permanent contracts using the following information from Osuna (2005): (i) 20 days of wages per year of seniority for legal indemnities in fair dismissals with a maximum of 12 monthly wages; (ii) 45 days of wages per year of seniority for unfair dismissals with a maximum of 42 monthly wages dismissals; (iii) the mean job tenure X^j for each worker-age group; (iv) procedural wages of around two monthly wages; and (v) the fact that 72% of all firing processes were declared unfair in 1996. In turn, Garibaldi and Violante (2005) estimate a tax component between 19% and 34% of total firing costs, depending on the layoff scenario. We consider the most conservative of these scenarios and set it equal to 20%. Thus, the firing tax component amounts to near 24% of severance payments. According to García-Serrano and Jimeno (1999), the average job tenure in 1996 was one year for employees younger than 25 years, 7 years for those workers between 25 and 45 years, and 16 years for employees older than 45 years old. Thus, the firing tax components for each group of worker amounts to $\gamma^y = 0.26 \times w^y$, $\gamma^m = 0.87 \times w^m$ and $\gamma^o = 1.78 \times w^o$.[8]

Following the standard assumption in the literature, as in den Haan, Ramey, and Watson (2000), the idiosyncratic productivity z_t is assumed to be log-normally distributed with mean μ and standard deviations σ_z. We normalize the mean of $\log z_t$ to zero, $\mu = 0$. With respect to σ_z, the literature provides a range of values between 0.1 (den Haan, Ramey, and Watson (2000)) and 0.4 (Trigari (2004)) and we choose an intermediate case, $\sigma_z = 0.2$. See also Burgess and Turon (2005); and Walsh (2005), who use values within this range. We also normalized the aggregate labour productivity for the group of workers with more than 45 years old, $A^o = 1.00$, and fix $A^y = 0.695$ and $A^m = 0.90$ to match the observed wage gap among these workers.

Finally, the hiring cost c is calibrated together with the employment opportunity costs b^j. We select these parameters such that in the steady-state job creation is equal to job destruction and equilibrium satisfies our remaining calibration targets: $u^y = 34.7\%$, $u^m = 18.0\%$, $u^o = 12.3\%$. This yields $c = 0.030$, $b^y = 0.549$, $b^m = 0.691$ and $b^o = 0.228$.

[8] For X^j years of job tenure severance payments in days are SP^j=$0.72 \times X^j \times 45$ days per year + $0.28 \times X^j \times 20$ days per year + 60 days. Thus, the quarterly firing tax calculation amounts to $\gamma^j = \frac{SP^j}{90} \times 0.24 \times w^j$.

Table 7: **Simulated effects on wages of the 1997 reform**

The 1997 reform	w^y/w^m	w^o/w^m
Before the reform	0.777	1.070
After the reform	0.841	1.233
Simulated variation (%)	8.24	15.23
Estimated short-term post reform variation.		
First panel of Table 5 (%)	4.63	7.78
No reduction in τ		
Before the reform	0.777	1.070
After the reform (modified)	0.779	1.063
Variation (%)	0.26	-0.65

6.2.2 Long-term wage effects

The first principal change in legislation reduced severance payments by around 20% (33 days of wages per year of seniority, with a maximum of 24 monthly wages, rather than 45 days of wages per year of seniority with a maximum of 42 monthly wages in case of unfair dismissal). Thus, the firing tax parameters for workers under 30 and over 45 years of age and with new permanent contracts can be reduced to $\gamma^y = 0.238 \times w^y$ and $\gamma^o = 1.413 \times w^o$, respectively.[9] Since the reform did not change severance payments in the middle aged group, their firing tax component remains unchanged. Thus, $\gamma^m = 0.87 \times w^m$.

The second main modification of the reform was a reduction of 40% and 60% in the payroll tax for workers under 30 and over 45 years of age, respectively. Thus, τ^y and τ^o are reduced from 0.27 to 0.16 and 0.11, respectively while τ^m remains unchanged at 0.27.

This simulation takes into account the changes experienced by wages in terms of the steady-state values. The results of this exercise are displayed in the first panel of Table 7.

The simulated reform yields an increase in the relative wage of the two target groups,

[9]For $i = y, o$, the calculations are: SP^i=0.72$\times XX^i$ years$\times$**33 days** per year + 0.28$\times XX^i$ years$\times$20 days per year + 60 days. Thus, the quarterly firing tax calculation amounts to $\gamma^i = \frac{SP^i}{90} \times 0.24 \times w^i$

which is larger for workers older than 65 years. The simulated ratios w^o/w^m and w^y/w^m are increased by 15.23% and 8.24%, respectively. These results are in line with the estimated shorter-term effects reported in section 6.1. Moreover, the simulated post reform scenario yields larger longer term effects than the estimated short term results. For instance, the estimated short-term wage effect for young unemployed men who do a transition to permanent contract is a wage increase of around 5% during the reform period relative to their middle-aged counterpart, while for older unemployed men who do the same transition wages increase around 8% (see Table 5).

Since our simulated model captures the total cumulative effects of the reform, we can say that, during the first year of the reform, the estimated increase in the relative wages of young and old workers accounts for over half of the predicted long-term effects – 56 and 51 percent respectively. The partial transmission of the adjustment in the non-wage labour costs occurs because the estimated short-term causal effects pick up the effect of the change in provisions for newly signed permanent contracts, whereas the simulations capture the effects that would obtain if the change in provisions affected all employed workers belonging to the treatment groups. Recall that it is not possible to identify and estimate the mid- and long term effects of the 1997 reform because in 1999 a new labour market reform was introduced in Spain. Thus, the long-term effects of the reform can only be predicted through the simulation of the theoretical model.

The simulation also permits to separately identify and quantify the effects of each policy change. That is, we can compute the impact of changing either firing costs or payroll taxes. To calculate the impact of reducing solely firing costs, we simulate a scenario with no reduction in payroll taxes (i.e. $\tau_y = \tau_m = \tau_o = 0.27$), keeping the rest of post-reform parameters constant The results of this exercise are presented in the bottom panel of Table 7. According to our model (see equation 11), the direct effect of job firing costs increases wages because these costs increase the workers implicit bargaining power. That is, in the absence of an indirect effect, we should expect a reduction in the wages of treated groups due to the reduction of severance payments. In contrast, our findings show that the (overall) effect of firing costs on wages is nil, suggesting that the indirect effects offset the direct effects of firing costs. An immediate corolary of this finding is that the increase in relative wages of young and older workers with respect to middle aged workers is mostly accounted for by the large reduction in payroll taxes.

7 Final remarks

This paper provides short- and long-term empirical evidence of the effect on wages of two important elements of non-wage labour costs, using a labour market reform in Spain which reduced firing costs and payroll taxes after 1997 for certain population subgroups.

To gain a theoretical insight into the effects of these two provisions we extend the matching model with heterogeneous workers (Dolado, Jansen, and Jimeno (2007)) to accommodate the salient features of the reform. Our model predicts an ambiguous effect on wages, since the effect of firing costs and payroll taxes go in opposite directions. Firing costs are expected to increase wages, through their increased bargaining power of workers, while the effect of payroll taxes is negative.

For the empirical analysis we use a unique longitudinal data set, which contains information on individual job histories from social security records and basic individual characteristics from the census. Since we have information on each and every single job spell, we avoid the possibility of aggregation bias.

Our empirical strategy exploits the substantial reduction in firing costs and payroll taxes brought about by the 1997 Spanish labour market reform for young and old workers who got a permanent job from unemployment. Since the changes did not cover all workers, we use a difference-in-difference estimator to obtain short-term causal effects. The possible sample selection bias that arises because firing cost and labour tax reductions apply only to workers transiting from unemployment to permanent employment is addressed with a two-step Heckman correction model. The first step of the model is identified with information on previous unemployment spells. Identification of the causal effects of the reform may be threatened if employers substitute workers not affected by the reform for targeted workers. We show that substitution of workers does not take place. Our estimates suggest that decreased firing costs and payroll taxes have a positive effect on wages (and also on unemployment). We find larger effects for older than for younger workers and for men than for women.

The long-term evidence comes from calibrating the model and simulating the reform, and shows the cumulative effect in the hyopothetical scenario where the reduced firing costs and payroll taxes affect all employed workers in the treatment groups, rahter than only newly signed permanent contracts. Simulated long-term effects are larger than estimated short-run ones, and their comparison shows that about 50 percent of the predicted cumulative increase on wages takes place during the first year of the reform. Finally, our simulations also show that the increase in wages is mostly due to the reduction in payroll taxes, and that the overall effect of firing costs is nil because direct and indirect effects of firing costs offset each other.

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A Appendix

A.1 Employment Effects of the Reform

Although the paper focuses on wage effects we also evaluate the impact on employment. Employment effects are interesting *per se* –e.g. the main objective of the reform was to promote permanent employment– and also allow us to compare our results with previous studies that use survey quarterly data (LFS) instead of administrative records on job spells. To this end, we compare the change in employment probabilities before and after the reform of treated and control groups. That is we estimate a linear probability model similar to (20), where now $e_{it} = 1$ if the individual is employed with a permanent contract and $e_{it} = 0$ otherwise. As for the wage equation (19), the θs capture the employment effects of the reform in the years after the reform for the two treatment groups.

The upper panel of Table 8 reports probit marginal effects estimates using equation 20 to assess whether the reduction in dismissal costs and payroll taxes increased permanent employment. The results show a positive effect of the reform on permanent employment probabilities for younger and older male and female workers relative to middle-aged workers. The effect is larger for the younger than for the older: permanent employment probabilities increased by 2.5% for the older (both men and women) and by 4.5% and 3% for younger men and women, respectively. Our estimates are similar to those reported by Kugler, Jimeno, and Hernanz (2002), who examine employment effects of the reform with LFS data. They also find positive but smaller positive effects for younger workers (of roughly half our size), but insignificant effects for older workers. The discrepancy may be explained by the nature of the data sets (the MCVL being administrative records and including all job spells while the LFS being quarterly survey data) and the cyclical effects, which we do not control for.

The lower panel of Table 8 shows results for flows from unemployment to permanent employment. Now the dependent variable of equation (20) takes the value of one if the individual does the transition from unemployment to permanent employment and zero otherwise. The results show increased transitions from unemployment to permanent employment of younger and older workers relative to middle-aged workers, as result of the reform. Such increase is now larger for older workers: transitions increased by 2% and

1.6% for men and women, while for youger workers the increase was about 0.6%.[10]

Table 8: Employment and Transition Probabilities for Men and Women

	Men		Women	
	Coefficient	t-stat.	Coefficient	t-stat.
	Permanent Employment			
Age < 30	-0.307	51.43	-0.235	-97.20
Age > 45	-0.168	54.75	0.147	32.85
(Age < 30) × (Post 1997)	0.047	15.72	0.031	9.68
(Age > 45) × (Post 1997)	0.025	6.76	0.024	4.75
N	650,674		355,578	
Log-likelihood	-356,480.69		-238,122.44	
	Unemployment to Permanent Employment			
Age < 30	-0.008	-8.18	-0.019	-14.95
Age > 45	-0.002	2.60	-0.015	6.70
(Age < 30) × (Post 1997)	0.006	4.31	0.007	4.17
(Age > 45) × (Post 1997)	0.020	9.47	0.016	5.61
N	398,333		226,333	
Log-likelihood	-168,989.81		-110,478.41	

Notes: The Table reports **probit** marginal effects. Control group are individuals aged 30 to 45 years.

[10]As compared to Kugler, Jimeno, and Hernanz (2002), our estimated effects on this transition are smaller for younger workers but larger for older workers (theirs being not statistically significant). However, Kugler, Jimeno, and Hernanz (2002) examine a different transition, which includes out of the labour market as well as unemployment as origin state of the transition.

A.2 Main changes in dismissal costs and payroll taxes Due to the 1997 Reform for temporary workers

Table 9: **Principal Changes in Dismissal Cost and Payroll Tax due to the Labour Market Reform of 1997 which permit identification for Temporary Contracts**

		Dismissal cost under existing permanent contracts (pre-reform)	Dismissal cost under new permanent contracts (post-reform)	Payroll tax reductions for newly hired workers under permanent contracts after 1997
Treated group	Older (>45 years)	45 days' wages per year of seniority with a maximum of 42 months' wages	33 days' wages per year of seniority with a maximum of 24 months' wages	60% of employer contribution for 24 months, 50% thereafter
Control group	Young and Middle-aged (≤45 years)	45 days' wages per year of seniority with a maximum of 42 months' wages	33 days' wages per year of seniority with a maximum of 24 months' wages	50% of employer contribution for 24 months

A.3 Do employers replace non-targeted for targeted workers?

Table 10: Employment and Transition Probabilities for Men and Women (restricted sample)

	Men		Women	
	Coefficient	t-stat.	Coefficient	t-stat.
	Permanent Employment			
Age $<$ 30	-0.191	-53.49	-0.141	-33.41
Age $>$ 45	0.146	17.37	0.111	9.29
(Age $<$ 30) $\times$ (Post 1997)	0.034	4.82	0.029	3.53
(Age $>$ 45) $\times$ (Post 1997)	0.021	2.63	0.021	3.11
N	221,173		145,319	
Log-likelihood	-128,614.63		-87,306.55	
	Unemployment to Permanent Employment			
Age $<$ 30	-0.006	-8.09	-0.012	-5.62
Age $>$ 45	-0.004	-2.58	-0.014	-3.00
(Age $<$ 30) $\times$ (Post 1997)	0.005	4.39	0.006	3.11
(Age $>$ 45) $\times$ (Post 1997)	0.017	10.22	0.028	3.93
N	68,173		46,324	
Log-likelihood	-58,997.51		-37,683.79	

Notes: The Table reports **probit** marginal effects estimated on a sample of individuals aged 27 to 32 years and 42 to 47 years.

A.4 Sensibility checks with wider time windows (2 years)

Table 11: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment (**95-96 vs 98-99**)

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Wage equation				
Age<30	-0.303	-11.17	-0.221	-8.98
Age>45	0.116	4.54	0.037	2.47
(Age<30)*(Post 1997)	0.031	3.97	0.026	2.65
(Age>45)*(Post 1997)	0.068	4.72	0.059	3.66
Selection coeff (λ)	21.771	5.63	2.284	19.5
Selection equation				
Age<30	-0.285	-12.25	-0.215	-7.23
Age>45	-0.131	-4.88	0.027	4.84
(Age<30)*Reform	0.026	5.42	0.025	3.36
(Age>45)*Reform	0.053	14.73	0.047	7.34
Unemployment spells	-0.020	-32.22	-0.011	-18.54
Duration	-0.002	-14.64	-0.001	-29.60

Notes: All coefficients are significant at 5%. Control group are men and women aged 30 to 45 years.

A.5 Full estimates of wage and selection regressions

Table 12: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Wage equation				
Age<30	-0.328	-32.01	-0.251	-18.98
Age>45	0.063	5.11	0.047	2.42
(Age<30)*(Post 1997)	0.046	3.37	0.025	2.54
(Age>45)*(Post 1997)	0.078	4.63	0.061	2.57
(a)Education				
Unknown	0.139	4.70	0.050	1.34
Primary incomplete	0.038	1.28	0.194	5.22
Secondary incomplete	0.215	7.24	0.404	10.84
Secondary completed	0.253	6.94	0.522	12.68
Graduate	0.550	16.84	0.702	17.32
Postgraduate and doctorate	0.662	11.69	0.888	13.96
(b)Occupation				
Agriculture and Fish Emp.	0.117	9.53	0.406	2.90
Extractive industry	0.290	11.47	0.716	5.64
Alimentary industry	0.420	12.59	0.985	7.64
Plant and machine operatives	0.997	8.85	0.694	5.37
Administrative employees	0.986	8.78	0.584	4.62
Transport employees	0.997	9.48	0.809	6.37
Education and sanitary emp.	0.852	8.14	0.523	4.14
Other social activities	0.349	7.50	0.631	4.98
Professional	0.916	3.09	0.395	3.10
Selection coeff (λ)	23.655	5.27	1.173	18.5
Selection equation				
Age<30	-0.080	-11.14	-0.127	-13.89
Age>45	-0.034	-3.99	0.041	2.95
(Age<30)*Reform	0.042	4.53	0.014	3.25
(Age>45)*Reform	0.053	13.62	0.021	8.39
Unemployment spells	-0.015	-31.01	-0.009	-17.65
Duration	-0.0001	-24.54	-0.0001	-30.70
(a)Education				
Unknown	0.005	0.26	0.038	0.68
Primary incomplete	0.013	0.70	0.018	0.46
Secondary incomplete	0.062	3.22	0.012	0.95
Secondary completed	0.105	4.39	0.027	3.84
Graduate	0.211	9.83	0.107	5.70
Postgraduate and doctorate	0.266	6.91	0.254	4.79
(b)Occupation				
Agriculture and Fish Emp.	0.336	4.40	0.443	5.06
Extractive industry	0.377	5.16	0.414	3.17
Alimentary industry	0.375	5.11	0.265	1.18
Plant and machine operatives	0.075	1.03	0.099	4.21
Administrative employees	0.465	6.37	0.344	1.82
Transport employees	0.301	4.12	0.150	0.20
Education and sanitary emp.	0.075	1.02	0.017	0.23
Other social activities	0.011	0.15	0.019	2.31
Professional	0.322	4.39	0.210	2.55

Notes: Control group are men and women aged 30 to 45 years.