

Job Security and New Restrictive Permanent Contracts. Do Spanish Workers Fear Losing Their Jobs?*

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This paper investigates the impact of the introduction of new restrictive permanent contracts on the perceived job security of workers in Spain. The perceived job security is strongly influenced by the characteristics of individuals and their distribution within groups. Comparing heterogeneous groups could make the traditional DID estimator biased. To address this issue I combine the propensity score matching with a fixed effect estimator. The analysis is conducted using data from the ECHP Survey for Spain from 1995 to 2000. The result is that the reform has a positive impact only for one targeted group, i.e. the young workers and no effect for the others. Several robustness checks are performed.

JEL Classification: C14, C33, J28.

Key Words: Job security, Firing Costs, Evaluation Policy, Fixed-effect estimator.

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1 Introduction

From the beginning of the 1980's in Spain the high rate of unemployment pushed towards the possible benefits that would derive from increasing 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 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 poor results obtained in 1994, a new reform in 1997, aimed at increasing stability.

This reform, differently with respect to the majority of the reform introduced in Western Europe in the nineties, was not a reform "at the margin". In fact, instead of introducing further elements of "pure" flexibility (i.e. new kind of temporary contracts), it tried to increase the use of permanent contracts by reducing the costs of firing. Moreover, this reform can be seen as a "natural experiment"¹. In particular, a "natural experiment" occurs when some exogenous event - like a change in the government policy - change the environment in which agents operate and the impact of this change can be evaluated comparing the mean before and after the event (Besley and Case (1994))². The evaluation of the impact of this kind of reforms of labor market has stimulated research³ and the nature of "natural experiment" of the Spanish reform represented an interesting base for several studies⁴.

This paper evaluates the impact of the 1997 reform in Spain on the perceived job security of the workers. A study of this reform is particularly compelling because, in contrast with the majority of the European reforms, it marks a sharp 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 the study is the focus on the impact of the reform on the

¹I 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.

²This 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 reform that can be used for measurement. Most institutional changes in the European context have been either so gradual or general that it is difficult to identify control groups that can be used to establish a non-reform baseline necessary for comparison (Kugler et al. (2003)).

³See, for example, Blanchard and Landier (2002), Acemoglu and Angrist (2001), Bauer et al. (2004).

⁴See, for example, Kugler et al. (2003), Dolado et al. (2004), Arellano (2004).

satisfaction of workers with respect to job security. In fact, the change in the level of job satisfaction among workers due to changes in the institutional regime is not frequently evaluated despite its increasing importance. There is, instead, an increasing attention towards the determinants of the level of job satisfaction and job security⁵ and less attention about how these levels are affected by the macro characteristics of the labor market⁶. In fact, elements like the rate of unemployment, the Employment Protection Legislation (EPL), the Unemployment Benefits (UI), the previous institutional and economic framework affect the perceived job security of workers and the behavior of the employees significantly; the change on the behavior of employees could, indirectly, affect the results of the reforms. Moreover, changes in the level of perceived job security have several potentials broad implications related to investment in job-specific skills, job mobility, consumption and savings, health job-related issues.

In the empirical analysis, data drawn from the European Community Household Panel (ECHP) for Spain from 1995 to 2000 are used. The ECHP is a cross sectional longitudinal survey that focuses on household income and living conditions.

In the evaluation analysis, I combine the propensity score matching with a fixed effect estimator. The latter gives the possibility to exploit the panel characteristics of the data set while the propensity score matching technique, applied to the treated and untreated groups in 1995, addresses the problem of heterogeneity between groups.

Given the subjective nature of the variable of interest and the risk of some unobserved heterogeneity, some robustness checks are performed. First, I check if there are some evidences of reform's effects on other satisfaction variables. Secondly, in order to test the robustness of the fixed effect estimator, I perform the analysis on the control groups. Finally, I estimate the treatment effect using a propensity score matching DID estimator.

Next section gives a brief description of the institutional framework of the Spanish labor market and the 1997 reform. Section 3 presents the data and the empirical strategy. Section 4 presents the estimation results. Section 5 provides some robustness checks. Finally, in section 6 I presents 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 the 20%. Accordingly, the employment creation has been one of

⁵See, for example, Clark (1997), Clark (1998), Clark et al. (1996), Clark et al. (2001), Givord and Maurin (2003), Manski and Straub (1999).

⁶See, for example, Clark and Postel-Vinay (2005), Clark et al. (2001).

the primary challenge facing the Spanish government since 1980 (Martin (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 level⁷.

The most significant attempt to reduce the strictness of EPL was the liberalization of the fixed-term contracts in late 1984. This reform introduced a whole variety of temporary contracts which, by contrast to the permanent ones, entailed much lower severance payments, if any, and whose termination couldn't be appealed (Dolado et al. (2004)).

Subsequently, in the middle of 90s, two labor market reform (1994 and 1997) aimed to reverse the effects of the 1984 liberalization, trying to reduce the proportion of temporary employment. In the early nineties, in effect, one third of Spanish labor force worked under temporary contracts (32.5%) and more than 90% of all new signed contracts were temporary⁸. In Spain, at the moment, there was a pressing need for combining flexibility and security (Martin (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" dismissals. 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 kinds of jobs. On the other side, the approval of 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 dismissal 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 an 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 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 Agreements on Promoting

⁷See Jimeno and Toharia (1993) for further details.

⁸The percentage of temporary contracts in Spain was one of the highest in Europe. See Martin (2002) for further details.

Stable Part Time Employment on 13th November 1998, a series of measures were introduced to promote stable part time employment, permanent intermittent and replacement contracts combined with early retirement. Finally, the 2001 labor reform suppressed the ceiling for the number of part time hours and introduced a more flexible distribution of working hours group.

2.1 The 1997 reform

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. People refer to them as "reforms at the margin" because of the failure on introducing a fundamental liberalization. Instead, they may increase the wages of permanent workers (as a consequences of the creation of a dual labor market), having some undesirable consequences for output, employment and segmentation of the labor market⁹ (Kugler et al. (2003)).

The 1997 reform, conversely, represents 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. First, it promoted the use of permanent contracts to hire 18-29 years old, long-term unemployed adults, disabled persons and temporary workers; secondly, it reduced the use of fix-term contracts; finally, 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 contribution¹⁰; the reduction of dismissal costs during a period of two years for new permanent contracts¹¹; the limitation of the number of fixed-term contracts that can be offered; the introduction of new training policies.

In practise, the 1997 reform reduced 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

⁹See, for example, Blanchard and Landier (2002), Cahuc and Postel-Vinay (2002), Dolado et al. (2004), Hunt (2000), Garcia-Fontes and Hopenhayn (1996), Jimeno and Toharia (1993), Jimeno and Toharia (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.

¹⁰Employers are entitled to these reductions when they hire a person from one of the targeted group and offer a permanent contract.

¹¹These incentives had the aim of remove the barriers which prevent employers from offering such contracts.

years of age, long-term unemployed, women under-represented in their occupations and disabled workers. Severance payment for unfair dismissals of newly signed contracts for the workers in the targeted groups were reduced from 45 to 33 days pay per year of seniority and the maximum was reduced from 42 to 24 months. The payroll taxes reduction¹² was 40% for workers under 30 years of age and 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, payroll taxes were reduced again after the second year of employment (Kugler et al. (2003)).

3 Data and methodology

3.1 The data

Data are from the European Community Household Panel (ECHP) from 1995 to 2000¹³. 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 provided¹⁴.

A panel has been extracted from the ECHP for Spain including men and women between 16 and 65 years of age, employed¹⁵. The panel presents attrition, as typical 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 question 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 contains a subjective element regarding the meaning of "satisfied" or even "job security" that could

¹²In Spain, the average payroll tax rate was about the 33% of the salary of the workers. The uniform payroll tax rate was differentiated by age group and 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%.

¹³I exclude the first (1994) and the last wave (2001): the first one because there are no workers hired with permanent contracts; the last one because, in the same year, another reform that modifies and extends the 1997 reform has been introduced.

¹⁴See Peracchi (2002) for further details.

¹⁵The questions related to job satisfaction are asked only to employees. See tab. 4 in Appendix B for the composition of the sample.

vary from one person to another (Clark and Postel-Vinay (2005))¹⁶.

Tab. 4 and tab. 5¹⁷ describe the composition of the sample and of the treatment and control groups considered in the analysis, respectively. The groups differ for some characteristics, as sex, marital status, number of children, level of education, kind of contracts (permanent and temporary), sector (private or public), experienced past unemployment: the so-called observable pre-treatment characteristics that require a proper control.

3.2 The identification strategy

The variable of interest is the perceived job security of the worker that, as all the satisfaction variable, is a categorical variable (it takes the value 1-6: not satisfied-fully satisfied), with an underlying ordinal utility, that has been transformed into cardinal linearizing the outcome variable: any translation into numbers is suitable, provided that the order of the "values" is preserved (Van Praag et al. (2003)). Each observation of the ordinal output variable is set equal to the expected mean of a truncated normal distribution¹⁸.

Two different treatment groups are identified: the first group is composed by individuals under 30 years of age and the second by individuals above 45 years of age¹⁹. In each of the treatment groups the individuals are temporary workers and they are entitled, after the reform, to be hired with the new permanent contracts. Four control groups are constructed. The first is composed by individuals with less than 30 years of age, working with a "traditional" permanent contract; the second by temporary workers with an age between 30 and 36 years; the third by permanent workers with more than 45 years of age; finally, the fourth group is composed by temporary workers with an age between 38 and 44 years.

The first treatment group (i.e. temporary workers with less than 30 years of age) is compared with, first, with permanent workers with less than 30 years of age and, secondarily, with temporary workers aged between 30 and 36 years. In the same way, the second treatment group (i.e. temporary workers with more than 45 years of age) is compared with permanent workers with more than 45 years of age and with the group of temporary workers aged between 38 and 44 years (tab. 1).

¹⁶This implies that it could be not compared across individuals or countries in a obvious way. This element has to be taken in account in a proper way.

¹⁷See Appendix B

¹⁸See Appendix A for further details.

¹⁹I don't consider the long-term unemployed because I cannot observe them in the pre-reform period. I don't look specifically at women under-represented in their workplace because they can be self-selected and I cannot distinguish them (Kugler et al. (2003)). I exclude also the disabled workers because of the lack of observations.

Tab. 1.1: Treatment and control groups comparisons.

Treatment groups	Control groups
Below 30, temporary contracts	Below 30, permanent contracts
Below 30 , temporary contracts	Age 30-36 , temporary contracts
Above 45, temporary contracts	Above 45, permanent contracts
Above 45 , temporary contracts	Age 38-44 , temporary contracts

To each treatment group two control groups are assigned. In the first case, treated and untreated have the same age, but different contracts (temporary and permanent respectively); in the second case, treated and untreated have the same kind of contract but different age²⁰.

Figure 1 shows that the first treatment group presents a change in the level of perceived job security starting in 1997. After the reform, this group presents an increasing level of job security. The first control group (i.e. permanent workers with the same age), instead, presents a more stable pattern along all the period. In the same way, the second control group has a less stable pattern, but does not show a precise trend.

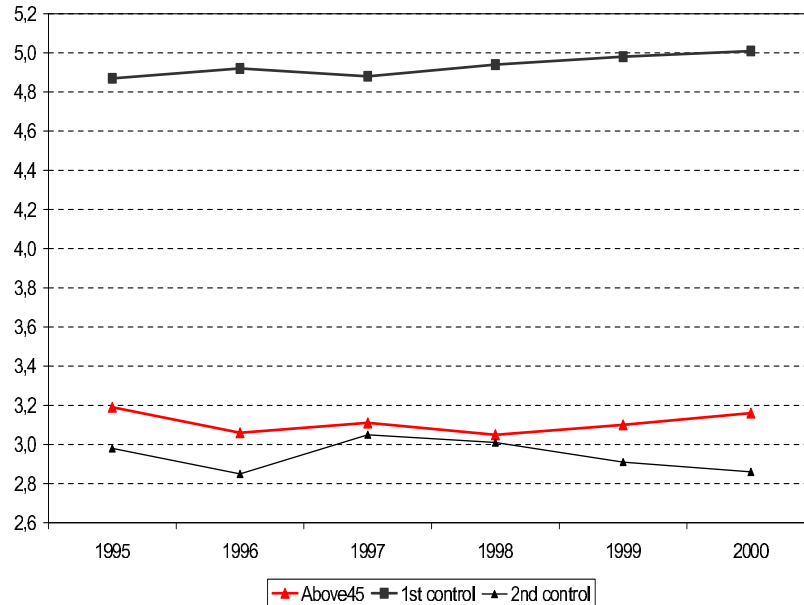
Fig. 1: Average level of job security for the "below 30" treatment group versus the two control groups (yearly means)



Note: ECHP dataset. The first control group is composed by permanent workers with less than 30 years of age; the second is composed by temporary workers aged between 30 and 36 years.

²⁰The first control group is composed by individuals who are better off than the treated, while the second is composed by individuals who are worse off than the treated.

Fig. 2: Average level of job security for the "below 30" treatment group versus the two control groups (yearly means)



Note: ECHP dataset. The first control group is composed by permanent workers with more than 45 years of age; the second is composed by temporary workers aged between 38 and 44 years.

Considering the second treatment group (see figure 2) there is no evidence of some effect on the level of perceived job security introduced by the reform. In fact, in the observed period, the perceived job security of the target group doesn't change, as well as the level of job security of the first control group (i.e. permanent workers with more than 45 years of age). As before, the pattern of the second control group is less stable than that of the first control, but there is no evidence of a trend change.

3.3 Estimation strategy

3.3.1 The traditional Differences-In-Differences estimator

In a natural experiment, the most used estimation strategy is based on the Differences-In-Differences (DID) estimator. The DID allows a comparison between pre-treatment and 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 are not due to the treatment exposure (Abadie (2005)).

The basic DID framework can be described as follow. 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 the 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:

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

$\hat{\gamma}_3$ is the Differences-In-Differences estimator:

$$\hat{\gamma}_3 = (\bar{\gamma}_{D=1,t=1} - \bar{\gamma}_{D=1,t=0}) - (\bar{\gamma}_{D=0,t=1} - \bar{\gamma}_{D=0,t=0}) \quad (2)$$

that measures the effect of the treatment.

The DID estimator is based on a crucial 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 untreated, in absence of treatment, would have followed parallel paths over time (Abadie (2005)). This is implausible if 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 Heckmann et al. (1997), this bias can be split in three parts: a first component due to the *non overlapping support* (i.e. the population have completely different characteristics, X), a second due to the different distributions of X, within the two populations, a third due to the differences in outcomes that remain even after controlling for the first two biases²¹.

The differences in observed characteristics, in fact, might create non parallel dynamics in the perceived job security for the treated and the untreated. The bias due to these differences might become relevant and the evaluation problem could not be addressed with the traditional DID.

Looking at figure 1 and 2, it appears that in the pre-reform period the treatment and the control groups don't follow parallel paths. This evidence supports the fact that in this case the assumption of the DID is not valid.

3.3.2 The propensity score matching and the fixed effect estimator

Two main aspects have to be considered to set up the evaluation strategy in a proper way. The first relates to the heterogeneity between and within treatment and control groups. The second refers to the panel characteristics of the sample.

²¹The latter is the selection bias and it is due to the selection of the unobservable. This bias can become less relevant if, as in this case, the data are administrated with the same questionnaire and the treated and the untreated reside in the same local labor market (Heckmann et al. (1997)).

Given those considerations, the empirical strategy used is composed by three steps. First, the panel has been balanced in order to have the same individuals in all the years considered²². In this way, I address the problem related to the heterogeneity overtime, due to the possibility that, in different years, treatment and control groups could be composed by different individuals. Secondly, to control for the heterogeneity between groups, are matched on the basis of a set of pre-treatment observable characteristics²³, using a propensity score matching method. Finally, the effect of the reform is estimated using a fixed effect estimator.

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

$$P(X) \equiv Pr[D = 1|X] = E[D|X] \quad (3)$$

where $D = \{0, 1\}$ is the indicator of exposure to treatment and X is the multidimensional vector of pre-treatment characteristics.

Several methods are available in order to match treatment and control groups on the basis of the propensity score²⁴. I use the *Kernel Matching* method that 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 the controls (Becker and Ichino (2002)). In particular, the kernel weight function is:

$$W(i, j) = \frac{G\left(\frac{P_j - P_i}{a_n}\right)}{\sum_{k \in I_0} G\left(\frac{P_k - P_i}{a_n}\right)} \quad (4)$$

where $G(\cdot)$ is the kernel function and a_n is a bandwidth parameter.

Formally, two hypothesis are required in the matching process in order to derive, successively, the treatment effect. The balancing property of treatment variables²⁵, as well as, the unconfoundedness given the propensity score must hold²⁶ (Becker and Ichino (2002)).

The use of propensity score addresses properly the issue of common support and miss-weighting²⁷. In fact, the balancing property implies that obser-

²²Vedi paper Stefano.

²³Demographics variables: sex, age, level of education, marital status, immigrant status, number of children; job conditions: professional level, sector, industry, working hours, firm size, experience in the labor market, experience of past unemployment, length of the unemployment spell; income: income of the household.

²⁴The most common methods are: Nearest Neighbor Method, Radius Matching, Kernel Matching and Stratification Matching.

²⁵If $P(X)$ is the propensity score, then $D \perp X|P(X)$.

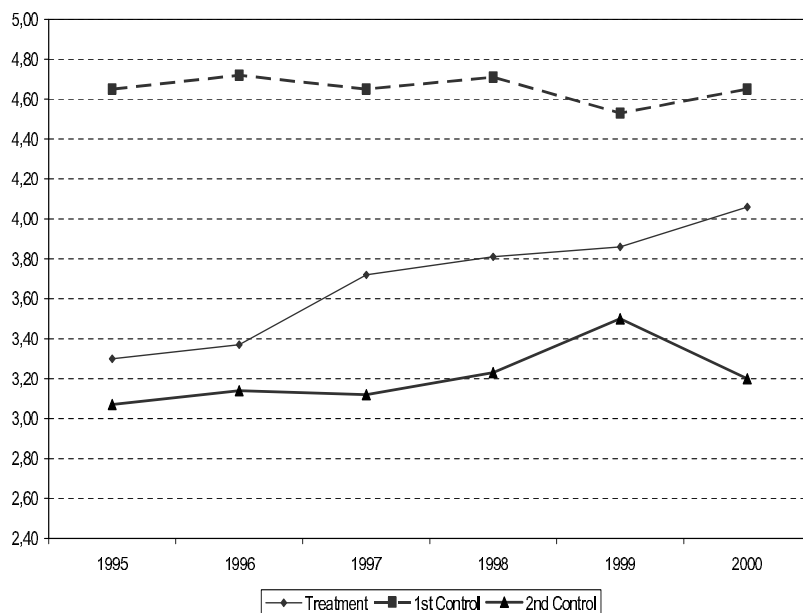
²⁶Suppose the assignment to the treatment is unconfounded, i.e. $Y_1, Y_0 \perp D|X$ (CIA - unconditional independence assumption). Then, assignment to treatment is unconfounded given the propensity score, i.e. $Y_1, Y_0 \perp D|P(X)$.

²⁷See Heckmann et al. (1997).

variations 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 a control unit having the closest propensity score.

In figure 3, the yearly average of perceived job security for matched treatment and control groups are shown. From the graph, it appears that the pre-treatment paths are now parallel and only the treatment group shows a change after 1997.

Fig. 3: Average level of job security for matched treatment and control groups (yearly means)



Note: ECHP dataset. The treatment groups is composed by temporary workers below 30 years of age; the 1st control group is composed by permanent workers below 30 years of age; the 2nd control group is composed by temporary workers aged between 30 and 36 years.

Eventually, the treatment effect of the reform on the perceived job security of the workers, using a fixed effect estimator on the matched and balanced sample, is estimated.

In practise, I estimate the following equation:

$$Y_i = \alpha_i + \delta_t + \gamma D_{i,t} + \beta D_{i,t} \cdot Post_{1997} + \varepsilon_{i,t} \quad (5)$$

where α_i indicates the individual effect, δ_i indicates the time effects, $D_{i,t}$ is a dummy variable assuming value 1 when the individual is treated and 0 otherwise, $Post_{1997}$ is a dummy variable having value 1 in the post-reform period and 0 otherwise. β is the parameter of interest that measures the effect of the reform.

The key assumption of this approach is that the unobservable α_i (i.e. individual effects) are time invariant (Cameron and Trivedi (2005)). The fixed effect model has the attraction of allowing one to use panel data to establish causation under weaker assumptions than those needed to establish causation with cross-section data or with panel data model without fixed effect, such as pooled models or random effects models (Cameron and Trivedi (2005)).

4 Results

The aim of the reform introduced in Spain in 1997 was to reduce the instability in the labor market through a new restrictive permanent contract. The reform introduced in Spain had an impact on the "real" level of protections of workers, as shown in tab. 2:

Tab. 2: Oecd EPL Index for Spain

	Late 80s	Late 90s
Individual dismissal	3.9	2.6
Temporary employment	3.8	3.3
Collective dismissal	-	3.1
Overall EPL	3.8	2.9

Note: Oecd (1999) *Employment Outlook*

The overall index of strictness of EPL is decreased in the late 90s and this decrease is mainly due to the reduction in the level of protection relative to the individual dismissal.

The expected effect on the level of job security might be ambiguous. On one hand, an increase in the level of job security for the targeted groups, due to an increasing probability to access to permanent contracts, is expected. On the other hand, the reduction of firing costs, that accompany this contract, can introduce a 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 higher probability to be hired and the higher probability to be fired.

To analyze the impact of this reform I use a fixed effect estimator after having matched treatment and control groups on the basis of the propensity score technique. In practise, I first balance the panel, then I obtain the propensity score²⁸ and I identify, for each participant, all non participants who match on the propensity score, using kernel weights²⁹. Finally, on these matched samples I estimate the treatment effect of the reform using a fixed effect model. A separate analysis for blue-collar workers has also been performed. Tab. 6 reports the estimation results.

The reform has a positive and significant effect for temporary workers with

²⁸See Becker and Ichino (2002) for details on the procedure to calculate the propensity score.

²⁹See, Leuven and Sianesi (2003) for details on the matching procedure.

less than 30 years of age, in both the comparison performed. This results is coherent with the literature on the relationship between job security and strictness of EPL. In Clark and Postel-Vinay (2005), for example, the authors, using 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. Also in Spain, the reduction of strictness of EPL seems to have introduced a higher sense of security. On the other hand, my results are coherent also with the conclusions of Kugler et al. (2003), who investigate the impact of the reform on the probability of transition between different states. They noticed an increasing probability, in particular for young individuals, to move from temporary to permanent employment.

There is no effect instead on the perceived job security for the workers more than 45 years old. This is explained by the fact that for this group together with an higher probability to move from temporary to permanent positions, there is an higher probability of transition from permanent employment to non employment as well³⁰. Moreover, there are evidences of increasing access to early retirement through unemployment and disability benefits for older workers in Spain³¹. The existence of these mechanism reduces the impact of the reform on the perceived job security of older workers.

These results are confirmed also by the analysis performed only on the blue collar workers. The temporary workers with less than 30 years of age show an increase in the perceived job security due to the introduction of the reform in both comparisons. Tab. 1.7 shows the estimation results.

5 Sensitivity analysis

5.1 The outcome variable

The outcome variable of my analysis - the perceived job security of the workers - is subjective and this might rise some doubts about the estimation results. In other words, it might be that the effect captured by the estimator is affected by subjective bias even after controlling for individual characteristics.

One way to check for the existence of a possible subjective bias is to explore the overall satisfaction with respect to job and other dimensions related or unrelated to the changes introduced by the 1997 reform.

The ECHP questionnaire contains a set of satisfaction questions related to different aspects of the job conditions. I plot the yearly average level of different satisfaction variables for the first treatment group - i.e. temporary workers with less than 30 years of age - and the first control group, com-

³⁰See Kugler et al. (2003).

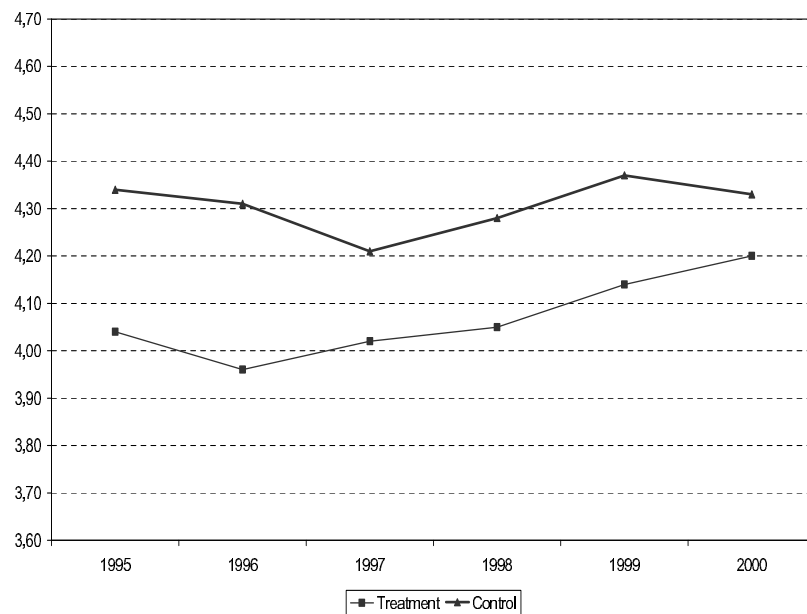
³¹See Boldrin et al. (1997) for further details.

posed by permanent workers with less than 30 years of age³². In figure 4, the overall job satisfaction variable that comes out from the following questions:

How much satisfied are you with your work or main activity?

The answers are ranked from 1 to 6, where 1 means not satisfied and 6 fully satisfied.

Fig. 4: Average level of job satisfaction for the "below 30" treatment group versus the "below 30" control group (yearly means)



Note: ECHP dataset.

The overall job satisfaction of the treated is increased starting from 1996 and there is not a specific change after the 1997 reform. The control group, on the other side, shows a similar increasing pattern on the level of job satisfaction, since the year of the reform. Moreover, the differences in the level of job satisfaction among the two groups is not so wide as in case of the perceived job security.

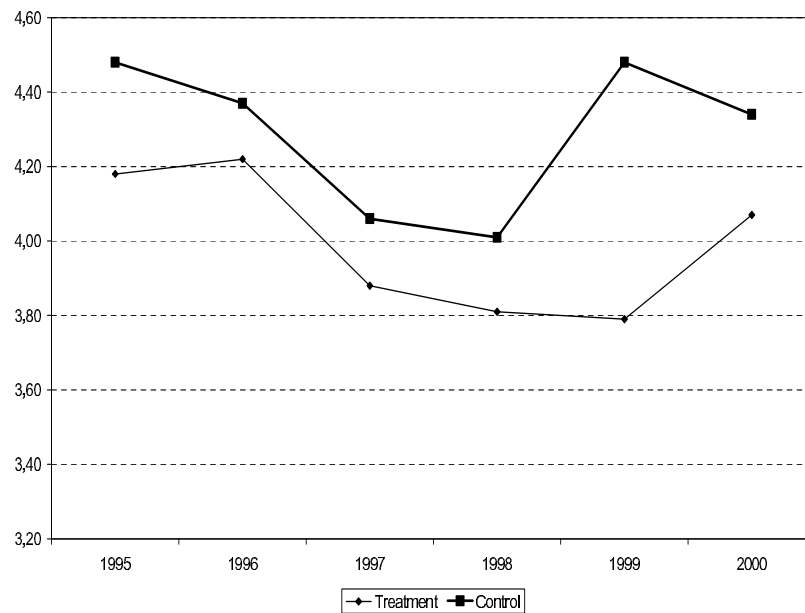
The satisfaction related to the distance from the workplace is interesting because is totally unrelated to the reform. The question is:

³²In the paper, I proposed this check only for the first treatment-first control groups comparison and only for some satisfaction variable for simplicity reasons. For more detailed information, please contact the author.

How satisfied are you with your presents job in terms of distance to job/commuting?

Figure 5 makes clear that there are no changes in the level of the satisfaction after the 1997 reform both for the treatment and control groups and, moreover, the both follow the same pattern.

Fig. 5: Average level of distance satisfaction for the "below 30" treatment group versus the "below 30" control group (yearly means)



Note: ECHP dataset.

Figure 6 and 7, represents the yearly average level of satisfaction related to working time and working/environment conditions respectively. The two question are:

How satisfied are you with your present job in terms of working times (day time, night time, shifts, etc.)?

How satisfied are you with your present job in terms of working conditions/environment?

Fig. 6: Average level of working time satisfaction for the "below 30" treatment group versus the "below 30" control group (yearly means)

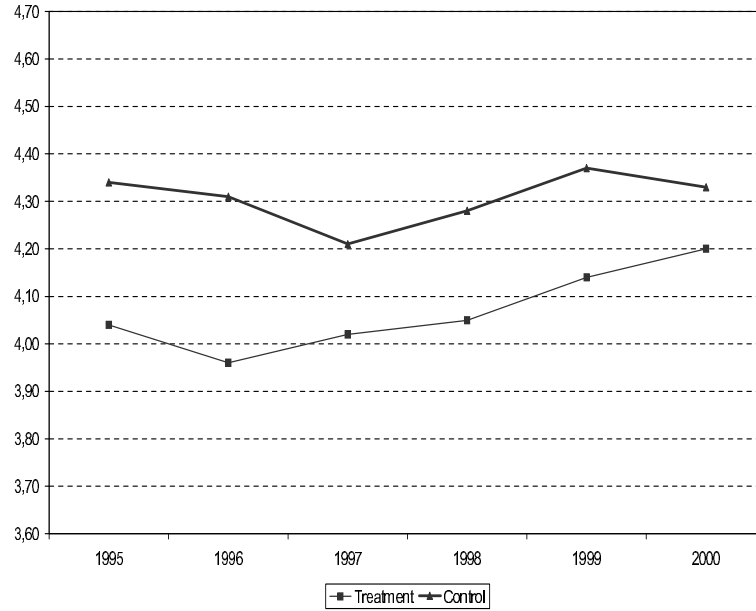
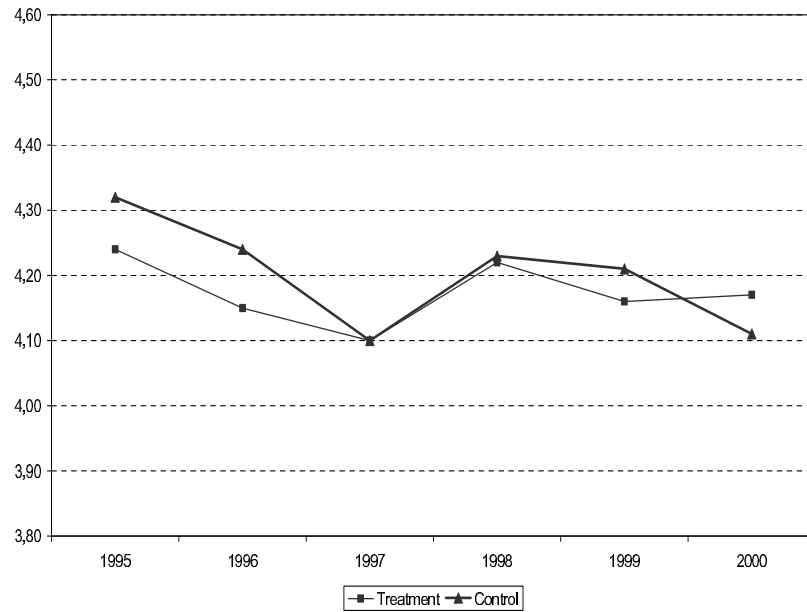


Fig. 7: Average level of environment satisfaction for the "below 30" treatment group versus the "below 30" control group (yearly means)



Note: ECHP dataset.

Also in these cases, there are no clear changes in variables trajectories after the 1997 reform and the two groups follow the same pattern and have almost the same level in respect to both working time and working conditions satisfaction.

At the end, the reform's impact estimated on the perceived job security of the workers seems to capture the real effect and not a subjective bias, given that the other job satisfaction variables are not affected by the reform.

5.2 The fixed effect analysis of the control groups

The application of the fixed effect estimation technique to control groups allows to test the correctness of the group construction and the possible heterogeneity between treated and untreated. In case of correct set up of our analysis, the coefficient γ - i.e. the coefficient related to the treatment/control dummy - should be 0 and there should not be any treatment effect, i.e. β not significantly different from 0.

More specifically, I estimate the equation (1.5) twice. In the first case, D should be equal to 1 if individuals are permanent workers with less than 30 years of age (i.e. first control group related to first treatment group) and 0 if individuals are permanent workers with more than 45 years of age (i.e. first control group related to second treatment group). In the second case, D should be equal to 1 if individuals are temporary workers with age between 30 and 36 (i.e. second control group related to first treatment) and 0 if individuals are temporary workers with age between 38 and 44 years (i.e. second control group related to second treatment). In both cases, the coefficients γ and β are significantly different from 0³³.

5.3 The propensity score matching DID

The robustness of the results obtained with the fixed effect estimator is checked by performing the same analysis using a propensity score matching DID estimator. First, I balance the panel in order to have the same individuals each year. Secondly, in each year we match treatment and control groups on the basis of the propensity score³⁴, using kernel matching technique. Finally, we evaluate the differences-in-differences for couples of year (i.e. one year after the reform versus one year before the reform). This procedure has been done for each treatment-control comparison. The structure of the analysis is as follow:

Tab. 1.8 shows the estimation results. The treatment effects estimated with the propensity score matching DID confirms the results obtained with the fixed effect analysis. There is a positive and significant effect for the temporary workers with less than 30 years of age in both comparisons with

³³For detailed results please contact the author.

³⁴For the variables used in the definition of the propensity score see footnote 23.

Tab. 3: Differences-in-Differences structures.

DID	Structure
DID_{98-95}	$ATT_{1998} - ATT_{1995}$
DID_{99-95}	$ATT_{1999} - ATT_{1995}$
DID_{00-95}	$ATT_{2000} - ATT_{1995}$
DID_{98-96}	$ATT_{1998} - ATT_{1996}$
DID_{99-96}	$ATT_{1999} - ATT_{1996}$
DID_{00-96}	$ATT_{2000} - ATT_{1996}$

the two control groups in almost all the cases. There are no effects for the temporary workers with more than 45 years of age.

6 Concluding remark

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 allow me to set up a research design to evaluate its impact on the perceived job security of some targeted groups of Spanish workers.

The introduction of these new restrictive permanent contracts could produces a double effects. On one side the probability to be hired with a permanent contract is higher - and this rises the sense of security of the workers - on the other it becomes easier to be fired. Estimates using ECHP data for Spain suggest that the reform increased the perceived job security for workers with less than 30 years of age and there were no effect for workers with more than 45 years of age.

The results are robust even using the blue-collar workers' sample and the propensity score matching DID technique.

In previous studies³⁵, 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 security and viceversa. My analysis, then, confirms that in Spain, the introduction of looser EPL did increase the job security and changed the distribution of the perceived job security from less to more satisfied position in the satisfaction ranking.

³⁵See, for example, Clark and Postel-Vinay (2005)

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A Linearization based on truncated normal distribution

Let's suppose that a random variable X is $N(0, 1)$ and let's consider a double truncated distribution $c_1 \leq X \leq c_2$. The mean of this truncated distribution is given by³⁶:

$$E(X) = \frac{\phi(c_1) - \phi(c_2)}{\Phi(c_2) - \Phi(c_1)} = M \quad (6)$$

In the analysis, X is the ordered variable describing the level of job security, c_1 and c_2 are respectively the level 1 and 6 of the satisfaction ranking. Following the previous procedure a new continuous variable (i.e. *security_pols*) is created simply setting $security_pols_i = E(security_sat | \mu_{i-1} < security_sat \leq \mu_1)$.

³⁶See Maddala (1986) for further details.

B ECHP for Spain

Tab. 4: Sample Composition before matching

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

Tab. 5: Composition of the sub-samples before and after the 1997's labor market reform (before matching)

	Below30		Age30_36		Age38_44		Above45	
	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
Cohabitant or married	29.1	32.9	79.5	72.3	85.7	83.6	85.9	83.9
Immigrant	2.6	1.7	2.0	1.6	1.4	1.5	1.4	1.3
N. Children>0	27.84	23.17	64.34	52.52	56.64	72.43	39.74	30.39
High education	25.2	31.7	34.2	37.7	33.6	36.2	24.2	27.7
Low education	48.7	41.6	43.1	39.6	45.8	42.8	64.5	57.5
Manager or professional	7.2	9.9	16.0	14.9	22.7	23.2	17.3	20.8
Blue-collar	68.3	69.3	57.4	60.0	50.4	42.8	62.1	58.2
Experience>0	83.16	77.34	83.46	89.81	47.55	65.24	34.41	40.23
Permanent contract	21.8	42.5	40.1	66.5	49.2	76.7	47.0	81.4
Public sector	14.8	12.0	29.3	23.3	35.7	32.5	30.2	30.7
Part time	8.9	8.3	6.4	6.0	5.3	5.5	4.6	5.0
Industry	33.9	36.6	29.5	32.6	30.6	30.3	36.6	34.6
Services	62.2	59.7	67.4	64.1	60.6	66.5	59.4	60.9
Firmsize 0 – 99	81.3	78.9	73.5	71.8	65.3	64.5	67.0	64.7
Experienced past unemployment	56.8	51.1	47.2	47.5	27.2	36.2	20.4	22.5
Long spell of past unemployment	17.8	14.3	18.3	14.3	12.0	13.9	8.3	8.9
Income of the household>0	91.35	92.01	74.22	78.26	69.33	73.00	77.63	79.36
N. Obs.	3,599	4,484	2,813	3,455	2,557	2,797	3,135	3,708

Notes: In table 1.4 and 1.5 the data are drawn from the ECHP for Spain.

C Estimation Results

Tab. 6: Estimation results fixed effect estimator: overall sample

	1st treat - 1st control	1st treat - 2nd control	2nd treat - 1st control	2nd treat - 2nd control
N.Obs.				
Treatment	422	260	154	139
Control	458	385	345	95
$\hat{\gamma}$	-0.526 *** (0.096)	0.396 ** (0.170)	-0.523 *** (0.150)	0.212 (0.284)
$\hat{\beta}$	0.221 ** (0.102)	0.256 ** (0.103)	-0.009 (0.147)	-0.105 (0.220)
R-squared	0.1646	0.1441	0.1767	0.0035

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

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Tab. 7: Estimation results fixed effect estimator: Blue-collar

	1st treat - 1st control	1st treat - 2nd control	2nd treat - 1st control	2nd treat - 2nd control
N.Obs.				
Treatment	339	312	147	132
Control	287	203	221	84
$\hat{\gamma}$	-0.563 *** (0.116)	0.199 (0.158)	-0.551 *** (0.169)	0.160 (0.293)
$\hat{\beta}$	0.259 ** (0.115)	0.190 * (0.109)	0.002 (0.166)	-0.144 (0.231)
R-squared	0.1593	0.0925	0.1809	0.0049

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

Tab. 8: Estimation results: Propensity score matching DID estimator

	Ist treat - 1st control	Ist treat - 2nd control	2nd treat - 1st control	2nd treat - 2nd control
<i>DID</i> ₉₈₋₉₅	0.301 (0.239)	0.445 (0.369)	0.199 (0.346)	-2.005 (1.019)**
N.Obs. treat.	165	164	57	6
N.Obs. control	189	116	163	60
<i>DID</i> ₉₉₋₉₅	0.432** (0.238)	0.145 (0.487)	0.349 (0.373)	-1.519* (0.913)
N.Obs. treat.	158	133	77	6
N.Obs. control	171	97	171	51
<i>DID</i> ₀₀₋₉₅	0.485** (0.278)	-	0.124 (0.283)	-1.211 (1.090)
N.Obs. treat.	154	-	84	18
N.Obs. control	137	-	182	49
<i>DID</i> ₉₈₋₉₆	0.384** (0.215)	0.416* (0.308)	0.448 (0.407)	-
N.Obs. treat.	168	151	45	-
N.Obs. control	190	108	160	-
<i>DID</i> ₉₉₋₉₆	0.515*** (0.207)	0.116 (0.388)	0.598* (0.432)	-
N.Obs. treat.	161	120	65	-
N.Obs. control	172	89	168	-
<i>DID</i> ₀₀₋₉₆	0.568*** (0.239)	-	0.373 (0.335)	-
N.Obs. treat.	157	-	72	-
N.Obs. control	138	-	179	-

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