Promoting Permanent Employment: Lessons from Spain

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Abstract

This paper presents the first joint evaluation of the two major labour market reforms implemented in Spain to foster permanent employment in 1994 and 1997. 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. To evaluate these non-targeted treatments I present a family of semiparametric estimators that predict the outcome that would have been observed in the absence of a treatment by exploiting the time series variation of the outcome in the pre-treatment period. Alternative counterfactuals are also explored by means of conventional between-groups estimators. Estimates using the Spanish Labour Force Survey indicate that employers did not change their contract conversion practices in response to either the 1994 or the 1997 reform. I also find that the 1997 reform succeed in increasing unemployment to permanent employment transition probabilities for most groups of unemployed workers, including the middle-aged. This result rejects the natural experiment research design in existing papers analyzing the effects of the 1997 reform.

JEL Classification: J23, J32, J38, J63, J65.

Keywords: Permanent employment, Temporary employment, dismissal costs, payroll taxes.

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1 Introduction

Following the notable growth of unemployment rates until mid-1980s, France, Germany, Greece, Italy, Netherlands, Portugal and Spain increased the flexibility of their labour markets by allowing employers to recruit under non-causal fixed-term (also called temporary) contracts.[1] Although regulations vary, a common feature of fixed-term contracts is that severance pay and dismissal protection are lower than those for indefinite or permanent contracts. Since their introduction, fixed-term contracts have accounted for most new hirings in these countries (OECD, 1993).

Spain is a fascinating case to study. Soon after the reform liberalizing fixed-term contracts in 1984 the share of temporary employment was highest within developed countries. The 1984 reform led to a dual labour market with a third of employees permanently employed on a temporary basis, receiving lower wages than otherwise equivalent permanent employees (Bentolila and Dolado, 1994; De la Rica, 2004), facing a higher work accident risk (Guadalupe, 2003), and also a lower probability of receiving formal training (Alba-Ramirez, 1994), of marrying and of entering into parenthood (De la Rica and Iza, 2005).[2]

The magnitude of the phenomenon placed fixed-term contracts and its consequences at the center of the political debate and motivated the application over the 1990s of countervailing reforms aimed at promoting permanent employment.[3] The 1994 reform restored the principle of causality in the application of temporary contracts (i.e. there needed to be an objective cause to use them) and offered fiscal incentives for their conversion into permanent ones for certain groups of workers. The 1997 reform further lowered the cost gap between temporary and permanent hiring

[1] Other six European countries already had no limits on the use of temporary contracts. See OECD (2004) for an overview of employment protection reforms.

[2] The literature evaluating the outcome of these reforms *at the margin* show that they increase both job creation and job destruction and the variability of employment. However, the overall impact on equilibrium unemployment is ambiguous. Alonso-Borrego *et al.* (2005) calibrate a general equilibrium model with firing costs to Spanish data and find that fixed-term contracts increase unemployment, reduce output, and raise productivity. Blanchard and Landier (2002) and Zhou (2006) find that the introduction of non-causal fixed-term contracts in France increased equilibrium unemployment. Finally, Kahn (2007) evaluates the impact of employment protection reforms in several European countries and finds no evidence that policies making it easier to create temporary jobs raise employment.

[3] In France, the applicability of fixed-term contracts was reduced in 1990.

by introducing a new permanent contract with lower payroll taxes and dismissal costs than the ordinary one, whose regulation remained unchanged. Any worker except for the unemployed aged 30 to 45 years old could be hired under the new permanent contract.

The goal of this paper is to evaluate the success of the 1994 and 1997 reforms in promoting transitions into permanent employment. By providing an assessment of the effectiveness of these policies, this paper contributes to the debate on the consequences of wage and dismissal cost reductions on employers' hiring and conversion practices.[4]

While little attention has been paid to the 1994 reform,[5] the effect of the 1997 reform on the proportion of temporary workers, transitions into permanent employment, earnings and workers' perception of job insecurity have been analyzed in Dolado *et al.* (2002), Kugler *et al.* (2003), Plá and Ramos (2007) and Trevisan (2007), respectively. By assuming that middle-aged workers were not elegible for the new permanent contract, they find that the 1997 reform reduced the incidence of temporary employment in the private sector, improved elegible workers' transitions into permanent employment and earnings and reduced their perception of job insecurity.

However, the identification strategy in these papers is not correct and, thus, their conclusions are misleading. Middle-aged unemployed workers could easily recover eligibility by simple being hired under a temporary contract, since no age eligibility criteria was stated for temporary workers. That is, the 1997 reform is a non-targeted reform (i.e. it applies to all employers and workers) and, thus, those papers do not identify the effect of dismissal cost reductions but the effect of differences in payroll tax reduction schemes across groups of workers.

Furthermore, the post-reform period in those papers confounds the effect, if any, of the 1997 reform with that coming from the 1999 National Employment Plan (NEP). The 1999 NEP, passed on 30th December 1998, announced that payroll tax reductions would last for one additional year for permanent contracts signed until May 1999 but

[4] See OECD (1999) and Nickell and Layard (1999) for an overview of this debate.

[5] Güell and Petrongolo (2007) provide an exception. By estimating duration models for temporary employment before and after the 1994 reform, they find that the 1994 reform improved contract conversions for women, the youth and the less-skilled. However, they do not control for cyclical effects, which seems relevant given that the 1994 reform coincides with the recovery of the Spanish economy after a short but severe recession.

they would be significantly lower in magnitude and length after that date.

The identification strategy in this paper allows me to identify the effect of non-targeted treatments like the 1994 and 1997 reforms and the 1999 NEP. I present a family of semiparametric estimators that predict the outcome that would have been observed in the absence of a non-targeted treatment by exploiting the time series variation of the outcome in the pre-treatment period. Alternative counterfactuals are also explored by means of between-groups estimators. Moreover, I separately identify the effect of the 1997 reform from that of the 1999 NEP.

Estimates using the Spanish Labour Force Survey (LFS) indicate that employers did not change their contract conversion practices in response to either the 1994 or the 1997 reform. The restrictions on the use of one type of temporary contract (the non-causal one) probably led to a greater use of other types but not to encourage the use of permanent contracts. Additionally, wage and dismissal cost reductions had no effect on contract conversions, which primarily respond to employers' flexibility needs and union pressures for increased employment stability (Amuedo-Dorantes, 2001).

Regarding unemployed workers, while the 1994 reform had no impact on the hiring of permanent workers, the 1997 reform increased transition into permanent employment probabilities for most groups of unemployed workers, including the middle-aged. This empirical evidence rejects the natural experiment research design in preceding papers evaluating the 1997 reform.

Finally, estimates attest that employers reacted to the announced reduction in fiscal incentives for permanent contracts by increasing permanent hires and contract conversions in the first half of 1999. This transitory effect is found for most groups of workers, including the middle-aged, and is, for unemployed workers, substantially larger than that coming from the 1997 reform.

To resume, estimates in this paper do not support the hypothesis that wage and, in particular, dismissal costs are at the center of Spanish employers' reliance on temporary employment.

The rest of the paper is organized as follows. Section 2 reviews the content of the major labour market reforms implemented in Spain in the last decades. Section 3 presents the identification strategy. Section 4 describes the data used in the esti-

mation. Section 5 presents and discusses the estimation results and, finally, Section 6 concludes.

2 The Institutional Framework

Until 1984 temporary contracts in Spain were restricted to seasonal, occasional or temporary jobs and they accounted for less than 10 percent 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 three years. After that period the firm had to convert the temporary worker to a permanent status or to dismiss him.

In the early 1990s the share of temporary employment was a third. The rapid increase in the number of temporary contracts was the result of 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.[6] In sharp contrast, dismissed temporary workers received an indemnity of 12 days' wages per year worked, which could not be appealed in labour courts. Moreover, the compensation was zero if the employer waited until the end of the contract, which, moreover, tends to be very short.[7]

The first reform designed 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, in an attemp to reduce dismissal costs for permanent contracts the procedural requirements for "fair" dismissals were relaxed and notice periods were shortened.

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

[6] Spanish labour courts tend to rule in favor of workers. Over 70 percent of terminations appealed to courts between 1986 and 2003 were ruled in favor of workers (Galdon-Sanchez and Güell, 2000).

[7] Own calculations using the Spanish Labour Force Survey indicate that a fourth of temporary contracts signed between 1987 and 2000 lasted for up to three months. The corresponding percentage for those lasting up to one year is 70 percent.

socialist Government in 1996 and its replacement by a conservative Government with a different labour policy explain the new attemp to promote permanent employment.

The 1997 reform was the result of several months of tough negotiations between employers' organizations and the two major unions.[8] The reform was enacted in May 1997 and it introduced a new permanent contract 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 permanent contract. Second, payroll tax reductions ranging from 40 to 80 percent and lasting for at least two years were introduced for permanent hires and contract conversions under the new contract. Third, middle-aged unemployed workers were not elegible for the new contract. However, this restriction was not binding in practice since they could easily recover eligibility by simply being hired under a temporary contract.

Finally, on 30th December 1998 the Spanish government passed the National Employment Plan (NEP) for 1999. The 1999 NEP 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. Table 1 summarizes fiscal incentives for permanent contracts included in the 1994 and 1997 reforms and the 1999 NEP.

3 The identification strategy

In this section I adapt the estimators commonly used in the treatment effects literature for 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). For simplicity, the identification strategy is presented for unemployed workers and for the case in which there is only one treatment. The same reasoning works for temporary workers and for the other treatments under evaluation.

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

[8] The agreement was totally unexpected. Spanish newspapers informed that negotiations were likely to break down only one month before the agreement was announced (*El Pais*, March 3, 1997).

a permanent contract in that period and zero otherwise. Additionally, let $X(i,t)$ be a vector including information recorded at the beginning of period t relative to individual i that is a priori thought to influence his probability of obtaining a permanent contract. A non-targeted policy aimed at promoting the hiring of permanent workers is enacted at the beginning of period $t = 1$. Variable D_i indicates whether individual i is observed in the pre-treatment period ($D_i = 0$, $t < 1$) 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 a given individual at a given period. Thus, the evaluation literature analyzes average measures of the effect of the treatment. I focus on the average gain of receiving treatment for those who effectively receive the treatment.[9] This quantity is known as the average treatment effect on the treated (ATET) and is written as follows:

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

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 would be to approximate the proportion of treated unemployed workers that would have obtained a permanent job in the absence of the treatment by the proportion observed in the last pre-treatment period. The credibility of this approximation is higher once differences in the distribution of covariates are controled for. Under this approximation the ATET is written as follows:[10]

$$ATET = E[Y(i,t) | X(i,t), t = 1] - E[Y(i,t) | X(i,t), t = 0] \quad (2)$$

This is a before-after-type (BA) estimator and its power to identify the ATET relies on temporal stability (Holland, 1986). In particular, two conditions must be met: (i)

[9] The average effect of the treatment for the treated is equivalent to that for an individual randomly drawn from the population in the evaluation of non-targeted treatments.

[10] Existence of expectations is assumed throughout.

unobserved individual characteristics and changing aggregate labour 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 assumption underlying this estimator is formally written as follows:

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

Following Meyer (1995), Assumption 3.1 is examined by sequentially estimating equation (2) in the pre-treatment period.[11] Under Assumption 3.1 the vector of pre-treatment estimates is not significantly different from zero and, thus, conditioning on observables suffices to identify the ATET.[12] If Assumption 3.1 is rejected, the BA estimator does not provide information on the outcome that would have been observed in the post-treatment period in the absence of the treatment and, thus, it does not identify the ATET. In that case I consider an alternative estimator and examine its identification assumption in the pre-treatment period.

The next estimator that I consider is a difference-in-differences-type (DD) estimator assuming that the average conditional (on X) outcome experiences a constant increment over time in the absence of the treatment. Equivalently, in the evaluation of targeted treatments the DD estimator identifies the ATET under the assumption that the average conditional outcome experiences the same increment over time for the treated and control groups in the absence of the treatment.[13] The identification assumption underlying the DD estimator is the following:[14]

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

[11] Meyer (1995) stated that "an underemphasized advantadge of a long time-series for outcome measures is that they may allow the researcher to examine the hypothesis underlying the implemented estimator".

[12] This approach is named selection on observables (Barnow, Cain, and Goldberger (1981)), ignorable treatment assignment (Rosenbaum and Rubin (1983)) or conditional independence assumption (Lechner (1999)).

[13] See Angrist and Krueger (1999) for an overview of the difference-in-differences estimator in the evaluation of targeted treatments.

[14] Hereinafter the individual and time arguments i and t will dropped out to simplify the notation.

Under Assumption 3.2 the ATET can be expressed as follows:

$$\begin{aligned} ATET &= \{E[Y|X, t=1] - E[Y|X, t=0]\} \\ &\quad - \{E[Y|X, t=0] - E[Y|X, t=-1]\}. \end{aligned} \tag{3}$$

As before, Assumption 3.2 is tested by sequentially estimating equation (3) in the pre-treatment period. If the vector of pre-treatment DD estimates is significantly different from zero I move to an estimator identifying the ATET under the assumption that the average conditional outcome increases at a constant rate in the absence of the treatment. This assumption is formally written as follows:

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*

$$\Delta_1^0 - \Delta_0^0 = \Delta_0^0 - \Delta_{-1}^0$$

To compactly write the estimator that identifies the ATET under Assumption 3.3, let Δ_τ represent 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

$$ATET = \{\Delta_1 - \Delta_0\} - \{\Delta_0 - \Delta_{-1}\} \tag{4}$$

This estimator is a difference-in-difference-in-differences-type (DDD) estimator.[15] The DD and BA estimators are particular cases of the DDD estimator. If the average conditional outcome follows a linear time trend or remains constant in the absence of the treatment Assumption 3.3 simplifies to Assumption 3.2 or Assumption 3.1, respectively. More sophisticated estimators could be defined for the case in which Assumption 3.3 is rejected. However, the discussion is limited to those implemented in the analysis.

Abadie (2005) develops a simple two-step method to estimate the effect of a targeted treatment on the treated using the DD estimator. That procedure is now adapted to the case in which the DDD estimator is used to identify the effect of a non-targeted treatment. Assumption 3.4 is necessary for the evaluation problem

[15] See Meyer (1995) for an overview of the DDD estimator in the evaluation of targeted treatments.

to be well defined. 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.

ASSUMPTION 3.4: $P(D=1)>0$ *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 unemployed 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*

$$\rho = (\Upsilon_1 - \Upsilon_0) - (\Upsilon_0 - \Upsilon_{-1})$$

A formal proof of Lemma 3.1 can be easily derived from Abadie (2005). The expression for ρ is obtained by replacing conditional expectations at $t=\tau$ by terms like $(D_\tau/P(t=\tau|X))$ in equation (4). Under Assumptions 3.3 and 3.4 the ATET is identified by:

$$\begin{aligned}
E[Y_1(1)-Y_0(1)|t=1] &= \int E[Y_1(1)-Y_0(1)|X,t=1]\, dP(X|t=1) \\
&= \int E[\rho Y|X]\, dP(X|t=1) \\
&= E\left[\rho Y \frac{P(t=1|X)}{P(t=1)}\right] \\
&= E\left[\frac{Y}{P(t=1)} \underbrace{P(t=1|X)\,\rho}_{\omega}\right] \qquad (5)
\end{aligned}$$

where ω can be written after some algebra as:

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

Equation (5) suggests a simple two-step method to estimate the ATET under Assumptions 3.3 and 3.4. First, conditional probabilites are estimated by means of a

multinomial logit model and fitted values of $P(t=1|X)$ and $P(t=k|X)$ are computed for the unemployed at pre-treatment period $t=k$, for $k=\{0,-1,-2\}$.[16] Second, fitted values are plugged into the sample analog of equation (5).[17] Under Assumptions 3.3 and 3.4 a simple weighted average of the outcome variable recovers the ATET. The weighting function ω imposes the distribution of covariates for treated unemployed workers at any pre-treatment period.[18]

The expressions for ρ and ω in a general T-period estimator are the following:

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

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

The expressions for the DDD, DD and BA estimators are those for $T=4$, $T=3$ and $T=2$, respectively. The asymptotic properties of the general T-period estimator are presented in the Appendix.

Finally, the discrete nature of the dependent variable may imply that the assumptions underlying the estimators 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.

4 The Data

The data is drawn from the rotating panel version of the Spanish Labour Force Survey (LFS). 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 avaliable sample period ranges from the second quarter of 1987 to the fourth quarter of year 2000. That is, it starts soon after the liberalization of

[16] The conditional probability of receiving treatment given individual characteristics ($P(t=1/X)$) is known as the propensity score.

[17] Following Abadie (2005), I assume that $\rho=-1$ if $P(t=1|X)=0$. That choice is inconsequential since the objects of interest will be integrals over the distribution of the X conditional on $t=1$.

[18] See Abadie (2005) for a detailed description of the weighting scheme.

temporary contracts and it covers the 1994 and 1997 counter-reforms and the 1999 NEP.

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 last 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 LFS 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 2A and 2B describe the data at hand for temporary and unemployed workers, respectively. Transition probabilities are computed as the proportion of temporary and unemployed workers at a given quarter that hold a permanent contract in the following quarter. Transition into permanent employment probabilities are much lower after the 1994 reform than before. Regarding the 1997 reform, contract conversions decrease but transitions into permanent employment for younger and middle-aged unemployed workers increase once the 1997 reform is enacted.

A more detailed description of the data is provided in Figures 1 to 4. Figures 1 and 2 (Figures 3 and 4) plot transition into permanent employment probabilities for male and female temporary (unemployed) workers, respectively. The information in these figures can be summarized in four points. First, transition probabilities follow a loosely monotonically decreasing time trend over the sample period. Second, transition probabilities become stable by approximately 1994, that is, coinciding with the introduction of the 1994 reform and the recovery of the Spanish economy. Third,

no significant change in transition probabilities is observed once the 1997 reform is enacted. Finally, permanent hires and contract conversions increase in the first half of 1999, that is, before the reduction in fiscal incentives for permanent contracts announced in the 1999 NEP.

Obviously, no causal conclusion regarding the efficacy of these policies can be reached at this point. Differences in the distribution of covariates among employees at different periods and/or changes in business cycle conditions might totally or partially explain the observed pattern of transitions into permanent employment. In the next section I implement the identification strategy outlined in the preceding section to properly isolate the effects of interest.

5 Empirical results

In the empirical analysis period $t = 1$ collects flows into permanent employment between quarters τ and $\tau + 1$, for $\tau = \{1997{:}3,\ 1997{:}4,\ 1998{:}1\}$ and, thus, it captures the effect, if any, of the 1997 reform. Equivalently, period $t = 2$ is a three-quarter period ranging from 1998:3 to 1999:1 that includes the period of higher fiscal incentives for permanent contracts defined in the 1999 NEP. Remaining periods are defined to include the same distribution of quarters than these two periods. I control for seasonal effects by including the quarter at which the worker is observed in X. Estimation results are separately discussed for temporary and unemployed workers.

5.1 Temporary workers

Table 3 presents BA estimates for younger, middle-aged and older male and female temporary workers. Under Assumption 3.1, the estimates obtained when comparing periods t and $t + 1$, for $t = -3$, $t = 0$ and $t = 1$, identify the effect on temporary to permanent transition probabilities of the 1994 and 1997 reforms and the 1999 NEP, respectively.

Estimates indicate that Assumption 3.1 holds for older female temporary workers and that none of the policies under evaluation succeed in increasing their probability of working under a permanent contract. The same holds for older men regarding the 1997 reform and the 1999 NEP, since Assumption 3.1 is not rejected from 1993

onwards and, thus, BA estimates provide information on the effect of these two policies. This is an interesting result since, as shown in Table 1, fiscal incentives for the conversion of temporary contracts into permanent ones were highest in the 1994 and 1997 reforms for workers aged over 45 years. The significant estimates obtained in the pre-reform period indicate that the effect of the 1994 reform for older men cannot be analyzed using the BA estimator.

Regarding middle-aged workers, their probability of obtaining a permanent position decreases until 1994, particularly so for men. Then, coinciding with the recovery of the Spanish economy and the introduction of the 1994 reform there starts a period of stability that is replaced in 1996 by a new decreasing trend that continues once the 1997 reform is enacted. Finally, estimates indicate that transitions from temporary to permanent employment were significantly higher than expected in period $t = 2$.

The picture for younger workers is the same as for the middle-aged but in the period following the introduction of the 1997 reform. In period $t = 1$ contract conversions decrease for the middle-aged while they remain unchanged for younger workers. For women, the differential in the stability of temporary to permanent transition probabilities between younger and middle-aged workers is observed from 1994.

These estimates are not informative about the effect of the 1994 and 1997 reforms for younger and middle-aged workers. First, the stability of contract conversions from 1994 onwards could totally or partially be due to the recovery of the Spanish economy after a short but severe recession in which more than 800,000 employees were dismissed between 1991 and 1993.[19] Second, Assumption 3.1 is rejected for younger men and middle-aged workers in some estimates between the 1994 and 1997 reforms and, thus, the BA estimator does not approximate the outcome that would have been observed for these workers in period $t = 1$ in the absence of the 1997 reform. This assumption only holds from 1994 onwards for younger women and estimates show that their probability of working under a permanent contract did not increase in response to the 1997 reform.

[19] Amuedo-Dorantes and Malo (2005) find, using data on Spanish establishments, that net employment growth expectations for the short-run are met with increases in net permanent job creation and hiring rates, where net employment growth expectations are proxies of expected booms and crises.

Conversely, estimates attest that employers reacted to the reduction in fiscal incentives for permanent contracts announced in the 1999 NEP by increasing transitions from temporary to permanent employment for younger and middle-aged workers in the first half of 1999. The implementation of the BA estimator over the sample period allows me to examine if movements of a given magnitude are more or less common than standard errors suggest (Meyer, 1995).[20] The only positive and significant estimate in Table 3 is that for period $t = 2$. Furthermore, estimates of a similar magnitude (in absolute value) to that for period $t = 2$ are only found in the late 1980s and early 1990s, that is, five or six years before the 1999 NEP.

This result confirms that middle-aged temporary workers were not excluded by 1997 reformers and, thus, it rejects the natural experiment research design in preceding papers evaluating the 1997 reform. The estimated effect for younger women amounts to an improvement of 32.7 percent in their probability of working under a permanent contract in period $t = 1$. The magnitude of the effect for the other groups of temporary workers is derived from the estimator whose identification assumption holds in the pre-treatment period.

In Table 4 I analyze if the DD estimator identifies the effects of interest for younger and middle-aged workers and older men. Assumption 3.2 is not rejected for middle-aged workers and estimates indicate that the 1994 and 1997 reforms failed at improving their temporary to permanent transition probabilities. Additionally, estimates confirm that their probability of working under a permanent contract was significantly higher than expected in period $t = 2$. Middle-aged males' (females') probability of obtaining a permanent contract improved by 43.2 (71.0) percent following the introduction of the 1999 NEP. The negative and significant estimate obtained in period $t = 3$ amounts to an equivalent reduction in their temporary to permanent transition probabilities and, thus, it confirms the transitory nature of the latter improvement.

Regarding younger men, Assumption 3.2 cannot be rejected from $t = -2$ onwards and, thus, these estimates provide information on the effect of the 1997 reform and the 1999 NEP for these workers. Estimates indicate that the 1997 reform had no

[20] Blundell *et al.* (2001) proceed in a similar way when estimating the impact of a mandatory job search assistance program. They implement the estimator in the pre-treatment period to analyze whether the estimated effect of the programme lies whithin typical values of the historical estimates.

impact on their contract conversion prospects and that their probability of obtaining a permanent contract improved by 37.4 percent in the period including the first half of 1999. Assumption 3.2 also holds in the pre-1994 reform period for older men. Estimates show that this reform had no effect on their "temp-to-perm" transition probabilities. Conversely, this Assumption is rejected for younger women in the pre-1994 reform period.

Next, I examine whether the DDD estimator identifies the effect of the 1994 reform for younger workers. Estimates in Table 5 show that this estimator properly captures the decreasing trend that dominates younger workers' temporary to permanent transition probabilities in the pre-reform period and that the 1994 reform had no impact on that probability.

To resume, estimates indicate that the 1994 and 1997 reforms did not improve contract conversions for any group of temporary workers. This result suggests that temporary hiring was a deep-rooted practice among Spanish employers at the time of the 1994 reform and, thus, the restrictions to the use of one type of temporary contract (the non-causal one) probably led to a greater use of other types but not to encourage the use of permanent contracts. Regarding the 1997 reform, estimates suggest that employers might be unwilling to forgo employment flexibility through contract conversion regardless of the employment cost.

Within-group estimators in the preceding tables indicate that a treatment is effective if the value of the outcome in the post-treatment period exceeds that expected given the time trend it follows in the pre-treatment period. However, it might be the case that an effective treatment provokes no significant difference between the observed and the expected values of the outcome in the post-treatment period. For example, it might be argued that younger and older workers' probability of obtaining a permanent position would have decreased in period $t = 1$ in the absence of the 1997 reform, as it did for middle-aged workers.

To account for this alternative I implement a between-groups analysis where I compare, for each period t, temporary to permanent transition probabilities for younger (older) and middle-aged workers once differences in the distribution of covariates are controlled for. By implementing this estimator in the pre-treatment period I exam-

ine an alternative counterfactual assuming that the average conditional outcome that would have been observed for younger and older workers in the absence of a treatment is that for the middle-aged.

Estimates in Table 6 show that younger males' temporary to permanent transition probability is not significantly different from that for middle-aged men until 1996.[21] Then, middle-aged males' probability of obtaining a permanent contract becomes significantly lower than that for younger men and it remains at lower values once the 1997 reform is enacted. That is, middle-aged men are not a valid control group for analyzing the effect of the 1997 reform for younger men using this estimator. Between-groups DD estimates, not shown to save space, capture the difference in the outcome variable that emerges between these two groups prior to the 1997 reform and show no significant difference following the introduction of the reform.[22]

Additionally, estimates show a significant increase in the transition from temporary to permanent employment for older relative to middle-aged men following the introduction of the 1999 NEP. No significant different is found in the preceding periods.

Regarding women, I find that the probability of obtaining a permanent contract cannot be rejected to be the same for younger and middle-aged workers except for the period following the introduction of the 1997 reform, when that probability was higher for younger women. This result suggests that younger females' probability of working under a permanent contract would have decreased in the absence of the 1997 reform, as it did for middle-aged women. However, between-groups estimates for unemployed workers in the following subsection suggest an alternative interpretation to this result.

Estimates in Table 6 indicate that the 1994 reform and the 1999 NEP had no effect on younger and older workers' temporary to permanent transition probabilities. However, finding that a non-targeted treatment is not effective using a between-groups estimator has two alternative and untestable implications. On the one hand, it might

[21] Güell and Petrongolo (2007) also find, using the Spanish LFS, that the age of a temporary worker has a limited effect on his probability of obtaining a permanent contract. In particular, they find that this probability increases from the category 16 to 24 years old to the category 24-34 years old and stays constant afterwards.

[22] These estimates are available upon request to the author.

be that the treatment has no effect for any of the two groups and, on the other hand, it might be that it has similar effects on both groups. Within-group estimates clarify the interpretation of non-significant estimates in Table 6 by showing that the 1994 reform failed at improving contract conversions and that the reduction in fiscal incentives for permanent contracts announced in the 1999 NEP provoked a transitory and sizeable increase in transitions from temporary to permanent employment in the first half of 1999.

Finally, Kugler *et al.* (2003) find that the 1997 reform increased transitions from temporary to permanent employment for younger relative to middle-aged workers during the reform years. An equivalent result emerges from estimates in Table 6 if the post-reform period includes periods $t = \{1, 2, 3\}$, as in Kugler *et al.* (2003). Furthermore, they find a negative effect for older relative to middle-aged men, as I do in period $t = 2$. That is, Kugler *et al.* (2003) only identify the effect of differences in payroll tax reduction schemes across groups of workers.

5.2 Unemployed workers

As shown in Table 7, permanent hires decrease from the beginning of the sample period until mid 1990s and then become stable. That is, Assumption 3.1 cannot be rejected from 1994 onwards and, thus, BA estimates provide information on the effect of the 1997 reform and the 1999 NEP. According to these estimates, younger workers' unemployment to permanent employment transition probability improved as a result of these two policies, with the effect of the 1999 NEP being substantially larger than that of the 1997 reform. In particular, younger males' (females') probability of working under a permanent contract increased by 22.9 (26.2) and 42.5 (62.0) percent following the introduction of the 1997 reform and the 1999 NEP, respectively. Equivalently, the estimated effect of the 1997 reform for older men amounts to an improvement of 59.3 percent in their probability of moving from unemployment to a permanent contract.

Interestingly, I find that middle-aged unemployed workers also benefited from these two policies. This result further rejects the identification strategy in preceding papers analyzing the effects of the 1997 reform. Middle-aged females' probability of

obtaining a permanent contract increased by 61.1 and 35.7 percent following the introduction of the 1997 reform and the 1999 NEP, respectively. Conversely, middle-aged men only benefited from the 1999 NEP. Their probability of obtaining a permanent contract increased by 35.7 percent in period $t = 2$.

Moving to the analysis of the 1994 reform, Assumption 3.1 is not rejected for older women and for middle-aged unemployed workers in the pre-reform period. Estimates for these groups show no significant effect coming from the 1994 reform. Estimates in Table 8 confirm that Assumption 3.2 holds in the pre-reform period for the remaining groups of unemployed workers and they allow me to conclude that the 1994 reform failed at promoting permanent hires for any group of unemployed workers.

As for temporary workers, I explore alternative counterfactuals with between-groups estimates in Table 9. I find that unemployment to permanent employment transition probabilities cannot be rejected to be the same for younger and middle-aged women but in the period following the introduction of the 1997 reform, when that probability was higher for middle-aged women.

Between-group estimates for temporary workers suggested that younger women benefited from the 1997 reform since their probability of obtaining a permanent contract did not decrease once the reform was enacted, as it did for middle-aged women. An alternative interpretation to this result emerges if between-group estimates for temporary and unemployed workers are jointly considered. This interpretation states that the 1997 reform had no impact on contract conversions but improved permanent hires for some groups of unemployed workers, including middle-aged women. To be elegible for the new permanent contract middle-aged women were hired under a temporary contract. These temporary jobs were rapidly turned into permanent ones, explaining the negative (positive) between-group estimate obtained in the period following the introduction of the 1997 reform for middle-aged female temporary (unemployed) workers.

Evidence in Amuedo-Dorantes (2001) supports this interpretation. She examines the determinants of Spanish employers' reliance on temporary workers and finds that wage and dismissal cost reductions for permanent contracts promote 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.

Finally, if the post-1997 reform period includes periods $t = \{1, 2, 3\}$, estimates in Table 9 suggest that the 1997 reform increased unemployment to permanent employment transition probabilities for younger relative to middle-aged men, as in Kugler *et al.* (2003). This result only captures the differential effect across groups of workers of the reduction in fiscal incentives for permanent contracts announced in the 1999 NEP. That is, estimates in Kugler *et al.* (2003) provide no information on the effect of dismissal cost reductions on transitions into permanent employment.

6 Conclusions

This paper presents the first joint evaluation of the two major labour market reforms implemented in Spain to promote permanent employment in 1994 and 1997. The 1994 reform restored the principle of causality in the application of temporary contracts and offered fiscal incentives for their conversion into permanent ones for certain groups of workers. The 1997 reform further lowered the cost gap between temporary and permanent hiring by introducing a new permanent contract with lower payroll taxes and dismissal costs than the ordinary one, whose regulation remained unchanged. Any worker except for the unemployed aged 30 to 45 years old could be hired under the new permanent contract.

This is not the first paper that evaluates the 1997 reform. Its effect on the proportion of temporary workers, net flows into permanent employment, earnings and workers' perception of job insecurity have been analyzed in Dolado *et al.* (2002), Kugler *et al.* (2003), Plá and Ramos (2007) and Trevisan (2007), respectively. I argue that estimation results in these papers are misleading since their identification strategy is not correct. They implement a natural experiment research design by assuming that middle-aged workers were not elegible for the new permament contract. However, middle-aged unemployed workers could easily recover eligibility by simple being hired under a temporary contract, since no age eligibility criteria was stated for temporary workers. That is, the 1997 reform is a non-targeted treatment and, thus, preceding papers only identify the effect of differences in payroll tax reductions

schemes across groups of workers.

Furthermore, the post-reform period in those papers confounds the effect, if any, of the 1997 reform with that of the 1999 National Employment Plan (NEP). The 1999 NEP, passed on 30th December 1998, announced that fiscal incentives would last for one additional year for permanent contracts signed until May 1999 but would be significantly lower in magnitude and length after that date.

The identification strategy in this paper allows me to identify the effect of non-targeted treatments like the 1994 and 1997 reforms and the 1999 NEP. I present a family of semiparametric estimators that predict the outcome that would have been observed in the absence of a non-targeted treatment by exploiting the time series variation of the outcome in the pre-treatment period. Alternative counterfactuals are also explored by means of between-groups estimators.

Estimates using the Spanish Labour Force Survey (LFS) do not support the hypothesis that wage and, in particular, dismissal costs are at the center of Spanish employers' reliance on temporary employment. Employers did not change their contract conversion practices in response to either the 1994 or the 1997 reform. The restrictions on the use of one type of temporary contract (the non-causal one) probably led to a greater use of other types rather than encouraging the use of permanent contracts. Additionally, wage and dismissal cost reductions had no effect on contract conversions, which primarily respond to employers' flexibility needs and union pressures for increased employment stability (Amuedo-Dorantes, 2001). Employers might be unwilling to forgo employment flexibility through contract conversion regardless of the employment cost.

Regarding unemployed workers, while the 1994 reform had no impact on permanent hires, the 1997 reform succeeded in increasing permanent hires for most groups of unemployed workers, including the middle-aged. This empirical evidence rejects the natural experiment research design in existing papers evaluating the 1997 reform.

Finally, estimates attest that employers reacted to the announced reduction in fiscal incentives for permanent contracts by increasing permanent hires and contract conversions in the first half of 1999. This transitory effect is found for most groups of workers, including the middle-aged, and is, for unemployed workers, substantially

larger than that coming from the 1997 reform.

The 1997 reform failed at reducing the proportion of temporary workers because it had no effect on contract conversions, which account for 85 percent of permanent contracts signed in Spain. Cebrian *et al.* (2005) indicate another reason why this proportion has resisted to decrease since the introduction of the 1997 reform. They analyze administrative records on permanent contracts in Spain and find that permanent hires under the new contract increase the hazard rate of ending the contract by 15-30 percent relative to that of ordinary permanent contracts. They conclude that Spanish employers took advantadge of wage and dismissal cost reductions to substitute permanent contracts for otherwise temporary ones.

7 Appendix. Estimation and asymptotic variance

The two-step method developed in Abadie (2005) to estimate the the effect of a targeted treatment on the treated using the DD estimator is now adapted to the evaluation of non-targeted treatments using the general T-period estimator. Consider the following estimator of the ATET:

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

where

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

and $\pi_k\left(X\right) = P\left(t = k/X\right)$, for $k = \{1, 0, -1, -2, ...\}$. Let n be the total number of observations involved in the estimation of β_0 and n_k the number of observations at period $t = k$. Let me consider the following estimator of β_0:

$$\overset{\wedge}{\beta} = \left(\frac{1}{n} \sum_{i=1}^{n} \overset{\wedge}{\pi}_1 \left(X_i\right) \right)^{-1} \frac{1}{n} \sum_{i=1}^{n} \overset{\wedge}{\pi}_1 \left(X_i\right) \overset{\wedge}{\rho}_i Y_i,$$

where

$$\overset{\wedge}{\rho}_i = \sum_{j=0}^{T-1} \left[\frac{D_{(j-(T-2))i} \left(-1\right)^{j+T+1} \binom{T-1}{j}}{\overset{\wedge}{\pi}_{j-(T-2)}\left(X_i\right)} \right],$$

and $\overset{\wedge}{\pi}_k \left(X_i\right)$ is an estimator of $\pi_k \left(X_i\right)$, for $k = \{1, 0, -1, -2, ...\}$. Under the conditions stated in the following theorem $\overset{\wedge}{\beta}$ is well-defined with probability approaching one.

ASSUMPTION A1: (i) γ_0 is an interior point of a compact set $\Gamma \subset \mathbb{R}^k$, where $k = r\left(T-1\right)$ and r is the dimension of X; (ii) the support of X is a subset of a compact set S, $E\left[XX'\right]$ is nonsingular; (iii) for $k = \{1, 0, -1, -2, ...\}$ there is a (known) function $\pi_k : \mathbb{R} \to [0, 1]$ such that $\pi_k\left(X\right) = \pi_k\left(X'\gamma_0\right)$; (iv) let $\upsilon = \{x'\gamma : x \in S, \gamma \in \Gamma\}$; for $v \in \upsilon$ and for $k = \{1, 0, -1, -2, ...\}$, $\pi_k\left(v\right)$ is bounded away from zero and one,

strictly increasing and continuously differentiable with derivative bounded away from zero and one; (v) β_0 is an interior point of a compact set $\Theta \subset \mathbb{R}$; (vi) $EY^2 < \infty$.

Under Assumption A1, γ_0 can be estimated by Maximum Likelihood:

$$\overset{\wedge}{\gamma} = \arg\max_{\gamma \in \Gamma} \frac{1}{n} \sum_{i=1}^{n} \sum_{j=0}^{T-1} D_{(j-(T-2))i} \log \pi_{j-(T-2)} \left(X_i' \gamma \right).$$

Then $\overset{\wedge}{\pi}_k \left(X_i \right) = \pi_k \left(X_i' \overset{\wedge}{\gamma} \right)$, for $k = \{1, 0, -1, -2, ...\}$. Assumption A1 allows us to estimate previous discrete choice model by multinomial logit or probit models. Let $\dot{\pi}_k = \partial \pi_k \left(v \right) / \partial v$ and $\dot{\pi}_{k0} = \dot{\pi}_k \left(X' \gamma_0 \right)$. Under standard regularity conditions (e.g., Assumption A1$(i) - (iv)$), $\overset{\wedge}{\gamma}$ is asymptotically linear:

$$n^{1/2} \left(\overset{\wedge}{\gamma} - \gamma_0 \right) = n^{-1/2} \sum_{i=1}^{n} \psi_{\gamma_0} \left(Z_i \right) + O_p \left(1 \right),$$

where

$$\psi_{\gamma_0} \left(Z \right) = E \left[i_{\gamma_0} i_{\gamma_0}' \right]^{-1} i_{\gamma_0},$$

and

$$i_{\gamma_0} = X \left[\sum_{j=0}^{T-1} D_{j-(T-2)} \pi_{j-(T-2)}^{-1} \dot{\pi}_{(j-(T-2))0} \right].$$

Let

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

with

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

THEOREM A1: If $n_k \to \infty$ for each k, and assumptions 3.4 and A1 hold, then $\sqrt{n} \left(\overset{\wedge}{\beta} - \beta_0 \right) \xrightarrow{d} N \left(0, V \right)$, where $V = Q^{-1} \Sigma Q^{-1}$, $Q = E \left[D_1 \right]$, $\Sigma = E \left[\psi \psi' \right]$, $\psi = m \left(Z, \beta_0, \gamma_0 \right) + M_{\gamma_0} \psi_{\gamma_0}$, and $m \left(Z, \beta_0, \gamma_0 \right) = \pi_1 \left(\rho Y - \beta_0 \right)$.

Let $\hat{V} = \hat{Q}^{-1} \overset{\wedge}{\Sigma} \hat{Q}^{-1}$, where

$$\hat{Q} = \frac{1}{n}\sum_{i=1}^{n} D_{1i}, \qquad \overset{\wedge}{\Sigma} = \frac{1}{n}\sum_{i=1}^{n} \overset{\wedge}{\psi}_i \overset{\wedge}{\psi}_i{}',$$

$$\hat{M}_{\overset{\wedge}{\gamma}} = \frac{1}{n}\sum_{i=1}^{n} \left(\hat{W}_{\overset{\wedge}{\gamma} i} - \dot{\pi}_1 \left(X_i' \overset{\wedge}{\gamma} \right) \overset{\wedge}{\beta} \right) X_i',$$

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

$$\overset{\wedge}{\psi}_{\overset{\wedge}{\gamma}} (Z_i) = E \left[i_{\overset{\wedge}{\gamma}} i_{\overset{\wedge}{\gamma}}' \right]^{-1} i_{\overset{\wedge}{\gamma}}$$

with

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

and $\overset{\wedge}{\psi}_i = \overset{\wedge}{\pi}_1 (X_i) \left(\overset{\wedge}{\rho}_i Y_i - \overset{\wedge}{\beta} \right) + \hat{M}_{\overset{\wedge}{\gamma}} \overset{\wedge}{\psi}_{\overset{\wedge}{\gamma}} (Z_i)$. A formal proof of Theorem A1 can be easily derived from the proof of Theorem 4.3 in Abadie (2005) by properly replacing ρ and the first step likelihood function by its particular expressions for the T-period estimator. Similarly, it can be easily shown that under the assumptions of Theorem A1 and assuming that $\pi_k (v)$ is twice differentiable with bounded second derivative in v, $\hat{V} \xrightarrow{p} V$ (see Theorem4.4 in Abadie (2005)).

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

Age group	May 1994	May 1997	January 1999	May 1999
		Unemployed workers		
16-29	-	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
		Temporary workers		
16-45	2.400 euros*	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

Note: * For workers aged less than 25 years old.

Table 2A. Descriptive statistics by sex and age group before and after the 1994 and 1997 reforms. Temporary workers

	Pre-1994 reform			Between-reforms			Post-1997 reform		
	< 30	30 to 45	> 45	< 30	30 to 45	> 45	< 30	30 to 45	> 45
					MEN				
Age	20.9	33.9	49.8	21.5	33.9	49.5	21.4	34.0	49.5
	(3.3)	(4.0)	(4.7)	(3.2)	(4.0)	(4.7)	(3.2)	(4.0)	(4.6)
Tenure (in months)	29.1	34.6	33.7	17.1	22.7	23.2	26.9	39.4	38.1
	(36.5)	(44.4)	(44.4)	(26.8)	(37.5)	(35.9)	(38.5)	(47.4)	(53.2)
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	75207	32787	14565	37356	21974	9210	49695	29304	12351
					WOMEN				
Age	20.6	34.0	49.3	21.4	34.0	48.6	21.6	34.0	48.7
	(3.3)	(4.0)	(4.8)	(3.2)	(4.0)	(4.5)	(3.1)	(4.0)	(4.4)
Tenure (in months)	30.5	38.1	50.4	17.9	28.4	31.7	25.1	42.9	48.6
	(37.9)	(51.5)	(66.1)	(31.5)	(51.2)	(57.9)	(38.0)	(68.0)	(74.4)
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	50103	17305	6561	25188	13425	4420	35258	19826	5806

Note: The table reports means and percentages for continuous and discrete variables, respectively. Standard deviations are in parenthesis for continuous variables.

Table 2B. Descriptive statistics by sex and age group before and after the 1994 and 1997 reforms. Unemployed workers

	Pre-1994 reform			Between-reforms			Post-1997 reform		
	< 30	30 to 45	> 45	< 30	30 to 45	> 45	< 30	30 to 45	> 45
					MEN				
Age	20.5	33.9	51.0	21.0	33.9	50.3	21.0	34.0	50.5
	(3.3)	(4.0)	(5.0)	(3.3)	(4.0)	(4.9)	(3.4)	(4.0)	(4.8)
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	56850	19170	11560	31819	15555	8785	26551	13393	7639
					WOMEN				
Age	20.5	33.6	48.9	21.1	33.7	48.8	21.3	33.8	48.8
	(3.3)	(4.0)	(4.5)	(3.3)	(3.9)	(4.4)	(3.2)	(3.9)	(4.5)
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	68950	18727	5131	38057	18160	5396	37619	20967	6555

Note: The table reports means and percentages for continuous and discrete variables, respectively. Standard deviations are in parenthesis for continuous variables.

Table 3. Before-After estimates. Temporary workers

		Men			Women		
Period	Quarters	16 to 29	30 to 45	46 to 64	16 to 29	30 to 45	46 to 64
$t=-9$	1987:3-1988:1	-0.0317***	-0.0078	0.0015	-0.0529*	-0.0510*	0.0088
$t=-8$	1988:3-1989:1	[-3.13]	[-1.59]	[0.73]	[-2.87]	[-3.05]	[0.87]
$t=-8$	1988:3-1989:1	-0.0205***	-0.0325***	-0.0159	-0.0015	0.0007	-0.0218
$t=-7$	1989:3-1990:1	[-2.78]	[-2.64]	[-1.42]	[-0.29]	[0.47]	[-1.17]
$t=-7$	1989:3-1990:1	-0.0223***	-0.0176**	-0.0225*	-0.0167***	-0.0235***	-0.0104
$t=-6$	1990:3-1991:1	[-2.71]	[-2.39]	[-1.68]	[-2.93]	[-2.89]	[-1.38]
$t=-6$	1990:3-1991:1	-0.0044	-0.0127*	-0.0111	-0.0005	0.0018	-0.0026
$t=-5$	1991:3-1992:1	[-1.39]	[-1.88]	[-1.43]	[-0.11]	[0.78]	[-0.54]
$t=-5$	1991:3-1992:1	-0.0170***	-0.0218***	-0.0374***	-0.0220***	-0.0196***	-0.0033
$t=-4$	1992:3-1993:1	[-2.65]	[-2.79]	[-3.05]	[-3.04]	[-2.78]	[-0.48]
$t=-4$	1992:3-1993:1	-0.0066*	-0.0142**	-0.0080	-0.0044	-0.0091	-0.0046
$t=-3$	1993:3-1994:1	[-1.78]	[-2.08]	[-1.32]	[-1.06]	[-1.47]	[-1.15]
$t=-3$	1993:3-1994:1	0.0011	0.0006	0.0029	0.0074**	-0.0011	0.0049
$t=-2$	1994:3-1995:1	[0.52]	[0.28]	[0.33]	[1.99]	[-0.63]	[0.74]
$t=-2$	1994:3-1995:1	0.0041	-0.0017	0.0019	-0.0058	0.0048	-0.0023
$t=-1$	1995:3-1996:1	[0.81]	[-0.36]	[0.84]	[-0.93]	[0.47]	[-0.19]
$t=-1$	1995:3-1996:1	-0.0070**	-0.0079**	-0.0061	-0.0032	-0.0107**	-0.0034
$t=0$	1996:3-1997:1	[-2.13]	[-2.09]	[-0.73]	[-0.31]	[-2.07]	[-0.24]
$t=0$	1996:3-1997:1	-0.0010	-0.0114***	-0.0074	-0.0027	-0.0173***	-0.0059
$t=1$	1997:3-1998:1	[-0.30]	[-2.74]	[-0.51]	[-0.63]	[-2.84]	[-0.61]
$t=1$	1997:3-1998:1	0.0129***	0.0167***	0.0015	0.0117***	0.0179***	0.0100
$t=2$	1998:3-1999:1	[3.47]	[3.19]	[0.90]	[2.96]	[3.41]	[0.94]
$t=2$	1998:3-1999:1	-0.0090***	-0.0157***	-0.0122*	-0.0120***	-0.0145***	-0.0139*
$t=3$	1999:3-2000:1	[-2.78]	[-3.08]	[-1.72]	[-3.12]	[-2.96]	[-1.78]

Notes: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The table reports t-statistics in brackets. Standard errors are calculated using the delta method.

Table 4. Difference-in-differences estimates. Temporary workers

		Men			Women	
Period	Quarters	16 to 29	30 to 45	46 to 64	16 to 29	30 to 45
$t=-9$	1987:3-1988:1	0.0114	-0.0233	-0.0163	0.0506***	0.0415
$t=-8$	1988:3-1989:1	[1.47]	[-1.39]	[-1.10]	[2.86]	[1.50]
$t=-7$	1989:3-1990:1					
$t=-8$	1988:3-1989:1	-0.0016	0.0142	-0.0071	-0.0150	-0.0270
$t=-7$	1989:3-1990:1	[-0.41]	[1.28]	[-0.78]	[-1.31]	[-1.43]
$t=-6$	1990:3-1991:1					
$t=-7$	1989:3-1990:1	0.0166**	0.0030	0.0055	0.0174**	0.0258
$t=-6$	1990:3-1991:1	[1.98]	[0.37]	[0.82]	[2.43]	[1.18]
$t=-5$	1991:3-1992:1					
$t=-6$	1990:3-1991:1	-0.0105*	-0.0115	-0.0226	-0.0121	-0.0169
$t=-5$	1991:3-1992:1	[-1.90]	[-1.27]	[-1.07]	[-1.32]	[-1.07]
$t=-4$	1992:3-1993:1					
$t=-5$	1991:3-1992:1	0.0124**	0.0132	0.0032	0.0308*	0.0161
$t=-4$	1992:3-1993:1	[2.07]	[0.84]	[1.34]	[2.77]	[1.02]
$t=-3$	1993:3-1994:1					
$t=-4$	1992:3-1993:1	0.0073	0.0113	0.0118	0.0157*	0.0092
$t=-3$	1993:3-1994:1	[0.51]	[0.67]	[0.76]	[1.79]	[0.93]
$t=-2$	1994:3-1995:1					
$t=-3$	1993:3-1994:1	0.0028	-0.0020	0.0018	-0.0131*	0.0075
$t=-2$	1994:3-1995:1	[0.79]	[-0.59]	[0.58]	[-1.82]	[0.60]
$t=-1$	1995:3-1996:1					
$t=-2$	1994:3-1995:1	-0.0089	-0.0059	-0.0072	0.0023	-0.0165
$t=-1$	1995:3-1996:1	[-1.09]	[-0.51]	[-1.14]	[0.45]	[-0.76]
$t=0$	1996:3-1997:1					
$t=-1$	1995:3-1996:1	0.0061	-0.0025	-0.0002	0.0002	-0.0056
$t=0$	1996:3-1997:1	[0.33]	[-0.73]	[-0.87]	[0.52]	[-0.36]
$t=1$	1997:3-1998:1					
$t=0$	1996:3-1997:1	0.0141***	0.0299***	0.0136	0.0134**	0.0397***
$t=1$	1997:3-1998:1	[2.85]	[2.74]	[1.51]	[2.26]	[3.28]
$t=2$	1998:3-1999:1					
$t=1$	1997:3-1998:1	-0.0166**	-0.0225**	0.0001	-0.0233***	-0.0347***
$t=2$	1998:3-1999:1	[-2.31]	[-2.18]	[0.94]	[-2.63]	[-3.13]
$t=3$	1999:3-2000:1					

Notes: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The table reports t-statistics in brackets. Standard errors are calculated using the delta method.

Table 5. DDD estimates. Young temporary workers

Period	Quarters	Men	Women
$t=-9$	1987:3-1988:1	-0.0102	-0.0662**
$t=-8$	1988:3-1989:1	[-1.48]	[-1.98]
$t=-7$	1989:3-1990:1		
$t=-6$	1990:3-1991:1		
$t=-8$	1988:3-1989:1	0.0172	0.0344**
$t=-7$	1989:3-1990:1	[0.83]	[2.11]
$t=-6$	1990:3-1991:1		
$t=-5$	1991:3-1992:1		
$t=-7$	1989:3-1990:1	-0.0213	-0.0191
$t=-6$	1990:3-1991:1	[-1.32]	[-1.17]
$t=-5$	1991:3-1992:1		
$t=-4$	1992:3-1993:1		
$t=-6$	1990:3-1991:1	0.0194	-0.0220
$t=-5$	1991:3-1992:1	[1.26]	[-0.74]
$t=-4$	1992:3-1993:1		
$t=-3$	1993:3-1994:1		
$t=-5$	1991:3-1992:1	-0.0037	-0.0915
$t=-4$	1992:3-1993:1	[-0.55]	[-0.63]
$t=-3$	1993:3-1994:1		
$t=-2$	1994:3-1995:1		
$t=-4$	1992:3-1993:1	-0.0047	-0.0352**
$t=-3$	1993:3-1994:1	[-0.53]	[-2.38]
$t=-2$	1994:3-1995:1		
$t=-1$	1995:3-1996:1		
$t=-3$	1993:3-1994:1	-0.0139	0.0147
$t=-2$	1994:3-1995:1	[-1.22]	[1.36]
$t=-1$	1995:3-1996:1		
$t=0$	1996:3-1997:1		
$t=-2$	1994:3-1995:1	0.0171**	-0.0021
$t=-1$	1995:3-1996:1	[2.07]	[-1.18]
$t=0$	1996:3-1997:1		
$t=1$	1997:3-1998:1		
$t=-1$	1995:3-1996:1	0.0064	0.0117
$t=0$	1996:3-1997:1	[1.53]	[1.61]
$t=1$	1997:3-1998:1		
$t=2$	1998:3-1999:1		
$t=0$	1996:3-1997:1	-0.0274	-0.0430**
$t=1$	1997:3-1998:1	[1.47]	[2.30]
$t=2$	1998:3-1999:1		
$t=3$	1999:3-2000:1		

Notes: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The table reports t-statistics in brackets. Standard errors are calculated using the delta method.

Table 6. Between-groups estimates. Temporary workers

		Men		Women	
Period	Quarters	16 to 29	46 to 64	16 to 29	46 to 64
$t=-9$	1987:3-1988:1	-0.0128	-0.0183	0.0236	0.0058
		[-0.50]	[-1.13]	[0.64]	[1.16]
$t=-8$	1988:3-1989:1	0.0099	-0.0034	-0.0110	-0.0475**
		[0.87]	[-0.52]	[-0.76]	[-1.98]
$t=-7$	1989:3-1990:1	-0.0032	-0.0056	-0.0106	-0.0090
		[-0.53]	[-0.87]	[-0.90]	[-0.57]
$t=-6$	1990:3-1991:1	0.0029	-0.0409*	-0.0615**	-0.0031
		[0.38]	[-1.67]	[-2.07]	[-0.34]
$t=-5$	1991:3-1992:1	0.0033	-0.0646*	-0.0067	-0.0148
		[0.37]	[-1.85]	[-0.54]	[-0.51]
$t=-4$	1992:3-1993:1	0.0046	-0.0290	0.0005	-0.0052
		[0.46]	[-0.54]	[0.39]	[-0.50]
$t=-3$	1993:3-1994:1	-0.0131	0.0016	0.0043	-0.0062
		[-0.93]	[0.32]	[0.21]	[-0.38]
$t=-2$	1994:3-1995:1	0.0037	-0.0111	-0.0121	-0.0175
		[0.76]	[-0.31]	[-0.94]	[-1.04]
$t=-1$	1995:3-1996:1	-0.0028	-0.0021	-0.0034	-0.0052
		[-0.44]	[-0.46]	[-0.42]	[-0.30]
$t=0$	1996:3-1997:1	-0.0136**	-0.0074	-0.0045	-0.0209*
		[-2.17]	[-0.72]	[-0.22]	[-1.68]
$t=1$	1997:3-1998:1	-0.0128**	-0.0069	-0.0130**	0.0065
		[-1.83]	[-0.88]	[-2.29]	[0.18]
$t=2$	1998:3-1999:1	-0.0137*	0.0129*	-0.0017	-0.0060
		[-1.85]	[1.95]	[-0.30]	[-0.71]
$t=3$	1999:3-2000:1	-0.0139*	0.0015	-0.0066	0.0071
		[-1.91]	[0.71]	[-0.41]	[0.39]

Notes: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The reference group are middle-aged workers. The table reports t-statistics in brackets. Standard errors are calculated using the delta method.

Table 7. Before-After estimates. Unemployed workers

		Men			Women		
Period	Quarters	16 to 29	30 to 45	46 to 64	16 to 29	30 to 45	46 to 64
$t=-9$	1987:3-1988:1	-0.0024	-0.0090	-0.0191	-0.0009	0.0026	-0.0177
$t=-8$	1988:3-1989:1	[-0.75]	[-0.99]	[-1.11]	[-0.54]	[0.19]	[-0.28]
$t=-8$	1988:3-1989:1	-0.0100***	-0.0022	-0.0078	-0.0044*	0.0006	-0.0189
$t=-7$	1989:3-1990:1	[-2.79]	[-0.32]	[-1.41]	[-1.88]	[0.43]	[-0.71]
$t=-7$	1989:3-1990:1	-0.0004	-0.0108*	0.0075	-0.0033*	-0.0024	-0.0064
$t=-6$	1990:3-1991:1	[-1.02]	[-1.78]	[0.53]	[-1.75]	[-0.43]	[-0.57]
$t=-6$	1990:3-1991:1	-0.0066***	-0.0027	-0.0129	-0.0008	-0.0027	-0.0007
$t=-5$	1991:3-1992:1	[-2.81]	[-0.76]	[-1.39]	[-0.17]	[-0.85]	[-0.49]
$t=-5$	1991:3-1992:1	-0.0082***	-0.0061*	-0.0039	-0.0041***	-0.0051*	-0.0011
$t=-4$	1992:3-1993:1	[-2.57]	[-1.91]	[-1.17]	[-2.97]	[-1.78]	[-0.41]
$t=-4$	1992:3-1993:1	-0.0004	-0.0022	-0.0007	0.0008	0.0006	0.0049
$t=-3$	1993:3-1994:1	[0.87]	[-0.50]	[-0.51]	[0.32]	[0.41]	[0.95]
$t=-3$	1993:3-1994:1	-0.0005	0.0028	0.0061	-0.0027***	-0.0016	-0.0008
$t=-2$	1994:3-1995:1	[-0.74]	[0.27]	[0.89]	[-2.39]	[-0.54]	[-0.61]
$t=-2$	1994:3-1995:1	0.0003	-0.0032	-0.0106	0.0012	0.0022	-0.0016
$t=-1$	1995:3-1996:1	[-0.35]	[-0.57]	[0.28]	[0.43]	[0.65]	[-0.31]
$t=-1$	1995:3-1996:1	0.0013	0.0022	-0.0001	0.0002	-0.0009	0.0036
$t=0$	1996:3-1997:1	[-1.05]	[0.11]	[-0.19]	[0.74]	[-0.84]	[0.51]
$t=0$	1996:3-1997:1	0.0022*	0.0004	0.0094***	0.0017*	0.0033**	0.0005
$t=1$	1997:3-1998:1	[1.68]	[0.15]	[2.71]	[1.71]	[2.28]	[0.49]
$t=1$	1997:3-1998:1	0.0051**	0.0070**	0.0010	0.0049***	0.0030*	-0.0033
$t=2$	1998:3-1999:1	[2.13]	[2.15]	[0.93]	[3.11]	[1.81]	[-0.50]
$t=2$	1998:3-1999:1	-0.0001	-0.0027	0.0023	-0.0015	0.0001	0.0043**
$t=3$	1999:3-2000:1	[-0.22]	[0.51]	[0.27]	[0.42]	[0.19]	[-1.99]

Notes: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The table reports t-statistics in brackets. Standard errors are calculated using the delta method.

Table 8. Difference-in-differences estimates. Unemployed workers

		Men		Women	
Period	Quarters	16 to 29	30 to 45	16 to 29	30 to 45
$t=-9$	1987:3-1988:1	-0.0077	0.0085	-0.0042	-0.0053
$t=-8$	1988:3-1989:1	[-0.89]	[0.91]	[-0.18]	[-0.73]
$t=-7$	1989:3-1990:1				
$t=-8$	1988:3-1989:1	0.0113**	-0.0081	0.0020	-0.0043
$t=-7$	1989:3-1990:1	[2.08]	[-1.37]	[-0.75]	[-1.52]
$t=-6$	1990:3-1991:1				
$t=-7$	1989:3-1990:1	-0.0050	0.0050	0.0022	0.0003
$t=-6$	1990:3-1991:1	[-0.42]	[0.81]	[0.93]	[0.47]
$t=-5$	1991:3-1992:1				
$t=-6$	1990:3-1991:1	-0.0031	-0.0024	-0.0023	-0.0057
$t=-5$	1991:3-1992:1	[-0.31]	[-1.01]	[-0.59]	[-0.53]
$t=-4$	1992:3-1993:1				
$t=-5$	1991:3-1992:1	0.0076*	0.0044	0.0039	0.0079
$t=-4$	1992:3-1993:1	[1.76]	[0.71]	[1.31]	[0.73]
$t=-3$	1993:3-1994:1				
$t=-4$	1992:3-1993:1	0.0005	0.0047	-0.0033	-0.0027
$t=-3$	1993:3-1994:1	[0.17]	[0.59]	[-1.47]	[-0.95]
$t=-2$	1994:3-1995:1				
$t=-3$	1993:3-1994:1	0.0010	-0.0065	0.0038	0.0039
$t=-2$	1994:3-1995:1	[0.47]	[-0.44]	[0.61]	[1.18]
$t=-1$	1995:3-1996:1				
$t=-2$	1994:3-1995:1	0.0012	0.0051	-0.0012	-0.0031
$t=-1$	1995:3-1996:1	[0.52]	[0.83]	[-0.92]	[-0.31]
$t=0$	1996:3-1997:1				
$t=-1$	1995:3-1996:1	0.0010	-0.0012	0.0017	0.0045*
$t=0$	1996:3-1997:1	[0.63]	[-1.08]	[1.48]	[1.87]
$t=1$	1997:3-1998:1				
$t=0$	1996:3-1997:1	0.0033	0.0076***	0.0032	0.0002
$t=1$	1997:3-1998:1	[1.45]	[2.77]	[1.31]	[0.57]
$t=2$	1998:3-1999:1				
$t=1$	1997:3-1998:1	-0.0049**	-0.0095**	-0.0074***	-0.0026
$t=2$	1998:3-1999:1	[-2.28]	[-2.31]	[-2.03]	[-1.47]
$t=3$	1999:3-2000:1				

Notes: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The table reports t-statistics in brackets. Standard errors are calculated using the delta method.

Table 9. Between-groups estimates. Unemployed workers

		Men		Women	
Period	Quarters	16 to 29	46 to 64	16 to 29	46 to 64
$t = -9$	1987:3-1988:1	-0.0114	0.0036	-0.0072	-0.0181
		[-0.82]	[1.47]	[-0.62]	[-0.57]
$t = -8$	1988:3-1989:1	-0.0043	0.0199	-0.0028	-0.0049
		[-0.31]	[1.27]	[-0.31]	[-0.39]
$t = -7$	1989:3-1990:1	0.0096	0.0238	0.0036	-0.0044
		[0.39]	[1.63]	[0.38]	[-0.41]
$t = -6$	1990:3-1991:1	0.0061	0.0107*	0.0078**	0.0099
		[0.85]	[1.95]	[1.98]	[0.64]
$t = -5$	1991:3-1992:1	-0.0121*	0.0093	-0.0022	-0.0058
		[-1.72]	[0.39]	[-0.61]	[0.79]
$t = -4$	1992:3-1993:1	-0.0227*	0.0025	-0.0006	0.0033
		[-1.89]	[0.97]	[-0.63]	[0.31]
$t = -3$	1993:3-1994:1	-0.0043	0.0062	0.0011	-0.0051
		[-0.63]	[0.36]	[0.41]	[-0.38]
$t = -2$	1994:3-1995:1	0.0020	-0.0033	0.0014	-0.0048
		[0.42]	[-0.42]	[0.49]	[-0.53]
$t = -1$	1995:3-1996:1	-0.0009	-0.0023	0.0018	-0.0047
		[-0.48]	[-0.98]	[0.37]	[-0.79]
$t = 0$	1996:3-1997:1	0.0027	0.0089	-0.0031	-0.0007
		[0.94]	[1.53]	[-0.28]	[-0.29]
$t = 1$	1997:3-1998:1	-0.0003	-0.0001	0.0035**	0.0032
		[-0.31]	[-0.72]	[2.18]	[1.25]
$t = 2$	1998:3-1999:1	-0.0125*	0.0090***	-0.0009	0.0021
		[-1.94]	[2.38]	[-0.27]	[0.79]
$t = 3$	1999:3-2000:1	-0.0129*	-0.0090*	-0.0202*	0.0001
		[-1.77]	[-2.25]	[-1.73]	[0.71]

Notes: *, ** and *** denote significance at the 10%, 5% and 1% level, respectively. The reference group are middle-aged workers. The table reports t-statistics in brackets. Standard errors are calculated using the delta method.

Figure 1. Temporary to permanent contract transition probability between quarters t and $t+1$. Men

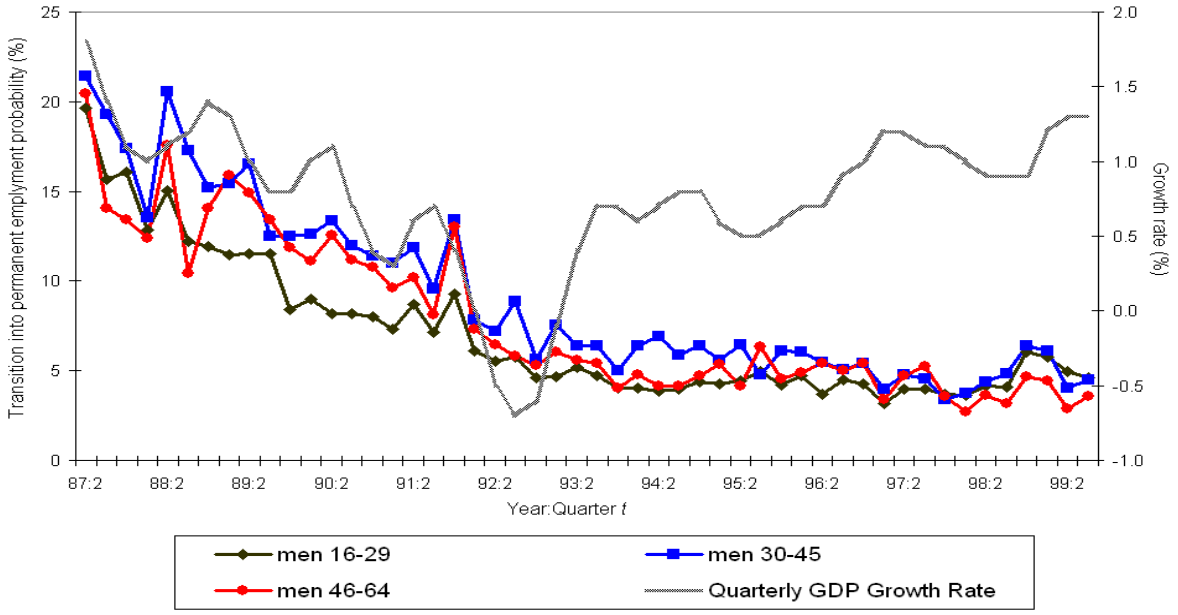

Figure 2. Temporary to permanent contract transition probability between quarters t and $t+1$. Women

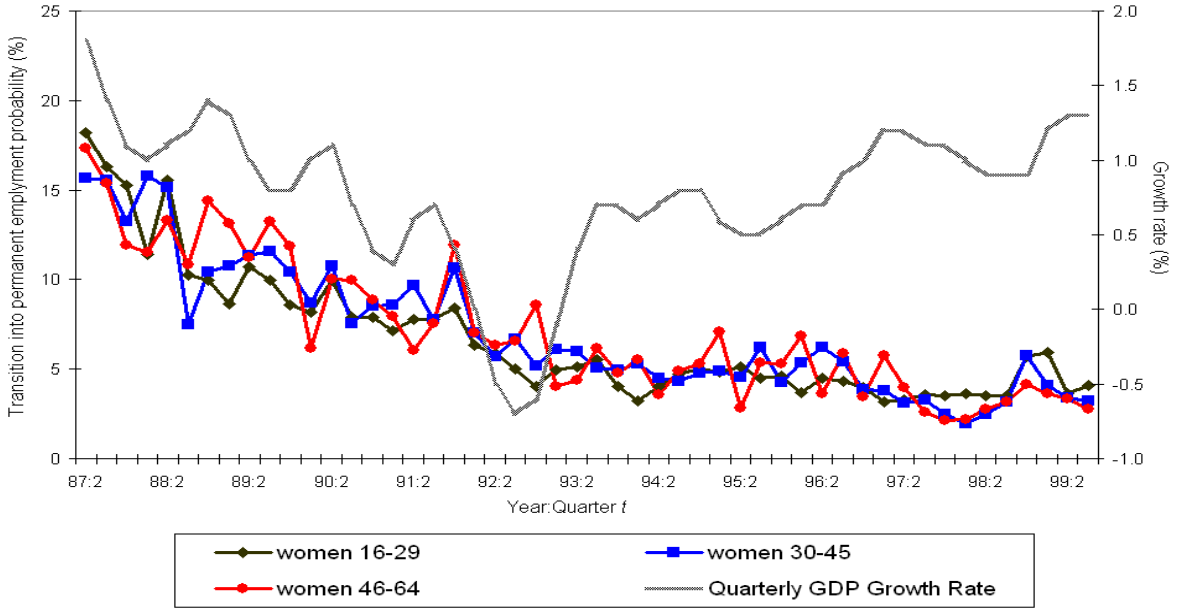

Figure 3. Unemployment to permanent contract transition probability between quarters t and $t+1$. Men

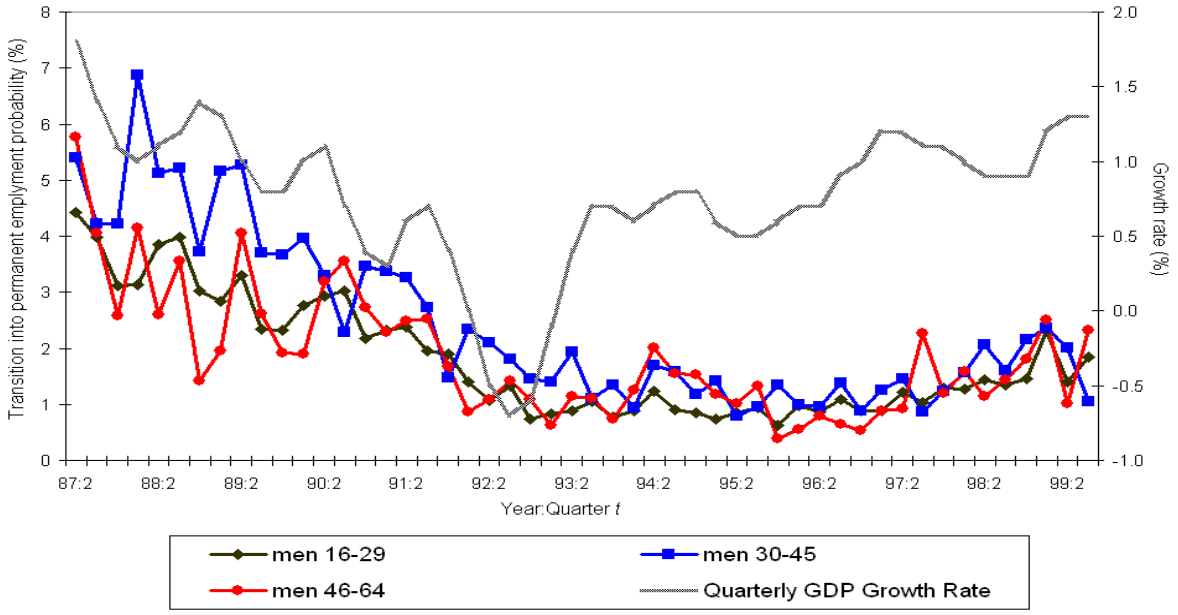

Figure 4. Unemployment to permanent contract transition probability between quarters t and $t+1$. Women