Earnings Effects of Permanent Employment Promoting Reforms: Analysis of the 1997 Labour Market Reform in Spain

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Abstract

Temporary employment accounts in Spain for about one third of total salaried employment since the mid-Eighties, which triples European figures. In the Nineties, two labour market reforms were implemented in an attempt to reduce the very high incidence of temporary employment, achieving some positive effects on employment. However, we still have a very limited knowledge of the likely effects on earnings. Since the reform applied only to certain age groups, in this paper we use a natural experiment research design to assess the impact of the 1997 reform on earnings levels. Using data on the Spanish component of the ECHP, we find positive effects on earnings of young men signing new contracts from unemployment.

Keywords: Evaluation of labour market reforms, temporary employment, experimental design, difference-in-difference, Spain.

JEL classification: C23, D31, J31.

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1 Introduction

In the last decades, several European countries have employed asymmetric reforms in employment protection legislation (EPL) to improve the performance of the labour market. Such reforms have increased flexibility 'at the margin' by liberalising the use of temporary or fixed term contracts while keeping employment protection levels of permanent workers largely unchanged (Boeri & Garibaldi (2007)). While there is a substantial amount of research on the labour market effects of employment protection legislation (EPL),[1] much less is known about the effects of the counter-reforms designed to reduce the employment protection gap (between the two contractual forms) introduced or enlarged by previous reforms. The analysis of such counter-reforms is important, and Spain offers an excellent study case.

The Spanish labour market has experienced five reforms during the last twenty years.[2] Up to the early 1980s, the intervention in the labour market was especially important. While wages were kept very low, the permanence in the labour market was guaranteed by difficulties in laying-off workers and very rigid conditions to hire employees. Permanent work contracts represented more than 90% of all jobs. The main objective of the 1984 reform was to ease access to employment, making hiring temporary workers easier for employers. Although the reform achieved certain employment dynamism, it also generated a dual labour market (insider-outsider) and segmentation problems. Temporary employment increased from around 10% in the mid-Eighties to more than 30% in the early Nineties. This figure doubles the European average. As the proportion of temporary jobs surged, the recent reforms in the 1990s and the 2000s have aimed at achieving a more 'balanced' situation between both types of contracts both by restricting the use of fixed-term contracts and by reducing mandatory firing costs under new permanent contracts.[3]

The employment effects of these reforms have captured most of the attention in the literature, and recent research shows that they were rather successful in terms of fostering the 'right' employment transitions from temporary to permanent contracts

[1] The two featured numbers of the *Economic Journal* of June 2007 and June 2002 devoted to EPL and temporary work is a good indicator of the interest of the profession in the topic.

[2] They took place in 1984, 1994, 1997, 2001 and more recently in 2006. The next section provides some details on these labour market reforms.

[3] Segura (2001) and Dolado, Garca-Serrano & Jimeno (2002) analyse the impact of these reforms in more detail.

–especially the later one of 1997. [4] As a consequence, the share of temporary employment declined from 35.4% in 1995 to 32% in 2001.

Such reforms are also likely to have a bearing on wage levels –as well as on wage instability, possibly reducing wage mobility during the period analysed. Notwithstanding, the wage effects of these reforms have not received much attention at all. In this paper, we try to bridge this gap and investigate the consequences of the 1997 labour market reform on Spanish male wage levels –and leave the study of the effects on wage stability for future work.

The impact on average wages of increases in the proportion of permanent jobs (with stricter EPL) is ambiguous. The bargaining hypothesis (Nickell & Andrews (1983)) argues that job security and wages are complements because of union bargaining power; workers on permanent contracts are also likely to be more productive since on average they report higher job satisfaction, accumulate more specific human capital and enjoy longer seniority (A. Booth & Frank (2002)). Other models, however, predict the opposite relationship. Standard competitive labour market models that view employment protection as mandatory employment benefits, predict a wage fall to cover the cost of the benefit, provided the mandate is efficient (Lazear (1990)). [5] Wages ought to fall also, in accordance to Rosen's (1986) theory of equalizing differences, which views job protection and wages as substitutes.

Empirical research, however, finds consistent evidence of positive earnings differentials in favor of permanent workers (Jimeno & Toharia (1993), Bentolila & Dolado (1994), De la Rica & Felgueroso (2003), De la Rica (2004)), mostly accounted for by differences in characteristics (Davia & Hernanz (2004)).

However, the overall average impact is not the effect we seek to unveil in this paper. We are interested in the incidence of the reform *per se*, which is bound to depend on the policy instruments used to implement it, i.e payroll tax and firing costs. Section 3 uses a simple matching model (Pissarides (2000)) to gain some theoretical insights on the behaviour of wages, as this two policy instruments come into play.

[4] See Dolado et al. (2002), Kugler, Jimeno & Hernanz (2002) and Guell-Rotllan & Petrongolo (2000).

[5] Inefficient mandatory dismissal protection –i.e. when workers value dismissal protection at less than its marginal cost of provision– may decrease or increase labour productivity, thus having an ambiguous effect on wages. Decreasing labour productivity may come from the retention of unproductive workers who do not want to separate from the firm due to (perceptions of) reduced termination benefits, while the capital deepening which may derive from such retention may raise labour productivity.

Since the reform applied only to certain age groups, we follow Kugler et al. (2002) and use a natural experiment research design to identify the impact of the 1997 reform on earnings levels[6]. We use the eight waves of the Spanish component of the European Community Household Panel (ECHP). Overall, our evidence suggests that the reform had rather limited impact on earnings. Actually, only young men entering employment from unemployment experience a positive effect on earnings because of the reform.

The rest of the paper is structured as follows. The next section describes the institutional framework and the Spanish labour market reforms and Section 3 accommodates the relevant and salient features of those reforms into a very simple matching model. Section 4 explains the natural experiment research design used to evaluate the impact of the 1997 reform, while Section 5 describes the data and our methodological decisions. Section 6 presents the main empirical results. Finally, section 7, summarises the main findings and suggests the future research agenda.

2 The Spanish Labour Market Reform

From an institutional point of view, the Spanish labour market has experienced five reforms during the last twenty years (in 1984, 1994, 1997, 2001 and more recently in 2006). Up to the early 1980s, permanent work contracts -open-ended contracts subject to mandatory severance payments- accounted for more than 90% of all contracts, with the remaining temporary contracts applying mainly to seasonal jobs, e.g. in agriculture or tourism. In 1984, with an unemployment rate of 20.1%, the Spanish government tried to implement a significant change in the Employment Protection Legislation (EPL) by liberalising temporary contracts in two main respects: first, their use was extended to all types of jobs; and second, they entailed much lower dismissal costs than regular permanent contracts. Although the reform achieved certain employment dynamism, it also generated a dual labour market (insider-outsider) and segmentation problems between unstable low-paying jobs and stable high-paying jobs. Temporary

[6]Kugler et al. (2002) set up a natural experiment research design to assess the impact of the 1997 reform on permanent employment. The use of natural experiments to evaluate treatment effects in the absence of truly experimental data has gained wide acceptance in social sciences. Simple comparisons of pre-treatment and post-treatment outcomes for those individuals exposed to a treatment are likely to be contaminated by temporal trends in the outcome variable or by the effect of events, other then the treatment, that occurred between both periods. However, when only a fraction of the population is exposed to the treatment, an untreated comparison group can be used to identify temporal variation in the outcome that is not due to treatment exposure.

employment increased from around 10% in the mid-Eighties to more than 30% in the early Nineties. This figure doubles the European average[7].

Between 1985 and 1994, over 95% of all new hires were employed through temporary contracts and the conversion rate from temporary to permanent contracts after 1984 was only around 10%.[8]

Temporary employment becomes a new and increasing concern, and in the 1990s, the reforms aim at reducing the incidence of temporary employment by partly undoing the liberalisation introduced in 1984. The new regulations introduced with the 1994 reform restrict the usage of fixed-term temporary contracts to seasonal jobs[9] and try 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. In practice, however, not much changed: employers continued to hire workers under temporary contracts for all type of jobs -and not just for seasonal jobs-, and judges did not change their criteria or behaviour when appraising dismissals, despite the new regulations[10].

The very limited success of the 1994 reform led to a new reform in 1997, which was eventually extended in 2001. As with the 1994 reform, the main objective of the 1997 and 2001 reforms was to reduce the use of temporary contracts. In 1997, the employers organisation (CEOE) and the two major unions (UGT and CC.OO) reached an agreement to reform the system of work contracts and the structure of collective bargaining. As shown in Table 1, the 1997 reform reduced dismissal costs for unfair dismissals by about 25% and lowered payroll taxes between 40% and 90% for newly signed permanent contracts and for conversions of temporary into permanent contracts after the second quarter of 1997 for certain groups[11]. In principle, the 1997

[7]A clear sign that employers took full advantage of the newly available flexibility device is that a large fraction of temporary workers have been hired under fixed-term contracts while other types of temporary contracts (probationary, seasonal, etc.), which are more representative in other European labour markets, have remained relatively unimportant.

[8]See Guell-Rotllan & Petrongolo (2000).

[9]In the case of workers over 45 years of age, temporary contracts could be continued to be used for all types of jobs and not only for seasonal jobs until 1995. After 1995, however, the use of temporary contracts for the over 45 age group, as for the rest of the workers, was limited to seasonal jobs.

[10]In particular, dismissals under "economic reasons" continued to be granted mainly when there was agreement between employers and workers and labour courts continued to rule most dismissals as unfair.

[11]These groups are: unemployed workers under 30 years of age, over 45 years of age, workers over 45 years holding a temporary contract, the long-term unemployed, women under-represented in their

reform envisaged the new contract to be in effect for a period of four years. In 2001, however, when the new permanent contracts were supposed to expire, fearing that their elimination would exert a negative effect on job creation, the government allowed them to remain in effect and extended their use to hire other groups of workers. That is, the 1990s and 2001 reforms could be considered counter-reforms as they purposed to reduce the temporality in the labour market generated by the 1984 reform.

We analyse the income effects of the 1997 reform because the design of the reform invites to set up a natural experiment research design to identify the effects of the reform. More precisely, the new regulations affected dismissal costs and payroll taxes differently for given population groups: younger and older workers, the long-term unemployed, women under-represented in their occupations and disabled workers.

3 A simple framework

This section uses a simple theoretical framework put forth by Kugler et al. (2002) to illustrate the effects on wages of reductions in payroll taxes and firing costs for permanent contracts, as implemented by the 1997 reform. As in recent contributions, we use a stripped-down version of the Diamond-Mortensen-Pissarides equilibrium unemployment model (Mortensen & Pissarides (1994); Pissarides (2000)), where firms create and fill vacancies using temporary and permanent contracts.

Firms are risk-neutral value maximizers. Positions can be created at cost c, and filled with temporary or permanent contracts, according to matching technology that has 'workers waiting at the gate' –i.e. positions can be filled instantaneously from a pool of unemployed workers. In accordance to the share of fixed term contracts in new hires for Spain –persistently over 90% for the last twenty years–, we assume that all vacancies are initially filled with temporary contracts. Productivity of a job-worker pair is match specific, but depends on the type of contract. Productivity may change because both types of jobs are exposed to productivity shocks with instantaneous probability s. Temporary or 'entry-level' jobs show constant productivity, p_0, while the productivity level of permanent jobs is drawn from a distribution with cumulative distribution function $H(p)$, whose expected value is $E(p)$. After a shock, the new

occupations, and disabled workers.

level of productivity is also drawn from the same distribution $H(p)$. Temporary jobs may be converted into permanent jobs provided the productivity level of the worker exceeds a productivity threshold p_c; otherwise workers are dismissed, and the job is filled again with a temporary contract. Permanent workers may also be dismissed if the new productivity level after a productivity shock is smaller than p_r .

Central to our model, firms are obliged to pay firing cost F if a permanent worker is dismissed. Dismissal of transitory workers or separations due to retirement are not subject to firing costs.[12] Both permanent and transitory contracts are subject to payroll (proportional) taxes, with tax rate t. Hence, the net return of a job is $[p - w(1-t)]$, where w is the wage. Finally, the interest rate relevant to the firm is r.

The expected value of profits from a permanent job, $J^P(p)$, is given by the following Bellman equation

$$rJ^P(p) = [p - (1-t)\,w\,(p)] + sE\left(J^P(p') - J^P(p) \mid p' \geq p_r\right) + s\left(J^T(p_0) - J^P(p) - F\right)H(p_r).$$

As we just pointed out, the first term on the right gives the flow profit of the job. The second term reflects the expected change in the value of the job if the worker is retained in the permanent job, while the third term gives the value of the job if the permanent worker is dismissed and the firm hires a new worker with 'entry-level' productivity p_0.

Similarly, the expected value of profits from a permanent job, $J^T(p_0)$, is given by

$$rJ^T(p_0) = [p_0 - (1-t)\,w\,(p_0)] + sE\left(J^P(p') - J^T(p_0) \mid p' \geq p_c\right).$$

The second term reflects the change in the value if the temporary job turns into a permanent job, as a result of a new productivity level p'.

Workers are also assumed risk-neutral, receive wages that depend on their productivity level, $w(p)$, and have a discount rate r. Dismissed workers[13] and those who separate from a temporary job become unemployed. The value of being unemployed is normalised to zero. Recall that unemployed workers only find new employment in

[12] Allowing for firing costs for separations due to dismissal of workers holding transitory contracts would introduce an additional parameter to our model, but not change anything of substance Recall that our interest is to predict whether, and in what direction, wages change as a result of a fall in firing costs for permanent contracts only.

[13] Recall that only permanent workers can be laid off, since temporary jobs display constant productivity, p_0.

temporary jobs, which they do with probability $x = h/u$, with h being the flow of hires and u the unemployment rate. The labour force is normalised to 1.

The value to a worker being employed in a permanent job, $W^P(p)$, is given by

$$rW^P(p) = w(p) + sE\left(W^P(p') - W^P(p) \mid p' \geq p_r\right) + s\left(U - W^P(p)\right) H(p_r).$$

The first term on the right is the wage the worker receives with 'entry-level' productivity p_0. The second term reflects the expected change in value if the worker is retained in the permanent job, while the third term gives the loss in value if the worker is laid off.

The value to a temporary employee, $W^T(p_0)$, is given by

$$rW^T(p_0) = w(p_0) + sE\left(W^P(p') - W^T(p_0) \mid p' \geq p_c\right) + s\left(U - W^T(p_0)\right) H(p_c).$$

The first term on the right is the wage the worker receives with 'entry-level' productivity p_0. The second term reflects the expected change in value if the temporary contract is converted to a permanent one, while the third term gives the loss in value if the worker enters unemployment upon termination of the temporary contract.

Finally, the value for an unemployed worker, U, is given by

$$rU = x\left(W^T(p_0) - U\right).$$

Wages in both types of jobs are set by symmetric Nash bargaining, with continuous renegotiation. The Nash bargaining condition for temporary jobs equals the losses incurred by firms and workers from terminating a temporary job and not convert it to a permanent job.

$$J^T(p_0) - c = W^T(p_0) - U$$

The term on the left reflects that the firm will have to create a new temporary job, and according to the free entry condition the value of such new job must equal the cost of creating it, c. The term on the right gives the change in vlaue for the worker who enters unemployment from the temporary job.

Similarly, the Nash bargaining condition for permanent jobs equals the losses incurred by firms and workers from terminating a permanent job.

$$J^P(p) - J^T(p_0) + F = W^P(p) - U$$

The term on the left reflects that the firm will have to create a new temporary job and also pay the firing costs. The term on the right gives the change in vlaue for the worker who enters unemployment from the permanent job.

Now, permanent wages are given by

$$w(p) = \frac{p - r(c - F)}{2 + t}$$

from where it is immediate to see that firing costs and payroll taxes have offseting effects on permanent wages. While wages increase with firing costs ($\frac{\partial w(p)}{\partial F} > 0$), they fall with payroll taxes ($\frac{\partial w(p)}{\partial t} < 0$). Therefore, the final effect of the 1997 reform on permanent wages is entirely an empirical question.

4 Identification strategy

We use the evaluation literature to assess the impact of the 1997 Labour Market Reform on earnings. As it is traditional in this literature, the programme analysed is called "treatment". Then, the group that receives the treatment (i.e. being targeted by the programme) is termed "treated group", while the comparison group –which remains unaffected by the programme– is the "control group".

In particular, Spain's 1997 labour market reform marks a sharp change for some groups (i.e. young workers, older workers, the long-term unemployed, women under-represented in their occupations, and disabled workers), while leaving other groups unaffected. As in Kugler et al. (2002), we exploit such differential treatment by age groups to set up a natural experiment research design and assess the impact of the 1997 reform on wages. That is, we shall compare treated groups (unemployed workers under 30 and over 45 years of age, temporary workers under 45 years) with the control group (middle-aged unemployed workers, over 45 years temporary workers) before and after the 1997 reform by means of the Difference in Differences estimator.

We also employ the simpler Before and After estimator to illustrate other relevant aspects of the reform and gain some intuition about the causal effects.

4.1 Before and After

The Before and After estimator (BA) takes the difference in mean outcome for the group of treated individuals before and after the treatment occurs. Thus, this estimator

is not very demanding on the data (it requires only to observe the treated group before and after the treatment) but builds on a set of rather strong assumptions to identify the effect of the programme –see below.

Let $W_0(i,t)$ be the earnings that individual i would earn at time t had she not been affected by the treatment, and $W_1(i,t)$ the earnings that individual i would obtain at time t had she received the treatment. Suppose that the policy (treatment) is implemented at the begining of $t = 1$. Then, the real effect of the treatment on earnings of individual i is defined as $[W_1(i,1) - W_0(i,1)]$, which cannot be identified since the counterfactual $W_0(i,1)$ cannot be observed.

However, under some assumptions, i.e. temporal stability (Holland (1986))[14], the average treatment effect on the treated (ATT) can be identified by

$$\beta_{BA} = E[W(i,t)/X(i,t), t = 1] - E[W(i,t)/X(i,t), t = 0] \tag{1}$$

We estimate this equation for various treatment groups, such as those obtaining permanent from temporary employment or those making the transition to permanent employment from unemployment (see Tables 5 and 6).

4.2 Difference in Differences

Since temporal stability is typically rejected and only a fraction of the population is affected by the 1997 reform, we also consider the Difference in Differences (DID) estimator. The conventional DID estimator assumes that the average conditional outcome for the treated and control groups would have followed parallel paths over time in absence of the treatment. Letting $D = 1$ denote individuals belonging to the treated group, and $D = 0$ to the control (or untreated) group, the temporal stability assumption may be now expressed as:

$$E[W_0(1) - W_0(0)|X, D = 1] = E[W_0(1) - W_0(0)|X, D = 0]$$

[14]Temporal stability requires that the effect of unobserved heterogeneity, omitted variables, or the changing aggregate labour market conditions on the outcome of interest be nil or constant over time, and that other events contemporaneous to the treatment do not have any bearing on the outcome.

and, the estimator that identifies the ATT is given by:

$$\beta_{DID} = E[W(1)|X, D = 1] - E[W(0)|X, D = 1] - E[W(1)|X, D = 0] - E[W(0)|X, D = 0] \quad (2)$$

Given the particularities of the 1997 Reform, we focus on two different contrasts by age group (see table 1). First, we asses the impact of reducing dismissal costs and payroll tax for the young (less than 30 years) and the older (over 45) unemployed –here the control group are the middle-aged unemployed. Second, we analyse the earnings effect of the a larger reduction in the payroll tax for the older workers holding a temporary contract –the control group thus being the younger workers who also hold temporary contracts. The latter can be thought of a reform 'at the margin', while the former is definitely a substantial change in provisions [15].

The following regression is used to implement the estimation strategy

$$W_{it} = \alpha_0 + \alpha_1 D_t + \alpha_2 D_i + \alpha_{31} time + \alpha_{32} D_i * time + \beta' D_i * D_t + X'\gamma + \theta_i + \epsilon_{it} \quad (3)$$

where D_i is a vector of dummies for treated groups. The variable *time* is a time-trend, so α_{31} captures the evolution of earnings over time, the impact of macro shocks affecting earnings in both treated and control groups, while α_{32} allows for different age-group specific slopes. The most important coefficients in this regression are the βs, which represent the treatment effects; that is, capture the effects on earnings of the reform in the years after the reform.

5 Data and methodological decisions

We employ the eight waves of the Spanish component of the ECHP, covering the period 1993-2000[16].

[15] As in Kugler et al. (2002), we only exploit age groups for identification purposes, for other treated groups –the long-term unemployed and women under-represented in certain occupations– may be self-selected.

[16] The ECHP is a specially conducted survey and belongs to the ever more complete set of harmonised statistical operations for European Union (EU) countries. ECHP uses fixed panel techniques for 8 annual cycles. The population scope is the private households who live in main family dwellings and the set of people who are members of the household. In the case of Spain, the geographic scope is national with the exception of Ceuta and Melilla. The sample size is 70.000 household in the whole EU and some 8000 in Spain. The collection method is personal and phone interviews. Although the ECHP questionnaires cover the period 1994-2001, our sample period is 1993-2000 because the earnings questions refer to the year prior to the interview.

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

		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
Unemployed Workers				
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
Workers under Temporary contract				
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

As most previous studies, we study only men. This allows mitigating issues of endogenous female labour market participation. In particular, the sample is restricted to full-time male employees who reported positive earnings in at least one of the eight waves. Therefore, individuals are allowed to enter the panel at any wave and to re-enter the panel if they do exit. Such a sample selection produces an unbalanced panel since not all persons are present for all eight waves. Movements into and out of the earnings sample may be due to unemployment, retirement, mobility to or from self-employment and attrition[17]. We select male workers aged between 21 and 60, so the

[17]Attrition is typically a problem with panel data; members of the original sample are lost at successive interviews, causing the panel to decline in size and, quite possibly, to became unrepresentative. The incompleteness of the panel data may be due to a variety of reasons: people may refuse cooperation, households may not be located due to unnotified change of residence, or may split up losing subsequent track of some original household members.

chosen age range selects out the extremes of the earnings life cycle. The final sample consists of 5,268 individuals and a total of 20,605 individual-year observations.

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

Table 2 provides summary statistics by relevant age group of our sample. Descriptive statistics are presented for the period before and after the 1997 Reform. The last three rows suggest that the probability of getting a permanent contract or to make a transition from temporary to permanent employment might have increased after the reform. In the next section we investigate whether such figures can be taken at face value or, on the contrary, are rather misleading.

Table 2: **Descriptive Statistics by Age Group, Pre- and Post-Reform**

Variable	Age<30		Age 30-45		Age>45	
	Pre	Post	Pre	Post	Pre	Post
Log earnings	9,129	9,232	9,687	9,675	9,816	9,86
Age	25,64	25,51	36,78	36,42	51,27	51,17
% Married	30,00	25,00	80,09	76,14	91,53	88,79
% No Education	2,18	0,89	2,58	1,72	12,07	8,75
% Primary Education	14,63	8,00	22,17	13,24	41,97	37,02
% Secondary and Technical Education	75,28	77,06	55,92	66,00	27,86	34,75
% University Education	7,92	14,06	19,32	19,03	18,10	19,48
% with Permanent Contract	41,89	51,28	73,54	74,69	81,93	83,81
% with Transitory Contract	58,11	48,67	26,46	25,28	18,07	16,19
% with Transition from Temp to Perm	6,19	10,33	3,38	4,59	1,69	4,74
N	3070	1800	6153	3315	4027	2069

6 Estimated Effects of the 1997 Reform

This section presents the results of the implementation of the identification strategy discussed above. Table 3 reports fixed effects estimates of the earnings effect of the reform on men who have been unemployed at least once. The results in column (1) show a large and statistically significant increase in average earnings for young relative to middle-aged unemployed workers after the 1997 reform, but insignificant effects for

older unemployed men[18]. More precisely, young unemployed workers experienced an 8% earnings increase during the reform period relative to their middle-aged counterpart. Thus, our first important finding is that the reform had an overall positive effect on average earnings for the young unemployed but not for the older ones.

The estimates in column (2) suggest that such positive effect on young unemployed may be due to the impact the reform had on men who signed new (post-reform) permanent contracts (at t) transiting from unemployment (at $t-1$). Notice that such effect is absent for the older group. As expected, it is also nil for those transiting from unemployment to temporary employment, as the reform did not target such transitions. Such differential incidence on the age groups parallels the employment effects (increase in the transition probabilities from unemployment to permanent employment) found by Kugler et al. (2002) and Mendez (2005).

Table 3: **Effects of the Reform on Earnings Levels for Unemployed**

	Ever Unemployed (1)	Unemployed to Permanent (2)	Unemployed to Temporary (3)
Age < 30	-0.251**	-0.222**	-0.244**
Age > 45	0.008	0.055	0.104
(Age < 30)*Reform	0.088**	0.117*	-0.001
(Age > 45)*Reform	-0.051	0.028	0.056
N	14050	4523	5546

Notes: * denotes statistically significant at 10% and ** at 5%. Coefficients from fixed effect panel regressions. Control group is unemployed aged 30-45 years.

As pointed out above, the main goal of the reform was to lower the incidence of temporary employment. Thus, the reform promoted temporary to permanent employment transitions for all age groups by lowering dismissal costs and reducing payroll taxes. The somewhat larger reduction in payroll taxes for men aged over 45 years permits identification. How did the earnings of this group change as a consequence of the reform, relative to younger men under temporary contracts? The statistically insignificant estimates of the interaction term in Table 4 suggest that the reform appears to have no effect on older workers under temporary contracts, i.e. the larger payroll tax reductions did not bear any effect on the earnings of our treated group.

[18]Reported standard errors are Huber-White.

So, not only the reform did not induce transitions into permanent employment from temporary contracts, as Mendez (2005) finds out, but our results suggest that it also had a negligible effect on wage levels.

Table 4: **Effects of the Reform on Earnings Levels for Temporary Workers**

	Ever Temporary (1)	Temporary to Permanent (2)	Temporary to Temporary (3)
Age > 45	-0.078	-0.299**	-0.059
(Age > 45)*Reform	-0.052	0.009	-0.040
N	9085	5032	5349

Notes: * denotes statistically significant at 10% and ** at 5%. Coefficients from fixed effect panel regressions. Control group is temporary workers aged less than 46 years.

So far we have assessed the effects of the reform by means of comparison between treated and untreated groups. Comparisons between individuals of the same age group but on different employment transitions provide valuable complementary information on the effects of the reform. Table 5 shows that unemployed men who manage to get a permanent contract during the reform years do not see any earnings increase relative to those from the same age group but making different transitions, unless they are aged over 45 years. This result is especially relevant because unemployment to permanent transitions for this age group do not yield on average any earnings premium over the entire sample period. However, for the young and middle-aged groups our estimates suggest the opposite. As expected, unemployment to permanent transitions yield on average and over the sample period a (noticeable 10%) earnings increase, but such increase does not differ in the pre- and the post-reform years.

Table 5: **Effects of the Reform on Earnings Levels for Unemployed Workers Within Age Groups**

	Age <30	Age 30-45	Age > 45
Unemployment to Permanent Transition	0.105**	0.092**	0.002
Unemployment to Permanent Transition*Reform	0.027	-0.017	0.150**
N	4365	6170	3515

Notes: ** denotes statistically significant at 5%.

The last column in Table 6 provides information on the differential earnings effects

of the reform on older workers switching from temporary to permanent contracts as compared to equally older workers under a temporary contract at $t-1$ but not on such transition. As the estimated coefficient of the interaction between the reform and the temporary to permanent transition dummy shows, conditional on holding a temporary contract at $t-1$, those who jump to a permanent contract during the reform years obtain a substantial 16% earnings increase, relative to those not transiting to permanent employment. The first column shows that workers younger than 46 years transiting from temporary to permanent contracts after the reform experience no earnings increase, despite the 19% increase observed on average for men transiting from temporary to permanent employment within this age group.

Table 6: **Effects of the Reform on Earnings Levels for Workers under Transitory Contract at $t-1$ and Selected Within Age Groups**

	Age <45	Age > 45
Temporary to Permanent Transition	0.190**	0.147**
Temporary to Permanent Transition * Reform	0.018	0.160**
N	7702	1383

Notes: ** denotes statistically significant at 5%.

7 Final remarks

In this study we have analysed the consequences of the 1997 labour market reform on earnings. Since the reform applied only to certain age groups, we use a natural experiment research design and difference in difference estimators to assess the impact of the 1997 reform.

We find rather limited effects of the reform on earnings levels, affecting only the earnings of young workers who enter employment from unemployment. In particular, our results indicate that the earnings of these young workers increased as a result of the reform. In the light of a simple matching model, where lower dismissal costs result in lower wages while reductions in payroll taxes have the opposite effect, our results indicate that the effect of the former more than offseted the latter.

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