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Wage Effects of Non-wage Labour Costs*

María Cervini-Plá † Xavier Ramos ‡§ José Ignacio Silva ¶

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Abstract

We study 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 who got a permanent job. A matching model with heterogeneous workers predicts positive wage effects of reducing firing costs but ambiguous wage effects of reducing payroll taxes, for both new entrant and incumbent workers. Difference-in-differences estimates and simulation of the model show positive wage effects for both new entrant and incumbent workers. The reduction in firing costs accounts for up to half of the overall wage increase for new entrants but only 10 per cent for incumbents.

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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 have 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 regulation affects employment flows (Autor, Donohue, and Schwab (2004, 2006); Kugler and Pica (2003, 2008); Marinescu (2009); Martins (2009)). However, evidence on wage effects is very scarce and not very conclusive. Leonardi and Pica (2014) analyse an increase in firing costs implemented in Italy for small firms and find that more stringent employment protection has a negative impact on entry and subsequent wages. Martins (2009) finds no reaction of wages to an increase in dismissal-for-cause costs only for large firms in Portugal, and van der Wiel (2010) finds positive wage effects of extending employer's term of notice in the Netherlands.[2]

The incidence of payroll taxes also gathers mixed evidence. Generally speaking, when

[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]The extent to which severance costs are shifted from employers to employees in the form of lower wages depends on maket imperfections or information problems. For instance, in situations of low institutional trust, the cost of severance pay may not be entirely shifted to workers under the standard severance pay system, where employers are required to pay severance at the time of separation, due to a problem of moral hazard, if workers fear that firms will declare bankruptcy and will not pay. Kugler (2005) studies the effect of a new system of severance payment savings account in Colombia, where firms are required to deposit a percentage of wages into guaranteed individual accounts available to workers in the event of job separation, and finds a substantial wage reduction that results from reducing the moral hazard problem.

employees perceive 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 (see Gruber (1997) for Chile or Bennmarker, Mellander, and Öckert (2009) for Sweeden). However, with a loose link between taxes and benefits, payroll taxes are usually not fully passed on to employees and employment decreases.[3] In a recent study for Argentina, Cruces, Galiani, and Kidyba (2010) exploit regional variations in tax rates and find that changes in payroll tax rates are only partially shifted onto wages with negligible effects on employment, due to rigid labour demand and supply functions. Small changes have also been found easier to pass on to employees than large changes (Gruber (1997)).

Our analysis focusses on the wage effects of firing costs and payroll taxes.[4] To do so, we extend 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 consider the joint effect of payroll taxes and firing costs on wages, and since the reform basically targets the entry wage of two groups of workers, we consider a different wage bargaining process for new entrants than for incumbent workers.

The theoretical model predicts an ambiguous effect on wages for both new entrant and incumbent workers. While payroll taxes have an unclear effect on wages, firing costs in new entry positions reduce wages. This result takes place in new entrants because, since firing costs are not operational in entry jobs, firms can translate part of them to the new jobs, reducing the workers 'implicit' bargaining power. In the case of incumbents, an increase in firing costs of new entry positions decrease their wages because they expect a lower match surplus in case of moving to a new job position. As a result, incumbent employees are more willing to decrease their current wage in order to reduce the probability of being separated from the firm.

We provide two sets of complimentary evidence, from estimations and from simulations, which yield consistent results. Estimates come from a microeconometric analysis of panel individual administrative records, while simulations are obtained by first calibrating the model and then simulating the reform.

[3]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.

[4]We also discuss briefly the implications of changing firing costs and payroll taxes on employment. In Cervini Plá, Ramos, and Silva (2010), the companion and more extensive working paper, we provide a more detailed discussion.

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 the wages (and employment) of new hired workers. Estimated effects are larger for older than for younger workers and for men than for women. Wages of incumbent workers also increase, but to a lesser extent. Our simulations show that the reduction in firing costs accounts for up to half of the overall wage increase for new entrants but only 10 per cent for incumbents.

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. The source of identification in this paper, the age discontinuity, is unique compared to previous literature examining the impact of firing costs or payroll taxes on wages, which has relied on firm size discontinuities (Martins (2009), Leonardi and Pica (2014)), differences in tenure (van der Wiel (2010), Marinescu (2009)) or regional differences (Bennmarker, Mellander, and Öckert (2009), Cruces, Galiani, and Kidyba (2010)). 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 selection into treatment problem we face when estimating the causal effects on wages, which results from those getting new permanent employment not being a random sample of the unemployment pool. Moreover, our theoretical model fits the salient features of the policy change and helps disentangle the impact of firing costs and payroll taxes.

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,[5] the 1997 reform substantially lowered firing costs for unfair dismissals[6] and payroll taxes to newly signed permanent contracts, when the worker belonged to certain population groups. This is the so-called Permanent Emploment Promotion (PEP) contract. In particular, severance payments for unfair dismissals were cut by about 25%

[5]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 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.

[6]Workers can be dismissed on disciplinary grounds, due to improper individual behaviour, such as misconduct or unjustified absenteeism, or objective grounds, related to economic, technical or organisational reasons. If a dismissal is found to be unfair, a Labour Court can order the employer to re-instate an employee with immediate effect, or alternatively to pay her a compensation payment based on the length of service with the employer. The 1997 Reform substantially reduced such compensation payment.

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, long-term unemployed women who entered under-represented 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 received treatment irrespective of their age, 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 14. 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. Furthermore, unemployment is the main source of entry into permanent employment –as many as 75 per cent of all new permanent contracts come from unemployment–, while conversions from temporary employment are a much smaller share of new permanent hirings. 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.

Not all firms could use Permanent Employment Promotion (PEP) contracts to hire new permanent workers after the reform, and thus benefit from the reduction in firing costs and payroll taxes introduced by the reform. Firms that dismissed workers for 'objective' reasons but that were proved wrong in court or that engaged in collective dismissals over the 12 moths prior to the reform, could not use PEP contracts when signing new permanent contracts. To take due account of this, our sample selects only newly signed PEP contracts (see Section 5). Furthermore, we will use the sample of newly signed permanent contracts that do not use PEP contracts as control group in an alternative identification strategy, in order to check the robustness of our findings (see Section 6.1.5).

Social security contribution rebates decreased slightly for newly signed contracts in

Table 1: **Principal Changes in Dismissal Cost and Payroll Tax for Unfair Dismissals, due to the 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

1999 and these changes were eventually extended in 2001.[7] 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 two extensions.[8] First, we illustrate the joint effects of payroll taxes and firing costs on wages. Second, since the reform basically targets the entry wage of two groups of workers (less than 30 years and more than 45 years old, respectively), we consider a different wage bargaining process for new entrants than for incumbents workers. This

[7]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. The 2001 reform applied lower payroll tax reductions and did not modify dismissal costs.

[8]Although the reform targeted age-worker groups, we do not include a complete life-cycle model because this reform had a limited duration of two years. Thus, we only analyze its short-run impact on wages.

distinction is relevant because the firm does not incur in firing costs when the firm and the worker do not agree on a wage in the first encounter since a contract has not been signed yet. This second assumption permits deliver theoretical predictions specific to the entry wage, which is the main dependent variable of the micro estimates. Finally, to avoid capturing the effects of a different reform introduced in 1999, and thus be consistent with our empirical estimations, we consider a model where the 1997 reform only modified the non-wage labour costs of new hired workers with permanent contract and keep unchanged the firing costs and payroll taxes of incumbent employees.

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 elderly (e) workers who can be either unemployed or employed. The employed can be either new entrants or incumbents. Thus, there are six type of employed workers who earn w_{0t}^j and w_t^j, where subscript 0 indicates new entrants and superscript $j = y, m, e$ denotes the age-group of workers. 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 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. 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 $[0, \bar{z}]$.

Every period, a proportion ϕ^j of each type of employed worker separate exogenously from the employment status and flow into the unemployment pool. Firms may voluntarily terminate employment relationships, for which they may incur in a firing cost. In particular, firms lose γ_0^j or γ^j when a match with a new entrant or and incumbent worker is destroyed by the firm, respectively.[9] In both cases, a proportion ψ of this cost is assumed to be a transfer to the worker in form of severance payment whereas the rest $(1-\psi)$ is assumed to be fully wasted, reflecting firing restrictions imposed by the government.[10] The second policy parameter is the wage payroll tax to be paid by the firm, which is τ_0^j and τ^j for new entrant and incumbent positions, respectively.

The equations characterizing the value of vacancies, V_t, and filled positions for new jobs, $J_{0t}^j(z_t)$ and incumbent jobs $J_t^j(z_t)$ are, [11]

$$\begin{aligned}
V_t &= -c + \lambda_t^y \delta \left[q(\theta_t) \int_{\tilde{z}_{0t+1}^y}^{\bar{z}} J_{0t+1}^y(z) dG(z) + [1 - q(\theta_t)(1 - G(\tilde{z}_{0t+1}^y))] V_{t+1} \right] \\
&+ \lambda_t^m \delta \left[q(\theta_t) \int_{\tilde{z}_{0t+1}^m}^{\bar{z}} J_{0t+1}^m(z) dG(z) + [1 - q(\theta_t)(1 - G(\tilde{z}_{0t+1}^m))] V_{t+1} \right] \\
&+ (1 - \lambda_t^y - \lambda_t^m) \delta \left[q(\theta_t) \int_{\tilde{z}_{t+1}^e}^{\bar{z}} J_{0t+1}^e(z) dG(z) + [1 - q(\theta_t)(1 - G(\tilde{z}_{0t+1}^e))] V_{t+1} \right], \quad (2) \\
J_{0t}^j(z_t) &= A_t^j z_t - (1+\tau_0^j) w_{0t}^j(z_t) + \delta(1-\phi^j) \left[\int_{\tilde{z}_{t+1}^j}^{\bar{z}} J_{t+1}^j(z) dG(z) + G(\tilde{z}_{t+1}^j) \left(V_{t+1} - \gamma_0^j \right) \right] \\
&\quad + \delta \phi^j V_{t+1}, \quad (3) \\
J_t^j(z_t) &= A_t^j z_t - (1+\tau^j) w_t^j(z_t) + \delta(1-\phi^j) \left[\int_{\tilde{z}_{t+1}^j}^{\bar{z}} J_{t+1}^j(z) dG(z) + G(\tilde{z}_{t+1}^j) \left(V_{t+1} - \gamma^j \right) \right] \\
&\quad + \delta \phi^j V_{t+1}, \quad (4)
\end{aligned}$$

where $\tilde{z}_{0t+1}^j$ and $\tilde{z}_{t+1}^j$, $j = \{y, m, e\}$, are match-specific productivity thresholds, defined such that nonprofitable matches (i.e., with negative surplus) are severed. These thresholds

[9] In this section, we assume that firing costs are constant along the duration of the contract. In the calibration exercise, however, we will assume that severance payments change with the average duration of the job position.

[10] We introduce the wasted firing costs component to avoid the 'bonding critique' (Lazear, 1990). Therefore, our model allows the presence of employment effects after the reform.

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

or reservation productivities must satisfy the following conditions:

$$J_{0t}^{j}(\widetilde{z}_{0t}^{j}) - V_t = 0, \tag{5}$$

$$J_{t}^{j}(\widetilde{z}_{t}^{j}) - V_t + \gamma^j = 0. \tag{6}$$

Expression (5) defines 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_0^j in the absence of agreement since the job has not been created yet. In turn, (6) defines the reservation productivity for job destruction of incumbent positions. In this case, firing costs γ^j are operational.

It follows that each type of worker separate and find jobs with probabilities,

$$s_t^j = \phi^j + (1-\phi^j)G(\widetilde{z}_t^j), \tag{7}$$

$$\chi_t^j = f(\theta_{t-1})(1 - G(\widetilde{z}_{0t}^j)). \tag{8}$$

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. The values of the different statuses –unemployed, U_t^j, new hired $W_{0t}^j(z_t)$ or incumbent, $W_t^j(z_t)$– are given by the following expressions:

$$U_t^j = b^j + \delta\left[f(\theta_t)\int_{\widetilde{z}_{0t+1}^j}^{\bar{z}} W_{0t+1}^j(z)dG(z) + [1 - f(\theta_t)(1 - G(\widetilde{z}_{0t+1}^j))]U_{t+1}\right], \tag{9}$$

$$\begin{aligned} W_{0t}^j(z_t) &= w_{0t}^j(z_t) + \delta\left[(1-\phi^j)\left(\int_{\widetilde{z}_{t+1}^j}^{\bar{z}} W_{t+1}^j(z)dG(z) + G(\widetilde{z}_{t+1}^j)\left(U_{t+1}^j + \psi\gamma_0^j\right)\right)\right] \\ &+ \delta\phi^j U_{t+1}^j, \end{aligned} \tag{10}$$

$$\begin{aligned} W_t^j(z_t) &= w_t^j(z_t) + \delta\left[(1-\phi^j)\left(\int_{\widetilde{z}_{t+1}^j}^{\bar{z}} W_{t+1}^j(z)dG(z) + G(\widetilde{z}_{t+1}^j)\left(U_{t+1}^j + \psi\gamma^j\right)\right)\right] \\ &+ \delta\phi^j U_{t+1}^j. \end{aligned} \tag{11}$$

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

$$V_t = 0. \tag{12}$$

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. It is known that this form of wage bargaining produces flexible wages. We decide to adopt it because the relevant wages for the reform are wages of newly hired workers with permanent contracts. Along this line, recent evidence by Haefke, Sonntag, and van Rens (2009) and Pissarides (2009) suggest that wages in new jobs display similar variability than the one obtained from a Nash wage equation in the search model. Moreover, using also the Spanish Social Security data, De la Roca (2008) also finds that wages of newly hired workers are substantially more volatile than those of ongoing employees.[12]

The first-order conditions for new and incumbent employees yield the following conditions,

$$(1-\beta)(1+\tau_0^j)(W_{0t}^j(z_t) - U_t^j) = \beta(J_{0t}^j(z_t) - V_t), \tag{13}$$

$$(1-\beta)(1+\tau^j)(W_t^j(z_t) - U_t^j - \psi\gamma^j) = \beta(J_t^j(z_t) - V_t + \gamma^j). \tag{14}$$

Note that the Nash condition for the incumbents displays two extra terms depending on γ^j. Since separation costs are operational in continuing jobs, they are explicitly considered in the wage negotiation. This implies that the firm's threat point when negotiating with an incumbent is no longer the value of a vacancy V_t, but $(V - \gamma^j)$; and that the worker's threat point depends on the proportion of firing costs (ψ) obtained in case of disagreement. In the case of new entrants, the term γ_0^j does not appear in condition (13) because the firm does not have to pay it if the new job position is not finally created. Defining the total surplus for new and incumbent jobs as,

$$S_{0t}^j(z_t) = (1+\tau_0^j)(W_{0t}^j(z_t) - U_{0t}^j) + (J_{0t}^j(z_t) - V_t), \tag{15}$$

$$S_t^j(z_t) = (1+\tau^j)(W_t^j(z_t) - U_t^j - \psi\gamma^j) + (J_t^j(z_t) - V_t + \gamma^j), \tag{16}$$

[12]To check the robustness of our results, we also include a simulated scenario with wage rigidity in ongoing jobs.

and using (2)-(14), the equilibrium wage equation for new entrants and incumbents are

$$
\begin{aligned}
w_{0t}^{j}(z_t) &= (1-\beta)b^{j} - \left(1-\phi^{j}\right)\delta\left(1-\beta\right)\psi\gamma_0^{j} - \frac{\beta}{(1+\tau_0^{j})}\left(1-\phi^{j}\right)\delta\gamma_0^{j} \\
&\quad + \frac{\beta}{(1+\tau_0^{j})}\left[A_t^{j}z_t + \delta f(\theta_t)(1-\beta)\int_{\tilde{z}_{0t+1}^{j}}^{\bar{z}} S_{0t+1}^{j}(z)dG(z)\right], \qquad (17)\\
w_{t}^{j}(z_t) &= (1-\beta)b^{j} + \left[1-\left(1-\phi^{j}\right)\right]\delta\left(1-\beta\right)\psi\gamma^{j} + \frac{\beta}{(1+\tau^{j})}\left[1-\left(1-\phi^{j}\right)\right]\delta\gamma^{j} \\
&\quad + \frac{\beta}{(1+\tau^{j})}\left[A_t^{j}z_t + \delta f(\theta_t)(1-\beta)\int_{\tilde{z}_{0t+1}^{j}}^{\bar{z}} S_{0t+1}^{j}(z)dG(z)\right], \qquad (18)
\end{aligned}
$$

where

$$
\begin{aligned}
S_{0t}^{j}(z_t) &= A_t^{j}z_t - (1+\tau_0^{j})b^{j} - \delta\left(1-\phi^{j}\right)\left[1-\left(1+\tau_0^{j}\right)\psi\right]\gamma_0^{j} \\
&\quad - \delta f(\theta_t)\beta\int_{\tilde{z}_{0t+1}^{j}}^{\bar{z}} S_{0t+1}^{j}(z)dG(z) + (1-\phi^{j})\delta\int_{\tilde{z}_{t+1}^{j}}^{\bar{z}} S_{t+1}^{j}(z)dG(z), \qquad (19)
\end{aligned}
$$

According to equation (17), it is immediate to see that firing costs in new permanent contracts, γ_0^j, decrease the wages of new hired workers. This result takes place because, although these costs are not operational at the entry level jobs, firms can translate part of them to the new entry positions, reducing the workers 'implicit' bargaining power.[13] Notice that the higher the proportion of severance payments in total firing costs ψ, the larger the negative effect of firing costs on w_{0t}^j. In contrast, the effect of payroll taxes, τ_0^j, on entry wages is not clear since the right hand side element in equation (17) interacting with firing costs increases with τ_0^j, while the one not interacting with γ_0^j decreases with payroll taxes in new hired positions.

Wages in incumbent jobs are also affected by γ_0^j and τ_0^j. In this case, the reform affects the future surplus of incumbent workers moving to a new job position, S_{0t+1}^j. According to equation (19), γ_0^j decreases the future surplus in new positions and, therefore, the wage of incumbent workers (18). Since incumbent workers know that entry wages decrease with γ_0^j , they are more willing to reduce their current wage to decrease the probability of being separated from the firm. As for new entrants, the effect of τ_0^j on incumbent wages w_t^j is not clear since there are two terms in S_{0t}^j that go in opposite directions.

In sum, according to our theoretical model the overall wage effect of the 1997 reform on

[13]Notice that this result implies that new hired workers in permanent positions earn, on average, less than incumbent workers, which is consistent with our own wage gap estimates across age groups, which range from 26% to 40%.

both new entrants and incumbent wages is not clear and it is thus entirely an empirical question. On the one hand, the reduction in firing costs in new created job positions, γ_0^j, should increase both w_{0t}^j and w_t^j. On the other hand, the reduction in payroll taxes generates an unclear effect on both type of wages.

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:

$$
\begin{aligned}
n_t^j &= n_{t-1}^j + f(\theta_{t-1})\lambda_t^j(1 - G(\widetilde{z}_{0t}^j))u_{t-1}^j - s_t^j n_{t-1}^j && (20)\\
n_t &= n_t^y + n_t^m + n_t^e && (21)\\
u_t^j &= u_{t-1}^j + s_t^j n_{t-1}^j - f(\theta_{t-1})\lambda_t^j(1 - G(\widetilde{z}_{0t}^j))u_{t-1}^j, && (22)\\
u_t &= u_t^y + u_t^m + u_t^e, && (23)\\
1 &= u_t + n_t, && (24)
\end{aligned}
$$

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 prime 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:

$$\beta_{DID} = (E\{w^T_{post}\} - E\{w^T_{pre}\}) - (E\{w^C_{post}\} - E\{w^C_{pre}\}) \tag{25}$$

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. The graphical evidence of Figure 1 thus hints clearly at a strong and clear effect of the reform.

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

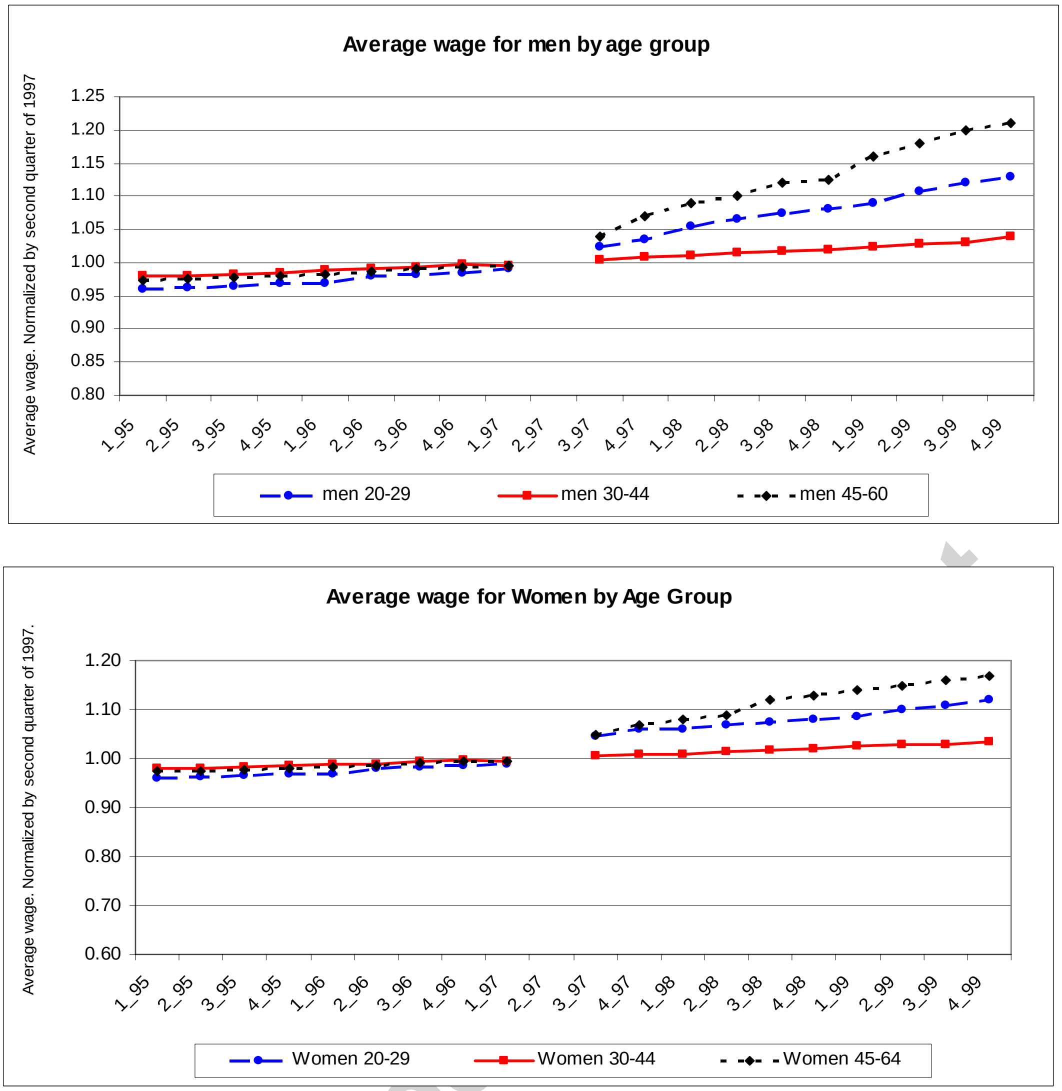
$$W_{it} = \alpha_0 + \alpha_1 D_t + \alpha_2 D_i + \beta' D_i \times D_t + X'\gamma + \epsilon_{it} \tag{26}$$

where W_{it} is the log of average gross monthly earnings for those who transit from unemployment to permanent contract, 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)[14] and D_t is a vector of dummies that identify the post-reform years. The vector X includes time-varying covariates such as professional category, experience, industry, whether working full- or part-time, private or public sector job, firmsize in the current and previous job spells, regional effects, type of contract in the previous job spell, number of permanent contract held previously, having received unemployment benefits in the last unemployment spell, and duration of the last unemployment spell. 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.

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-

[14]The excluded category is then the control group, which comprises workers aged 30 to 45 who transit from unemployment to permanent employment.

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



Source: MCVL, own calculations.

targeted workers (our control group) for targeted workers (our treatment group) who are otherwise deemed similar. To see whether the assumption 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 estimated the effects on employment of the reform with the sample restricted to workers with ages close to the policy thresholds, i.e.

tratment groups include only workers aged 27 to 29 and 45 to 47, and the control group includes only workers aged 30 to 32 and 42 to 44. If substitution took place then we would find larger effects in the restricted sample. Results of these regressions, presented in Cervini Plá, Ramos, and Silva (2010), 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.[15]

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

	Men		**Women**	
Age	Pre-reform	Post-reform	Pre-reform	Post-reform
27	70.1%	76.0%	44.9%	49.1%
28	72.3%	79.2%	45.5%	50.1%
29	75.8%	80.7%	47.6%	52.1%
30	77.6%	81.5%	50.2%	52.3%
31	78.3%	83.0%	52.9%	55.7%
32	78.4%	83.1%	53.2%	57.2%
42	79.1%	80.3%	41.2%	46.7%
43	80.1%	81.5%	41.3%	46.8%
44	82.3%	81.2%	42.6%	48.2%
45	83.1%	86.4%	43.8%	50.4%
46	83.5%	86.8%	45.3%	52.3%
47	84.6%	87.2%	46.1%	53.5%

Table 2 also shows that employment increases in treated groups after the reform.[16] Workers who were hired because of the reform but who would had not been hired otherwise, are likely to be lower productivity workers and thus earn lower wages. We expect this selection to play against our results and introduce a downward bias. Hence, our estimates may be interpreted as a lower bound estimate of the true effects.

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

[16]Kugler, Jimeno, and Hernanz (2002) reach the same conclusion when examining the employment effects of the reform with data from the Labour Force Survey. Section 6.1.2 also reports positive employment effects of the reform for our sample.

5 Data and methodological decisions

We employ a unique administrative dataset with Social Security records called Continuous Sample of Job Histories (Muestra Continua de Vidas Laborales, MCVL) for the years 2004 to 2012, 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 1,692,308 individuals, which include labour market status and type of contract for each and every job spell.[17] The MCVL is very rich and detailed as regards job histories, and it also has information on basic individual characteristics, such as sex, education and age, which come from municipal registries (padrones).

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 drop the long-term unemployed[18] and disabled workers, since they all received treatment irrespective of their age. Third, we drop incomplete or incorrect registers. Fourth, we consider workers who are in the general scheme (Regimen General) when making the relevant transition from unemployment to permanent employment, which includes 90 per cent of all workers.[19] Fifth, as outlined in Section 2 not all firms could use Permanent Employment Promotion contracts to hire new permanent workers after the reform. Since the MCVL has information on the exact contract of the worker, we employ only PEP contracts in the analysis, which covers 78.3% of newly signed permanent contracts from unemployment for workers younger than 30 and older than 45 in 1998. 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). Sensitivity checks are performed with slightly wider time windows (i.e.1995-1996 and 1998-1999), but results do not change substantially (see Appendix Table 15).

The wage measure is the log of average gross monthly wage or salary, deflated by the

[17]Since the dataset contains information also on pensioners, we do *not* face attrition problems due, for instance, to the larger likelihood of workers with poorer employment performance and lower wages to exit the labour market sooner.

[18]Long-term unemployment includes unemployed with unemployment spells longer than 12 months.

[19]We thus only exclude workers in self-employment, agriculture, fishing and other minor special cases, when making the relevant transition. When *not* making the relevant transition, the above types of workers are included in the analysis. Using workers who are in the general regime 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.

consumer price index. As it often occurs with Social Security records, wages in the MCVL are top- and bottom-coded, that is, they are censored. Although for the entire sample this is a significant problem (Bonhomme and Hospido (2009)), such an issue is likely not to be empirically relevant in our case as wages are censored only for very few observations.[20]

Tables 3 and 4 provide summary statistics for the period before and after the 1997 reform by relevant age groups of our sample, and for men and women separately.

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	1114.67	1376.65	1384.56	1436.89	1465.23	1628.33
Log wages	7.02	7.23	7.23	7.27	7.29	7.40
Age	25.11	25.16	36.06	36.26	51.70	51.60
% Unskilled jobs	69.35	64.58	60.93	58.25	62.67	62.25
% Semi-skilled workers and semi-skilled clerks	23.51	25.65	24.13	23.48	20.57	19.77
% Engineers and graduates, chief and dep. heads	7.15	9.76	14.94	18.27	16.76	17.98
Unemployment spells	6.90	6.42	12.51	12.93	15.68	14.49
Experience (in days)	1445.79	1301.61	4257.29	4328.89	7661.83	7941.70
N	4,928	5,975	5,175	5,301	1,780	2,745

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

[20]There are hardly any bottom-coded observations in our sample, while top-coded wages represent between 0.16% and 0.85% of the sample, depending on the year and age/gender group. Such small incidence is likely to be due to the fact that individuals in our sample have experienced a recent spell of unemployment, so their wages are less likely to be affected by top-coding. Nonetheless, we have also estimated our main parameters of interest of Table 5 with censored models and have found very similar results. Top-coding is more prevalent among incumbent workers, see Section 6.1.4.

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	1006.89	1173.78	1223.17	1256.41	1141.87	1322.70
Log wages	6.91	7.07	7.11	7.14	7.04	7.19
Age	25.04	25.17	36.25	36.36	51.58	51.31
% Unskilled jobs	44.99	40.13	52.97	52.65	74.21	72.01
% Semi-skilled workers and semi-skilled clerks	47.14	50.38	32.89	32.42	20.99	21.26
% Engineers and graduates, chief and dep. heads	7.87	9.49	14.14	14.94	4.80	6.73
Unemployment spells	7.13	6.41	19.46	19.58	24.26	23.84
Experience (in days)	1199.82	1146.18	3292.83	3354.17	3702.48	4272.64
N	1,719	3,127	3,904	4,763	1,620	3,238

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 microeconometric estimates (Section 6.1) and then evidence which results from calibrating and simulating the model of Section 3 (Section 6.2). Difference-in-differences estimates will yield results for newly hired and incumbent workers,[21] men and women separately, while results from simulations provide effects on average wages across gender. Simulations will also allow us to calculate the separate effect of dismissal costs and payroll taxes.

6.1 Microeconometric estimates

Table 5 reports the estimates of interest of the wage equation (26), 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

[21] Incumbent workers are those hired before the reform.

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 6.5% wage increase for young unemployed men transiting to a permanent contract; the increase for women of the same age is lower (4.5%). For the older unemployed workers doing the same transition, wages increased 9.4% and 7.7% for men and women, respectively.[22] The larger effect on wages for the older group may reflect the somewhat larger reduction in firing costs, which applied to this age group (see Table 1), as according to the simulations of our model shown in Section 6.2.2, the reduction in firing costs accounts for over 60% of the overall wage increase of new entrants. Likewise, it is reasonable to find larger wage effects for the older group irrespective of the gender, as changes in both firing costs and payroll taxes are the same across gender groups.

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

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Age<30	0.047	1.72	0.036	1.12
Age>45	-0.024	-0.66	0.127	2.91
Post 1997	0.079	4.47	0.075	3.63
(Age<30)*(Post 1997)	**0.065**	2.66	**0.045**	3.43
(Age>45)*(Post 1997)	**0.094**	2.85	**0.077**	2.07
N	25,904		18,371	

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

Controls have the expected sign. For instance, experience, working full-time, in the private sector or firmsize shows a positive effect on wages, while the duration of the last unemployment spell has a negative influence on wages. Full estimates of the wage regressions are shown in Appendix Table 16.

These estimates are unbiased for the group of workers who go from unemployment

[22]Recall that long-term unemployed and disabled workers were dropped from the sample because all individuals belonging to these two groups receive treatment irrespective of their age.

to permanent employment, and thus are relevant to assess the effect on wages of policies which seek to promote employment by changing employment protection legislation provisions, which is normaly the case. Notice, however, that since there may be selection into treatment, the estimates of Table 5 may be biased if applied to the whole active population. The next section addresses such selection.

6.1.1 Selection into treatment

We only observe wages of the unemployed who obtain a permanent job, and these may differ in important unmeasured ways from the unemployed that are not hired with a permanent contract. The latter are arguably less productive, have lower bargaining power and thus earn lower wages. This selection, thus, may introduce an upward bias. 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 the number of unemployment spells prior to the transition into permanent employment. It is difficult to find a convincing exclusion restriction for our case and this is the best option available in our data set. We thus assume that the number of previous unemployment spells affects the probability of getting a permanent job from unemployment but that conditional on selection it does not affect wages. There is a substantial amount of evidence of state dependence scarring effects in individual unemployment histories (see Arulampalam, Booth, and Taylor (2000), Biewen and Steffes (2010), Rebollo-Sanz (2011) and Ayllón (2013) for recent studies for Spain), which should explain a negative effect of the number of unemployment spells on the probability of finding permanent employment. It may be argued, that our exclusion restriction captures in part unobserved productivity and it is thus correlated with wages. We however contend that this is less likely to occur in our case, as our empirical analysis uses entry wages. Importantly, our exclusion restriction does not correlate with entry wages, when it is included in our baseline wage equation (26).[23] Moreover, our exclusion restriction is not correlated with the duration of the last unemployment spell, a variable which is often used to capture unobserved productivity.[24] We thus expect that conditional on selection,

[23]Our exclusion restriction, i.e. the number of previous unemployment spells, is neither statistically significant (p-value of 0.45 for men and 0.68 for women) nor economically important (point estimate very close to zero, i.e. -0.005, for men, and -0.002 for women).

[24]The correlation coeffcient between number of unemployment spells and the duration of the last unemployment spell is 0.037 and 0.032 for men and women, respectively.

unobserved productivity does not affect entry wages much –and certaintly much less than longer-term wages.

We use a probit model for the first stage of the Heckman procedure,

$$Pr[e_{it} = 1|X_{it}] = \Phi[\mu_o + \mu_1 D_t + \mu_2 D_i + \theta^{'} D_i \times D_t + X^{'}\vartheta + Z^{'}\eta] \quad (27)$$

where $e_{it} = 1$ if individual i transits from unemployment to permanent employment and $e_{it} = 0$ otherwise. Z is the exclusion restriction, that is, the number of unemployment spells prior to the transition.

Table 6 shows the estimates of interest for the selection and the wage equations. For men, selection into the relevant transition from unemployment to permanent employment is negative, i.e. unobservables are negatively correlated with both doing the transition and wages, and the coefficient of the number of unemployment spells η, which identifies selection into the relevant transition, is negative and statistically significant –see Appendix Table 17 for the full set of estimates of the wage and selection regressions. Selection is positive for women, however, which introduces a downward bias.[25] Such positive selection is entirely driven by low skilled older women, i.e. selection is negative for younger women and intermediate and high skilled older women, as it is for men.[26]

Once we correct for selection, the wage effects of the reform are not much different from those reported in the previous section, and because of the negative selection for men and the positive selection for women, the wage effects of the reform converge between genders. The reform brings about a wage increase of 6.0% and 4.7% for young men and women, respectively, and an increase of 8.8% and 7.7% for older male and female workers,[27] which are statistically significant according the the corrected standard errors. That is, as we pressumed, the selection brings about a slight upward bias for men and a slight downward bias for women.

To check the robustness of our estimates of interest to different exclusion restrictions,

[25]This is also the case when we extend the sample period to include two years around the reform date.

[26]We have estimated the two-stage Heckman procedure for each professional category separately and found selection to be positive only for low skilled older women. Results are available from the authors upon request.

[27]Since the two treatment variables appear in both the selection and outcome equations, the marginal effects (and their standard errors) are not simply given by the second stage estimates reported in the upper panel of Table 6. Instead, they are given by two components, the direct effect of the reform on wages of those making the transition from unemployment to permanent employment and the indirect effect through an increased probability of making the transition (see Greene (2008), p. 885).

Table 6: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment. Heckman selection model (two stage method)

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
1. WAGE EFFECTS				
Uncorrected wage effects (wage equation)				
(Age<30)*(Post 1997)	0.059	2.39	0.049	3.44
(Age>45)*(Post 1997)	0.088	2.65	0.080	2.08
N	25,904		18,371	
Selection coefficients				
Inverse Mill's ratio (λ)	-0.103	-3.48	0.254	7.54
ρ	-0.113		0.389	
σ	0.911		0.880	
Corrected wage effects*				
(Age<30)*(Post 1997)	0.060	10.41	0.047	5.38
(Age>45)*(Post 1997)	0.088	15.37	0.077	8.85
2. EMPLOYMENT EFFECTS				
Employment effects (selection equation)				
(Age<30)*(Post 1997)	0.079	4.72	0.064	3.19
(Age>45)*(Post 1997)	0.119	4.95	0.083	4.40
Spells	-0.063	-108.34	-0.051	-79.42
N	277,394		181,432	
Marginal prob. of emp. effects at the mean				
(Age<30)*(Post 1997)	0.009	3.71	0.011	3.15
(Age>45)*(Post 1997)	0.014	4.30	0.013	3.16

Notes: Control group are men and women aged 30 to 45 years. ρ is the correlation between error terms of the wage and selection equation. σ is the standard error of the residual in the wage equation.
*Since the two treatment variables appear in both the outcome and selection equations, the marginal effects and their standard errors are not simply given by the second stage estimates reported in the upper panel (see footnote 27).

we run the two-step Heckman procedure with two alternative exclusion restrictions: the type of contract held in the previous job spell and the number of permanent contracts held previously. The results shown in Appendix Table 18 indicate that when using these two alternative exclusion restrictions, there is no sample selection, as the statistically insignificant inverse Mills' ratio suggest. With no sample selection, then, our estimates are robust to using different exclusion restrictions.

6.1.2 Employment effects

A thorough analysis of the employment effects of the reform is beyond the scope of this paper. However, since the reform was meant to improve the permanent employment prospects of certain population groups, it is interesting to examine its employment effects. As we want to analyse the transition probability from unemployment to a permanent contract, we use duration models and estimate a specification with the same controls as the selection model in equation (27). This model shall provide a first insight into the employment effects of the reform. Table 7 reports the estimated hazard ratios of interest from a Cox proportional hazards model. In line with previous studies (e.g. Kugler, Jimeno, and Hernanz (2002)), our findings reveal that the reform has large, positive and significant permanent employment effects for the sample of new permanent hires with PEP contract. Such effects are slightly larger for younger than for older male workers –for whom permanent employment probabilies increase by 22 and 16 per cent, respectively–, and about the same for younger and older female workers –whose permanent employment probability increases by 18 per cent.

Table 7: Effects of the Reform on Employment. Hazard ratios of interest from a Cox proportional hazards model

	Men		Women	
	Hazard Rates	t-stat	Hazard Rate	t-stat
Age<30	1.057	1.66	0.991	-0.23
Age>45	1.118	2.60	1.451	6.90
Post 1997	0.791	-11.23	0.901	-4.05
(Age<30)*(Post 1997)	**1.219**	6.73	**1.177**	4.49
(Age>45)*(Post 1997)	**1.160**	3.81	**1.179**	4.37
N	277,394		181,432	

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

As expected, the reform has no permanent employment effects for the sample of new permanent hires with no PEP contract, as the interaction terms between the trated groups and the post reform dummy are not statistically significant (not shown).

6.1.3 Who benefits more? Heterogeneous effects of the Reform

As Dolado, Jansen, and Jimeno (2007) and the model in Section 3 suggests, the change in payroll taxes and firing costs brought about by the Reform is likely to have heterogenous effects possibly stemming from differences in the bargaining positions of workers vis-à-vis employers. Heterogeneous effects are estimated by interacting the variable that proxies the bargaining position of workers, and that will cause the heterogeneity, with the interaction variable that identifies the effect of the reform. That is, we estimate the following extended wage regression

$$W_{it} = \alpha_0 + \alpha_1 D_t + \alpha_2 D_i + \alpha_3 H_i + \beta' D_i \times D_t + \zeta_1' D_i \times H_i + \zeta_2' D_t \times H_i + \varphi' D_i \times D_t \times H_i + X'\gamma + \epsilon_{it}$$

where H_i is the variable that causes heterogeneous effects, and thus φ is our parameter of interest, capturing the heterogeneity brought about by the different bargaining position of workers.

In order to examine whether individuals with different bargaining power experienced different wage changes from the Reform, we employ three observable variables to proxy the bargaining position of workers: professional category, whether working in the private or public sector, and earned wage.[28]

Professional category Arguably higher professional category workers will have more bargaining power –recall also that given the unreliability of the education variable in our data set, professional category is often used in previous studies as proxy for education attainment. The upper panel of Table 8 shows that indeed this is the case, for both men and women. For instance, because of the reform, unskilled young male workers (the reference category) experience a wage increase of 5.9%, while semi-skilled young male workers have a wage increase of 7.2% (i.e. 5.9+1.3), and engineers and graduates young male workers see their wages increase by 8.1%.

[28]Other recent studies have used similar observables, such as white or blue collar, and the age group of the worker –that we cannot use for obvious reasons– to proxy the bargaining position of workers (Leonardi and Pica (2014)).

Private or public sector The second manner we have to proxy the bargaining position of workers is to distinguish those who work in the private sector from those in the public sector. Wage setting in public sector employment in Spain is more rigid and more subject to regulations than in the private sector. Hence, we expect private sector employees to have a better bargaining position than public sector employees, and to benefit more from the Reform. The second panel of Table 8 shows that this is the case, for both men and women. Because of the Reform, young male (female) workers in the private sector earn 2.7% (1.4%) higher wages than their counterpart workers in the public sector (reference category), while older male (female) workers earn 3.2% (2.9%) more in the private sector than in the public sector.

Wages Finally we also estimate the coefficient of the interaction term φ at different quantiles of the wage distribution. Since workers earning higher wages are likely to have a better bargaining position, we expect φ to increase monotonically with the position of the worker in the wage distribution. The bottom panel of Table 8 shows estimates of φ at the 10th, 25th, 50th, 75th, and 90th quantiles from quantile regressions. Results show that the positive effect of the Reform increases with the position of the worker in the wage distribution. Differences across the wage distribution are substantial. The effect of the reform is 4 times larger for a young male worker at the 90th quantile of the wage distribution than for a woker at the 10th quantile; such difference is a bit smaller for older male workers (a factor of 3.5). Differences for younger women are larger than for younger men. The effect of the reform on younger female workers at the 10th quantile is about 5.5 times larger than those at the 90th quantile, while it is 3.2 times larger for older women.

6.1.4 Effect on incumbents

Even thought the reform did not target incumbent workers, i.e. those hired before the reform, our model suggests that their wages may be also affected by the reform. According to our thoretical model, once incumbent employees know that wages of new entrant positions are higher than before the reform, they use their bargaining power and negotiate an increment in their wages. Moreover, the reform also reduces unemployment rates for younger and older incumbent workers, increasing their bargaining power. We empirically test the wage effects by estimating the wage equation (26) on incumbent workers, as we

Table 8: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment. **Heterogeneous effects.** OLS estimates

		Men		Women	
		Coeff.	t-stat	Coeff.	t-stat
1. PROFESSIONAL CATEGORY					
(Age<30)*(Post 1997)		0.059	3.91	0.043	4.04
(Age>45)*(Post 1997)		0.074	3.77	0.058	4.24
(Age<30)*(Post 1997)*(Semi-skilled workers)		0.013	4.05	0.011	5.18
(Age<30)*(Post 1997)*(Engineers and graduates)		0.022	4.27	0.024	5.15
(Age>45)*(Post 1997)*(Semi-skilled workers)		0.017	4.21	0.014	5.15
(Age>45)*(Post 1997)*(Engineers and graduates)		0.047	4.53	0.028	5.18
2. PRIVATE SECTOR					
(Age<30)*(Post 1997)		0.052	3.61	0.047	3.42
(Age>45)*(Post 1997)		0.079	3.75	0.062	3.59
(Age<30)*(Post 1997)*(Private)		0.027	4.31	0.014	3.84
(Age>45)*(Post 1997)*(Private)		0.032	4.29	0.029	3.40
3. QUANTILE EFFECTS					
	Quantiles				
(Age<30)*(Post 1997)	0.10	0.024	3.78	0.017	3.51
(Age>45)*(Post 1997)	0.10	0.032	3.44	0.037	3.39
(Age<30)*(Post 1997)	0.25	0.030	2.50	0.028	5.63
(Age>45)*(Post 1997)	0.25	0.052	3.29	0.048	6.94
(Age<30)*(Post 1997)	0.50	0.060	3.66	0.043	3.66
(Age>45)*(Post 1997)	0.50	0.085	3.71	0.057	3.66
(Age<30)*(Post 1997)	0.75	0.063	5.47	0.073	5.41
(Age>45)*(Post 1997)	0.75	0.088	5.75	0.112	6.20
(Age<30)*(Post 1997)	0.90	0.101	6.80	0.095	5.82
(Age>45)*(Post 1997)	0.90	0.112	10.08	0.119	9.65
N		25,904		18,371	

Notes: Control group are men and women aged 30 to 45 years. Reference categories: Professional category (Unskilled jobs), Private (Work in the public sector)

can safely assume that there is little substitutability of workers across age categories –see Table 2.

As pointed out in Section 5, observed wages of incumbent workers are censored. In particular, 8.1% of male wages and 4.7% of female wages in the sample of incumbents are top-coded. In order to address the problem of censored wages we report estimates of a Tobit model for the wage equation. Moreover, since our dataset has many job spells for each incumbent worker, we now make use of the panel structure of the dataset and estimate panel models, which allow us to control for unobserved individual heterogeneity. Both fixed and random effect models yield simlar results.

Table 9: Effects of the Reform on Wages for the incumbents. Censored models with FE estimation

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Age<30	-0.018	-2.07	0.024	2.55
Age>45	0.021	2.66	0.046	4.14
Post 1997	0.020	3.10	0.123	3.60
(Age<30)*(Post 1997)	**0.018**	3.83	**0.020**	3.60
(Age>45)*(Post 1997)	**0.025**	3.73	**0.026**	3.52
N	203,053		114,897	

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

In line with our model predictions, the Tobit fixed effect estimates shown in Table 9 suggest that incumbent workers also experience a modest wage increase due to the reform. This increase is larger for older men and women (2.5% and 2.6%, respectively) than for younger workers (1.8% and 2.0%). The full set of estimates are presented in Appendix Table 19.

6.1.5 Robustness checks

To show the robustness of our key findings, this section presents the results of three sensibility checks. In particular, we show that our results are robust to alternative identification stategies, to widening the time window around the year of the reform, and to restricting the sample to ages around the policy thresholds.

An alternative identification strategy As outlined in Section 2, not all firms could use Permanent Employment Promotion contracts to hire new permanent workers after the reform. Two circumstances precluded firms from using PEP contracts: dismissing workers for 'objective' reasons and having been proved wrong in court or engaging in collective dismissals over the 12 moths prior to the reform. Unfortunately, our data do not contain information on firms' dismissals, and so we cannot check why the worker is not hired with a PEP contract, but we have grounds to believe that the group of workers hired with no PEP contract after the reform is not endogenous to the reform, and thus we can use it as control group.

We presume that firms that dismissed and were proved wrong in court should not behave differently from the pool of other firms, which includes firms that dismissed and were not proved wrong in court and firms that did not dismiss over the 12 months prior to the reform. As regards collective dismissals, very few workers (only 0.9 per cent, according to official statistics) were affected by such dismissals. Therefore the sample of workers with no PEP contracts should be exogenous to the reform. Our data supports this premise. To start with, t and χ^2 tests (not shown) reveal that workers with and without PEP contracts have similar observable characteristics. Moreover, difference-in-differences regressions (as in equation (26)) for the sample of workers with no PEP contract show that the reform has no effect on this group of workers, which supports the validity of our alternative identification strategy. Furthermore, the estimates of all the other control variables are nearly identical to those obtained from the sample of workers with PEP contracts (see Appendix Table 20).

Therefore, we can estimate the causal effect of the reform by simply comparing workers with and without Permanent Employment Promotion contracts amongst the pool of workers who entered permanent employment from unemployment after the reform. That is, now our control group is the workers who got permanent employment from unemployment after the reform and were not hired with a PEP contract. To that end, we run a regression similar to our baseline regression (26), for each age group and gender, where the effect of the reform is estimated by a dummy variable that indicates whether the worker holds a PEP contract. The estimation sample is now restricted to workers who obtained a permanent job from unemployment after the reform.[29] Tables 10 and 11 show that

[29]Selection into treatment could also be an issue here. However, the results from a Heckman selection model (not shown, but available upon request) indicate that the sample does not suffer from selection into treatment, as the inverse Mills' ratio of the four regressions is not statistically significant. Thus Tables 10 and 11 show simple OLS estimates.

this alternative identification strategy yields reassuringly similar results as those obtained when identification relies on age groups. Estimates are slightly smaller for younger men (5.7 and 9.7 per cent, to be compared to the baseline estimates of 6.5 and 9.4 per cent, for younger and older men respectively) and slightly larger for women (5.1 and 8.1 per cent, instead of the baseline estimates of 4.5 and 7.7 per cent, for younger and older women respectively).

Table 10: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment. **Alternative identification strategy:** compares workers with and with no Employment Promotion contract for those with **less than 30 years old.** OLS estimates.

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Employment Promotion contract	0.057	3.31	0.051	3.78
Age	0.093	3.85	0.321	5.44
Age square	-0.001	-3.40	-0.006	-4.98
Experience	0.0008	12.71	0.0001	10.20
Unemployment benefits	0.049	3.32	0.098	5.83
Full time	0.521	23.80	0.462	25.65
Firm size	0.0004	9.19	0.0001	2.91
Private	0.037	4.62	0.086	3.75
Duration last spell	-0.0001	-4.48	-0.0003	-3.54
Previous firm size	3.68e-07	0.98	-4.54e-07	-1.01
Num. of perm. contracts	-0.009	-23.88	-0.006	-12.75
(a) Previous type of contract				
Permanent	0.235	10.23	0.183	7.05
Temporary	-0.037	-2.32	-0.003	-0.17
(b)Profesional Category				
Semi-skilled workers, skilled and semi-skilled clerks	0.143	10.00	0.227	15.05
Engineers and graduates, chief and departmental heads	0.410	19.70	0.594	23.24
(c)Industry				
Industry.	-0.062	-0.88	0.278	1.79
Construction	-0.131	-1.84	0.271	1.67
Trade, Transport and Hotels	0.256	-3.67	0.165	1.06
Finance	-0.247	-3.47	0.097	0.62
Public Administration	-0.362	-6.88	0.149	0.95
(d)Region				
Dummys included	YES		YES	
N	10,734		7,256	

Reference categories: Previous type of contract (no contract), Professional category (Unskilled jobs), Industry (Agriculture)

Wider time windows To avoid capturing the effects of the 1999 reform, our baseline regression compares the year prior to the reform (1996) with the year after the reform (1998). Appendix Table 15 shows that results do not change when we slightly widen the

Table 11: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment. **Alternative identification strategy:** compares workers with and with no Employment Promotion contract for those with **more than 45 years old**. OLS estimates.

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Employment Promotion contract	0.097	8.06	0.081	3.73
Age	0.057	1.02	-0.111	-1.50
Age square	-0.0005	-1.05	0.0009	1.31
Experience	7.85e-06	2.36	0.0001	12.86
Unemployment benefits	0.138	6.98	0.014	0.45
Full time	0.748	14.60	0.601	17.82
Firm size	0.0001	3.65	0.0004	2.29
Private	0.055	5.44	0.081	4.19
Duration last spell	-0.0003	-2.51	-0.0009	-6.96
Previous firm size	1.51e-07	0.35	6.66e-07	0.87
Num. of perm. contracts	-0.003	-12.78	-0.003	-10.29
(a) Previous type of contract				
Permanent	0.098	3.80	0.134	4.45
Temporary	-0.042	-2.07	-0.012	-0.39
(b)Profesional Category				
Semi-skilled workers, skilled and semi-skilled clerks	0.125	5.57	0.136	4.62
Engineers and graduates, chief and departmental heads	0.499	21.21	0.447	9.42
(c)Industry				
Industry.	0.533	6.09	0.441	4.23
Construction	0.369	4.18	0.442	3.29
Trade, Transport and Hotels	0.204	2.35	0.445	4.46
Finance	0.387	4.24	0.436	4.18
Public Administration	0.057	0.62	0.451	4.30
(d)Region				
Dummys included	YES		YES	
N	5,278		3,025	

Reference categories: Previous type of contract (no contract), Professional category (Unskilled jobs), Industry (Agriculture)

time windows to years 1995-1996 and 1998-1999.[30]

Restrict the sample to ages around the policy thresholds Since our identification strategy compares different age groups and the difference-in-differences estimator does not impose common support on the distribution of the control variables, treated and control groups might face different common supports. We address this problem by estimating our baseline equation (26) on a restricted sample of workers with ages close to the policy thresholds (i.e. 26-29, 30-33 and 40-44, 45-49). Appendix Table 21 corroborates the positive effect of the reform on wages. Actually, the estimated effects of the reform for

[30] Results are also very similar to the baseline case, i.e. where only years 1996 and 1998 are used, when we correct for endogenous selection into treatment by means of the two-stage Heckman procedure.

the narrower and more homogeneous age groups are larger than the baseline estimates, suggesting that the true effect of the reform might be larger than what our baseline estimates suggest.

6.2 Calibration and simulated results of the model

In this section we quantify the impact in relative wages when only new hired 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 annual 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 taxes in the targeted age-groups during the 1997 reform. Finally, we analyze the simulated post reform effects on the level of wages of each target group with respect to the non targeted group of workers (m). The simulated results complement the estimated effects presented in section 6.1 by predicting not only the impact on wages of newly hired workers, w_0^j, but also on wages of continuing workers not directly affected by the reform, w^j.

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 12. The first three targets consist of the average unemployment rates for workers younger than 30 years old, $u^y = 35.1\%$, between 30 and 45 years old, $u^m = 18.5\%$, and older than 45 years old, $u^e = 12.4\%$. Using data from the Spanish National Institute of Statistics (INE), we apply Shimer (2005)'s methodology to target an annual job finding rate of 0.547 for young workers, 0.427 for middle age employees and 0.460 for older employees. We also target the average wage differential among these groups, $\overline{w}^y/\overline{w}^m = 0.777$ and $\overline{w}^e/\overline{w}^m = 1.069$. Finally, we target the proportion of each type of workers looking for jobs. Therefore, $\lambda^e = 0.166$, $\lambda^y = 0.486$ and $\lambda^m = 1 - \lambda^e - \lambda^y$.

With respect to the calibration of our parameters, we set the discount factor $\delta = 0.95$, which matches an annual real interest rate of nearly 5 percent observed in 1996. Petrongolo and Pissarides (2001) identify an elasticity of unemployment with respect to the matching function in the range 0.5-0.7. We take 0.6 as reference and thus set the matching parameter φ at 0.879.

Table 12: **Benchmark Calibration. Spain, 1996**

		Value	Source
Targets:			
Unemployment rate (< than 30 years old)	u^y	0.351	[A]
Unemployment rate (between 30 and 45 years old)	u^m	0.185	[A]
Unemployment rate (> than 45 years old)	u^e	0.124	[A]
Job finding rate > than 45 years old	χ^e	0.460	[A]
Job finding rate between 30 and 45 years old	χ^m	0.427	[A]
Job finding rate < than 30 years old	χ^y	0.547	[A]
Wage gap for young workers	$\frac{\overline{w}^y}{\overline{w}^m}$	0.777	[A]
Wage gap for old workers	$\frac{\overline{w}^e}{\overline{w}^m}$	1.069	[A]
Workers looking for jobs(< than 30 years old)	λ^y	0.486	[A]
Workers looking for jobs(> than 45 years old)	λ^e	0.166	[A]
Parameters:			
Aggregate labour productivity > than 45 years old	A^e	1.00	Normalized
Aggregate labour productivity between 30 and 45	A^m	1.116	[C]
Aggregate labour productivity < than 30 years old	A^y	1.509	[C]
Mean of log z	μ	0.000	Normalized
Standard deviation of log z	σ_z	0.10	[D]
Discount rate	δ	0.950	[A]
Exogenous exit probability > than 45 years old	ϕ^e	0.065	[C]
Exogenous exit probability between 30 and 45	ϕ^m	0.097	[C]
Exogenous exit probability < than 30 years old	ϕ^y	0.297	[C]
Employment opportunity cost < than 30 years old	b^y	1.565	[C]
Employment opportunity cost between 30 and 45	b^m	3.147	[C]
Employment opportunity cost > than 45 years old	b^e	3.603	[C]
Employers payroll tax	τ^j	0.300	[B]
Cost of vacancy	c	0.021	[C]
Parameter of the Matching function	φ	0.879	[D]
Worker's bargaining power	β	0.623	[C]
New entrant total firing costs parameter < than 30 years old	γ_0^y	$0.785w_0^y$	[A,B]
New entrants firing costs between 30 and 45	γ_0^m	$1.882w_0^m$	[A,B]
New entrants firing costs > than 45 years old	γ_0^e	$2.680w_0^e$	[A,B]
Incumbents firing costs < than 30 years old	γ^y	$0.785w^y$	[A,B]
Incumbents firing costs between 30 and 45	γ^m	$1.882w^m$	[A,B]
Incumbents firing costs > than 45 years old	γ^e	$2.680w^e$	[A,B]
Proportion of severance payments	ψ	0.66	[B]

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

Before the 1997 reform, payroll taxes are equal for both new entrant and incumbent jobs. Thus, we assume that $\tau_0^j = \tau^j$. Using data from the OECD Tax Database, we set the payroll tax at 0.30 for all groups. Thus, $\tau^y = \tau^m = \tau^e = 0.30$.

We now turn to the firing costs γ_0^j and γ^j. We first estimate the total severance payments in years of wages for both new entrant and incumbent workers with permanent contracts, $\psi\gamma_0^j$ and $\psi\gamma^j$, respectively. We use 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 $X^j = 1/s^j$ 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.

According to our target unemployment and job finding rates, the calibrated job exit rates of each group are $s^y = 0.294$, $s^m = 0.097$ and $s^e = 0.065$. These rates imply that the average job tenure in 1996 was 3.4 years for employees younger than 30 years, 10.3 years for those workers between 30 and 45 years, and 15.4 years for employees older than 45 years old.[31] Thus, severance payments for new entrants amount to $\psi\gamma_0^y = 0.518 \times w_0^y$, $\psi\gamma_0^m = 1.242 \times w_0^m$ and $\psi\gamma_0^e = 1.769 \times w_0^e$ of annual wages while for incumbents are $\psi\gamma^y = 0.518 \times w^y$, $\psi\gamma^m = 1.242 \times w^m$ and $\psi\gamma^e = 1.769 \times w^e$.[32]

We next calculate the firing tax costs, $(1-\psi)\gamma_0^j$ and $(1-\psi)\gamma^j$. Garibaldi and Violante (2005) estimate it between 19% and 34% of total firing costs, depending on the layoff scenario. We consider the last scenario and set ψ equal to 0.66. Thus, the firing tax component amounts to near 51.5% of severance payments, which implies that for new entrants the firing tax costs are $(1-\psi)\gamma_0^y = 0.267 \times w_0^y$, $(1-\psi)\gamma_0^m = 0.640 \times w_0^m$ and $(1-\psi)\gamma_0^e = 0.911 \times w_0^e$, while for incumbents these costs are equal to $(1-\psi)\gamma^y = 0.267 \times w^y$, $(1-\psi)\gamma^m = 0.640 \times w^m$ and $(1-\psi)\gamma^e = 0.911 \times w^e$.[33] As a result, total firing costs for new entrants are equal to $\gamma_0^y = 0.785 \times w_0^y$, $\gamma_0^m = 1.882 \times w_0^m$ and $\gamma_0^e = 2.680 \times_0 w^e$, while for incumbents these costs are $\gamma^y = 0.785 \times w^y$, $\gamma^m = 1.882 \times w^m$ and $\gamma^e = 2.680 \times w^e$.

[31] In our model, entrant and incumbent workers with permanent contracts have the same job destruction probability, s_t^j, and, therefore, the same average job duration.

[32] For X^j years of job tenure, severance payments expressed as a proportion of wages are $\frac{\psi\gamma_0^j}{w_0^j} = \frac{\psi\gamma^j}{w^m}$=(0.72×$X^j$×45 days per year + 0.28×X^j×20 days per year + 60 days)/365.

[33] The annual firing tax calculation for new entrants and incumbents amounts to $(1-\psi)\gamma_0^j = \psi\gamma_0^j \times 0.515 \times w_0^j$, and $(1-\psi)\gamma^j = \psi\gamma^j \times 0.515 \times w^j$, respectively.

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 deviation σ_z. We normalize the mean of $\log z_t$ to zero, $\mu = 0$. With respect to σ_z, and similar to den Haan, Ramey, and Watson (2000), we set it equal to 0.1. We also normalized the aggregate labour productivity for the group of workers with more than 45 years old, $A^e = 1.00$, and fix A^y and A^m to match the observed wage gap among these group of workers. These two parameters are calibrated together with the hiring cost c, the wages bargaining power, β, the employment opportunity costs b^j and with the exogenous job exit probabilities ϕ^j. We select these parameters to satisfy the calibration targets: $u^y = 35.1\%$, $u^m = 18.5\%$, $u^e = 12.4\%$, $\chi^y = 0.547$, $\chi^m = 0.427$ and $\chi^e = 0.459$, $\lambda^e = 0.166$, $\lambda^y = 0.486$, $\overline{w}^y/\overline{w}^m = 0.777$ and $\overline{w}^e/\overline{w}^m = 1.069$. This yields $\beta = 0.623$, $c = 0.021$, $A^y = 1.509$, $A^m = 1.02$, $b^y = 1.565$, $b^m = 3.147$, $b^e = 3.600$, $\phi^y = 0.297$, $\phi^m = 0.097$ and $\phi^e = 0.065$.

6.2.2 Simulated effects

The first principal change in legislation reduced severance payments by around 20% for workers who made the transition from unemployment to permanent jobs (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).[34] 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 who made the transition from unemployment to permanent jobs. Thus, for new hired workers, τ_0^y and τ_0^e are reduced from 0.30 to 0.18 and 0.12, while it remains unchanged at 0.30 for both the middle aged group and the continuing positions of the young and elderly groups. As in the empirical part, the simulation takes into account the changes experienced by wages just after the reform. The results of this exercise are displayed in the first panel of Table 13.

The simulated reform yields an increase in the relative wage of the two target groups, which goes in line with the microeconometric estimated effects reported in section 6.1. This result lends credibility to the simulation results that try to disentangle the effects of

[34] In this case, for new hired workers with age-group $i = y, e$, the calculations are: $\frac{\psi\gamma_0^i}{w_0^i}$=(0.72×$XX^i$ years×**33 days** per year + 0.28×XX^i years×20 days per year + 60 days)/365.

Table 13: **Simulated effects of the 1997 reform**

Simulated post reform variation		Var.(%)
New hired worker wages ratio: w_0^y/w_0^m		9.28
New hired worker wages ratio: w_0^e/w_0^m		13.22
Incumbent worker wages ratio: w^y/w^m		0.54
Incumbent worker wages ratio: w^e/w^m		0.80
Transition probability from unemp. to perm. (%):		Var. perc. points
Job finding probability:χ^y		0,90
Job finding probability:χ^e		8.76
Unemployment rate (%):	Pre-reform	Post-reform
u^y	35.2	35.4
u^i	18.5	18.7
u^e	12.4	10.7
Estimated effects of the 1997 reform.		
Weighted average* of Table 5		Var.(%)
New hired worker wages ratio:Age<30		5.73
New hired worker wages ratio:Age>45		8.69
Weighted average* second panel of Table 6		Var. perc. points
Transition from unemp to perm: Age<30		7.46
Transition from unemp to perm: Age>45		10.45
Weighted average* of Table 9		Var.(%)
Incumbent worker wages ratio:Age<30		1.87
Incumbent worker wages ratio::Age>45		2.54
Simulated post reform variation with no reduction in γ		Var.(%)
New hired worker wages ratio: w_0^y/w_0^m		4.36
New hired worker wages ratio: w_0^e/w_0^m		0.09
Incumbent worker wages ratio: w^y/w^m		0.49
Incumbent worker wages ratio: w^e/w^m		0.74
Simulated post reform variation with wage rigidity in continuing jobs.		Var.(%)
New hired worker wages ratio: w_0^y/w_0^m		8.83
New hired worker wages ratio: w_0^e/w_0^m		12.50

*Note: Weighted average of the estimates of the referred Table, where weights are population shares of the two gender groups.

the two policy instruments. With respect to the group of new hired workers older than 45 who made the transition from unemployment to jobs with permanent contracts, the simulated ratio w_0^e/w_0^m increased by 13.22%. This result is somewhat higher than the increase in the weighted average of the estimated effects of 8.69%, reported in the second panel of Table 13. In turn, the relative wage for younger unemployed workers who do the same transition increases by 9.28%, which is also larger than the estimated one of 5.73%.[35]

The simulated employment effects of the reform also go in the same direction as the ones observed in the empirical model, showing a higher increase in the transition probability from unemployment to permanent employment in the group of people above 45 years. In this group, the simulated transition probability from unemployment to permanent employment increases by 8.76 percentage points, which is larger than that for the younger group (0,90 percentage points). Empirical estimates show a weighted increase of 10.45 and 7.46 percentage points in each group, respectively.[36] As a result of the change in job finding probability, unemployment rates fall in the elderly group and remain almost constant in the younger group. According to our simulated results, the unemployment rate of older workers decreases from 0.124% to 0.107% while the unemployment rate of younger workers increases from 0.352% to 0.354%. Notice that the unemployment rate of the middle age workers also remains almost unchanged, going from 18.5% to 18.7%.

In spite of the absence of adjustment in the firing costs and payroll taxes of continuing workers with permanent contracts, the model simulates an increase of 0.54% and 0.80% in the ratios w^y/w^m and w^e/w^m, which are again in line with a weighted average of the estimated effects of 1.87% and 2.54%, reported in the second panel of Table 13. As we mentioned in the theoretical section of the model, these positive effects take place because the reform increases the surplus of new employment positions. Incumbent employees know that wages of new entrant positions are higher than before the reform, which make them less worry about losing their job and starting a new employment relationship. As a result, incumbent employees increase their implicity bargaining power and, therefore, negotiate an increment in their current wages.

[35] The counterpart weighted average estimates that correct for sample selection are very close to the uncorrected ones: 5.49% and 8.49% for younger and older workers, respectively.

[36] Notice, once again, that the simulated increase in the employment probabilities go in line with the positive employment effects estimated by Kugler, Jimeno, and Hernanz (2002).

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 payroll taxes, we simulate a scenario with no reduction in firing costs, keeping the rest of post-reform parameters constant. The results of this exercise are presented in the third panel of Table 13. Remember that, according to our theoretical model, the reduction in payroll taxes of new hired workers have an unclear effect on wages. Our simulated results show that payroll taxes only account for 46% and 1% of the increase in new entrant wages of young and older workers with respect to middle aged workers. However, the reduction in payroll taxes explains most of the the increase in the wages of young and older workers with incumbent positions with respect to middle aged workers (90% and 92.5% of the wage increase, respectively).

Our final simulated scenario considers wage rigidity in continuing permanent contracts. Wage rigidities vary a lot across countries and they are correlated to employment protection legislation. For example, in a recent study, Babeckandyacute, Du-Caju, Kosma, Lawless, Messina, and Randotilde (2010) show that wage rigidity is positively associated with the extent of permanent contracts and this effect is stronger in countries with stricter employment protection regulations. For the Spanish case, De la Roca (2008) shows that wages for newly hired workers are more volatile than wages of ongoing employees. To check the sensitivity of the model to the degree of wage rigidity, we next keep the Nash bargaining assumption in the wages of new hired workers with permanent contracts but set the wages of incumbent workers to their calibrated values before the reform. The results of this exercise are presented in the bottom panel of Table 13 and are very similar to the simulated post-reform variation in the first panel.

7 Final remarks

This paper provides 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. Since the firm does not incur in firing costs when there is no agreement on a wage between the firm and the employee in the first encounter, we permit the wage bargaining process to differ between new entrants and incumbent workers. A reduction in firing cost in new created positions increases wages of new entrants through an increase in their implicit bargaining power. The reduction in firing costs also increases the wages of incumbent workers not directly affected by the reform. In this case, with higher wages in new entry positions, incumbent employees are less worried about losing their jobs and starting a new employment relationship. This increases the implicit bargaining power of incumbents and, therefore, their wages. In turn, the reduction of payroll taxes has an unclear effect on both new entrant and incumbent wages. In sum, the theoretical model shows that the overall wage effect of the 1997 reform on both new entrants and incumbent wages is entirely an empirical question.

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. Ours is the first study that relies on this source of indentification to examine the effect of firing costs and payroll taxes. Since the changes did not cover all workers, we use a difference-in-differences estimator to obtain short-term causal effects. The possible selection into treatment 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 employment). We find larger effects for older than for younger workers and for men than for women. The reform also has an positive impact on incumbent workers' wages, which is somewhat smaller than the effect on entry wages.

These findings are robust to (i) an alternative identification strategy that exploits an exogenous variation in the use of Permanent Employment Promotion contracts, which reduce firing costs and payroll taxes, (ii) to enlarging the sample period to four years, two before and two after the Reform, and (iii) to restricting the sample to workers with ages close to the cut-off set by the Reform to define the two targeted groups, i.e. our treatment groups.

Consistent with our model, we find heterogeneous effects, larger for workers with larger bargaining power. That is, the positive effect of the Reform is larger for workers in the private sector, with higher professional category, and earning higher wages.

Calibrating the model and simulating the reform provides a robustness check of the estimated effects and allows to separately identify and quantify the effects of each policy change, which cannot be estimated since the two provisions changed at the same time. Simulated effects are consistent with the estimated effects, though somewhat larger for new hired targeted workers and somewhat smaller for incumbent employees. Regarding the relative impact of each provision, our simulations suggest that more than 90% of the increase in incumbent wages is due to the reduction in payroll taxes, while the reduction in firing costs accounts for less than 40% of the increase in the wages of new entrants.

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A Appendix

A.1 Main changes in dismissal costs and payroll taxes Due to the 1997 Reform for temporary workers

Table 14: **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.2 Sensibility checks with wider time windows (2 years)

Table 15: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment **(95-96 vs 98-99)**. OLS estimation

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Age<30	0.037	1.96	0.057	2.70
Age>45	-0.002	-0.07	0.067	2.18
Post 1997	0.076	6.48	0.096	7.17
(Age<30)*(Post 1997)	**0.072**	4.33	**0.055**	2.99
(Age>45)*(Post 1997)	**0.109**	4.51	**0.075**	2.75
N	46,877		33,400	

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

A.3 Full estimates of wage regression

Table 16: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment. OLS estimates.

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Age<30	0.047	1.72	0.036	1.12
Age>45	-0.025	-0.66	0.127	2.91
Post 1997	0.079	4.47	0.075	3.63
(Age<30)*(Post 1997)	0.066	2.66	0.045	3.43
(Age>45)*(Post 1997)	0.094	2.85	0.076	2.07
Age	0.041	5.90	0.033	4.11
Age square	-0.0005	-5.63	-0.0005	-4.94
Experience	0.0003	7.88	0.0001	16.17
Unemployment benefits	0.131	9.63	0.077	4.46
Full time	0.537	20.33	0.506	24.97
Firm size	0.0005	9.33	0.0002	3.97
Private	0.045	4.67	0.064	4.61
Duration last spell	-0.0001	-3.94	-0.0003	-3.27
Previous firm size	2.87e-07	0.91	7.26e-07	0.00
Num. of perm. contracts	-0.002	-9.98	-0.0011	-6.27
(a) Previous type of contract				
Permanent	0.106	5.47	0.076	3.54
Temporary	-0.026	-9.98	- 0.002	-0.11
(b)Profesional Category				
Semi-skilled workers, skilled and semi-skilled clerks	0.185	12.76	0.267	17.11
Engineers and graduates, chief and departmental heads	0.524	28.76	0.650	26.58
(c)Industry				
Industry.	0.366	6.26	0.564	6.83
Construction	0.215	3.56	0.470	4.83
Trade, Transport and Hotels	0.090	1.56	0.461	5.65
Finance	0.190	3.16	0.435	5.24
Public Administration	-0.015	-0.25	0.497	6.02
(d)Region				
Dummys included	YES		YES	
N	25,904		18,371	

Notes: Control group are men and women aged 30 to 45 years. Reference categories: Previous type of contract (no contract), Professional category (Unskilled jobs), Industry (Agriculture)

A.4 Full estimates of wage and selection regressions

Table 17: Effects of the Reform on Wages for men and women who experience a transition from unemp. to perm. employment. Heckman selection model (two stage method)

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Wage equation				
Age<30	0.048	1.76	0.036	1.12
Age>45	-0.034	-0.91	0.127	2.91
Post 1997	0.098	5.37	0.075	3.58
(Age<30)*(Post 1997)	0.059	2.39	0.049	3.44
(Age>45)*(Post 1997)	0.088	2.65	0.080	2.08
Age	0.033	4.67	0.034	4.07
Age square	-0.0004	-4.15	-0.0005	-4.88
Experience	0.00002	5.40	0.0001	14.51
Unemployment benefits	0.124	8.79	0.077	4.45
Full time	0.538	20.30	0.506	24.99
Firm size	0.00005	9.21	0.00002	3.97
Private	0.052	5.32	0.046	4.04
Duration last spell	-0.00001	-5.18	-0.0003	-3.21
Previous firm size	3.56e-07	1.12	-6.43e-07	-0.00
Num. of perm. contracts	-0.003	-10.77	-0.0011	-5.23
(a) Previous type of contract				
Permanent	0.122	5.98	0.076	3.36
Temporary	-0.006	-0.44	- 0.002	-0.11
(b)Profesional Category				
Semi-skilled workers, skilled and semi-skilled clerks	0.180	12.35	0.268	17.13
Engineers and graduates, chief and departmental heads	0.513	27.92	0.650	26.60
(c)Industry				
Industry.	0.359	6.19	0.564	6.83
Construction	0.218	3.63	0.470	4.83
Trade, Transport and Hotels	0.081	1.40	0.461	5.65
Finance	0.185	3.10	0.435	5.24
Public Administration	-0.014	-0.22	0.497	6.01
(d)Region				
Dummys included	YES		YES	
N	25,904		18,371	
Selection equation				
Age<30	0.002	0.08	0.025	1.08
Age>45	0.074	2.68	0.189	5.36
Post 1997	-0.251	-19.71	-0.146	-9.45
(Age<30)*(Post 1997)	0.080	4.72	0.065	3.19
(Age>45)*(Post 1997)	0.119	4.95	0.083	4.40
Age	0.105	22.99	0.057	9.94
Age square	-0.001	-25.35	-0.0008	-10.41
Experience	0.0001	29.13	0.0001	30.96
Unemployment benfits	0.040	3.81	0.032	2.53
Duration last spell	-0.0008	-16.54	-0.0011	-19.95
Previous firm size	1.61e-07	6.19	1.30e-07	3.86
Num. of perm. contracts	0.053	199.69	0.060	163.34
(a) Dummys of Previous type of contract included	YES		YES	
(b) Dummys of Lag Profesional Category included	YES		YES	
(c) Dummys of Lag Industry included	YES		YES	
(d) Dummys of Region included	YES		YES	
Unemployment spells	-0.063	-108.34	-0.052	-79.42
N	277,394		181,432	
Selection coefficients				
Inverse Mill's ratio (λ)	-0.103	-4.56	0.254	7.54
ρ	-0.113		0.389	
σ	0.911		0.880	

Notes: Control group are men and women aged 30 to 45 years. Reference categories: Previous type of contract (no contract), Professional category (Unskilled jobs), Industry (Agriculture)). ρ is the correlation between error terms of the wage and selection equation. σ is the standard error of the residual in the wage equation.

A.5 Alternative Exclusion Restrictions

Table 18: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment. Heckman selection model (two stage method). Robustness check: Alternative Exclusion Restrictions

	Instrument: Number of permanet contracts				Instrument: Previous type of contract			
	Men		Women		Men		Women	
	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat	Coeff.	t-stat
1. WAGE EFFECTS								
Uncorrected wage effects (wage eq.)								
(Age<30)*(Post 1997)	0.062	2.50	0.046	3.44	0.067	2.70	0.045	3.86
(Age>45)*(Post 1997)	0.095	2.89	0.077	2.07	0.093	2.82	0.077	2.01
N	25,904		18,371		25,904		18,371	
Selection coefficients								
Inverse Mill's ratio (λ)	-0.198	-0.95	0.010	0.41	-0.005	-0.24	0.025	1.20
ρ	-0.021		0.011		-0.005		0.029	
σ	0.906		0.880		0.907		0.881	
Corrected wage effects*								
(Age<30)*(Post 1997)	0.062	10.91	0.046	5.26	0.067	11.74	0.045	5.10
(Age>45)*(Post 1997)	0.095	16.79	0.077	8.75	0.093	16.39	0.077	8.72
2. EMPLOYMENT EFFECTS								
Employment effects (selection eq.)								
(Age<30)*(Post 1997)	0.080	4.71	0.064	3.19	0.092	5.69	0.065	4.35
(Age>45)*(Post 1997)	0.115	4.77	0.083	3.40	0.086	3.82	0.080	3.66
Instrument	0.054	199.57	0.060	163.34	0.192	11.70	0.274	13.99
					-0.465	-38.11	-0.63	-42.96
N	277,415		181,432		277,415		181,432	
Marginal prob. of emp. effects								
(Age<30)*(Post 1997)	0.008	3.72	0.012	3.17	0.009	3.89	0.009	3.12
(Age>45)*(Post 1997)	0.013	4.31	0.014	3.18	0.015	4.54	0.010	3.10

Notes: Control group are men and women aged 30 to 45 years. ρ is the correlation between error terms of the wage and selection equation. σ is the standard error of the residual in the wage equation.
*Since the two treatment variables appear in both the outcome and selection equations, the marginal effects and their standard errors are not simply given by the second stage estimates reported in the upper panel (see footnote 27).

A.6 Full estimates for incumbents

Table 19: Effects of the Reform on Wages for the incumbents. Censored models with FE estimation.

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Age<30	-0.019	-2.07	0.024	2.55
Age>45	-0.021	2.66	0.046	4.14
Post 1997	0.020	3.10	0.123	14.40
(Age<30)*(Post 1997)	0.018	3.83	0.020	3.60
(Age>45)*(Post 1997)	0.025	3.73	0.026	3.52
Age	0.071	32.51	0.063	23.63
Age square	-0.0008	-32.30	-0.0007	-23.50
Experience	0.00002	23.52	0.0001	48.40
Unemployment benefits	-0.179	-4.82	-0.034	-3.66
Full time	0.573	29.63	0.925	180.87
Firm size	0.00002	26.61	9.24e-06	10.15
Private	0.066	7.13	0.068	4.79
Duration last spell	-8.97e-06	-2.48	-0.0003	-10.23
Previous firm size	-4.56e-07	0.91	-4.41e-07	-1.95
Num. of perm. contracts	-0.0004	-2.41	-0.0009	-4.14
(a) Previous type of contract				
Permanent	0.040	4.58	0.075	7.25
Temporary	0.013	1.88	- 0.041	-5.26
(b)Profesional Category				
Semi-skilled workers, skilled and semi-skilled clerks	0.235	59.55	0.321	68.10
Engineers and graduates, chief and departmental heads	0.544	118.22	0.738	111.10
(c)Industry				
Industry.	0.174	10.21	0.404	14.24
Construction	-0.0008	-0.04	0.226	7.03
Trade, Transport and Hotels	-0.011	-0.64	0.288	10.28
Finance	0.122	7.00	0.276	9.80
Public Administration	-0.017	-0.97	0.280	9.95
(d)Region				
Dummys included	YES		YES	
N	203,053		114,897	

Notes: Control group are men and women aged 30 to 45 years. Reference categories: Previous type of contract (no contract), Professional category (Unskilled jobs), Industry (Agriculture)

A.7 An alternative identification strategy

Table 20: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment, with **No Employment Promotion contract**. OLS estimates.

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
Age<30	0.037	1.18	0.021	0.61
Age>45	-0.022	-0.58	0.110	2.48
Post 1997	0.066	2.74	0.076	3.72
(Age<30)*(Post 1997)	0.017	0.55	0.016	0.54
(Age>45)*(Post 1997)	0.024	0.67	0.017	0.45
Age	0.042	5.59	0.035	4.06
Age square	-0.0004	-5.35	-0.0005	-4.70
Experience	0.0002	6.38	0.0006	14.83
Unemployment benefits	0.095	5.92	0.054	2.97
Full time	0.546	17.46	0.528	25.09
Firm size	0.0005	8.11	0.0002	4.17
Private	0.049	4.43	0.065	3.63
Duration last spell	-0.0002	-3.27	-0.0004	-4.48
Previous firm size	5.95e-07	1.68	1.17e-07	0.27
Num. of perm. contracts	-0.004	-16.79	-0.0026	-14.02
(a) Previous type of contract				
Permanent	0.253	11.13	0.254	11.62
Temporary	-0.029	-16.79	0.005	0.29
(b)Profesional Category				
Semi-skilled workers, skilled and semi-skilled clerks	0.177	10.71	0.265	16.25
Engineers and graduates, chief and departmental heads	0.501	24.14	0.639	25.44
(c)Industry				
Industry.	0.333	5.06	0.501	6.05
Construction	0.181	2.67	0.375	3.79
Trade, Transport and Hotels	0.063	0.98	0.369	4.52
Finance	0.158	2.35	0.357	4.29
Public Administration	0.067	21.48	0.401	4.85
(d)Region				
Dummys included	YES		YES	
N	22,675		17,599	

Notes: Control group are men and women aged 30 to 45 years. Reference categories: Previous type of contract (no contract), Professional category (Unskilled jobs), Industry (Agriculture)

A.8 Restrict the sample to ages around the policy thresholds

Table 21: Effects of the Reform on Wages for men and women who experience a transition from unemployment to permanent employment. **Restricted age groups.** OLS estimates

	Men		Women	
	Coefficient	t-stat	Coefficient	t-stat
(Age 26 to 29)*(Post 1997)	**0.075**	3.96	**0.067**	3.47
N	8,913		6,074	
(Age 45 to 49)*(Post 1997)	**0.111**	4.01	**0.082**	3.99
N	4,488		3,247	

Notes: Control group are men and women aged 30 to 33 years for the first restricted group and men and women aged 40 to 44 years for the second restricted group .