Job Security and New Restrictive Permanent Contracts. Are Spanish Workers More Worried of Losing Their Job?[*]

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October 2006

This paper investigates the impact of the introduction of new restrictive permanent contracts on the perceived job security of the workers in Spain. The reform in question represents a rare example of a "natural experiment". The perceived job security is strongly influenced by the characteristics of the individual and their distribution within the groups. Comparing heterogeneous groups could make the traditional estimator biased. To address this issue we apply a propensity score matching DID estimator. The combination of the two methods allow us to correct the bias due to the heterogeneity of the treatment and the control groups, re-establishing the fundamental assumption of the DID estimator. The analysis is conducted using data from the ECHP Survey for Spain. The result are that this reform has a negative impact on the perceived job security of the workers belonging to almost all the target group, with a particular strong effect for the older workers.

JEL Classification: C14, C21, J28.

Key Words: Job security, Propensity score matching DID, Restrictive permanent contract.

1 Introduction

During recent years, in the European Union, the issue of job security has become very important. The member states, in fact are confronted with a "double bind", that can be summed up as the flexibility-security nexus. On one hand, there is a demand for increasing flexibility in order to reinforce the competitiveness of the firms, the sectors and the countries. On the other hand, labor demand job security, wage security and employability (Wilthangen, 2003). This nexus can be addressed with a policy strategy in which both objectives are represented in a more or less integrated manner. More precisely, flexicurity could be defined as a policy-strategy that attempts to enhance on one hand, the flexibility of the labor market and security notably for weak groups inside and outside the labor market, on the other (Wilthagen, 2003).

In this respect, the European Union in its 2003 *Employment Guidelines for Member States* states:

> "*Member States will facilitate the adaptability of workers and firms to change, taking in account of the need for both flexibility and security [...]. Member States will review and, where appropriate, reform overly restrictive elements in employment legislation that affect labor market dynamics [...].*"

The key point, in this years, for all the European countries has been how to reconcile the demand for flexibility in the labor market expressed by the firms with the demand for job security expressed by the workers. For example, looser rules about hiring and firing may make easier for employers to hire workers, thus improving the job prospects of new entrants to the work force such as young people. At the same time, easing these restrictions can also male people, who have already a job, worry more about the risk of losing it. The question is controversial and the difficulties to find effective policy strategies are reflected by the history of the institutional framework of the national labor markets[1].

From the beginning of the 1980's in Spain the high rate of unemployment pushes towards the benefits that would derive from increasing the introduction of flexibility in the labor market. In this view, the first reform introduced in Spain, concerning the liberalization of the temporary contracts in 1984, has as the main goal an increase in flexibility. In the middle of the nineties, the high percentage of temporary workers and the high turnover determined a pressing need for combining flexibility and security. After the scarce results obtained in 1994, the reform in 1997 aimed at increasing stability in the labor market.

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[1]See Oecd (2004) for further considerations about the costs and benefits of greater job security.

This reform had two additional attractive features. First, differently with respect to the majority of the reforms introduced in the Western Europe in the nineties, it was not a reform "at the margin". In fact, instead of introducing further elements of "pure" flexibility, it tried to increase the use of permanent contracts by reducing the costs of firing. Secondly, this reform can be viewed as a "natural experiment"[2], i.e. we can compare the mean before and after some exogenous event (Besley and Case, 1994). In particular, a "natural experiment" occurs when some exogenous events - like a change in the government policy - change the environment in which agents operate. This characteristic permits to conduct several kind of impact evaluations[3]. Consequently, the issue on evaluation of the impact of this kind of reforms on labor market has stimulated research[4] and the nature of "natural experiment" of the Spanish reform represented an interesting base for several studies[5].

In this paper, we evaluate the impact of the 1997 reform in Spain on the perceived job security of the workers. As said before, a study of this reform if particularly compelling because, in contrast with the majority of the other European reforms, it marks a sharp of change for some groups (i.e. young workers, older workers, long-term unemployed, women under-represented in their occupations and disabled workers), while leaving other groups unaffected. This represents an opportunity to set up a treatment-control design that may provide reliable estimates (Kugler et al., 2003).

The novelty of our study is that we focus on the impact of the reform on the satisfaction of workers with respect to job security. In fact, the change in the level of job satisfaction among the workers due to changes in the institutional regime is not frequently evaluated despite its increasing importance. There is, in fact, an increasing attention towards the determinants of the level of job satisfaction and job security[6] and how these levels are affected by the macro aspects of the labor market[7]. Elements like the rate of unemployment, the

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[2]We consider this reform as a natural experiment because it presents the typical characteristics attributed to a natural experiment, i.e. it's an "exogenous" event that affects some targeted groups while leaving unaffected some other groups. Anyway, the exogeneity of this reform can be argued. It could be seen also as endogenous, considering that it come after a period of instability.

[3]This last feature is particularly valuable because, normally, one reason the causal effect of institutional changes has been difficult to establish is the lack of sharp changes or reforms that can be used for measurement. Most institutional changes in the European context have been either gradual or so general that it is difficult to identify control groups that can be used to establish a non-reform baseline necessary for the comparison (Kugler et al., 2003).

[4]See, for example, Blanchard and Landier (2002), Acemoglu et al. (2001), Bauer et al. (2004).

[5]See, for example, Kugler et al. (2003), Dolado et al. (2001), Arellano (2004).

[6]See, for example, Clark (1997,1998), Clark et al. (1996,2001), Givord and Maurin (2003), Manski and Straub (1999).

[7]See, for example, Clark and Postel-Vinay (2005), Clark et al. (2001).

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Employment Protection Legislation (EPL) and the Unemployment Benefit (UI), the previous institutional and economic frameworks affect the perceived job security of the workers and the behavior of the employees significantly; the change on behavior of the employees could, indirectly, affect the results of the reform.

In the empirical analysis data drawn from the European Community Household Panel (ECHP) for Spain from 1994 to 2001 are used. The ECHP is a cross sectional longitudinal survey that focuses on household income and living conditions.

In the evaluation analysis we combine the propensity score matching and the differences-in-differences analysis. The latter is the natural choice given the possibility to identify clearly a fraction of the population affected by the reform (treated groups) and another part that remains unaffected (non treated groups). The propensity score matching estimation permits us to solve the problem of heterogeneity of the treated/untreated groups.

The paper is organized as follow. Section 2 gives a brief description of the institutional framework of the Spanish labor market and the reform in 1997. Section 3 presents the data and the empirical model. Section 4 presents the estimation results. Finally, section 5 offers some concluding remarks.

2   The institutional framework

The performance of the Spanish labor market is, among the Oecd countries, one of the most disappointing, with an unemployment rate, during the 1990's, exceeding 20%. Accordingly, the employment creation has been one of the primary challenges facing the Spanish government since 1980 (Martìn, 2002).

The main peculiarity of the actual institutional framework of the Spanish labor market finds its origins in 1980, with the approval of the Worker's Statute (Ley del Estatuto de los Trabajadores). This law defined the two main institutional features of the Spanish system of labor market relations which, despite several reforms, still remain operative: a high degree of employment protection and the predominance of collective bargaining at the provincial/industrial level[8].

The most significative attempt to reduce the strictness of EPL was the liberalization of the fixed-term contracts in late 1984. This reform, in fact, introduced a whole variety of temporary contracts which, by contrast to the permanent ones, entail much lower severance payments, if any, and whose termination cannot be appealed (Dolado et al., 2001).

Subsequently, in the middle of 90s, two labor market reforms (1994 and 1997) aimed to reverse the effects of the liberalization of 1984, trying to reduce the proportion of temporary employment. In the early nineties, in effect, one

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[8]See Jimeno and Toharia (1993) for further details.

third of the Spanish labor force worked under temporary contracts (32, 5%) and more than 90% of all new signed contracts were temporary[9]. In Spain, at the moment, there was a pressing need for combining flexibility with security (Martìn, 2002).

The two main provisions of the reform in 1994 limited the use of temporary contracts to seasonal jobs and widened the conditions for "fair" dismissal. The reform had a weak impact on the Spanish labor market. On one side, the employers continued to hire workers under temporary contracts for all types of jobs. On the other side, the approval for dismissal for "economic reason" continued to be granted mainly when there was an agreement between employers and workers, while the labor courts continued to rule most dismissals as unfair.

At the beginning of 1997, the unemployment rate was 21.5% and there was a high level of insecure employment. In this context, the employers confederation (CEOE) and the major unions (UGT and CC.OO) reached and agreement to reform the system of employment contracts and the structure of collective bargaining. This reform aimed to reducing the use of temporary contracts by increasing the incentives for the firms to hire workers from certain population groups using permanent contracts. In practise, the reform introduced a new permanent contract with lower firing costs in case of unfair dismissal.

Since 1998, the Spanish government introduced several measures related to working time flexibility. In particular, with the Agreement on Promoting Stable Part Time Employment on 13th November 1998 a series of measures were introduced to promote stable part time employment, permanent intermittent employment and replacement contracts combined with early retirement.

More recently, the labor reform of 2001 modified again the regulation of the part time contracts suppressing the ceiling for the number of part time hours and introducing a more flexible distribution of working hours groups.

2.1 The reform in 1997

Until 1997 all the reforms introduced in Spain, and in the Western European countries as well, attempted at increasing flexibility through the liberalization of temporary contracts. They are called "reforms at the margin" because they fail to introduce a fundamental liberalization. Instead, they may increase the wages of permanent workers (as a consequence of the creation of a dual labor market), having some undesirable consequences for output, employment and segmentation of labor market[10] (Kugler et al., 2003).

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[9]The percentage of temporary contracts in Spain was one of the highest in Europe. See Martìn (2002) for further details.

[10]See, for example, Blanchard and Landier (2002), Cahuc and Postel-Vinay (2002), Dolado et al. (2001), Hunt (2002), Garcia-Fontes and Hopenhayn (1996), Jimeno and

The 1997's reform, conversely, represented the first attempt of the Spanish government to correct the distortions of the labor market, due to the large increase in temporary contracts of the previous years and, at the same time, to introduce new elements of flexibility, reducing the dismissal costs for permanent contracts.

This reform had three main characteristics. It promoted the use of permanent contracts to hire 18-29 years old, long-term unemployed adults, disabled persons and temporary workers; it reduced the fix term contracts; it promoted combined theoretical and practical education among the young to facilitate their entry into the labor market.

Some of the main incentives introduced by the government were: the reduction of social security contributions[11]; the reduction of dismissal costs during a period of two years for new permanent contracts[12]; the limitation of the number of fix-term contracts that can be offered; the introduction of new training policies.

In practise, the 1997 reform reduced the dismissal costs for unfair dismissals by about 25% and payroll taxes between 40% and 90% for newly signed permanent contracts and for conversions of temporary into permanent contracts after the second quarter of 1997 for workers under 30 years of age, over 45 years of age, long-term unemployed, women under-represented in their occupations and disabled workers. Severance payment for unfair dismissals of newly signed contracts of the workers in the target groups was reduced from 45 to 33 days pay per year of seniority and the maximum was reduced from 42 to 24 months. They payroll taxes reduction[13] was 40% for workers under 30 years of age and for long-term unemployed, 60% for workers above 45 years of age and women under-represented in their occupations and between 70% and 90% for disabled workers. Furthermore, in some cases, the payroll taxes were reduced again after the second year of employment (Kugler et al., 2003).

Toharia (1993,1996), Bertola and Ichino (1995), Bentolila and Dolado (1994); and Bentolila and Saint-Paul (1992) for theoretical and empirical analysis of the effect of temporary contracts.

[11] Employers are entitled to these reductions when they hire a persons from one of the target groups and offer them a permanent contracts.

[12] These incentives have the aim to remove the barriers which prevent employers from offering such contracts.

[13] In Spain, the average payroll tax rate was about 33% of the salary of the worker. The uniform payroll tax rate is differentiated by age group and kind of contract. For example, for young workers it was the 28.3% of the salary. The reduction of 40% implied a new payroll tax rate of about 16%.

3 Data and methodology

3.1 Data

Data are from the European Community Household Panel (ECHP) from 1994 to 2001[14]. The ECHP is a cross-sectional longitudinal survey focusing on household income and living conditions: information on health, education, housing, migration, demographics, employment characteristics and satisfaction are provided[15].

A panel has been extracted from the ECHP for Spain including men and women between 16 and 65 years of age, employed[16].

The panel present an attrition, as typically in a household panel, due to non response and changes in the life of respondents (death, moving, etc.). In Peracchi (2002) the estimated average attrition for Spain is 10%.

The question on job security is inserted in a wide range of questions regarding personal satisfaction of the ECHP questionnaire. The exact wording is as follow:

> **Question:** *"How satisfied are you with your present job or business in terms of job security? Using a scale from 1 to 6, please indicate your degree of satisfaction. Position 1 means that you are not satisfied at all, and 6 that you are fully satisfied"*

The typical formulation of this kind of questions contain subjective element regarding the meaning of "satisfied" or even "job security" that could vary from one person to another (Clark and Postel-Vinay, 2005)[17].

The expected impact of this reform might be ambiguous. First, the reduction of firing costs may increase the probability to access to permanent contracts[18], affecting positively the level of job security. Secondly, the lay-off procedures became "easier" and this could make the "new permanent" workers (e.g. individuals hired with permanent contracts after 1997) feel less secure.

To conclude, Table 1 and Table 2[19] describe the composition of the sample and of the treatment and the control groups considered in our analysis, respectively. We could notice that the treatment and the control groups differ for some characteristics, as sex, marital status, level of education, kind of contract (permanent and temporary), sector (public and private), experienced

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[14]We exclude the last wave (2001) because, in this year, another reform was introduced. This reform modifies and extends the one occurred in 1997.

[15]See Peracchi (2002) for further details.

[16]The questions related to job satisfaction are asked only to employees. See Tab. 1 in the Appendix B for composition of the sample.

[17]This implies that it could be not compared across individuals or countries in a obvious way. Considering the kind of estimation analysis we are going to exploit this element has to be taken in account.

[18]See Kugler et al. (2003).

[19]See Appendix B.

past unemployment. These represent the so-called observable pre-treatment characteristics for which, in the estimation, we have to control properly.

3.1.1 The identification strategy

The aim of this paper is to investigate the effect of the reduction on dismissal costs on the level of perceived job security.

The variable of interest is the perceived job security of the worker that, as all the satisfaction variables, is a categorical variable (it takes the value 1-6: not satisfied-fully satisfied), with an underlying ordinal utility, that is transformed into cardinal linearizing the outcome variable. Since it is an ordinal variable, one can use any translation into numbers provided that the order of the "values" is preserved (Van Praag et al., 2003). In our case, we set each observation of the ordinal output variable equals to the expected mean of a truncated normal distribution[20].

We have thus individualized three different treatment groups: the first group is composed by individuals under 30 years of age, the second is composed by individuals above 45 years of age and, the third is composed by disabled workers[21]. In each of the three treatment groups, the individuals are, before the reform, employed with a temporary contract, and they are entitled, after the reform, to be hired with a new restrictive permanent contract (i.e. permanent contract with lower firing costs).

Four control groups are constructed. The first three are composed by individuals under 30 years of age, above45 years of age and disabled respectively. In each of these groups the individuals are employed with a permanent contract signed before the second quarter of 1997. The fourth control group is simply composed by individuals aged between 30 and 44 years[22].

The treatment group of young workers is compared, first, with the young workers employed with permanent contracts and, successively, with the workers aged between 30 and 44 years. In the same way, we proceed to the analysis of the treatment group composed by old workers[23]. The third treatment group is analyzed separately and it is compared with the control group composed by disabled workers employed with permanent contracts.

The structure of the comparisons is summarized in the following table.

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[20]See Appendix A for further details.

[21]We don't consider the long-term unemployed because we cannot observe them. We exclude also the women under-represented in their work place because they may be self-selected (Kugler et al., 2003).

[22]A group of individuals aged between 30 and 36 is used as control group for the treatment group "below30". For the treatment group "above45", as control, is used a group of individuals aged between 38 and 44 years.

[23]In the analysis of these groups the disabled workers are always excluded.

Fig. 1: Treatment and control groups comparisons.

Treatment groups	Control groups
Below 30 with permanent contracts with lower firing costs	Below 30 with traditional permanent contracts
Below 30 with permanent contracts with lower firing costs	Age 30-36 with temporary or permanent contracts
Above 45 with permanent contracts with lower firing costs	Above 45 with traditional permanent contracts
Above 45 with permanent contracts with lower firing costs	Age 38-44 with temporary or permanent contracts
Disabled with permanent contracts with lower firing costs	Disabled with traditional permanent contracts

Looking at the distribution of the perceived job security for the treatment and the control groups (See Figure 3 to 6 in the Appendix B) before and after the reform we may notice a number of significant variations. The main feature of this variations seems to be a concentration of the answers and a small shift towards the central position of the satisfaction ranking, in particular for the young and the older workers. Conversely, from the responses of the disabled workers emerge a shift from less to more satisfied positions.

Fig. 2: Distribution of replies to Job Security question for first treatment and control groups
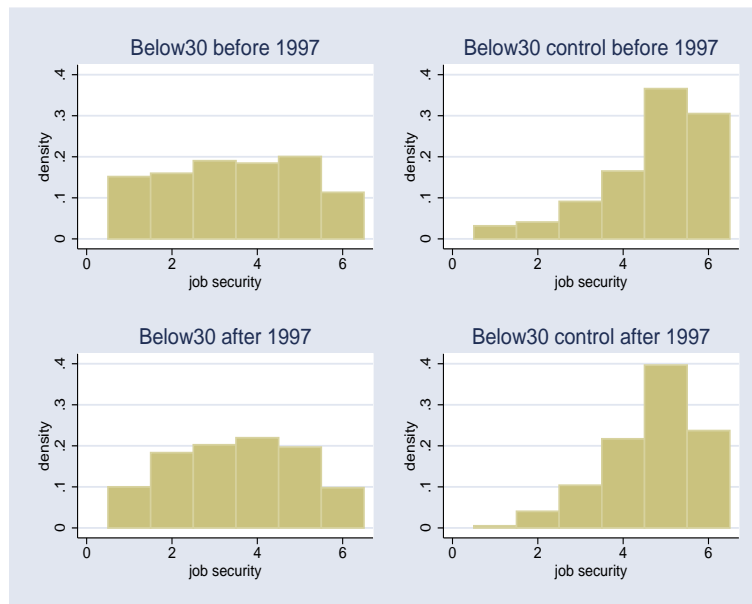
In other words, considering the period before and after the reform, we notice for the two treatment groups - below 30 and above 45 - a decrease in the level of perceived job security, while the disabled workers show an increase of the perceived job security. Nevertheless, we can notice the same kind of variations also for the four control groups.

The presence of variations from one period to another in both treatment and control groups makes difficult individualize, in the graphical analysis, a precise effect of the reform on the level of perceived job security.

The presence of observable differences between the treatment and the control groups requires the use of a set of control variables. In particular, we consider a set of demographic variables[24] and other related to the job conditions of the subjects and income[25].

Another aspects to consider is the possible existence of an age substitution effect[26]. To check for this effect we implement the analysis on restricted samples. These restricted samples are constructed starting from 1994. In this way, we obtain, for younger workers, a sample of individuals aged between 15 & 24, a sample of individual aged between 30 and 39 for the 'middle-age' group and a sample of individuals aged between 45 & 59 for older workers. If there is a age substitution effect we presume to find much larger effects in the restricted samples.

In order to reduce the impact of the subjectivity of the outcome variable, the long-run effect of the reform are evaluated[27].

3.2   The traditional Differences-in-Differences estimator

In the natural experiment, the most used estimation strategy is based on the Differences-in-Differences (DID) estimator. The DID allows a comparison between a pre-treatment and a post-treatment outcome for those individuals exposed to the treatment, using an untreated comparison group (the so-called *control group*) to control for temporal variations of the outcome that is not due to the treatment exposure (Abadie, 2005).

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[24]Sex, age, age squared, level of education (high, middle and low), marital status, immigrant, number of children.

[25]Starting year of actual job, public or private sector, professional level (manager, intermediate, blue-collar), part or full time, experience in the labor market, experience of past unemployment, length of spell of unemployment, size and economic sector of the firm, income of the household.

[26]The age substitution effect is the bias due to the age of the subjects. The individuals with an age at the limit of the cohort in given year could shift to another cohort the following year. For example, an individual 29 years old in 1994 belongs to the first treatment group. The same individual in 1995, will be 30 years old and he will belong to one of the control groups.

[27]The period considered goes from 1994 to 2000. The period of application of the reform started in the second quarter of 1997. In our sample, the observations in 1997 are available only for the last months of the year. This implies that the 1997 is always included in the analysis.

The basic DID framework can be described as follows. Let $Y(i,t)$ be the outcome of interest for individual i at time t. The population is observed in a pre-treatment and a post-treatment period. Let's denote $t = 0$ in the former case and $t = 1$ in the latter case. Between these two periods a fraction of the population is exposed to the treatment. Similarly, let's denote $D(i,t) = 1$ if individual i is exposed to the treatment and $D(i,t) = 0$ otherwise. (Abadie, 2005).

The conventional DID estimator is often specified using a linear parametric model:

$$Y(i,t) = \gamma_0 + \gamma_1 t + \gamma_2 D(i,t) + \gamma_3 t \cdot D(i,t) + \varepsilon(i,t) \tag{1}$$

where $\widehat{\gamma_3}$ will be the Differences-in-Differences estimator:

$$\widehat{\gamma_3} = (\overline{\gamma}_{D=1,t=1} - \overline{\gamma}_{D=1,t=0}) - (\overline{\gamma}_{D=0,t=1} - \overline{\gamma}_{D=0,t=0}) \tag{2}$$

and it measures the effect of the treatment.

Normally, it could be possible to individualized some observable differences between observations in the different groups, but the DID estimator in (2) doesn't capture the effect of these differences. A simple way to adjust for these differences is to introduce an additional vector of explanatory variables (i.e. $X_{i,t}$ (Meyer, 1995).

Equation (1) now becomes:

$$Y(i,t) = \gamma_0 + \gamma_1 t + \gamma_2 D(i,t) + \gamma_3 t \cdot D(i,t) + \beta X_{i,t} + \varepsilon(i,t) \tag{3}$$

3.3   An extension of the DID estimator: The matching DID

The DID estimator is based on a crucial and critical assumption. The error term has to be uncorrelated with the other variables (e.g. $cov(\varepsilon_i, D_i, t_i) = 0$). This means that the average outcomes of the treated and the untreated groups, in absence of treatment, would have followed parallel paths over time (Abadie, 2005).

This is implausible of the pre-treatment characteristics, associated with the dynamics of the outcome variable, are unbalanced between treated and control groups. As a consequence, the estimator will be biased (Abadie, 2005). As shown by Heckman et al. (1998), this bias can be split in three parts: a first component due to the *non overlapping support* (the populations have completely different characteristics, X), a second due to different distributions of X, within the two populations, a third due to differences in outcomes that remain even after controlling for the first two biases. The latter is the selection bias and it is due to the selection of the unobservables.

The issues of common support and miss-weighting could be properly addressed combining the propensity score matching with the DID estimator[28].

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[28]See Heckman et al. (1997).

In fact, the balancing property of the propensity score implies that observations with the same propensity score must have the same distribution of observable characteristics independently of the treatment status (Becker and Ichino, 2002). Furthermore, the matching method links to each treatment unit a control unit having the closest propensity score.

The third kind of bias could become less relevant if, as in our case, the data are administrated with the same questionnaire and the treated and the untreated reside in the same local labor market (Heckman et al., 1997).

The perceived job security has a subjective nature. This implies that the bias due to differences in the characteristics and their distribution within groups might become relevant. The differences in observed characteristics create, in fact, non parallel dynamics for the treated and the untreated and the evaluation problem cannot be addressed with the traditional DID estimator. The combination of the propensity score matching and the DID estimator permits to control properly for this differences, re-establishing the basic assumption of the DID estimator.

3.3.1 The propensity score and the matching estimator

The propensity score is defined by Rosenbaum and Rubin (1983) as the conditional probability of receiving a treatment given the pre-treatment characteristics:

$$P(X) \equiv Pr\{D = 1|X\} = E(D|X) \tag{4}$$

where $D = \{0, 1\}$ is the indicator of exposure to the treatment and X is the multidimensional vector of pre-treatment characteristics. Given a population denoted by i, if the propensity score $p(X_i)$ is known, the Average Treatment effect on the Treated (ATT) can be estimated as follows:

$$ATT \equiv E(Y_{1i} - Y_{0i}|D_i = 1)$$
$$= E[E(Y_{1i} - Y_{0i}|D_i = 1, p(X_i))]$$
$$= E[E(Y_{1i}|D_i = 1, p(X_i)) - E(Y_{1i}|D_i = 0, p(X_i))|D_i = 1] \tag{5}$$

where the outer expectations is over the distribution of $(p(X_i)|D_i = 1)$ and Y_{1i} and Y_{0i} are the potential outcomes in the two counterfactual situations of (respectively) treatment $(D = 1)$ and non treatment $(D = 0)$ (Becker and Ichino, 2002). Formally, the following two hypothesis are needed to derive (5) given (4): Balancing of pre-treatment variables given the propensity

score[29]; Unconfoundedness given the propensity score[30] (Becker and Ichino, 2002).

An estimate of the propensity score is not enough to estimate the Average Treatment effect on the Treated (ATT) of interest using equation (5). In the literature there are several methods and among them the most widely used are *Nearest Neighbor Matching*, *Radius Matching*, *Kernel Matching* and *Stratification Matching*.

In the *Nearest Neighbor* method the match between treated and untreated units consists on searching for the control with the closest propensity score. After the matching, the difference in the outcome between the matched treated and control units is computed, and the ATT of interest is obtained by averaging these differences[31]. The *Radius Matching* matches each treated unit only with the control units whose propensity score falls in a predefined neighborhood of the propensity score of the treated unit[32]. The *Kernell Matching* matches all the treated with a weighted average of all controls, with weights that are inversely proportional to the distance between the propensity score of the treated and controls (Becker and Ichino, 2002).

To conclude, the *Stratification* method consists on dividing the range of variation of the propensity score in intervals such that within each interval treated and untreated units have on average the same propensity score. The ATT is computed as an average of ATT of each block with weights given by the distribution of treated units across blocks (Becker and Ichino, 2002).

In general, the form of the matching estimator is given by:

$$\widehat{\gamma}_3 = \sum_{i \in T} \left\{ Y_i - \sum_{j \in C} W_{ij} Y_j \right\} w_i \tag{6}$$

where $T(D = 1)$ and $C(D = 0)$ represent the treatment and the control groups respectively, W_{ij} is the weight placed on comparison observation j for individual i and w_i accounts for the re-weighting that reconstructs the outcome distribution for the treated sample[33] (Blundell and Costa-Dias, 2002).

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[29]If $p(X)$ is the propensity score, then $D \perp X | p(X)$.

[30]Suppose the assignment to treatment is unconfounded, i.e. $Y_1, Y_0 \perp D | X$ (CIA - unconditional independence assumption). Then, assignment to treatment is unconfounded given the propensity score, i.e. $Y_1, Y_0 \perp D | p(X)$.

[31]Sometimes the matching obtained with this method could be very poor because, for some treated units, the nearest neighbor may have a very different propensity score (Becker and Ichino, 2002).

[32]If the dimension of the radius is set to be very small it is possible that some treated units are not matched because the radius does not contain control units (Becker and Ichino, 2002).

[33]The general form of the estimator changes on the base of the method used. See Becker and Ichino (2002) for further details.

3.3.2 The propensity score matching DID

A DID propensity score matching strategy, as defined in Heckman, Hichimura and Todd (1997), allows for temporary invariant differences in outcomes between treated and untreated individuals. This type of estimator is analogous to the standard DID regression estimator defined above, but it does not impose the linear functional form restriction in estimating the conditional expectation of the outcome variable and it reweights the observations according to the weighting function used by the matching estimator. The DID propensity score matching estimator requires that

$$E(Y_{0,t=1} - Y_{0,t=0}|P, D = 1) = E(Y_{1,t=1} - Y_{1,t=0}|P, D = 0) \qquad (7)$$

where $t = 1$ and $t = 0$ are time periods after and before the reform date. This estimator also requires the support conditions,

$$Pr(D = 1|Z) < 1 \qquad (8)$$

which must hold in both period $t = 1$ and $t = 0$ (a non trivial assumption given the attrition present in many panel data sets). The local linear Differences-in-Differences estimator is given by

$$\widehat{\alpha}_{KDM} = \frac{1}{n_1} \sum_{i \in I_{1t} \cap S_p} \left\{ (Y_{1,t=1,i} - Y_{1,t=0,i}) - \sum_{j \in I_{0t} \cap S_p} W(i,j)(Y_{0,t=1,i} - Y_{0,t=0,i}) \right\}$$
$$(9)$$

where the weights can correspond to either the kernel or the local linear weights. In particular, the kernel weight function is:

$$W(i,j) = \frac{G\left(\frac{P_j - P_i}{a_n}\right)}{\sum_{k \in I_0} G\left(\frac{P_k - P_i}{a_n}\right)} \qquad (10)$$

where $G(\cdot)$ is a kernel function and a_n is a bandwidth parameter. The local linear weighting function is:

$$W(i,j) = \frac{G_{ij} \sum_{k \in I_0} G_{ik}(P_k - P_i)^2 - [G_{ij}(P_j - P_i)]\left[\sum_{k \in I_0} G_{ik}(P_k - P_i)\right]}{\sum_{j \in I_0} G_{ij} \sum_{k \in I_0} G_{ij}(P_k - P_i)^2 - \left(\sum_{k \in I_0} G_{ij}(P_k - P_i)\right)^2}$$
$$(11)$$

With longitudinal data the ATT with Differences-in-Differences and propensity score matching becomes (Origo, 200):

$$\widehat{\alpha}_{KDM} = \sum_{i \in I_{1,t=1} \cap S_p} \left\{ (Y_{1,t=1,i} - \sum_{j \in I_{0,t=1} \cap S_p} W(i,j) Y_{0,t=1,i}) \right\} w_i -$$

$$\sum_{i \in I_{1,t=0} \cap S_p} \left\{ (Y_{1,t=0,i} - \sum_{j \in I_{0,t=0} \cap S_p} W(i,j) Y_{0,t=0,i}) \right\} w_i = \quad (12)$$

$$= ATT_{PSM,t=1} - ATT_{PSM,t=0}$$

where $I_{1,t=1}$, $I_{1,t=0}$, $I_{0,t=1}$ and $I_{0,t=0}$ denote the treatment and the comparison group data sets in each time period.

The procedure to implement the propensity score matching DID can be summarized as follows: Obtain the propensity score; For each participant, identify all non participants who match on the propensity score (i.e. determine common support set); Calculate the before-after differences for each participant; Calculate the before-after differences for multiple non participants using kernel weights or local linear weights; Evaluate the differences-in-differences.

3.4   Some problems: The effect of the business cycle

The level of perceived job security is significantly influenced by the macro aspects of the economy. In particular, decreasing rate of unemployment and increasing GDP growth rate could affect dramatically the level of job security. This aspect highlights the necessity to control properly for cyclical effects.

In the standard approach for Differences-in-Differences estimations the unobservables are assumed to have additive components, an individual effect uncorrelated across individuals, possibly time varying u_{it}, and a macro effect that may differ in its impact across individuals $k_i m_t$.

$$\varepsilon_{it} = u_{it} + k_i m_t \quad (13)$$

The basic assumption of the DID estimator requires the unobservables to be constant over the before and after periods. This requirements will clearly not be satisfied if there is any systematic change in the macro economy that affects the target groups of the reform. Consequently, the condition could be generalized by allowing a macro or general trend effect (Bell et al., 1999):

$$E[\varepsilon_{it}|t = s, i \in g, X_{is}] = u_g + k m_s \quad (14)$$

This macro effect is required to be the same across the target and the comparison groups. Since individuals belonging to different age cohorts and with different kinds of job contracts typically attract different macro effects over

a cycle, this requirement is unlikely to be met (Bell et al., 1999). To allow each group to respond differentially to the business cycle effects we could write:

$$E[\varepsilon_{it}|t = s, i \in g, X_{is}] = u_g + k_g m_s \qquad (15)$$

where k_g allows for differential macro effects across the two groups. It follows that the DID estimator in (2) consistently estimates:

$$plim\widehat{\gamma}_3 = \gamma + (k_{D=1} - k_{D=0})[m_{t=1} - m_{t=0}] \qquad (16)$$

The true effect of the reform is only recovered when $k_{D=1} = k_{D=0}$.
Accordingly, a simple way to obtain consistent estimates of γ is to take another time interval $\theta = (0, 1)$, over which a similar macro trend has occurred. Precisely, we require a period for which the macro trend matches the term $(k_{D=1} - k_{D=0})[m_{t=1} - m_{t=0}]$ in (14) and, then, run a "DID of DID":

$$\widehat{\gamma}_3 = \left\{ (\overline{Y}_{1,t=1} - \overline{Y}_{1,t=0}) - (\overline{Y}_{0,t=1} - \overline{Y}_{0,t=0}) \right\} - $$
$$- \left\{ (\overline{Y}_{1,\theta=1} - \overline{Y}_{1,\theta=0}) - (\overline{Y}_{0,\theta=1} - \overline{Y}_{0,\theta=0}) \right\} \qquad (17)$$

The comparison period chosen for this trend adjustment to Differences-in-Differences requires careful considerations. Normally, the most recent cycle is the most appropriate, since earlier cycles may have had systematically different effects across the target and the comparison groups (Bell et al., 1999). In our case, an ideal comparison period would be the expansion occurred in Spain in late 1980s (1987-1991). Unfortunately, we have no data on perceived job security related to this period. The ECHP survey start from 1994 and previous data surveys don't allow us to distinguish among different age groups. Consequently, our estimation of the treatment effect suffers from a positive bias (e.g. $k_T > k_C$), due to the fact that the target groups may benefit more during the expansion period. In fact, for the target groups the risk of job loss and consequently the perceived job security decrease in the negative and increase in the positive phases of the business cycle.

4 Results

The aim of the reform introduced in Spain in 1997 was to reduce the instability in the labor market through the introduction of new restrictive permanent contracts. The expected effect on the level of perceived job security might be ambiguous. On one hand, an increase in the level of job security for the target groups, due to an increasing probability to access to permanent

contract, is expected. On the other hand, the reduction of firing costs can introduce an higher level of insecurity among the workers. In practise, the final effect of the introduction of these new restrictive permanent contracts depends on the concern of the workers with respect to the higher probability to be hired and the higher probability to be fired.

To analyze the impact of this reform we use a propensity score matching DID analysis. The aim of combining these two techniques of estimation is to solve the problem of heterogeneity of the treatment and the control groups. In fact, the variable of interest is based on the subjective perception of the workers about the possibility to lose their job. In several studies[34], the variables like age, gender, kind of contract, past experiences of unemployment could affect significantly the perceived job security.

For this reason, we use the procedure proposed by Heckman, Hichimura and Todd (1997). First, we obtain the propensity score[35] and we identify, for each participant, all non participants who match on the propensity score[36]; we calculate the before-after differences for both treatment and control groups, using either kernel and local linear weights. At the end, we evaluate the differences-in-differences. A separate analysis for men and women, blue-collar and white-collar have also been performed.

The reform seems to have a negative and significant effect for almost all the target groups, with the exception of the disabled workers. This result is in contrast with some literature on the relationship between job security and strictness of EPL. In Clark and Postel-Vinay (2005), for example, the authors, using individual data from ECHP for 12 European countries, found a negative relationship between job security and job protection, i.e. workers feel less secure in countries where the jobs are more protected. In Spain, instead a reduction of the strictness of EPL seems to have introduced a higher insecurity. On the other hand, our results seems coherent with the conclusion of Kugler et al. (2003), who investigate the impact of the reform on the probability of transition between different states. In particular, they found an increasing probability of transition from permanent employment to non employment, mostly for older worker. They higher probability of losing the job explain the increasing job insecurity of the Spanish workers.

Table 3 reports the estimation results. The workers with less then 30 years of age are compared with a group of workers with a permanent contract signed before 1997. Successively, they are compared with a group of workers with age between 30 and 36 years. In both comparisons, the effect of the introduction of the reform is negative and, in the second case, highly significant. The analysis is also performed on a restricted sample (i.e. in 1994, 15-29

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[34]See, for example, Clark (1997,1998), Clark et al. (1996,2001,2005).

[35]To estimate the propensity score (See Becker and Ichino (2002)) we use a set of demographic variables and some related to the job conditions of the subjects. See footnotes 24 and 25.

[36]See Leuven and Sianesi (2003).

years for treatment group and 30-36 for control group), to test for the existence of the age substitution effect[37]. In the first case, there is no evidence of age substitution effect. In the second case, instead, the effect is higher pointing to an age substitution effect.

We perform the same kind of analysis for the group of workers with more than 45 years of age. In both treated-untreated evaluations, the effect is significant and negative. Also for this group, we perform the analysis on the restricted sample and we find no age substitution effects for both comparisons[38].

It is interesting to notice that the effect of the introduction of the reform is higher for older workers that for the younger workers. The result is coherent with the fact that the older workers are more concerned about the risk of job loss and the job security is decreasing in age.

The negative effect of the reform is higher in the comparison between different age groups. This result suggests a greater impact of belonging to one of the target (disadvantaged) groups than of working with a temporary versus permanent contract.

Particular attention has to be devoted to the case of the disabled workers. This group presents a positive reaction to the introduction of these new permanent contracts. This result is not unexpected. In fact, this reform creates a double effect. On one hand, it becomes easier to be hired with a permanent contract, on the other side, it becomes easier to be fired.

For younger and older workers, the risk of job loss introduced with the reform is the main reason of concern. The disabled workers, instead, are more concerned about the higher probability to obtain a permanent job, considering their difficulty to enter and remain in the job market in a more stable way.

Furthermore, we performed the analysis for young and old workers according to gender. Table 4 and table 4bis report the results of the estimation. The effect of the reform is found to be negative, both for men and women, and significant mostly for men. In general, the estimation results for the male almost replicate the ones obtained in the analysis of the overall sample. In particular, the workers below 30 years of age compared with the "middle-age" group are particularly concerned about the risk of job loss introduced by the reform. Also for the male sample, the workers with more than 45 years of age seem more affected by the reform than the younger worker, but this appear only in the comparison with the first control group. A slight higher effect for the disabled worker has been found.

The estimation results for the female workers are also coherent with the result for the general sample, but they are not significant (except for the

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[37]The result are not reported in the table. They are available under request to the author.

[38]See footnote 37.

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comparison between workers with more than 45 years and workers with age between 38 and 44 years).

The effects observed for the overall sample appear driven by men and/or women, differently depending on the treatment group. In particular, the negative effects for the "below 30" (in the comparison with the "middle-age" group) and the "above 45" groups (in the comparison with "above 45" workers with standard permanent contracts) seems mainly driven by the male sample, that shows a negative and highly significant effect. In the same way, the male disabled workers probably determine the positive and significant effect observed in the overall sample. The negative effect found for the workers with more than 45 years of age in the comparison with the "middle-age" group appears mainly driven by the female sample. In fact, the result for women is negative and highly significant and the magnitude is higher than the male and the overall sample.

In general, the reform seems to affect the young men more than the young women, who are probably more concerned about the possibility to have a permanent contract than to be fired. This is probably due to higher difficulty for young women to enter and remain in the labor market. In the same way, the older women are more worried than the older men. The higher cost of job loss and the increasing difficulty on finding a new job for women above 45 years might increase their job insecurity.

We perform also a separate analysis for blue-collar and white-collar. Table 5 and table 5bis report the estimation results. The results observed for the blue-collar replicate the ones obtained in the analysis of the overall sample. In other words, the effects of the reforms are negative and significant for almost all the target groups, with the exception of the disabled workers. The magnitude of the effects is lower, except for the comparison between the "below 30" and the "middle-age" groups.

Some interesting results can be observed in the analysis of the white-collar workers. In general, the effects are not significant, except in the comparisons between young and "middle-age" workers and old and "middle-age" workers, respectively. In the first case, the effect is positive and significant whereas it is strongly negative and significant, in the second case.

These different effects could be related to the high-skill profile of these kind of workers. In the case of young workers become more important the possibility to stabilize their careers with a permanent position and the probability (and the associate cost) of job loss is not so high. In the case of older workers, the higher probability of firing become more important, probably due to the higher cost, in term of wage loss, with respect to the same age blue-collar workers.

The heterogeneity between the treatment and the control groups can arise also over time. This means that the pre-treatment characteristics could be unbalanced in the two period considered (before and after the reform). To analyze this aspect, we perform the matching of the individuals considering

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both time and the pre-treatment characteristics. Table 6 shows the estimation results.

The analysis confirm, in general, the results obtained before. The unique exceptions is represented by the comparison between the "below 30" treatment group and the "middle-age" group: the effect almost disappear and it is not significantly different from 0.

To conclude, we perform in each analysis an estimation using the traditional DID estimator. The traditional and the propensity score matching DID estimations are, in most of the cases, rather close, although the traditional estimates over or under estimate the treatment effect.

5   Conclusion

This paper uses the labor market reform, that occurred in Spain in 1997, introducing new restrictive permanent contracts characterized by lower dismissal costs and lower payroll taxes. The 1997 reform represents a "natural experiment" and allows us to set up a research design to evaluate its impact on the perceived job security of some target groups of Spanish workers.

The analysis is performed using a propensity score matching DID technique. Estimates using the ECHP data for Spain suggest that the reform reduces the perceived job security for almost all the target groups. This result is robust with respect to the different matching weights and control groups used. The introduction of these new restrictive permanent contracts with lower firing costs and payroll taxes produces a double effect. On one side, the probability to be hired with a permanent contract is higher. On the other side, it becomes easier to be fired.

The young and the old workers are more concerned about the higher risk of job loss introduced by the reform. In fact, the effect for these groups is negative and significant. The worker with more than 45 years of age seems to become more affected by the introduction of these new restrictive permanent contracts.

Conversely, the disabled workers are more concerned about the higher probability to be hired with a permanent contract. In fact, the effect of the reform for this group is positive and significant.

Our results suggest also the presence of some age substitution effects.

Furthermore, the analysis differentiated by gender shows some differences between men and women. In particular, young women are more afraid to lose their job than young men. In the same way, women above 45 years of age are less secure than men.

Interesting is also the separate analysis of blue and white-collar. In particular, the young white-collar workers consider positively the possibility to access more easily to permanent contract, whereas the older ones are more worried about the high cost of a possible job loss.

In previous studies[39], the relationship between job security and strictness of EPL is found to be negative. This means that high level of Employment Protection is associated with low level of insecurity and viceversa.

Our analysis, then, shows that in Spain, the introduction of looser EPL did not increase the job security of the workers. More precisely, it changes the distribution of the perceived job security from more to less satisfied position in the satisfaction ranking.

These different results probably depend on the fact that the perceived job security is the sum of different aspects, like the risk of job loss, the cost of job loss and so on, and it is influenced by observable and unobservable characteristics of the individuals and by the macro conditions of the labor market. Some empirical works[40], in fact, pointed out a deterioration of job security since the end of the 80's, in Europe as well in the US. Yet, a broad range of indicators fails to point out a significant rise of job instability. This apparent paradox might be explained by the fact that the perceived job security expresses anxiety about the consequences of job loss (unemployment and wage loss) rather than an assessment of the risk of lay-offs. In other words, even when a worker is confident about keeping his job, he can still worry about the perspective of losing it (Deloffre and Rioux, 2004).

To conclude, perceived job security affect job stability and viceversa, via labor market institutions. Therefore, given the existence of this link between security and stability, it is very important to consider the effects on perceived job security on evaluating changes in the labor market institutions.

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[39]See, for example, Clark and Postel-Vinay (2005).
[40]See, for example, OECD Employment Outlook (1997), chapter 5.

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A Truncated normal distribution

Let us suppose that the random variable X is $N(0, 1)$, and we consider the truncated distribution $X \geq c_1$. The mean of this truncated distribution is given by[41]:

$$E(X) = \frac{\phi(c_1)}{1 - \Phi(c_1)} = \frac{\text{ordinate at} X = c_1}{\text{right} - \text{hand tail area}} = M_1 \tag{18}$$

If the truncation if from above, so that we consider the distribution $X \leq c_2$, then:

$$E(X) = \frac{-\phi(c_2)}{\Phi(c_2)} = M_2 \tag{19}$$

If the distribution is double truncated, so that we consider $c_1 \leq X \leq c_2$, then:

$$E(X) = \frac{\phi(c_1) - \phi(c_2)}{\Phi(c_2) - \Phi(c_1)} = M \tag{20}$$

In our case, X is the ordered variable that describe the level of job security, c_1 and c_2 are respectively the level 1 and 6 of the satisfaction ranking. We follow the procedure described above and we create a new continuous variable ($security\_pols$) simply setting $security\_pols_i = E(security\_sat | \mu_{i-1} < security\_sat \leq \mu_i)$.

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[41]See Maddala (1986) for further details.

B ECHP for Spain

Fig. 3: Distribution of replies to Job Security question for first treatment and second control groups
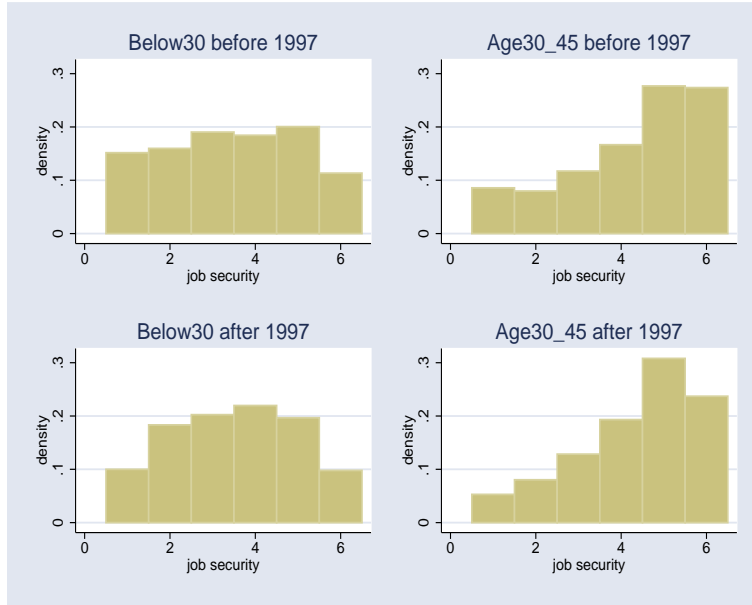

Fig. 4: Distribution of replies to Job Security question for second treatment and first control groups
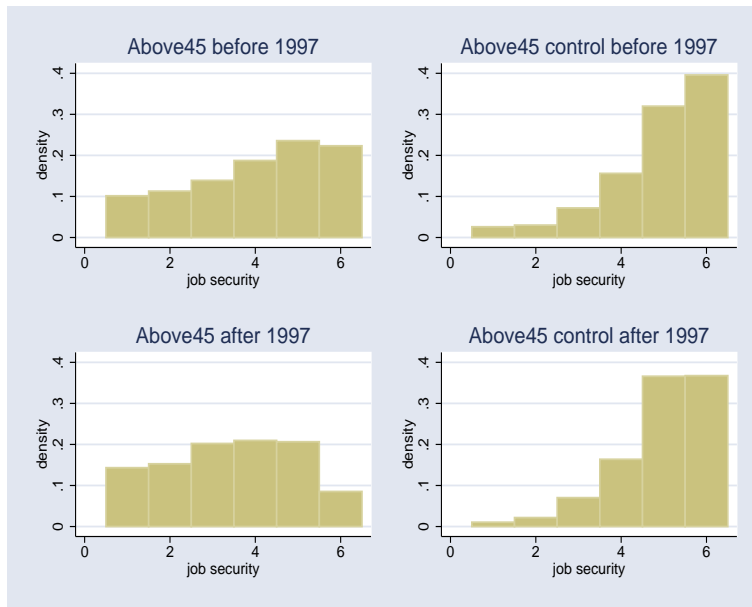

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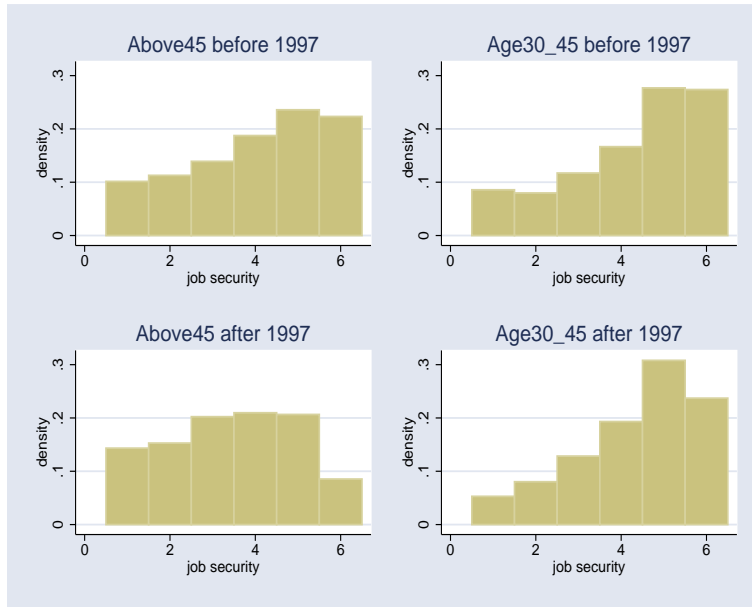Fig . 5: Distribution of replies to Job Security question for second treatment and control groups

Fig. 6: Distribution of replies to Job Security question for third treatment and control groups
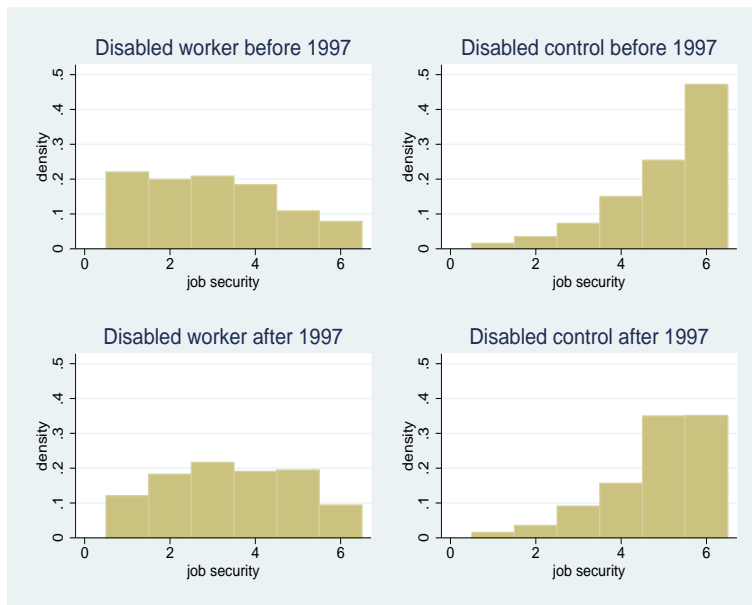
Table 1: Sample Composition

	1994	1995	1996	1997	1998	1999	2000
N. Obs.	4,746	4,400	4,255	4,194	4,135	4,141	4,030
Age < 30	1,267	1,279	1,221	1,236	1,217	1,201	1,127
Age 30 − 45	2,214	1,968	1,922	1,824	1,810	1,819	1,829
Age >= 45	1,265	1,153	1,112	1,134	1,108	1,121	1,074
Disabled worker	0	427	478	434	438	394	378
Female	1,616	1,546	1,496	1,470	1,526	1,534	1,519
Cohabitant or married	3,212	2,923	2,839	2,814	2,741	2,700	2,656
Immigrant	85	80	76	63	64	54	63
N. Children>0	2,542	2,205	1,987	1,827	1,772	1,703	1,607
High education	1,366	1,213	1,242	1,159	1,285	1,418	1,425
Low education	2,483	2,281	2,132	2,125	1,933	1,865	1,793
Manager or professional	704	659	662	674	629	684	657
Blue-collar	2,915	2,641	2,501	2,581	2,554	2,524	2,452
Experience>0	2,934	2,756	2,707	2,716	2,790	2,888	2,905
Permanent contract	0	2,701	2,677	2,651	2,644	2,754	2,717
Public sector	1,277	1,149	1,132	1,032	964	984	917
Part time	304	299	262	271	278	272	240
Industry	1,569	1,404	1,350	1,440	1,387	1,389	1,369
Services	3,012	2,762	2,633	2,577	2,578	2,605	2,506
Firm size 0 − 99	2,464	2,475	2,376	2,915	2,922	2,917	2,870
Experienced past unemployment	1,771	1,700	1,689	1,752	1,682	1,648	1,516
Long spell of past unemployment	659	652	623	647	555	507	434
Income of the household>0	3,771	3,501	3,354	3,403	3,351	3,389	3,320

Tab. 2: Composition of the sub-samples before and after the 1997's labor market reform

	Below30		Age30_36		Age38_44		Above45		Disabled workers	
	Before	After	Before	After	Before	After	Before	After	Before	After
Female	39.8	42.7	36.1	37.3	35.3	35.7	27.1	30.2	34.3	36.1
Cohabitant or married	29.1	32.9	79.5	72.3	85.7	83,6	85.9	83.9	70.4	70.2
Immigrant	2.6	1.7	2.0	1.6	1.4	1.5	1.4	1.3	0.4	1.2
N. Children>0	27.84	23.17	64.34	52.52	56.64	72.43	39.74	30.39	43.98	36.37
High education	25.2	31.7	34.2	37.7	33.6	36.2	24.2	27.7	22.1	23.8
Low education	48.7	41.6	43.1	39.6	45.8	42.8	64.5	57.5	62.1	59.0
Manager or professional	7.2	9.9	16.0	14.9	22.7	23.2	17.3	20.8	12.3	12.7
Blue-collar	68.3	69.3	57.4	60.0	50.4	42.8	62.1	58.2	66.7	69.0
Experience>0	83.16	77.34	83.46	89.81	47.55	65.24	34.41	40.23	56.02	65.45
Permanent contract	21.8	42.5	40.1	66.5	49.2	76.7	47.0	81.4	64.4	66.0
Public sector	14.8	12.0	29.3	23.3	35.7	32.5	30.2	30.7	29.2	24.2
Part time	8.9	8.3	6.4	6.0	5.3	5.5	4.6	5.0	6.9	7.1
Industry	33.9	36.6	29.5	32.6	30.6	30.3	36.6	34.6	32.8	33.8
Services	62.2	59.7	67.4	64.1	60.6	66.5	59.4	60.9	62.5	61.2
Firmsize 0 − 99	81.3	78.9	73.5	71.8	65.3	64.5	67.0	64.7	76.6	71.6
Experienced past unemployment	56.8	51.1	47.2	47.5	27.2	36.2	20.4	22.5	35.0	39.2
Long spell of past unemployment	17.8	14.3	18.3	14.3	12.0	13.9	8.3	8.9	14.9	13.9
Income of the household>0	91.35	92.01	74.22	78.26	69.33	73.00	77.63	79.36	80.55	80.47
N. Obs.	3,599	4,484	2,813	3,455	2,557	2,797	3,135	3,708	905	1,644

Notes: In table 1 and 2 the data are drawn from the ECHP for Spain.

C   The Common Support

Fig. 7bis: Common support (overall sample)
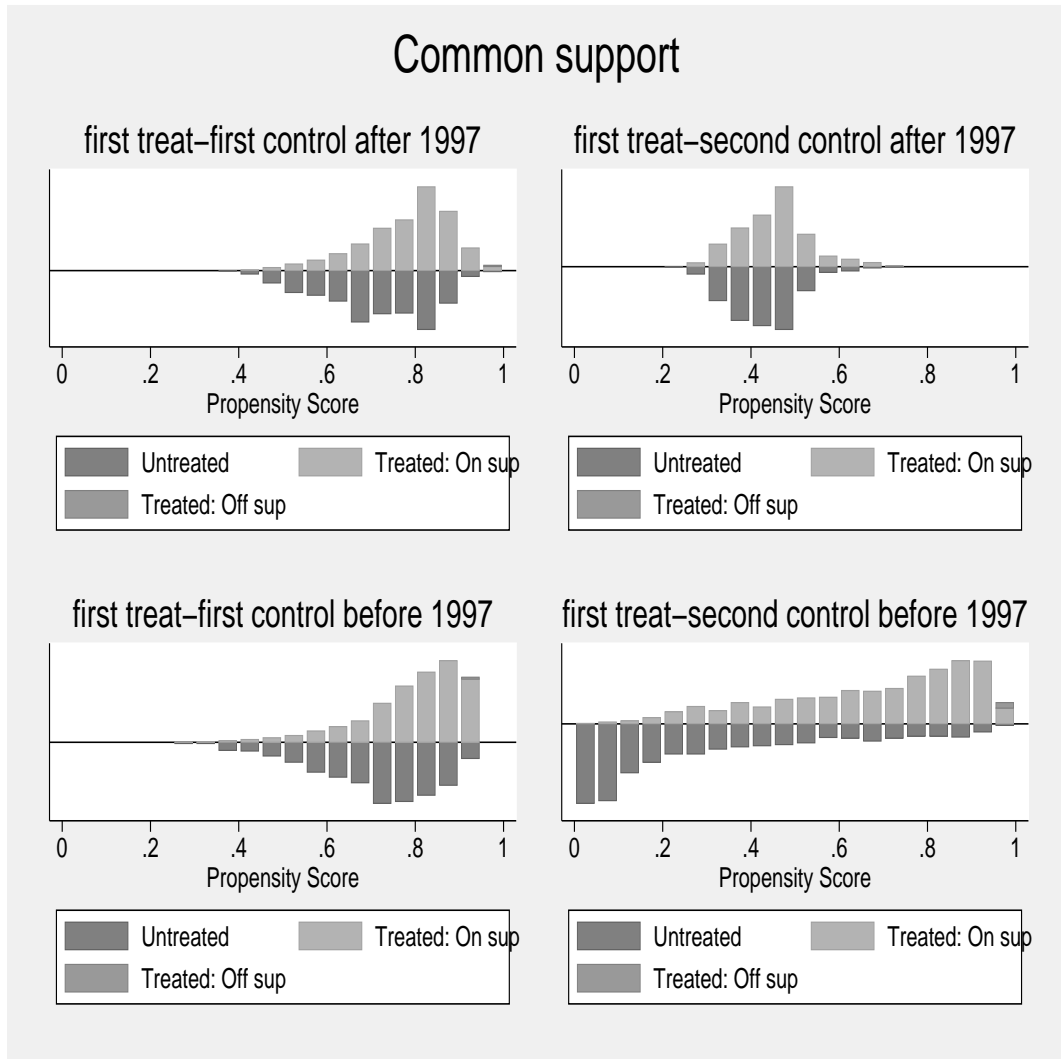

Common support

second treat–first control after 1997

second treat–second control after 1997

second treat–first control before 1997
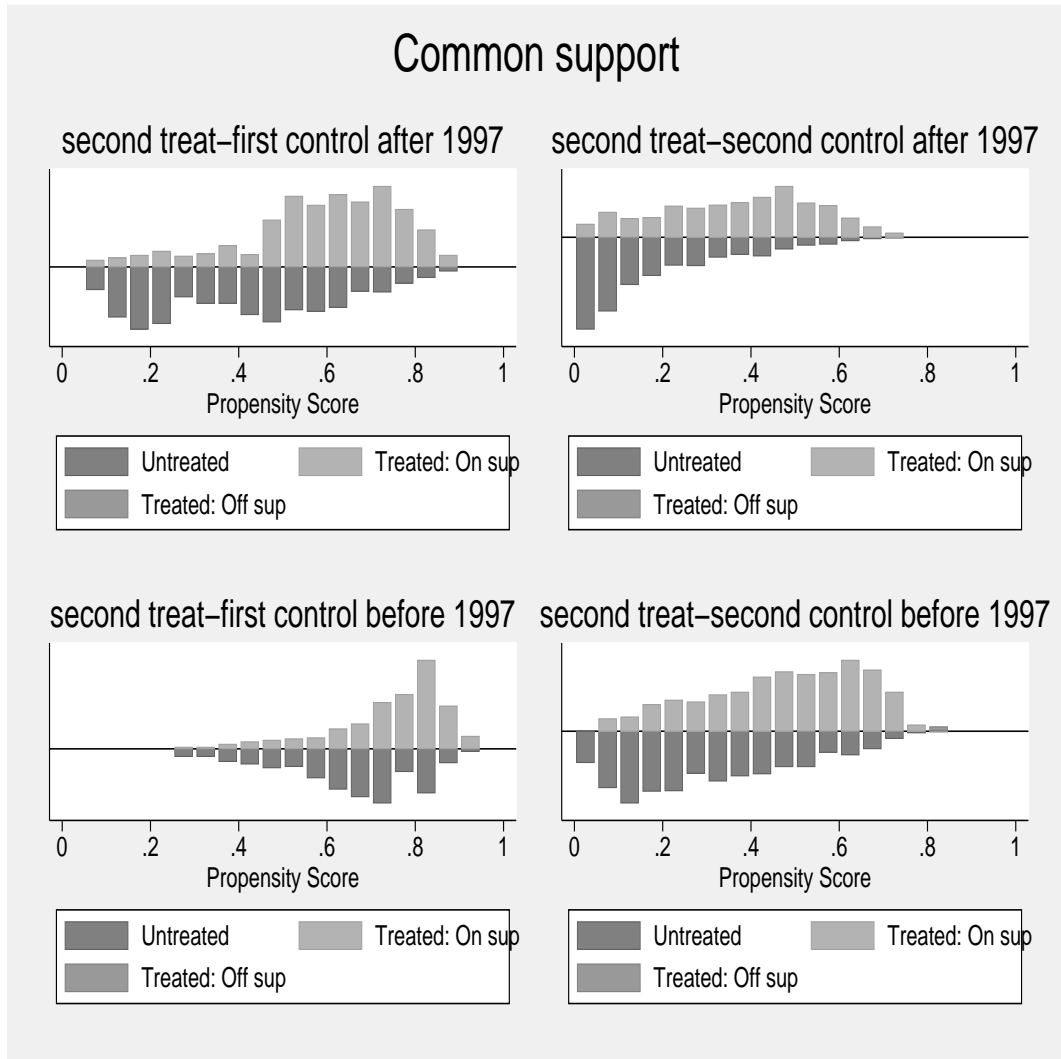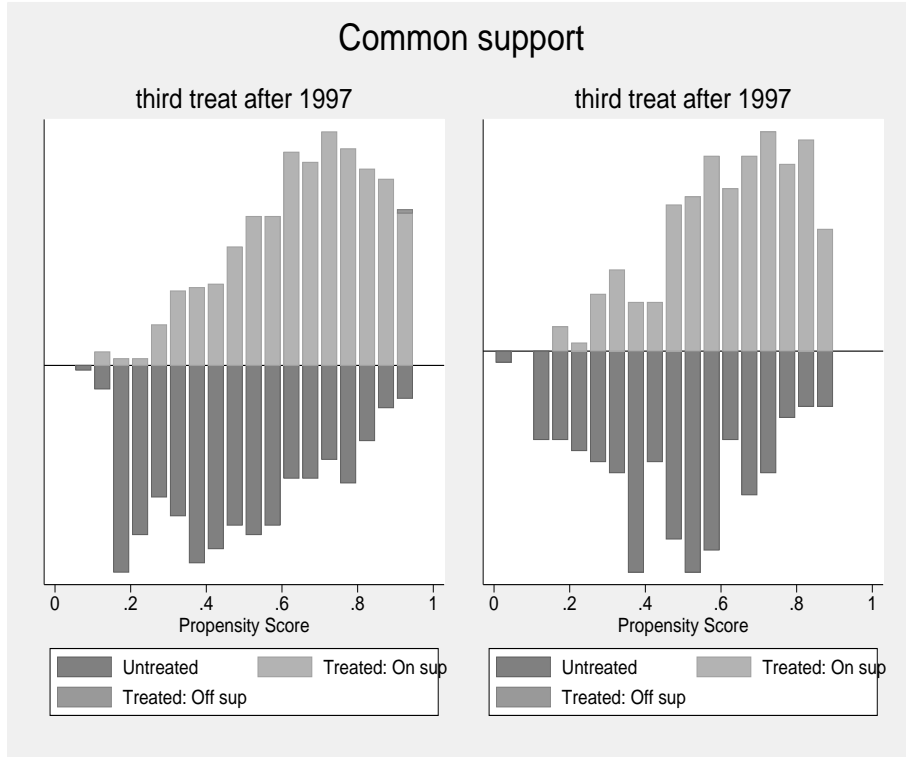
second treat–second control before 1997

Fig. 7ter: Common support (overall sample)

D Estimation Results

Tab. 3: Estimation results: overall sample

Treatment group	Control group	N. Obs.	OLS	DID estimator Propensity score kernel matching	combined with Propensity score local linear regression matching
Below 30 with permanent contracts w/lower firing costs	Below 30 with permanent contracts 1997	6,876	−0.030 (0.050)	−0.069*** (0.044)	−0.019 (0.055)
Below 30 with permanent contracts w/lower firing costs	Aged 30-36 with permanent or temporary contracts	12,088	−0.091* (0.034)	−0.131* (0.031)	−0.133* (0.031)
Above 45 with permanent contracts w/lower firing costs	Above 45 with permanent contracts	2,416	−0.170** (0.076)	−0.146*** (0.099)	−0.170** (0.093)
Above 45 with permanent contracts w/lower firing costs	Aged 38-44 with permanent or temporary contracts	5,206	−0.246* (0.058)	−0.183* (0.070)	−0.172* (0.082)
Disabled with permanent contracts w/lower firing costs	Disabled with permanent contracts	1,491	0.165 (0.101)	0.173*** (0.129)	0.189*** (0.121)

Notes: The standard error is indicated in parenthesis. * corresponds to 1%, ** to 5% and *** to 10% level of significance.

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Tab. 4: Estimation results by gender: Female

Treatment group	Control group	N. Obs	OLS	DID estimator Propensity score kernel matching	combined with Propensity score local linear regression matching
Below 30 with permanent contracts w/lower firing costs 1997	Below 30 with permanent contracts	2,775	−0.055 (0.081)	−0.056 (0.082)	−0.029 (0.092)
Below 30 with permanent contracts with lower firing costs	Aged 30-36 with permanent or temporary contracts	4,331	−0.182* (0.055)	0.078 (0.076)	0.068 (0.073)
Above 45 with permanent contracts with lower firing costs	Above 45 with permanent contracts	797	−0.148 (0.133)	−0.158 (0.162)	−0.119 (0.183)
Above 45 with permanent contracts w/lower firing costs	Aged 38-44 with permanent or temporary contracts	1,749	−0.268** (0.105)	−0.259* (0.112)	−0.220*** (0.142)
Disabled with permanent contracts w/lower firing costs	Disabled with permanent contracts	519	0.165 (0.183)	0.113 (0.213)	0.181 (0.242)

The standard error is indicated in parenthesis. * corresponds to 1%, ** to 5% and *** to 10% level of significance.

Tab. 4bis: Estimation results by gender: Male

Treatment group	Control group	N. Obs	OLS	DID estimator Propensity score kernel matching	combined with Propensity score local linear regression matching
Below 30 with permanent contracts w/lower firing costs	Below 30 with permanent contracts	4,079	−0.013 (0.064)	−0.011 (0.064)	−0.002 (0.073)
Below 30 with permanent contracts w/lower firing costs	Aged 30-36 with permanent or temporary contracts	7,096	−0.029 (0.042)	−0.145* (0.057)	−0.166* (0.060)
Above 45 with permanent contracts w/lower firing costs	Above 45 with permanent contracts	1,596	−0.181*** (0.094)	−0.180** (0.093)	−0.188** (0.091)
Above 45 with permanent contracts w/lower firing costs	Aged 38-44 with permanent or temporary contracts	3,415	−0.234* (0.068)	−0.030 (0.090)	−0.035 (0.081)
Disabled with permanent contracts w/lower firing costs	Disable with permanent contracts	943	0.216*** (0.122)	0.191*** (0.140)	0.185*** (0.147)

The standard error is indicated in parenthesis. * corresponds to 1%, ** to 5% and *** to 10% level of significance.

Tab. 5: Estimation results occupation: Blue-collar

Treatment group	Control group	N. Obs	OLS	DID estimator Propensity score kernel matching	combined with Propensity score local linear regression matching
Below 30 with permanent contracts w/lower firing costs 1997	Below 30 with permanent contracts	4,873	−0.023 (0.061)	−0.043 (0.060)	−0.012 (0.065)
Below 30 with permanent contracts with lower firing costs	Aged 30-36 with permanent or temporary contracts	7,570	−0.059 (0.040)	−0.191* (0.058)	−0.171* (0.056)
Above 45 with permanent contracts with lower firing costs	Above 45 with permanent contracts	1,833	−0.122 (0.091)	−0.145** (0.091)	−0.135*** (0.097)
Above 45 with permanent contracts w/lower firing costs	Aged 38-44 with permanent or temporary contracts	3,500	−0.207* (0.065)	−0.126** (0.069)	−0.117*** (0.077)
Disabled with permanent contracts w/lower firing costs	Disabled with permanent contracts	1,129	0.221*** (0.119)	0.140 (0.133)	0.175*** (0.136)

The standard error is indicated in parenthesis. * corresponds to 1%, ** to 5% and *** to 10% level of significance.

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Tab. 5bis: Estimation results by occupation: White-collar

Treatment group	Control group	N. Obs	OLS	DID estimator Propensity score kernel matching	combined with Propensity score local linear regression matching
Below 30 with permanent contracts w/lower firing costs	Below 30 with permanent contracts	1,864	−0.047 (0.090)	0.022 (0.098)	0.016 (0.103)
Below 30 with permanent contracts w/lower firing costs	Aged 30-36 with permanent or temporary contracts	3,732	−0.128** (0.064)	0.153** (0.091)	0.170** (0.101)
Above 45 with permanent contracts w/lower firing costs	Above 45 with permanent contracts	571	−0.213 (0.159)	−0.169 (0.179)	−0.209 (0.201)
Above 45 with permanent contracts w/lower firing costs	Aged 38-44 with permanent or temporary contracts	1,699	−0.257*** (0.143)	−0.253*** (0.212)	−0.275*** (0.214)
Disabled with permanent contracts w/lower firing costs	Disable with permanent contracts	383	0.188 (0.210)	−0.067 (0.291)	0.027 (0.400)

The standard error is indicated in parenthesis. * corresponds to 1%, ** to 5% and *** to 10% level of significance.

Tab. 6: Time Matching Estimation Results

Treatment group	Control group	N. Obs.	DID estimator Propensity score kernel matching	combined with Propensity score local linear regression matching
Below 30 with permanent contracts w/lower firing costs	Below 30 with permanent contracts	5, 478	−0.057 (0.052)	−0.040 (0.052)
Below 30 with permanent contracts w/lower firing costs	Aged 30-36 with permanent or temporary contracts	12, 046	−0.019 (0.039)	−0.015 (0.043)
Above 45 with permanent contracts w/lowef firing costs	Above 45 with permanent contracts	2, 410	−0.149*** (0.072)	−0.165** (0.075)
Above 45 with permanent contracts w/lower firing costs	Aged 38-44 with permanent or temporary contracts	5, 197	−0.196* (0.072)	−0.188* (0.075)
Disabled with permanent contacts w/lower firing costs	Disabled with permanent contracts	1, 467	0.176*** (0.117)	0.192*** (0.129)

The standard error is indicated in parenthesis. (*) corresponds to 1%, (**) to 5% and (***) to 10% level of significance.

Tab. 7: Propensity Score Test results

Treatment (Before)	N_1 (Before)	Control	N_0 (Before)	Probit Pseudo R^2 Before (1)	Probit Pseudo R^2 After (2)	$P > \chi^2$ After (3)	Median bias Before (4)	Median bias After (5)	% lost to common sup After (6)	Balancing property
Below30 (after 1997)	3,492	Below30 (after 1997)	991	0.061	0.004	0.095	12.0	2.5	10.2	OK
Below30 (before 1997)	757	Below30 (before 1997)	757	0.089	0.004	0.368	19.9	2.9	24.1	OK
Below30 (after 1997)	3,493	Age30-36 (after 1997)	4,426	0.018	0.001	0.770	9.3	1.1	38.2	OK
Below30 (before 1997)	2,842	Age30-36 (before 1997)	3,570	0.313	0.005	0.012	22.7	3.7	21.7	OK
Above45 (after 1997)	949	Above45 (after 1997)	2,759	0.140	0.002	0.999	28.4	1.7	22.6	OK
Above45 (before 1997)	1,675	Above45 (before 1997)	1,460	0.082	0.002	1.000	18.5	1,9	60.1	OK
Above 45 (after 1997)	949	Age38-44 (after 1997)	5556	0.176	0.001	0.989	33.9	1.4	20.2	OK
Above 45 (before 1997)	1675	Age38-44 (before 1997)	4,017	0.144	0.001	0.998	22.5	1.5	59.8	OK
Disabled (after 1997)	744	Disabled (after 1997)	900	0.149	0.005	0.988	23.3	2.5	15.8	OK
Disabled (before 1997)	331	Disabled (before 1997)	574	0.134	0.005	1.000	20.6	3.7	45.6	OK

Notes:

1. Pseudo R^2 from probit estimation of the conditional treatment probability, giving an indication of how well the 20 regressors X explain the perceived job security of the workers.

2. tiny Pseudo R^2 from a probit of D on X on the matched samples. to be compared with (1).

3. P-value of the likelihood-ratio test after matching, testing the hypothesis that the regressors are jointly insignificant, i.e. well balanced in the two matched groups.

4. Median absolute standardized bias before matching, median taken over all the 20 regressors[42].

5. Median absolute standardized bias after matching, median taken over all the 20 regressors.

6. Share of the treated group falling outside of the common support.