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ORIGINAL ARTICLE

Promoting permanent employment: lessons from Spain

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Abstract This paper analyzes whether the two major labor market reforms implemented in Spain in the 1990s to reduce the share of temporary employment succeed in promoting flows into permanent employment. The 1994 reform severely restricted temporary contracts and the 1997 reform introduced a new permanent contract figure with lower payroll taxes and dismissal costs than the ordinary. To evaluate these non-targeted treatments I present an estimation procedure that uses pre-treatment outcomes to predict the one that would have been otherwise observed in the post-treatment period in the absence of the treatment and I derive its large sample properties. Using data from the Spanish Labor Force Survey I find that both reforms failed at reducing the share of temporary employment because they had no impact on contract conversions, which account for most new permanent contracts. The 1997 reform succeed in increasing permanent hirings for some groups of workers. My findings suggest that Spanish employers took advantage of wage and dismissal cost reductions to substitute permanent contracts for otherwise temporary ones.

Keywords Permanent employment · Dismissal costs · Payroll taxes · Inverse probability weighted estimation · Semiparametric methods

JEL Classification J32 · J38 · J65

Introduction

Following the notable growth of unemployment rates until mid-1980s France, Germany, Greece, Italy, Netherlands, Portugal and Spain increased the flexibility of

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their labor markets by allowing employers to recruit under non-causal fixed-term (also called temporary) contracts. Although regulations vary, a common feature of fixed-term contracts is that severance pay and dismissal protection are lower than those for permanent contracts. Since their introduction, fixed-term contracts have accounted for most new hirings in these countries (OECD 1993).

Spain is a interesting case to study. Soon after the reform liberalizing temporary contracts the share of temporary employment was the highest within developed countries. The 1984 reform led to a dual labor market with a third of employees employed on a temporary basis, receiving lower wages than otherwise equivalent permanent employees (Bentolila and Dolado 1994), facing a higher work accident risk (Guadalupe 2003), and a lower probability of receiving formal training (Alba-Ramirez 1994), marrying or entering into parenthood (De la Rica and Iza 2005).

The magnitude of the phenomenon motivated the application of countervailing reforms aimed at promoting permanent employment. Firstly, the 1994 reform restored the principle of causality in the application of temporary contracts. Three years later a new major reform introduced a new permanent contract figure with lower payroll taxes and dismissal costs than the ordinary one, whose regulation remained unchanged. Any worker but the unemployed aged 30–45 years old could be hired under the new permanent contract.

In this paper I evaluate the two major labor market reforms implemented in Spain in the 1990s to lower the share of temporary employment. In particular, I analyze whether the 1994 and the 1997 reforms succeed in increasing transitions into permanent employment. The effect of the 1997 reform on net flows into permanent employment has already been analyzed in Kugler et al. (2005). By assuming that middle-aged non-employed workers were not eligible for the new contract, they implement a natural experiment research design and find that the reform improved eligible workers' transition into permanent employment probabilities, particularly so for men. Nevertheless, middle-aged workers were eligible for the new contract by simply being previously hired under a temporary contract, since no age eligibility criteria was stated for contract conversions.

Additionally, Kugler et al. (2005) do not separately identify the effect of the 1997 reform from that of the 1999 National Employment Plan (NEP), passed on December 1998. The NEP announced that payroll tax reductions would last for one additional year for permanent contracts signed until May 1999 and would be significantly lower in magnitude and length after that date. For these two reasons, the estimates in Kugler et al. (2005) are likely to lead to misleading conclusions about the effect of the 1997 reform.[1]

I present an estimation procedure that uses $T-1$ pre-treatment time periods to predict the outcome that would have been observed in the post-treatment period in the absence of the treatment and I derive its large sample properties. The before–after, the difference-in-differences and the difference-in-difference-in-differences estimators are particular cases of the general T periods estimator. I reinterpret these estimators in the context of non-targeted treatments (i.e. those applied to all employers and

[1] The same holds for the estimated earnings effects of the 1997 reform in Plá and Ramos (2007), since their identification strategy is that in Kugler et al. (2005).

workers) like the 1994 and 1997 reforms and the NEP. The identifying conditions of each estimator are tested using pre-treatment outcomes.

Estimates using the Spanish Labor Force Survey indicate that employers did not change their contract conversion practices in response to either the 1994 or the 1997 reforms. The restrictions on the use of the non-causal temporary contract led to a greater use of other types of temporary contracts rather than encouraging the use of permanent contracts. While the 1997 reform failed in its goal of promoting contract conversions, it succeed in improving permanent hirings for young workers, older men and middle-aged women. These findings, in line with existing evidence on the determinants of transitions into permanent employment in Spain, suggest that Spanish employers took advantage of wage and dismissal cost reductions to substitute permanent contracts for otherwise temporary ones.

Finally, estimates attest that employers reacted to the reduction in fiscal incentives for permanent contracts announced in the NEP by transitory increasing both permanent hires and contract conversions in the first half of the year 1999. This effect is identified for young and middle-aged workers and it is, for most groups of unemployed workers, substantially larger than that of the 1997 reform. The finding that middle-aged workers benefited from both the 1997 reform and the NEP empirically rejects the natural experiment research design in Kugler et al. (2005).

The rest of the paper is organized as follows. Section 1 reviews the major labor market reforms implemented in Spain from 1984 to 2000. Sections 2 and 3 describe the identification strategy and the data used in the estimation, respectively. Section 4 presents the estimation results and, finally, Sect. 5 concludes.

1 The institutional framework

Until 1984 temporary contracts in Spain were restricted to seasonal, occasional or temporary jobs and they accounted for less than 10 % of all existing jobs. The 1984 reform allowed employers to recruit under temporary contracts for all types of jobs and for a maximum length of 3 years. After that period the firm had to convert the temporary worker to a permanent status or to dismiss him.

In the early 1990s a third of Spanish employees held a temporary contract, the highest share of temporary employment within developed countries. The ratio increased rapidly in response to the cost gap between temporary and permanent hiring. Mandatory severance payments for permanent workers were 20 days' wages per year of tenure (up to one year's wages) if the dismissal was considered "fair", and 45 days' wages per year of tenure (up to 42 months of wages) if the worker disagreed with the dismissal and it was declared "unfair" in court.[2] In sharp contrast, dismissed temporary workers received an indemnity of 12 days' wages per year worked, which could not be appealed in labor courts. Moreover, the compensation was zero if the employer waited until the end of the contract length.

[2] According to Galdón-Sánchez and Güell (2003), during 1986–1998, on average, around 72 % of cases taken to court were declared unfair in Spain.

Table 1 Summary of payroll tax reductions for permanent contracts in Spain

Age group	May 1994	May 1997	January 1999	May 1999
Temporary workers				
16–45	2.400 euros[a]	50 %, 24 months 20 % third year	25 % additional year	25 %, 24 months
>45	50 % contract life and 3.000 euros	60 %, 24 months 50 % thereafter	Not modified	25 % contract life
Unemployed workers				
	–	40 %, 24 months	25 % additional year	35 % first year 25 % second year
>45	–	60 %, 24 months 50 % thereafter	Not modified	45 % first year 40 % thereafter

From 1997 onwards payroll tax reductions applied only to the new permanent contract figure and they were between 10 and 20 % points higher for women hired in occupations where women are underrepresented
[a] For workers aged less than 25 years old

The first major reform intended to reduce the incidence of temporary employment was enacted in 1994. The 1994 reform restored the principle of causality in the application of temporary contracts and introduced fiscal incentives for their conversion into permanent ones for workers aged less than 25 and over 45 years old. Furthermore, the procedural requirements for "fair" dismissals were relaxed and notice periods were shortened in an attempt to lower dismissal costs for permanent contracts.

Two years after this reform the share of temporary employment remained almost unchanged. The perceived inefficacy of the 1994 reform along with the fall of the socialist Government in 1996 and its replacement by a conservative Government with a different labor policy explain that a new major reform with the same goal were introduced only 3 years later.

The 1997 reform, enacted in May, was the first labor market reform agreed between trade unions and employers' organizations in Spain and, thus, the agreement was totally unexpected.[3] The reform introduced a new permanent contract figure with relevant differences with the existing one, whose regulation remained unchanged. First, mandatory severance pay for "unfair" dismissals was 33 days' wages per year of seniority (up to 24 months of wages) under the new contract. Second, payroll tax reductions ranging from 40 to 80 % and lasting for at least 2 years were introduced for the new permanent contract. The magnitude and length of tax reductions varied with workers' age and gender and they were, for young workers, higher for contract conversions than for permanent hirings. Table 1 summarizes payroll tax reductions for the different groups of workers.

Third, middle-aged unemployed workers could not be hired under the new contract. However, they could easily recover eligibility by simply being hired under a temporary contract since no age restriction was stated for contract conversions,

[3] Spanish newspapers informed that negotiations were likely to break down only one month before (El País, March 3, 1997).

as it has been highlighted in both economic and legal analysis of the reform like Toharia (2005) and Luján (2001), respectively. However, preceding economic evaluations of the 1997 reform have largely ignored this aspect of the reform and they have assumed that unemployed middle-aged workers were excluded by reformers.

Finally, on 30th December 1998 the Spanish government passed its National Employment Plan for 1999 (NEP) and announced that payroll tax reductions would last for one additional year for permanent contracts signed until May 1999, but they would be significantly lower in magnitude and length after that date.

2 The identification strategy

In this section I adapt the estimators commonly used in the evaluation of targeted treatments (i.e. those applied to certain employers and/or workers) to the evaluation of non-targeted treatments (i.e. those applied to all employers and workers). In particular, I present an estimation procedure that uses $T-1$ pre-treatment time periods to predict the outcome that would have been observed in the post-treatment period in the absence of the treatment and I derive its large sample properties. The before–after (BA), the difference-in-differences (DD) and the difference-in-difference-in-differences (DDD) estimators are particular cases of the general T periods estimator. The effect of interest is identified using the simplest estimator whose identifying condition is not rejected in the pre-treatment period.

For simplicity, the identification strategy is presented for unemployed workers. Let $Y(i,t)$ be the outcome of interest for individual i at time t. This variable equals one if individual i moves from unemployment at the beginning of period t to a permanent contract in that period and zero otherwise. Additionally, $X(i,t)$ is a vector of predetermined characteristics of individual i. A non-targeted policy aimed at promoting the hiring of permanent workers, i.e. the 1997 reform, is enacted at the beginning of period $t=1$. Variable D_i indicates whether individual i is observed in the last pre-treatment period ($D_i=0,\ t=0$) or in the post-treatment period ($D_i=1,\ t=1$).

Following Rubin (1974) and Heckman (1990) causality is defined in terms of potential outcomes. Variable $Y_0(i,t)$ is the outcome that individual i would attain at time t if he had not been affected by the treatment. Equivalently, variable $Y_1(i,t)$ is the outcome that individual i would experience at time t if he had received the treatment. Individual causal effects cannot be computed since just one of these potential outcomes is observed for given values of i and t and, thus, I focus on the average effect of the treatment on the treated (ATET):

$$ATET = E\left[Y_1(i,t) - Y_0(i,t)\,|t=1\right] = E\left[Y_1(i,t)\,|t=1\right] - E\left[Y_0(i,t)\,|t=1\right].$$

The ATET cannot be identified using observational data since $Y_0(i,t)$ is only observed for those unemployed in the pre-treatment period. A suitable solution is to approximate the proportion of treated unemployed workers that would have obtained a permanent position in the absence of the treatment by the proportion observed in the last pre-treatment period. The validity of this approximation increases once differences in the distribution of covariates are controlled for. Under this approximation the ATET is calculated as:

$$ATET = E\left[Y\left(i,t\right)|X\left(i,t\right), t=1\right] - E\left[Y\left(i,t\right)|\,X\left(i,t\right),\ t=0\right]. \tag{1}$$

This is a BA-type estimator and its power to identify the ATET relies on temporal stability Holland (1986). In particular, two conditions must be met for that to be the case: (i) unobserved individual characteristics and changing aggregate labor market conditions do not affect permanent hires or their overall average impact remains constant over time; (ii) the effect of events other than the treatment that happen between these two periods do not contaminate the causal analysis. The identification assumption for this estimator is written as:

Assumption 3.1 $E\left[Y_0\left(i,t\right)|X\left(i,t\right), t=1\right] = E\left[Y_0\left(i,t\right)|\,X\left(i,t\right),\ t=0\right]$.

This assumption is examined by sequentially estimating Eq. (1) in the pre-treatment period. Under Assumption 3.1, the vector of pre-treatment estimates is not significantly different from zero and conditioning on observables suffices to identify the ATET. This approach is named selection on observables (Barnow et al. 1981) in the treatment effects literature. However, if some pre-treatment estimates differ from zero, the BA estimator does not necessarily provide a suitable approximation to the effect of interest.[4] In that case I consider a DD-type estimator assuming that the average conditional (on X) outcome is not constant but experiences a constant increment over time in the absence of the treatment.[5] The identifying condition for this estimator is Assumption 3.2. Hereinafter the individual and time arguments i and t will dropped out to simplify the notation.

Assumption 3.2 $E\left[Y_0|X, t=1\right] - E\left[Y_0|X, t=0\right] = E\left[Y_0|X, t=0\right] - E\left[Y_0|X, t=-1\right]$.

The ATET is now written as:

$$\begin{aligned} ATET = &\{E\left[Y|X, t=1\right] - E\left[Y|\,X,\ t=0\right]\} \\ &- \{E\left[Y|\,X,\ t=0\right] - E\left[Y|\,X,\ t=-1\right]\}. \end{aligned} \tag{2}$$

As before, Assumption 3.2 is tested by sequentially estimating Eq. (2) using pre-treatment outcomes. If the vector of pre-treatment DD estimates significantly differs from zero I consider an estimator assuming that the average conditional outcome increases/decreases at a constant rate in the absence of the treatment.

Assumption 3.3 Let Δ_τ^0 denote the increment in the average conditional outcome that would have been observed in the absence of the treatment between periods $t=\tau-1$ and $t=\tau$, that is, $\Delta_\tau^0 = E[Y_0|X, t=\tau] - E[Y_0|X, t=\tau-1]$, then $\Delta_1^0 - \Delta_0^0 = \Delta_0^0 - \Delta_{-1}^0$.

4 Similarly, Blundell et al. (2001) check whether the estimated effect of a mandatory job search assistance program lies within typical values of the pre-treatment estimates.

5 The conventional DD estimator assumes that the increment in the average conditional outcome is independent of treatment assignment in the absence of the treatment (Angrist and Krueger 1999).

To compactly write this estimator let Δ_τ be the increment in the average conditional outcome between periods $t = \tau - 1$ and $t = \tau$, that is, $\Delta_\tau = E[Y|X, t = \tau] - E[Y|X, t = \tau - 1]$, then

$$ATET = \{\Delta_1 - \Delta_0\} - \{\Delta_0 - \Delta_{-1}\}. \tag{3}$$

This is a DDD-type estimator (Meyer 1995). More sophisticated estimators could be defined for the cases in which Assumption 3.3 is rejected, but that is not the case in the current application.

Implementation is now derived for the DDD estimator and discussed for the general estimator using $T - 1$ pre-treatment time periods. This estimator includes the DDD ($T = 4$), the DD ($T = 3$) and the BA ($T = 2$) estimators as three particular cases. The implementation and the asymptotic properties of the estimators are derived using the original results developed in Abadie (2005) for the DD estimator.

Since identification is attained after conditioning on covariates, it is required that for a given value of the covariates there is some fraction of the population in the pre-treatment period to be used as controls. That is, $P(D = 1) > 0$ and with probability one $P(D = 1|X) < 1$. Some additional notation is needed at this point. Let $D_\tau \in \{0, 1\}$ indicate whether the worker is observed at period $t = \tau$ ($D_\tau = 1$) and let Υ_τ be

$$\Upsilon_\tau = \frac{D_\tau}{P(t = \tau|X)} - \frac{D_{\tau-1}}{P(t = \tau - 1|X)}.$$

Lemma 3.1 *If Assumption 3.3 holds, and for values of X such that* $0 < P(t = 1|X) < 1$*, we have* $E[Y_1(1) - Y_0(1)|X, t = 1] = E[\rho Y|X]$ *where* $\rho = (\Upsilon_1 - \Upsilon_0) - (\Upsilon_0 - \Upsilon_{-1})$.[6]

The expression for ρ *is obtained by replacing the conditional expectations at* $t = \tau$ *by expressions like* $(D_\tau/P(t = \tau|X))$ *in Eq. (4). The ATET is now written as*

$$E[Y_1(1) - Y_0(1)|t = 1] = E\left[\frac{Y}{P(t = 1)} \underbrace{P(t = 1|X)\,\rho}_{\omega}\right], \tag{4}$$

where

$$\omega = D_1 - 3D_0\frac{P(t = 1|X)}{P(t = 0|X)} + 3D_{-1}\frac{P(t = 1|X)}{P(t = -1|X)} - D_{-2}\frac{P(t = 1|X)}{P(t = -2|X)}.$$

Equation (4) suggests a simple two-step method to estimate the ATET. First, conditional probabilities are estimated by means of discrete choice models and fitted values of $P(t = k|X)$ are computed for $k = \{1, 0, -1, -2\}$. Second, fitted values are plugged into the sample analog of Eq. (4). Under Assumption 3.3, a weighted average of the

[6] The proof of Lemma 3.1 immediately follows from the corresponding proof in Abadie (2005).

outcome variable recovers the ATET. The weighting function ω imposes the distribution of covariates for treated unemployed workers at any pre-treatment period. The expressions of ρ and ω in the general T-periods estimator are

$$\rho = \sum_{j=0}^{T-1} \left[\frac{D_{j-(T-2)} \left(-1\right)^{j+T+1} \binom{T-1}{j}}{P\left(t = j-(T-2)\,|X\right)} \right],$$

$$\omega = \sum_{j=0}^{T-1} \left[\frac{D_{j-(T-2)} \left(-1\right)^{j+T+1} \binom{T-1}{j} P\left(t=1|X\right)}{P\left(t = j-(T-2)\,|X\right)} \right].$$

The corresponding expressions for the DDD, the DD and the BA estimators are those for $T = 4$, $T = 3$ and $T = 2$, respectively. The asymptotic properties of the estimators are derived in the Appendix. Finally, the discrete nature of the dependent variable may imply that the identification assumptions do not hold for the expectations but for some transformation thereof. Following Blundell et al. (2001), I assume that the assumptions hold, if anything, for the inverse of the probability function, which I assume to be the inverse logistic.

3 The data

The data is drawn from the rotating panel version of the Spanish Labor Force Survey (*Encuesta de Población Activa: Estadística de flujos* (EPA)). This nationally representative survey is carried out on a quarterly basis on a sample of approximately 64,000 households. Each household is interviewed for a maximum of six consecutive quarters and every quarter one sixth of the sample is renewed. The sample period ranges from the second quarter of the year 1987 to the fourth quarter of the year 2000.

Employers, self-employed, agricultural and family workers, coop members and those aged 65 and over are dropped from each quarter total sample. Sociodemographic information such as gender, age, level of education, region of residence, marital status, whether the individual is the head of his household or not and the number of employed household members but him is included in the analysis. Equivalently, individual employment records such as tenure at current job and sector of activity are also included. The same information is considered for unemployed workers referred to their latest job, if any. Similarly, I also control for the length of their current unemployment spell, whether they receive unemployment benefits or not and whether they have previous work experience or not.

Information on tenure at current and previous job is based on self-reported elapsed duration. The EPA records the answers in months whenever elapsed duration is lower than one year and in years otherwise. Following Güell and Petrongolo (2007), I randomly replace each rounded elapsed duration by one of the quarterly durations implied.

Tables 2 and 3 summarize the data for temporary and unemployed workers, respectively. The outcome of interest is the share of unemployed or temporary workers at a given quarter that hold a permanent contract in the following quarter. Transition into permanent employment probabilities are substantially lower after the 1994 reform

Table 2 Descriptive statistics by gender and age groups (temporary workers)

	Pre-1994 reform			Between-reforms			Post-1997 reform		
	<30	30–45	>45	<30	30–45	>45	<30	30–45	>45
Men									
Age	20.9	33.9	49.8	21.5	33.9	49.5	21.4	34.0	49.5
Tenure (in months)	29.1	34.6	33.7	17.1	22.7	23.2	26.9	39.4	38.1
Private sector	91.4	89.3	87.8	92.4	90.0	89.3	92.7	89.4	88.4
Head of household	11.9	72.9	92.2	11.6	70.8	91.9	8.6	65.4	89.1
Married	15.2	78.1	91.6	13.5	75.7	91.5	9.1	70.1	89.2
No education	1.6	10.2	32.8	0.9	5.2	23.7	0.7	3.3	16.6
Primary education	21.8	53.3	58.4	14.7	38.2	59.5	12.2	31.0	60.4
Secondary education	53.5	23.6	5.4	54.8	38.4	11.5	57.1	45.5	16.4
Technical education	17.5	6.3	1.6	22.0	9.9	2.7	16.7	9.0	2.8
University education	5.6	6.7	1.9	7.6	8.3	2.6	13.2	11.1	3.8
Permanent contract									
Probability	8.55	11.01	9.72	4.21	5.61	4.70	4.62	4.69	3.68
N	75,207	32,787	14,565	37,356	21,974	9,210	49,695	29,304	12,351
Women									
Age	20.6	34.0	49.3	21.4	34.0	48.6	21.6	34.0	48.7
Tenure (in months)	30.5	38.1	50.4	17.9	28.4	31.7	25.1	42.9	48.6
Private sector	84.3	75.9	85.5	87.7	74.8	83.2	87.5	70.1	80.4
Head of household	1.8	11.2	22.4	2.9	12.6	21.4	3.5	14.9	25.5
Married	14.4	69.5	72.3	14.8	68.0	75.4	12.1	64.8	71.8
No education	1.1	9.4	35.0	0.6	4.2	23.4	0.3	2.0	13.4
Primary education	12.8	43.0	52.7	8.0	29.5	53.7	5.9	20.0	53.0
Secondary education	52.8	26.7	7.9	49.4	36.2	15.1	47.3	41.9	23.4
Technical education	19.4	6.3	1.5	24.3	12.4	3.7	18.2	11.2	4.2
University education	14.0	14.7	2.9	17.7	17.8	4.1	28.4	24.8	6.1
Permanent contract									
Probability	8.10	8.30	8.32	4.36	4.78	4.91	4.30	3.37	3.03
N	50,103	17,305	6,561	25,188	13,425	4,420	35,258	19,826	5,806

The table reports means and percentages for continuous and discrete variables, respectively

than before. The same holds for the 1997 reform regarding contract conversions. Conversely, unemployment to permanent employment transition probabilities increase once the 1997 reform is enacted for both young and middle-aged workers.

Figures 1 and 2 provide a more detailed description of the outcome of interest for men and women temporary workers, respectively. Figures 3 and 4 do so for men and women unemployed workers, respectively. Unlike preceding tables, these figures allow us to distinguish between the post-1997 reform and the post-NEP periods. It follows that transition into permanent employment probabilities follow a loosely monotonically decreasing time trend from the beginning of the sample period and they become

Table 3 Descriptive statistics by gender and age groups (unemployed workers)

	Pre-1994 reform			Between-reforms			Post-1997 reform		
	<30	30–45	>45	<30	30–45	>45	<30	30–45	>45
Men									
Age	20.5	33.9	51.0	21.0	33.9	50.3	21.0	34.0	50.5
Worked before	58.7	96.8	99.9	64.7	96.4	99.9	59.8	95.0	99.7
Head of household	6.6	62.2	89.8	5.8	53.8	88.2	4.4	50.6	83.5
Married	9.1	65.9	86.7	7.6	58.2	84.8	5.0	50.8	79.9
No education	2.5	12.4	38.4	1.8	7.3	28.5	1.1	4.1	19.3
Primary education	23.4	51.6	51.1	17.0	37.4	54.5	13.2	31.8	53.8
Secondary education	51.6	23.7	6.7	53.1	38.6	11.3	53.8	43.1	19.0
Technical education	15.0	5.6	1.6	18.7	9.4	3.2	15.3	8.8	3.2
University education	7.4	6.7	2.3	9.4	7.3	2.5	16.6	12.2	4.7
Permanent contract									
Probability	2.26	2.86	2.11	0.91	1.27	1.05	1.47	1.64	1.65
N	56,850	19,170	11,560	31,819	15,555	8,785	26,551	13,393	7,639
Women									
Age	20.5	33.6	48.9	21.1	33.7	48.8	21.3	33.8	48.8
Worked before	45.4	71.9	73.2	54.8	83.5	83.1	52.6	83.6	85.2
Head of household	1.0	8.8	20.2	1.6	90.1	22.0	2.2	11.9	21.6
Married	16.7	71.2	73.9	17.0	69.6	74.3	13.9	69.1	75.1
No education	1.3	6.7	27.3	1.0	4.1	21.9	0.6	2.8	14.1
Primary education	13.6	39.8	56.0	9.9	26.7	52.4	7.4	20.6	50.6
Secondary education	53.0	32.6	12.0	48.7	40.4	18.5	48.0	44.7	25.3
Technical education	18.1	7.7	1.8	22.6	14.7	4.0	18.8	13.5	4.9
University education	14.1	13.3	2.8	17.7	14.2	3.2	25.2	18.5	5.2
Permanent contract									
Probability	1.43	1.04	1.15	0.59	0.53	0.78	1.14	0.93	0.67
N	68,950	18,727	5,131	38,057	18,160	5,396	37,619	20,967	6,555

The table reports means and percentages for continuous and discrete variables, respectively

stable in 1994, coinciding with both the recovery of the Spanish economy and the 1994 reform. While no significant change is observed after the 1997 reform, transitions substantially increase during the first half of the year 1999, that is, during the transitory period of higher generosity in fiscal incentives for permanent contracts announced in the NEP.

4 Empirical results

Table 4 summarizes the estimates presented in Tables 5 (BA), 6 (DD) and 7 (DDD) for temporary workers and in Tables 8 (BA) and 9 (DD) for unemployed workers. For each policy under evaluation and for each group of workers Table 4 informs on which

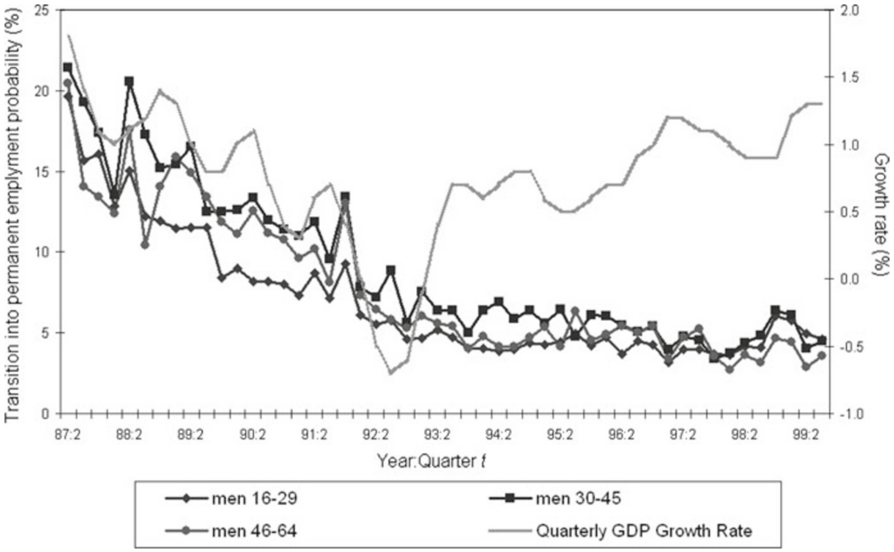

Fig. 1 Temporary (quarter t) to permanent employment ($t + 1$) transition probabilities (men)

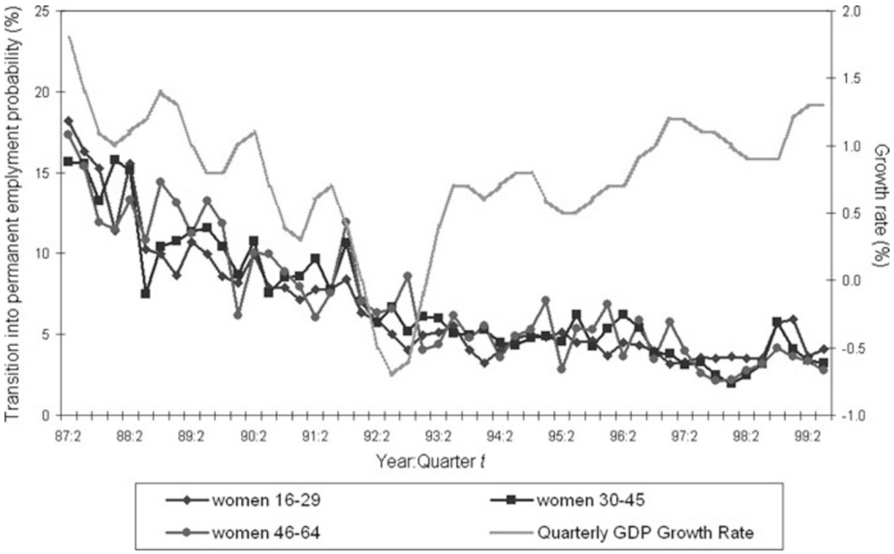

Fig. 2 Temporary (quarter t) to permanent employment ($t + 1$) transition probabilities (women)

is the estimator whose identifying conditions are held in the pre-treatment period, on the estimated effect and on how it reflects on the increase of the transition into permanent employment probability.

For estimation purposes, the sample period is organized as follows. As in Sect. 1, period $t = 1$ is the post-treatment period for the 1997 reform. It collects flows into permanent employment between quarters τ and $\tau + 1$, for $\tau = \{1997{:}3,\ 1997{:}4,\ 1998{:}1\}$.

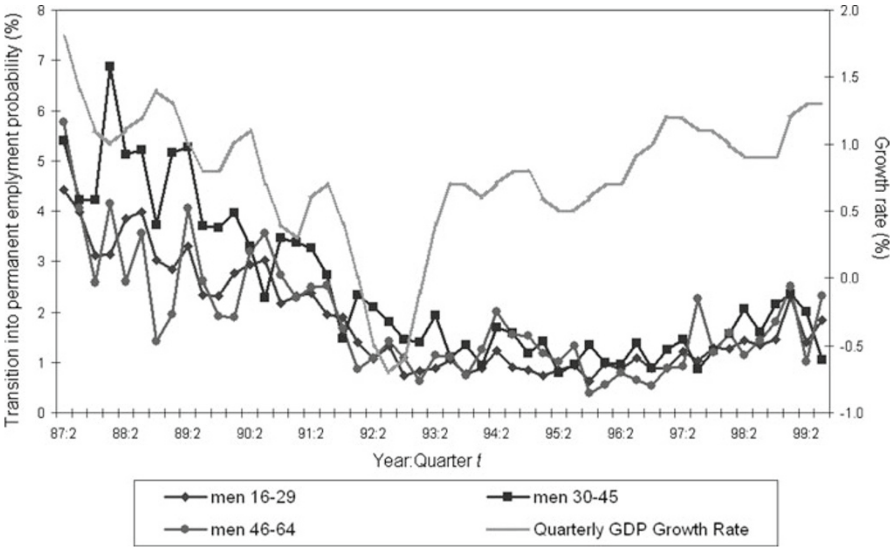

Fig. 3 Unemployment (quarter t) to permanent employment ($t+1$) transition probabilities (men)

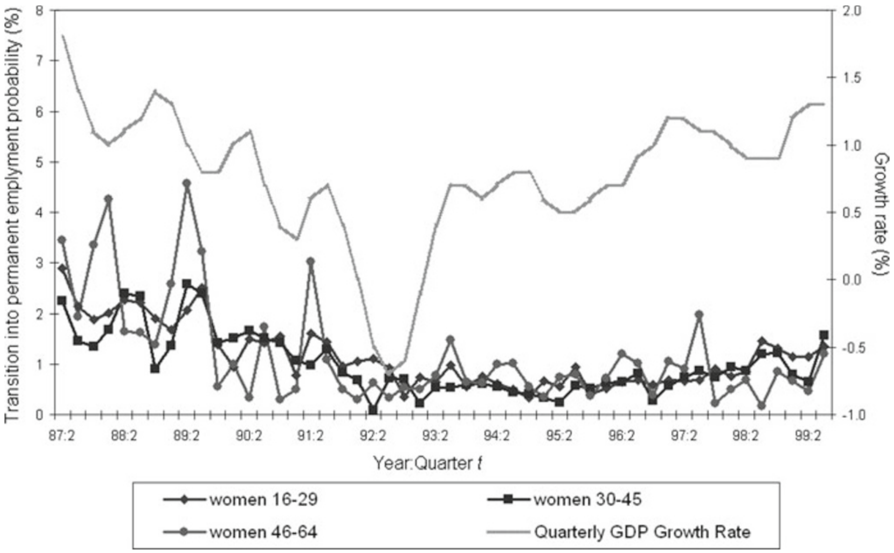

Fig. 4 Unemployment (quarter t) to permanent employment ($t+1$) transition probabilities (women)

Remaining time periods are defined to include the same distribution of quarters. In particular, period $t = -2$ is the post-treatment period for the 1994 reform and period $t = 2$ includes the first half of the year 1999 and, thus, the period of higher fiscal incentives for permanent contracts defined in the NEP. Seasonal effects are controlled for by including the quarter at which the worker is observed as an element of X.

Table 4 The effect of the 1994 and 1997 reforms and the NEP on transitions into permanent employment (summary of estimation results)

Policy	Period	Results	Men			Women		
			16–29	30–45	46–64	16–29	30–45	46–64
Temporary workers								
1994 reform	$t = -2$	Estimator	DDD	DD	DD	DDD	DD	BA
	1994:3–1995:1	Estimate	−0.0037	0.0113	0.0118	−0.0915	0.0092	0.0049
			[−0.55]	[0.67]	[0.76]	[−0.63]	[0.93]	[0.74]
		% increase[a]	0	0	0	0	0	0
1997 reform	$t = 1$	Estimator	DD	DD	BA	BA	DD	BA
	1997:3–1998:1	Estimate	0.0061	−0.0025	−0.0074	−0.0027	−0.0056	−0.0059
			[0.33]	[−0.73]	[−0.51]	[−0.63]	[−0.36]	[−0.61]
		% increase	0	0	0	0	0	0
1999 NEP	$t = 2$	Estimator	DD	DD	BA	BA	DD	BA
	1998:3–1999:1	Estimate	0.0141***	0.0299***	0.0015	0.0117**	0.0397***	0.0100
			[2.85]	[2.74]	[0.90]	[2.96]	[3.28]	[0.94]
		% increase	37.4	43.2	0	32.7	71.0	0
Unemployed workers								
1994 reform	$t = -2$	Estimator	DD	DD	BA	DD	DD	BA
	1994:3–1995:1	Estimate	0.0005	0.0047	0.0061	−0.0033	−0.0027	−0.0008
			[0.17]	[0.59]	[0.89]	[−1.47]	[−0.95]	[−0.61]
		% increase	0	0	0	0	0	0
1997 reform	$t = 1$	Estimator	BA	BA	BA	BA	BA	BA
	1997:3–1998:1	Estimate	0.0022*	0.0004	0.0094***	0.0017*	0.0033**	0.0005
			[1.68]	[0.15]	[2.71]	[1.71]	[2.28]	[0.49]
		% increase	22.9	0	59.3	26.2	61.1	0

Table 4 continued

Policy	Period	Results	Men			Women		
			16–29	30–45	46–64	16–29	30–45	46–64
1999 NEP	$t = 2$	Estimator	BA	BA	BA	BA	BA	BA
	1998:3–1999:1	Estimate	0.0051**	0.0070**	0.0010	0.0049***	0.0030*	−0.0033
			[2.13]	[2.15]	[0.93]	[3.11]	[1.81]	[−0.50]
		% increase	42.5	35.7	0	62.0	35.7	0

The table reports t-statistics in brackets. Standard errors are calculated using the delta method. The estimates control for the worker's level of education, region of residence, marital status, whether he is the head of his household or not, the number of employed household members but him and the quarter at which he is observed. For temporary workers, tenure at current job and for the establishment's sector of activity are also controlled for. The same information is considered for unemployed workers referred to their latest job, if any. Moreover, I also control for the length of their current unemployment spell, whether they receive unemployment benefits or not and whether they have previous work experience or not

BA, DD and DDD indicate before–after, difference-in-differences and difference-in-difference-in-differences estimates, respectively

*, ** and *** denote significance at the 10, 5 and 1 % level, respectively

[a] Percentage increase in the probability of getting a permanent contract with respect to what would have been otherwise observed in the absence of the treatment

Table 5 Before–after estimates (temporary workers)

Period	Quarters	Men			Women		
		16–29	30–45	46–64	16–29	30–45	46–64
$t=-8$	1988:3–1989:1	−0.0317***	−0.0078	0.0015	−0.0529*	−0.0510*	0.0088
		[−3.13]	[−1.59]	[0.73]	[−2.87]	[−3.05]	[0.87]
$t=-7$	1989:3–1990:1	−0.0205***	−0.0325 ***	−0.0159	−0.0015	0.0007	−0.0218
		[−2.78]	[−2.64]	[−1.42]	[−0.29]	[0.47]	[−1.17]
$t=-6$	1990:3–1991:1	−0.0223***	−0.0176**	−0.0225*	−0.0167***	−0.0235***	−0.0104
		[−2.71]	[−2.39]	[−1.68]	[−2.93]	[−2.89]	[−1.38]
$t=-5$	1991:3–1992:1	−0.0044	−0.0127*	−0.0111	−0.0005	0.0018	−0.0026
		[−1.39]	[−1.88]	[−1.43]	[−0.11]	[0.78]	[−0.54]
$t=-4$	1992:3–1993:1	−0.0170***	−0.0218***	−0.0374***	−0.0220***	−0.0196***	−0.0033
		[−2.65]	[−2.79]	[−3.05]	[−3.04]	[−2.78]	[−0.48]
$t=-3$	1993:3–1994:1	−0.0066*	−0.0142**	−0.0080	−0.0044	−0.0091	−0.0046
		[−1.78]	[−2.08]	[−1.32]	[−1.06]	[−1.47]	[−1.15]
$t=-2$	1994:3–1995:1	0.0011	0.0006	0.0029	0.0074**	−0.0011	0.0049
		[0.52]	[0.28]	[0.33]	[1.99]	[−0.63]	[0.74]
$t=-1$	1995:3–1996:1	0.0041	−0.0017	0.0019	−0.0058	0.0048	−0.0023
		[0.81]	[−0.36]	[0.84]	[−0.93]	[0.47]	[−0.19]
$t=0$	1996:3–1997:1	−0.0070**	−0.0079**	−0.0061	−0.0032	−0.0107**	−0.0034
		[−2.13]	[−2.09]	[−0.73]	[−0.31]	[−2.07]	[−0.24]
$t=1$	1997:3–1998:1	−0.0010	−0.0114***	−0.0074	−0.0027	−0.0173***	−0.0059
		[−0.30]	[−2.74]	[−0.51]	[−0.63]	[−2.84]	[−0.61]
$t=2$	1998:3–1999:1	0.0129***	0.0167***	0.0015	0.0117***	0.0179***	0.0100
		[3.47]	[3.19]	[0.90]	[2.96]	[3.41]	[0.94]
$t=3$	1999:3–2000:1	−0.0090***	−0.0157***	−0.0122*	−0.0120***	−0.0145***	−0.0139*
		[−2.78]	[−3.08]	[−1.72]	[−3.12]	[−2.96]	[−1.78]

The before–after estimate for period t compares transitions into permanent employment in periods t and $t-1$ as indicated in Eq. (1). The table reports t-statistics in brackets. Standard errors are calculated using the delta method. The estimates control for the worker's level of education, region of residence, marital status, whether he is the head of his household or not, the number of employed household members but him, the quarter at which he is observed, tenure at current job and the establishment's sector of activity
*, ** and *** denote significance at the 10, 5 and 1 % level, respectively

According to Table 4, transition into permanent employment probabilities are more stable for unemployed than for temporary workers since the estimators that identify the effects of interest are always at most as sophisticated for unemployed than for temporary workers. In particular, while the conditional probability of getting a permanent position lowers from the beginning of the sample period until 1994 for both young unemployed and temporary workers, it does so at a constant rate in the former case and at a decreasing rate in the latter one. Thus, the estimators that identify the effect of the 1994 reform for young unemployed and temporary workers are the DD and the DDD estimators, respectively.

Table 6 Difference-in-differences estimates (temporary workers)

Period	Quarters	Men			Women	
		16–29	30–45	46–64	16–29	30–45
$t = -7$	1989:3–1990:1	0.0114	−0.0233	−0.0163	0.0506***	0.0415
		[1.47]	[−1.39]	[−1.10]	[2.86]	[1.50]
$t = -6$	1990:3–1991:1	−0.0016	0.0142	−0.0071	−0.0150	−0.0270
		[−0.41]	[1.28]	[−0.78]	[−1.31]	[−1.43]
$t = -5$	1991:3–1992:1	0.0166**	0.0030	0.0055	0.0174**	0.0258
		[1.98]	[0.37]	[0.82]	[2.43]	[1.18]
$t = -4$	1992:3–1993:1	−0.0105*	−0.0115	−0.0226	−0.0121	−0.0169
		[−1.90]	[−1.27]	[−1.07]	[−1.32]	[−1.07]
$t = -3$	1993:3–1994:1	0.0124**	0.0132	0.0032	0.0308*	0.0161
		[2.07]	[0.84]	[1.34]	[2.77]	[1.02]
$t = -2$	1994:3–1995:1	0.0073	0.0113	0.0118	0.0157*	0.0092
		[0.51]	[0.67]	[0.76]	[1.79]	[0.93]
$t = -1$	1995:3–1996:1	0.0028	−0.0020	0.0018	−0.0131*	0.0075
		[0.79]	[−0.59]	[0.58]	[−1.82]	[0.60]
$t = 0$	1996:3–1997:1	−0.0089	−0.0059	−0.0072	0.0023	−0.0165
		[−1.09]	[−0.51]	[−1.14]	[0.45]	[−0.76]
$t = 1$	1997:3–1998:1	0.0061	−0.0025	−0.0002	0.0002	−0.0056
		[0.33]	[−0.73]	[−0.87]	[0.52]	[−0.36]
$t = 2$	1996:3–1997:1	0.0141***	0.0299***	0.0136	0.0134**	0.0397***
		[2.85]	[2.74]	[1.51]	[2.26]	[3.28]
$t = 3$	1997:3–1998:1	−0.0166**	−0.0225**	0.0001	−0.0233***	−0.0347***
		[−2.31]	[−2.18]	[0.94]	[−2.63]	[−3.13]

The difference-in-differences estimate for period t compares transitions into permanent employment in periods t, $t-1$ and $t-2$ as indicated in Eq. (1)

The table reports t-statistics in brackets. Standard errors are calculated using the delta method. The estimates control for the worker's level of education, region of residence, marital status, whether he is the head of his household or not, the number of employed household members but him, the quarter at which he is observed, tenure at current job and the establishment's sector of activity

*, ** and *** denote significance at the 10, 5 and 1 % level, respectively

Additionally, BA estimates in Tables 5 and 8 attest that while the probability of a permanent hire becomes stable from 1994 onwards for young and middle-aged workers, that of a contract conversion lowers again from 1996 onwards for young men and for middle-aged temporary workers. Differences in the dynamics of the outcome variable between unemployed and temporary workers almost vanish when looking at older workers. Their probability of a permanent position remains unchanged with respect to that in the preceding period during the whole sample period for any collective of older workers but for men before the 1994 reform. Thus, the BA estimator suffices to identify the effects of interest for older workers but that of the 1994 reform for older men temporary workers, identified using the DD estimator.

Table 7 Difference-in-difference-in-differences estimates (young temporary workers)

Period	Quarters	Men	Women
$t = -6$	1990:3–1991:1	−0.0102	−0.0662**
		[−1.48]	[−1.98]
$t = -5$	1991:3–1992:1	0.0172	0.0344**
		[0.83]	[2.11]
$t = -4$	1992:3–1993:1	−0.0213	−0.0191
		[−1.32]	[−1.17]
$t = -3$	1993:3–1994:1	0.0194	−0.0220
		[1.26]	[−0.74]
$t = -2$	1994:3–1995:1	−0.0037	−0.0915
		[−0.55]	[−0.63]
$t = -1$	1995:3–1996:1	−0.0047	−0.0352**
		[−0.53]	[−2.38]
$t = 0$	1996:3–1997:1	−0.0139	0.0147
		[−1.22]	[1.36]
$t = 1$	1997:3–1998:1	0.0171**	−0.0021
		[2.07]	[−1.18]
$t = 2$	1995:3–1996:1	0.0064	0.0117
		[1.53]	[1.61]
$t = 3$	1999:3–2000:1	−0.0274	−0.0430**
		[1.47]	[2.30]

The difference-in-difference-in-differences estimate for period t compares transitions into permanent employment in periods t, $t-1$, $t-2$ and $t-3$ as indicated in Eq. (3)
The table reports t-statistics in brackets. Standard errors are calculated using the delta method. The estimates control for the worker's level of education, region of residence, marital status, whether he is the head of his household or not, the number of employed household members but him, the quarter at which he is observed, tenure at current job and the establishment's sector of activity
*, ** and *** denote significance at the 10, 5 and 1 % level, respectively

Estimates in Table 4 indicate that the 1994 reform did not affect transition into permanent employment probabilities for either temporary or unemployed workers. This suggests that temporary hiring was a deep-rooted practice among Spanish employers at the time of the 1994 reform and neither fiscal incentives nor the restrictions to the use of one type of temporary contract (the non-causal one) led to a greater use of other types but not to encourage the use of permanent contracts. The evidence in Malo and Toharia (1999) supports this interpretation. They find that causal temporary contracts increased their relevance in total hirings by 20 % points from 1994 to 1997, an increment of similar magnitude to the relevance of non-causal temporary contracts in total hirings in the year 1991.

Estimates for period $t = 1$ in Table 4 show that the 1997 reform failed in its goal of promoting contract conversions. Conversely, it succeed in improving permanent hirings for young workers, older men and middle-aged women. The share of temporary employment remained almost unchanged after the reform since permanent hirings

Table 8 Before–after estimates (unemployed workers)

Period	Quarters	Men			Women		
		16–29	30–45	46–64	16–29	30–45	46–64
$t = -8$	1988:3–1989:1	−0.0024	−0.0090	−0.0191	−0.0009	0.0026	−0.0177
		[−0.75]	[−0.99]	[−1.11]	[−0.54]	[0.19]	[−0.28]
$t = -7$	1989:3–1990:1	−0.0100***	−0.0022	−0.0078	−0.0044*	0.0006	−0.0189
		[−2.79]	[−0.32]	[−1.41]	[−1.88]	[0.43]	[−0.71]
$t = -6$	1990:3–1991:1	−0.0004	−0.0108*	0.0075	−0.0033*	−0.0024	−0.0064
		[−1.02]	[−1.78]	[0.53]	[−1.75]	[−0.43]	[−0.57]
$t = -5$	1991:3–1992:1	−0.0066***	−0.0027	−0.0129	−0.0008	−0.0027	−0.0007
		[−2.81]	[−0.76]	[−1.39]	[−0.17]	[−0.85]	[−0.49]
$t = -4$	1992:3–1993:1	−0.0082***	−0.0061*	−0.0039	−0.0041 ***	−0.0051*	−0.0011
		[−2.57]	[−1.91]	[−1.17]	[−2.97]	[−1.78]	[−0.41]
$t = -3$	1993:3–1994:1	−0.0004	−0.0022	−0.0007	0.0008	0.0006	0.0049
		[0.87]	[−0.50]	[−0.51]	[0.32]	[0.41]	[0.95]
$t = -2$	1994:3–1995:1	−0.0005	0.0028	0.0061	−0.0027***	−0.0016	−0.0008
		[−0.74]	[0.27]	[0.89]	[−2.39]	[−0.54]	[−0.61]
$t = -1$	1995:3–1996:1	0.0003	−0.0032	−0.0106	0.0012	0.0022	−0.0016
		[−0.35]	[−0.57]	[0.28]	[0.43]	[0.65]	[−0.31]
$t = 0$	1996:3–1997:1	0.0013	0.0022	−0.0001	0.0002	−0.0009	0.0036
		[−1.05]	[0.11]	[−0.19]	[0.74]	[−0.84]	[0.51]
$t = 1$	1997:3–1998:1	0.0022*	0.0004	0.0094***	0.0017*	0.0033**	0.0005
		[1.68]	[0.15]	[2.71]	[1.71]	[2.28]	[0.49]
$t = 2$	1998:3–1999:1	0.0051**	0.0070**	0.0010	0.0049***	0.0030*	−0.0033
		[2.13]	[2.15]	[0.93]	[3.11]	[1.81]	[−0.50]
$t = 3$	1999:3–2000:1	−0.0001	−0.0027	0.0023	−0.0015	0.0001	0.0043**
		[−0.22]	[0.51]	[0.27]	[0.42]	[0.19]	[−1.99]

The before–after estimate for period t compares transitions into permanent employment in periods t and $t-1$ as indicated in Eq. (1)

The table reports t-statistics in brackets. Standard errors are calculated using the delta method. The estimates control for the worker's level of education, region of residence, marital status, whether he is the head of his household or not, the number of employed household members but him, the quarter at which he is observed, tenure and sector of activity referred to his latest job, if any, the length of their current unemployment spell, whether they receive unemployment benefits or not and whether they have previous work experience or not

*, ** and *** denote significance at the 10, 5 and 1 % level, respectively

accounted for less than 15 % of new permanent contracts at that time according to own calculations using the EPA. The estimated effects for young men and women amount to an improvement of 22.9 and 26.2 % in their probability of moving from unemployment to a permanent position, respectively. The increase was substantially higher for older men and middle-aged women. Their probability of obtaining a permanent position increased by approximately 60 % with respect to what would have been otherwise observed in the absence of the 1997 reform.

Table 9 Difference-in-differences estimates (unemployed workers)

Period	Quarters	Men		Women	
		16–29	30–45	16–29	30–45
$t = -7$	1989:3–1990:1	−0.0077	0.0085	−0.0042	−0.0053
		[−0.89]	[0.91]	[−0.18]	[−0.73]
$t = -6$	1990:3–1991:1	0.0113**	−0.0081	0.0020	−0.0043
		[2.08]	[−1.37]	[−0.75]	[−1.52]
$t = -5$	1991:3–1992:1	−0.0050	0.0050	0.0022	0.0003
		[−0.42]	[0.81]	[0.93]	[0.47]
$t = -4$	1992:3–1993:1	−0.0031	−0.0024	−0.0023	−0.0057
		[−0.31]	[−1.01]	[−0.59]	[−0.53]
$t = -3$	1993:3–1994:1	0.0076*	0.0044	0.0039	0.0079
		[1.76]	[0.71]	[1.31]	[0.73]
$t = -2$	1994:3–1995:1	0.0005	0.0047	−0.0033	−0.0027
		[0.17]	[0.59]	[−1.47]	[−0.95]
$t = -1$	1995:3–1996:1	0.0010	−0.0065	0.0038	0.0039
		[0.47]	[−0.44]	[0.61]	[1.18]
$t = 0$	1996:3–1997:1	0.0012	0.0051	−0.0012	−0.0031
		[0.52]	[0.83]	[−0.92]	[−0.31]
$t = 1$	1997:3–1998:1	0.0010	−0.0012	0.0017	0.0045*
		[0.63]	[−1.08]	[1.48]	[1.87]
$t = 2$	1998:3–1999:1	0.0033	0.0076***	0.0032	0.0002
		[1.45]	[2.77]	[1.31]	[0.57]
$t = 3$	1999:3–2000:1	−0.0049**	−0.0095**	−0.0074***	−0.0026
		[−2.28]	[−2.31]	[−2.03]	[−1.47]

The before–after estimate for period t compares transitions into permanent employment in periods t and $t-1$ as indicated in Eq. (1)

The table reports t-statistics in brackets. Standard errors are calculated using the delta method. The estimates control for the worker's level of education, region of residence, marital status, whether he is the head of his household or not, the number of employed household members but him, the quarter at which he is observed, tenure and sector of activity referred to his latest job, if any, the length of their current unemployment spell, whether they receive unemployment benefits or not and whether they have previous work experience or not

*, ** and *** denote significance at the 10, 5 and 1 % level, respectively

The estimates in Table 4 also attest that employers reacted to the NEP by increasing both permanent hires and contract conversions during the first half of 1999. This effect is identified for young and middle-aged workers and it is, for most groups of unemployed workers, substantially larger than that of the 1997 reform. The estimated effects amount to an improvement of at least 35 % in the probability of getting a permanent position. The increment becomes highest for middle-aged women temporary workers, whose probability of obtaining a permanent contract improved by 71 % after the NEP. Anyway, these were transitory effects, as suggested by the negative and significant estimates obtained for young and middle-aged temporary and unemployed workers in period $t = 3$ using the same estimator that identifies the effect of the NEP in Table 4.

These findings are in line with evidence on the determinants of transitions into permanent employment in Spain. In particular, Amuedo-Dorantes (2001) examines the determinants of Spanish employers' reliance on temporary workers between the years 1990–1996 using a representative sample of establishments with more than five employees.[7] She finds that changes in wage and dismissal costs for permanent contracts affect the hiring of permanent workers but have almost no impact on contract conversions, which primarily respond to employers' flexibility needs and unions' pressures for increased employment stability. She concludes that Spanish employers might be unwilling to forgo employment flexibility through contract conversion regardless of the employment cost.

Recent findings on the duration and total labor cost of the permanent contract introduced in 1997 are useful to interpret the estimates in Table 4. Cebrian et al. (2005) find, using administrative records on permanent contracts signed in Spain between the years 1996 and 1999, that permanent hires under the new contract increase the hazard rate of ending the contract by 17–32 % relative to that of ordinary permanent contracts and lead to less stable trajectories, i.e. to a higher probability of the worker being unemployed or out of the labor market in the subsequent years. In a related study, Cueto (2006) finds that hires under the new contract are more stable than those under the ordinary permanent contract while the former enjoys wage benefits, becoming less stable afterwards. Malo and Toharia (1999) calculate the total labor costs, including dismissal costs, of the new permanent contract and find that it is marginally cheaper than a temporary contract while wage benefits are operative.

These findings, jointly with the estimates in Table 4, suggest that Spanish employers took advantage of wage and dismissal costs reduction measures to substitute permanent contracts for otherwise temporary ones. This further explains why the share of temporary employment did not decrease after the 1997 reform. In a related setting, García-Pérez and Rebollo (2009) evaluate the effectiveness of regional wage subsidies in Spain to foster permanent employment and find that the outflow into permanent employment improves only minimally.

It is worth noting that middle-aged unemployed women increased their transition into permanent employment probabilities after both the 1997 reform and the NEP according to the estimates in Table 4. For middle-aged unemployed men, it was the NEP the policy that improved their permanent employment prospects. These findings empirically rejects the natural experiment research design for non-employed workers in Kugler et al. (2005). They recognize that no age eligibility criteria was stated for temporary workers but they abstract from the possibility that non-employed middle-aged workers at quarter τ can held a new permanent contract at the following quarter if they are previously hired under a temporary contract that is shortly promoted into the new permanent one. Indeed, that would also be the case for young workers since wage reductions for these workers were higher for contract conversions than for permanent hirings after the 1997 reform according to Table 1.

Own calculations using the EPA confirm that the average tenure of the temporary contracts converted into permanent ones lowered after the 1997 reform for young work-

[7] Own calculations using the EPA indicate that establishments with more than five employees account from 1990 to 1996, on average, for 84 and 71 % of men and women employees in Spain, respectively.

ers and, particularly so, for middle-aged women, while it remained almost unchanged for the other groups of workers. For young workers, the average tenure of the contracts converted in the year after the 1997 reform was 2 and 3 months lower than it was one year before the reform for men and women, respectively. For middle-aged women, the reduction in the average tenure of converted contracts amounts to 7 months. For these workers, temporary contracts lasting for at most 3 months increased their relevance in the total number of converted contracts by more than 11 % points in that period. The increment was of approximately 5 % points for young workers and it was negligible for the remaining groups of workers.

Another reason why the results in Kugler et al. (2005) might be misleading is because they do not separately identify the effect of the 1997 reform from that of the NEP. As Table 4 indicates, this is particularly relevant for temporary workers since they were not affected by the 1997 reform. While Kugler et al. (2005) present joint estimates for temporary workers of all age groups and find that contract conversions increased after the 1997 reform, estimates in Table 4 qualify that it was only after the NEP when contract conversions increased for these workers.

Regarding non-employed workers, Kugler et al. (2005) find that young and older men improved their transition into permanent employment probabilities after the 1997 reform relative to the "excluded" group of middle-aged workers. To investigate whether the definition of the post-reform period is driving their results, Table 10 presents between-group estimates where I compare, for each period t, young and older men unemployed workers' transition probabilities to that of the middle-aged.[8] I find no significant estimate from 1993 to 1998 for either young or older men. That is, by adopting a natural experiment research design I reach to the conclusion that it was the NEP not the 1997 reform the policy that improved young and older men's flows into permanent employment.

5 Conclusions

This paper analyzes whether the two major labor market reforms implemented in Spain in the nineties to lower the share of temporary employment succeed in promoting flows into permanent employment. The 1994 reform restored the principle of causality in the application of temporary contracts and the 1997 reform introduced a new permanent contract with lower payroll taxes and dismissal costs than the ordinary one, whose regulation remained unchanged.

Although this is not the first evaluation of the 1997 reform, the estimates in preceding studies are likely to lead to misleading conclusion for two reasons. First, they assume that non-employed middle-aged workers could not be hired under the new contract. However, no age eligibility criteria was stated for contract conversions and, thus, employers could hire non-employed middle-aged workers under a temporary contract and promote it to the new permanent contract. Second, they do not separately identify the effect of the 1997 reform from that of the 1999 National Employment

8 For men, the difference between unemployed and non-employed workers is not likely to be an issue leading to different estimation results.

Table 10 Between-groups estimates (men unemployed workers)

Period	Quarters	16–29	46–64
$t = -9$	1987:3–1988:1	0.0114	0.0036
		[0.82]	[1.47]
$t = -8$	1988:3–1989:1	0.0043	0.0199
		[0.31]	[1.27]
$t = -7$	1989:3–1990:1	−0.0096	0.0238
		[−0.39]	[1.63]
$t = -6$	1990:3–1991:1	−0.0061	0.0107*
		[−0.85]	[1.95]
$t = -5$	1991:3–1992:1	0.0121*	0.0093
		[1.72]	[0.39]
$t = -4$	1992:3–1993:1	0.0227*	0.0025
		[1.89]	[0.97]
$t = -3$	1993:3–1994:1	0.0043	0.0062
		[0.63]	[0.36]
$t = -2$	1994:3–1995:1	−0.0020	−0.0033
		[−0.42]	[−0.42]
$t = -1$	1995:3–1996:1	0.0009	−0.0023
		[0.48]	[−0.98]
$t = 0$	1996:3–1997:1	−0.0027	0.0089
		[−0.94]	[1.53]
$t = 1$	1997:3–1998:1	0.0003	−0.0001
		[0.31]	[−0.72]
$t = 2$	1998:3–1999:1	0.0125*	0.0090**
		[1.94]	[2.38]
$t = 3$	1999:3–2000:1	0.0129*	−0.0090*
		[1.77]	[−2.25]

The difference-in-differences estimate for period t compares transitions into permanent employment in periods t, $t-1$ and $t-2$ as indicated in Eq. (2)

The table reports t-statistics in brackets. Standard errors are calculated using the delta method. The estimates control for the worker's level of education, region of residence, marital status, whether he is the head of his household or not, the number of employed household members but him, the quarter at which he is observed, tenure and sector of activity referred to his latest job, if any, the length of their current unemployment spell, whether they receive unemployment benefits or not and whether they have previous work experience or not

*, ** and *** denote significance at the 10, 5 and 1 % level, respectively

Plan (NEP). The NEP was passed on December 1998 announcing that fiscal incentives for permanent contracts would be significantly lower for contracts signed after May 1999.

I present an estimation procedure that uses $T-1$ pre-treatment time periods to predict the outcome that would have been otherwise observed in the post-treatment period in the absence of the treatment and I derive its large sample properties. The before–after, the difference-in-differences and the difference-in-difference-in-differences estimators are particular cases of the general T periods estimator for $T = 2$, 3 and 4, respectively. I reinterpret these estimators in the context of non-targeted treatments (i.e. those applied to all employers and workers) like the 1994 and 1997 reforms and the NEP. The identifying conditions of each estimator are tested using pre-treatment outcomes.

Estimates using the Spanish Labor Force Survey suggest that employers did not change their contract conversion practices in response to either the 1994 or the 1997 reforms. The restrictions on the use of the non-causal temporary contract led to a greater use of other types of temporary contracts rather than encouraging the use of permanent contracts. While the 1997 reform failed in its goal of promoting contract conversions, it succeed in improving permanent hirings for young workers, older men and middle-aged women. Given the lower stability of permanent hires under the new contract relative to that of ordinary permanent contracts, this finding suggests that Spanish employers took advantage of wage and dismissal cost reductions to substitute permanent contracts for otherwise temporary ones.

Finally, I find that employers reacted to the NEP by transitory increasing both permanent hires and contract conversions during the first half of 1999. This effect is identified for young and middle-aged workers and it is, for most groups of unemployed workers, substantially larger than that of the 1997 reform.

The finding that middle-aged workers improved their transition into permanent employment probabilities after both the 1997 reform and the NEP rejects the natural experiment research design in preceding evaluations of the 1997 reform. I provide evidence suggesting that employers used temporary hiring as way of securing the access of middle-aged non-employed workers to the new permanent contract. In particular, the average tenure of temporary contracts converted into permanent ones lowered after the 1997 reform for middle-aged workers and, to a lesser extent, for workers for which payroll tax reductions were higher for contract conversions than for permanent hirings, as it was the case for young workers.

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Appendix: Asymptotic properties

This section adapts the asymptotic results developed in Abadie (2005) for the DD estimator to the general T-periods estimator. The expressions for the DDD, DD and BA estimates are those for $T = 3$, $T = 2$ and $T = 1$, respectively. Consider the following estimate of the ATET

$$\beta_0 = \arg\min_{\beta\,\in\Theta} E\left[\pi_1 \{\rho Y - \beta\}^2\right]$$

$$\rho = \sum_{j=0}^{T-1} \left[\frac{D_{j-(T-2)}(-1)^{j+T+1}\binom{T-1}{j}}{\pi_{j-(T-2)}(X)} \right],$$

and $\pi_k(X) = \pi_k(X_i'\gamma_0) = P(t = k/X)$, for $k = \{1, 0, -1, -2, \ldots\}$. Provided that the standard regularity conditions in Abadie (2005, Assumption 4.2(i)–(iv)) hold, γ_0 can be estimated by means of standard discrete choice models like the probit or logit models and $\widehat{\gamma}$ is asymptotically linear, that is, $n^{1/2}(\widehat{\gamma} - \gamma_0) = n^{-1/2}\sum_{i=1}^{n}\psi_{\gamma_0}(Z_i) + O_p(1)$, where

$$\psi_{\gamma_0}(Z) = E\left[i_{\gamma_0} i_{\gamma_0}'\right]^{-1} i_{\gamma_0},\ i_{\gamma_0} = X\left[\sum_{j=0}^{T-1} D_{j-(T-2)}\pi_{j-(T-2)}^{-1}\dot{\pi}_{(j-(T-2))0}\right].$$

Let

$$M_{\gamma_0} = E\left[\left(W_{\gamma_0} - \dot{\pi}_{10}\beta_0\right)X'\right],$$

with

$$W_{\gamma_0} = \sum_{j=0}^{T-2}(-1)^{j+T+1}\binom{T-1}{j} D_{j-(T-2)}\frac{\dot{\pi}_{10}\pi_{j-(T-2)} - \pi_1\dot{\pi}_{(j-(T-2))0}}{\pi_{j-(T-2)}^2},$$

where $\dot{\pi}_k = \partial\pi_k(v)/\partial v$ and $\dot{\pi}_{k0} = \dot{\pi}_k(X'\gamma_0)$, for $k = \{1, 0, -1, -2, \ldots\}$. Under the conditions stated in the following theorem, and provided that β_0 is an interior point of a compact set $\Theta \subset \mathbb{R}$ and $EY^2 < \infty$, $\widehat{\beta}$ is well-defined with probability approaching one.

Theorem A1 *If $n_k \to \infty$ for each k, and provided that the identification assumption of the general $T-$periods estimator holds, $\sqrt{n}(\widehat{\beta} - \beta_0) \xrightarrow{d} N(0, V)$, where $V = Q^{-1}\Sigma Q^{-1}$, $Q = E[D_1]$, $\Sigma = E[\psi\psi']$, $\psi = m(Z, \beta_0, \gamma_0) + M_{\gamma_0}\psi_{\gamma_0}$, and $m(Z, \beta_0, \gamma_0) = \pi_1(\rho Y - \beta_0)$. Let $\widehat{V} = \widehat{Q}^{-1}\widehat{\Sigma}\widehat{Q}^{-1}$, where*

$$\widehat{Q} = \frac{1}{n}\sum_{i=1}^{n} D_{1i},\ \widehat{\Sigma} = \frac{1}{n}\sum_{i=1}^{n}\widehat{\psi}_i\widehat{\psi}_i',\ \widehat{M}_{\hat{\gamma}} = \frac{1}{n}\sum_{i=1}^{n}\left(\widehat{W}_{\hat{\gamma}i} - \dot{\pi}_1\left(X_i'\hat{\gamma}\right)\widehat{\beta}\right)X_i',$$

$$\widehat{W}_{\hat{\gamma}i} = \sum_{j=0}^{T-2}(-1)^{j+T+1}\binom{T-1}{j} D_{j-(T-2)} \times\frac{\dot{\pi}_1\left(X_i'\hat{\gamma}\right)\widehat{\pi}_{j-(T-2)}(X_i) - \widehat{\pi}_1(X_i)\dot{\pi}_{j-(T-2)}\left(X_i'\widehat{\gamma}\right)}{\widehat{\pi}_{j-(T-2)}^2(X_i)},$$

$$\widehat{\psi}_{\hat{\gamma}}(Z_i) = E\left[i_{\hat{\gamma}} i_{\hat{\gamma}}'\right]^{-1} i_{\hat{\gamma}},\ i_{\hat{\gamma}} = X_i\left[\sum_{j=0}^{T-1} D_{j-(T-2)}\widehat{\pi}_{j-(T-2)}^{-1}(X_i)\dot{\pi}_{j-(T-2)}\left(X_i'\widehat{\gamma}\right)\right],$$

where $\widehat{\pi}_k(X_i) = \pi_k(X_i'\widehat{\gamma})$ *and* $\widehat{\psi}_i = \widehat{\pi}_1(X_i)(\widehat{\rho}_i Y_i - \widehat{\beta}) + \widehat{M}_{\widehat{\gamma}}\widehat{\psi}_{\widehat{\gamma}}(Z_i)$. *A formal proof of Theorem A1 can be easily derived from the proof of Theorem 4.3 in* Abadie (2005) *by simply replacing* ρ *and the first step likelihood function by its expressions for the general* T*-period estimator. Similarly, it can be shown that under the assumptions of Theorem A1 and assuming that* $\pi_k(\upsilon)$ *is twice differentiable with bounded second derivative in* υ, $\widehat{V} \xrightarrow{p} V$ *(see* Abadie 2005, Theorem 4.4*).*

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