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

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This paper investigates the impact of the introduction of new restrictive permanent contract on the perceived job security of the workers in Spain. The perceived job security is strongly influenced by the characteristics of individuals and their distribution within groups. Comparing heterogenous groups could make the traditional DID estimator biased. To address this issue with 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 are that this reform has a positive impact only for one targeted group, i.e. the young worker and no effect for the other. 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

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 demands 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, on the other, security notably for weak groups inside and outside the labor market (Wilthangen, 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 make 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 markets1

From the beginning of the 1980’s in Spain the high rate of unemployment pushed 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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1See 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", 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 event - like a change in the government policy - change the environment in which agents operate. This characteristic permits to conduct several kind of impact evaluations.

Consequently, the issue on evaluation of the impact of this kind of reforms on labor market has stimulated research and the nature of "natural experiment" of the Spanish reform represented an interesting base for several studies.

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 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 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 and how these levels are affected by the macro aspects of the labor market. Elements like the rate

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2We consider this reform 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.

3This 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).

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

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


7See, for example, Clark and Postel-Vinay (2005), Clark et al. (2001).
of unemployment, the 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 the behavior of the employees could, indirectly, affect the results of the reform. Moreover, the perceived job security has several potential 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 we combine the propensity score matching with a fixed effect estimator. The latter give us the possibility to exploit the panel characteristics of our data set and the propensity score matching technique, applied to our treated and untreated groups in 1995, permits us to solve the problem of heterogeneity of treated and control groups.

Given the subjective nature of the variable of interest and the risk to maintain some level of heterogeneity, we perform some robustness checks. First of all, considering that the subjectivity bias may have change reports of all job-related satisfaction components, we will check if there are some evidence of reform’s effect on other job satisfaction variables. Secondly, in order to test the robustness of our fixed effect estimation we will perform the analysis on the control groups. To conclude, to test in a different way the robustness of our findings, we will estimate the treatment effects using a propensity score matching DID estimator.

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 estimations results. Section 5 presents some robustness checks. Finally, section 6 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 employment rate, during the 1990’s, exceeding 20%. Accordingly, the employment creation has been one of the primary challenges 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 em-
ployment protection and the predominance of collective bargaining at the provincial/industrial level.

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, entailed 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 third of the Spanish labor force worked under temporary contracts and more than 90% of all new signed contracts were temporary. In Spain, at the moment, there was a pressing need for combining flexibility with 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" 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 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 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' federation (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 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 the 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.

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8 See Jimeno and Toharia (1993) for further details.
9 The percentage of temporary contracts in Spain was one of the highest in Europe. See Martin (2002) for further details.
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. 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 the labor market\(^\text{10}\) (Kugler et al., 2003).

The 1997 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 use of 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\(^\text{11}\); the reduction of dismissal costs during a period of two years for new permanent contracts\(^\text{12}\); 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 payments for unfair dismissals of newly signed contracts for the workers in the target groups were reduced from 45 to 33 days pay per year of seniority and the maximum was reduced form 42 to 24 months. The payroll taxes reduction\(^\text{13}\) was 40% for

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\(^{11}\) 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%.
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).

3 Data and Methodology

3.1 Data

Data are from the European Community Household Panel (ECHP) from 1995 to 2000\textsuperscript{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\textsuperscript{15}. A panel has been extracted from the ECHP for Spain including men and women between 16 and 65 years of age, employed\textsuperscript{16}. 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 questions regarding personal satisfaction of the ECHP questionnaire. The exact wording is as follow:

\begin{quote}
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"
\end{quote}

The typical formulation of this kind of questions contains a subjective element regarding the meaning of "satisfied" or even "job security" that could vary from one person to another (Clark and Postel-Vinay, 2005)\textsuperscript{17}. The expected impact of this reform might be ambiguous. First, the reduction of firing costs may increase the probability to access to permanent

\textsuperscript{14}We exclude the first (1994) and the last wave (2001). We exclude the first one because there are no workers hired with permanent contracts; the last one is excluded instead because, in this year, another reform was introduced. This reform modifies and extends the one occurred in 1997.

\textsuperscript{15}See Peracchi (2002) for further details.

\textsuperscript{16}The questions related to job satisfaction are asked only to employees. See Tab. 1 in the Appendix B for composition of the sample.

\textsuperscript{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.
contracts, affecting positively the level of job security. Secondly, the lay-off procedure 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 2 and Table 3 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, number of children, level of education, kind of contract (permanent and temporary), sector (public or private), experienced past unemployment: the so-called observable pre-treatment characteristics that require a proper control.

3.2 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 to the expected mean of a truncated normal distribution.

We have thus identified two different treatment groups: the first group is composed by individuals under 30 years of age and the second by individuals above 45 years of age. In each of the two treatment groups the individual are temporary workers and they are entitled, after the reform, to be hired with the new permanent contracts (i.e. permanent contracts with lower firing costs).

Four control groups are constructed. The first one is composed by individuals with less than 30 years of age, working with a "traditional" permanent contract; the second one is composed by temporary workers with an age between 30 and 36 years; the third is composed by permanent workers with more than 45 years of age; finally, the fourth 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, first, with permanent workers with less than 30 years

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18 See Kugler et al. (2003).
19 See Appendix B.
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). We exclude also the disabled workers because of a lack of observations.
of age and, secondarily, with temporary workers with an age 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 the control group composed by permanent worker with more than 45 years of age and with the group of temporary workers with an age between 38 and 44 years. The structure of the comparisons is summarized in the following table.

Tab. 1: Treatment and control groups comparisons.

<table>
<thead>
<tr>
<th>Treatment groups</th>
<th>Control groups</th>
</tr>
</thead>
<tbody>
<tr>
<td>Below 30 with temporary contracts</td>
<td>Below 30 with permanent contracts</td>
</tr>
<tr>
<td>Below 30 with temporary contracts</td>
<td>Age 30-36 with temporary contracts</td>
</tr>
<tr>
<td>Above 45 with temporary contracts</td>
<td>Above 45 with permanent contracts</td>
</tr>
<tr>
<td>Above 45 with temporary contracts</td>
<td>Age 38-44 with temporary contracts</td>
</tr>
</tbody>
</table>

It is important to notice that for each treatment group two control groups are chosen. In particular, one group represents a situation in which non treated individuals are in a better situation than the treated (e.g. when treated and no-treated have the same age, but different contracts), the other control group represents instead the case in which the untreated individuals are in a worse situation than the treated (e.g. when treated and untreated have the same temporary contract but different age).

Looking to figures 1 and 2, we could notice that the treatment group composed by temporary workers with less than 30 years of age shows a change in the level of perceived job security starting in 1997. After the reform, in fact, this group presents an increasing level of job security. The first control group (i.e. permanent workers with less than 30 years of age), instead, presents a more stable pattern along all the period observed. The second control group, finally, has a less stable pattern in the period of analysis, without showing a precise trend.

Considering the second treatment group (see figure 3 and 4) there are no evidence of same effect on the level of perceived job security induced 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 the one of the first control, but it doesn’t evidence a change in the trajectories.
Fig. 1: Average level of job security for the "below 30" treatment group versus the "below 30" control group (yearly means)

Note: Data drawn from ECHP data.

Fig. 2: Average level of job security for the "below 30" treatment group versus the "age 30-36" control group (yearly means)

Note: Data drawn from ECHP data.
Fig. 3: Average level of job security for the "Above 45" treatment group versus the "Above 45" control group (yearly means)

Note: Data drawn from ECHP data.

Fig. 4: Average level of job security for the "Above 45" treatment group versus the "age 38-44" control group (yearly means)

Note: Data drawn from ECHP data.
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\footnote{Sex, age, age squared, level of education (high, medium and low), marital status, immigrant status, number of children.} and another set related to the job conditions of the subjects and the income\footnote{Professional level (manager or professional, intermediate, blue-collar), sector (private or public), economic sector, working hours (full time or part time), size of the firm, experience in the labor market, experience of past unemployment, length of the unemployment spell, income of the household.}.

### 3.3 Estimation strategy

#### 3.3.1 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).

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 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:

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

where $\hat{\gamma}_3$ will be the Differences-in-Differences estimator:

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

and it measures the effect of the treatment.

The DID estimator is based on a crucial and critical assumption. The error term has to be uncorrelated with the other variables (e.g. $\text{cov}(\varepsilon_i, D_i, t_i) = 0$). 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 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. (1998), this bias can be split in three parts: a first component due to the non overlapping support (i.e., 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.\(^{24}\)

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, in fact, might create non parallel dynamics for the treated and the untreated and the evaluation problem could not be addressed with the traditional DID. Looking at figures 1 to 4, we could notice that in the pre-reform period the treatment and the control groups don’t seem to follow parallel paths in all the comparison considered. These empirical evidence, then, support the fact the basic assumption of the DID estimator is not valid.

3.3.2 The propensity score matching and the fixed effect estimator

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

Given these considerations, the empirical strategy used is composed by three steps. First, we balance the panel in order to have the same individuals in all the years considered. In this way, we address the problem related to the heterogeneity over time, due to the possibility that in different years the treatment and the control groups could be composed by individual totally different. Secondly, to control for the heterogeneity between groups, we match the treatment and the control groups on the basis of a set of pre-treatment observable characteristics, using a propensity score matching method. The propensity score is defined by Rosembaum and Rubin (1983) as the conditional probability of receiving a treatment given the pre-treatment characteristics:

\[
P(X) \equiv Pr\{D = 1|X\} = E\{D|X\}
\]

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

\(^{24}\)The latter is the selection bias and it is due to the selection of the unobservables. This kind of bias can 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 (Heckmann et al., 1997).
In the literature, there are several methods to match treatment and control groups on the basis of the propensity score\textsuperscript{25}. In our analysis, we use the \textit{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 control (Becker and Ichino, 2002). In particular, the kernel weight function is:

\[
W(i, j) = \frac{G\left( \frac{P_i - P_j}{\alpha_n} \right)}{\sum_{k \in I_0} G\left( \frac{P_k - P_i}{\alpha_n} \right)}
\]

where \( G(.) \) is the kernel function and \( \alpha_n \) is a bandwidth parameter.

Formally, two hypothesis are needed in the matching process in order to derive, successively, the treatment effect. The balancing property of treatment variables given the propensity score must hold\textsuperscript{26}, as well as, the unconfoundedness given the propensity score\textsuperscript{27} (Becker and Ichino, 2002).

As said before, the use of the propensity score address properly the issue of common support and miss-weighting\textsuperscript{28}. 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 a control unit having the closest propensity score.

In the last step of the analysis, we estimate the treatment effect of the reform on the perceived job security of the worker, using a fixed effect estimator on the matched and balanced sample.

In practice, we estimate the following equation:

\[
Y_i = \alpha_i + \delta_t + \gamma D_{i,t} + \beta D_{i,t} Post_{1997}
\]

where \( \alpha_i \) indicates the individual effect, \( \delta_i \) indicates the time effect, \( D_{i,t} \) is a dummy variable that assume value 1 when the individual is treated and 0 otherwise, \( Post_{1997} \) is a dummy variable that assumes value 1 if we observe the post reform period and 0 otherwise. \( \beta \) is the parameter of interest that measures the effect of the reform. The key assumption of this approach is that the unobservables \( \alpha_i \) (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 assumption than those needed to establish

\textsuperscript{25}The most common methods are: Nearest Neighbor Method, Radius Matching, Kernel Matching and Stratification Matching.

\textsuperscript{26}If \( P(X) \) is the propensity score, then \( D \perp X | p(X) \).

\textsuperscript{27}Suppose 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. \( Y_1, Y_0 \perp D | p(X) \).

\textsuperscript{28}See Heckman et al. (1997).
causation with cross-section data or with panel data models without fixed effects, such as pooled models and 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 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 a higher level of insecurity among the workers. In practice, 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 fixed effect estimator after having matched treatment and control groups on the basis of the propensity score technique.

The aim of combining these two techniques is to solve the problem of heterogeneity of the treatment and the control groups and to use the panel characteristics of our sample as well. In fact, the variable of interest is based on the subjective perception of the workers about the possibility to lose their job and, the variables like age, gender, kind of contract, past experiences of unemployment could affect significantly the perceived job security (see, for example, Clark (1997, 1998), Clark et al. (1996, 2001, 2005)).

For this reason, we first balance the panel, then we obtain the propensity score\textsuperscript{29} and we identify, for each participant, all non participants who match on the propensity score\textsuperscript{30}, using kernel weights. Finally, on these matched samples we estimate the treatment effects of the reform using a fixed effect estimator. A separate analysis for blue-collar workers has also been performed.

Tab. 4 reports the estimation results.

The reform have a positive and significant effect for the temporary workers with less than 30 years of age, in both the comparisons performed. This results is coherent with some 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

\textsuperscript{29}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 individuals. See footnotes 22 and 23.

\textsuperscript{30}See Leuven and Sianesi (2003).
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 security. On the other hand, our results seems coherent also with the conclusions of Kugler et al. (2003), who investigates 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 of the workers with more than 45 years old. This could be explained by the fact that for this group together with an higher probability to move from temporary to permanent position, there is an higher probability of transition from permanent employment to non employment as well\(^{31}\).

These results are confirmed also in 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 the comparisons. Table 5 shows the estimation results.

5 Sensitivity Analysis

5.1 The outcome variable

The outcome variable of our analysis - the perceived job security of the workers - is a subjective variable and this characteristic of the independent variable could generate some doubts about the estimation results obtained. In other words, it could be that the effect captured by our estimator could be affected by a 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 dimension 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.

For our purpose, we plot the yearly average level of different satisfaction variable for the first treatment group - i.e. temporary workers with less than 30 years of age - and the first control group, composed by permanent workers with less than 30 years of age\(^{32}\).

First, in figure 5, we look at the overall job satisfaction variable that come from the following question:

\[ \textit{How much satisfied are you with your work or main activity?} \]

\(^{31}\)See Kugler et al. (2003).

\(^{32}\)In the paper, we propose 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 ask to the author.
The replies are ranked from 1 to 6, where 1 means not satisfied and 6 fully satisfied.

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

Note: Data drawn from ECHP data.

We could notice that 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, started one year after with respect to the treatment groups. 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. Another satisfaction variable analyzed is the one concerning the distance from the workplace. This variable is interesting for our check because it’s totally unrelated with reform. The question is:

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

As we can see in figure 6, there are no changes in the level of distance satisfaction after the 1997 reform both for treatment and control groups and, moreover, they both follow the same pattern.
Fig. 6: Average level of distance satisfaction for the "below 30" treatment group versus the "below 30" control group (yearly means)

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

Note: Data drawn from ECHP data.
In figure 7 and 8, we could observe the yearly average level of satisfaction related to working times and working/environment conditions respectively. The two questions are as follows:

*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?*

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

Note: Data drawn from ECHP data.

We could conclude that the reform’s effect 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

A way to check if we choose properly the untreated groups and if we control properly for the heterogeneity - i.e. check for the selection bias - is to apply the fixed effect estimation technique to the control groups. In case of correct set up of our analysis, the coefficient $\gamma$ - i.e. the coefficient related to the treatment/control dummy - should be 0 and there should not be any treatment effect, i.e. $\beta$ not significantly different from 0.

More specifically, we estimate the equation (5) two times. In the first case, D will 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 will 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 $\gamma$ and $\beta$ are not significantly different from 0.

5.3 The propensity score matching DID

To check, in another way, for the robustness of the results obtained with the fixed effect estimator, we perform the same kind of analysis using a propensity score matching DID estimator. First, we 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 score, using kernel matching technique. Finally, we evaluate the differences-in-differences for different combination of year before and after the reform. This procedure will be done for each treatment-control comparison. More specifically, the structure of the analysis is as follows:

Tab. 1bis: Differences-in-Differences structures.

<table>
<thead>
<tr>
<th>DID</th>
<th>Structure</th>
</tr>
</thead>
<tbody>
<tr>
<td>DID98−95</td>
<td>ATT1998 − ATT1995</td>
</tr>
<tr>
<td>DID99−95</td>
<td>ATT1999 − ATT1995</td>
</tr>
<tr>
<td>DID00−95</td>
<td>ATT2000 − ATT1995</td>
</tr>
<tr>
<td>DID98−96</td>
<td>ATT1998 − ATT1996</td>
</tr>
<tr>
<td>DID99−96</td>
<td>ATT1999 − ATT1996</td>
</tr>
<tr>
<td>DID00−96</td>
<td>ATT2000 − ATT1996</td>
</tr>
</tbody>
</table>

Table 6 shows the estimation results. The treatment effects estimated with the propensity score matching DID confirm the results obtained with the

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33 For detailed results, please contact the author.
34 For the variables used in the definition of the propensity score see footnotes 22 and 23.
fixed effect analysis. There is a positive and significant effect for the temporary workers with less than 30 years of age in both the comparisons with 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 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 allow 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 fixed effect estimation technique. The introduction of these new restrictive permanent contracts could produces a double effects. One side the probability to be hired with a permanent contract is higher, on the other it becomes easier to be fired. Estimates using ECHP data for Spain suggest that the reform increases the perceived job security for workers with less than 30 years of age and there is no effect for workers with more than 45 years of age. The result are robust even using the blue-collar workers’ sample and the propensity score matching DID technique.

In previous studies\textsuperscript{35} 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, 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.

\textsuperscript{35}See, for example, Clark and Postel-Vinay (2005).
References


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:\footnote{See Maddala (1986) for further details.}

$$
E(X) = \frac{\phi(c_1)}{1 - \Phi(c_1)} = \text{ordinate at } X = c_1 = M_1
$$

(6)

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

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

(7)

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

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

(8)

In our case, $X$ 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. We follow the procedure described above and we create a new continuous variable (i.e. $\text{security\_pols}$) simply setting $\text{security\_pols}_i = E(\text{security\_sat}|\mu_{i-1} < \text{security\_sat} \leq \mu_1)$. 

\footnote{See Maddala (1986) for further details.}
## B ECHP for Spain

Tab. 2: Sample Composition before matching

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
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<td>N. Obs.</td>
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<td>4,255</td>
<td>4,194</td>
<td>4,135</td>
<td>4,141</td>
<td>4,030</td>
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<td>Age &lt; 30</td>
<td>1,279</td>
<td>1,221</td>
<td>1,236</td>
<td>1,217</td>
<td>1,201</td>
<td>1,127</td>
</tr>
<tr>
<td>Age 30 – 45</td>
<td>1,968</td>
<td>1,922</td>
<td>1,824</td>
<td>1,810</td>
<td>1,819</td>
<td>1,829</td>
</tr>
<tr>
<td>Age &gt; 45</td>
<td>1,153</td>
<td>1,112</td>
<td>1,134</td>
<td>1,108</td>
<td>1,121</td>
<td>1,074</td>
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<td>Disabled worker</td>
<td>427</td>
<td>478</td>
<td>434</td>
<td>438</td>
<td>394</td>
<td>378</td>
</tr>
<tr>
<td>Female</td>
<td>1,546</td>
<td>1,496</td>
<td>1,470</td>
<td>1,526</td>
<td>1,534</td>
<td>1,519</td>
</tr>
<tr>
<td>Cohabitant or married</td>
<td>2,923</td>
<td>2,839</td>
<td>2,814</td>
<td>2,741</td>
<td>2,700</td>
<td>2,656</td>
</tr>
<tr>
<td>Immigrant</td>
<td>80</td>
<td>76</td>
<td>63</td>
<td>64</td>
<td>54</td>
<td>63</td>
</tr>
<tr>
<td>N. Children &gt; 0</td>
<td>2,205</td>
<td>1,987</td>
<td>1,827</td>
<td>1,772</td>
<td>1,703</td>
<td>1,607</td>
</tr>
<tr>
<td>High education</td>
<td>1,213</td>
<td>1,242</td>
<td>1,159</td>
<td>1,285</td>
<td>1,418</td>
<td>1,425</td>
</tr>
<tr>
<td>Low education</td>
<td>2,281</td>
<td>2,132</td>
<td>2,125</td>
<td>1,933</td>
<td>1,865</td>
<td>1,793</td>
</tr>
<tr>
<td>Manager or professional</td>
<td>659</td>
<td>662</td>
<td>674</td>
<td>629</td>
<td>684</td>
<td>657</td>
</tr>
<tr>
<td>Blue-collar</td>
<td>2,641</td>
<td>2,501</td>
<td>2,581</td>
<td>2,554</td>
<td>2,524</td>
<td>2,452</td>
</tr>
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<td>Experience &gt; 0</td>
<td>2,756</td>
<td>2,707</td>
<td>2,716</td>
<td>2,700</td>
<td>2,888</td>
<td>2,905</td>
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<td>Permanent contract</td>
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<td>2,677</td>
<td>2,651</td>
<td>2,644</td>
<td>2,754</td>
<td>2,717</td>
</tr>
<tr>
<td>Public sector</td>
<td>1,149</td>
<td>1,132</td>
<td>1,032</td>
<td>964</td>
<td>984</td>
<td>917</td>
</tr>
<tr>
<td>Part time</td>
<td>209</td>
<td>262</td>
<td>271</td>
<td>278</td>
<td>272</td>
<td>240</td>
</tr>
<tr>
<td>Industry</td>
<td>1,404</td>
<td>1,350</td>
<td>1,440</td>
<td>1,387</td>
<td>1,389</td>
<td>1,369</td>
</tr>
<tr>
<td>Services</td>
<td>2,762</td>
<td>2,633</td>
<td>2,577</td>
<td>2,578</td>
<td>2,605</td>
<td>2,506</td>
</tr>
<tr>
<td>Firm size 0 – 99</td>
<td>2,475</td>
<td>2,376</td>
<td>2,915</td>
<td>2,922</td>
<td>2,917</td>
<td>2,870</td>
</tr>
<tr>
<td>Experienced past unemployment</td>
<td>1,700</td>
<td>1,689</td>
<td>1,752</td>
<td>1,682</td>
<td>1,648</td>
<td>1,516</td>
</tr>
<tr>
<td>Long spell of past unemployment</td>
<td>652</td>
<td>623</td>
<td>647</td>
<td>555</td>
<td>507</td>
<td>434</td>
</tr>
<tr>
<td></td>
<td>Before</td>
<td>After</td>
<td>Before</td>
<td>After</td>
<td>Before</td>
<td>After</td>
</tr>
<tr>
<td>-------------------------</td>
<td>---------</td>
<td>--------</td>
<td>---------</td>
<td>--------</td>
<td>---------</td>
<td>--------</td>
</tr>
<tr>
<td>Female</td>
<td>39.8</td>
<td>42.7</td>
<td>36.1</td>
<td>37.3</td>
<td>35.3</td>
<td>35.7</td>
</tr>
<tr>
<td>Cohabitant or married</td>
<td>29.1</td>
<td>32.9</td>
<td>79.5</td>
<td>72.3</td>
<td>85.7</td>
<td>83.6</td>
</tr>
<tr>
<td>Immigrant</td>
<td>2.6</td>
<td>1.7</td>
<td>2.0</td>
<td>1.6</td>
<td>1.4</td>
<td>1.5</td>
</tr>
<tr>
<td>N. Children&gt;0</td>
<td>27.84</td>
<td>23.17</td>
<td>64.34</td>
<td>52.52</td>
<td>56.64</td>
<td>72.43</td>
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<tr>
<td>High education</td>
<td>25.2</td>
<td>31.7</td>
<td>34.2</td>
<td>37.7</td>
<td>33.6</td>
<td>36.2</td>
</tr>
<tr>
<td>Low education</td>
<td>48.7</td>
<td>41.6</td>
<td>43.1</td>
<td>39.6</td>
<td>45.8</td>
<td>42.8</td>
</tr>
<tr>
<td>Manager or professional</td>
<td>7.2</td>
<td>9.9</td>
<td>16.0</td>
<td>14.9</td>
<td>22.7</td>
<td>23.2</td>
</tr>
<tr>
<td>Blue-collar</td>
<td>68.3</td>
<td>69.3</td>
<td>57.4</td>
<td>60.0</td>
<td>50.4</td>
<td>42.8</td>
</tr>
<tr>
<td>Experience&gt;0</td>
<td>83.16</td>
<td>77.34</td>
<td>83.46</td>
<td>89.81</td>
<td>47.55</td>
<td>65.24</td>
</tr>
<tr>
<td>Permanent contract</td>
<td>21.8</td>
<td>42.5</td>
<td>40.1</td>
<td>66.5</td>
<td>49.2</td>
<td>76.7</td>
</tr>
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<td>Public sector</td>
<td>14.8</td>
<td>12.0</td>
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<td>23.3</td>
<td>35.7</td>
<td>32.5</td>
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<td>Part time</td>
<td>8.9</td>
<td>8.3</td>
<td>6.4</td>
<td>6.0</td>
<td>5.3</td>
<td>5.5</td>
</tr>
<tr>
<td>Industry</td>
<td>33.9</td>
<td>36.6</td>
<td>29.5</td>
<td>32.6</td>
<td>30.6</td>
<td>30.3</td>
</tr>
<tr>
<td>Services</td>
<td>62.2</td>
<td>59.7</td>
<td>67.4</td>
<td>64.1</td>
<td>60.6</td>
<td>66.5</td>
</tr>
<tr>
<td>Firm size 0 – 99</td>
<td>81.3</td>
<td>78.9</td>
<td>73.5</td>
<td>71.8</td>
<td>65.3</td>
<td>64.5</td>
</tr>
<tr>
<td>Experienced past unemp</td>
<td>56.8</td>
<td>51.1</td>
<td>47.2</td>
<td>47.5</td>
<td>27.2</td>
<td>36.2</td>
</tr>
<tr>
<td>Long spell of unemp</td>
<td>17.8</td>
<td>14.3</td>
<td>18.3</td>
<td>14.3</td>
<td>12.0</td>
<td>13.9</td>
</tr>
<tr>
<td>Income of the household</td>
<td>91.35</td>
<td>92.01</td>
<td>74.22</td>
<td>78.26</td>
<td>69.33</td>
<td>73.00</td>
</tr>
<tr>
<td>N. Obs.</td>
<td>3,599</td>
<td>4,484</td>
<td>2,813</td>
<td>3,455</td>
<td>2,557</td>
<td>2,797</td>
</tr>
</tbody>
</table>

Notes: In table 2 and 3 the data are drawn from the ECHP for Spain.
C Estimation Results

Tab. 4: Estimation results fixed effect estimator: overall sample

<table>
<thead>
<tr>
<th>N.Obs.</th>
<th>1st treatment-1st control</th>
<th>1st treatment-2nd control</th>
<th>2nd treatment-1st control</th>
<th>2nd treatment-2nd control</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment</td>
<td>422</td>
<td>260</td>
<td>154</td>
<td>139</td>
</tr>
<tr>
<td>Control</td>
<td>458</td>
<td>385</td>
<td>345</td>
<td>95</td>
</tr>
<tr>
<td>( \hat{\gamma} )</td>
<td>-0.526*</td>
<td>0.396**</td>
<td>-0.523*</td>
<td>0.212</td>
</tr>
<tr>
<td>( (0.096) )</td>
<td>( (0.170) )</td>
<td>( (0.150) )</td>
<td>( (0.284) )</td>
<td></td>
</tr>
<tr>
<td>( \hat{\delta} )</td>
<td>0.027</td>
<td>0.021</td>
<td>0.045**</td>
<td>0.073</td>
</tr>
<tr>
<td>( (0.022) )</td>
<td>( (0.030) )</td>
<td>( (0.021) )</td>
<td>( (0.056) )</td>
<td></td>
</tr>
<tr>
<td>( \hat{\beta} )</td>
<td>0.221**</td>
<td>0.256**</td>
<td>-0.009</td>
<td>-0.105</td>
</tr>
<tr>
<td>( (0.102) )</td>
<td>( (0.103) )</td>
<td>( (0.147) )</td>
<td>( (0.220) )</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>0.1646</td>
<td>0.1441</td>
<td>0.1767</td>
<td>0.0035</td>
</tr>
</tbody>
</table>

Notes: The standard error is indicated in parenthesis. * corresponds to 1%, ** to 5% and *** to 10% level of significance.
Tab. 5: Estimation results fixed effect estimator: Blue-collar

<table>
<thead>
<tr>
<th></th>
<th>1st treatment-1st control</th>
<th>1st treatment-2nd control</th>
<th>2nd treatment-1st control</th>
<th>2nd treatment-2nd control</th>
</tr>
</thead>
<tbody>
<tr>
<td>N.Obs. Treatment</td>
<td>339</td>
<td>312</td>
<td>147</td>
<td>132</td>
</tr>
<tr>
<td>N.Obs. Control</td>
<td>287</td>
<td>203</td>
<td>221</td>
<td>84</td>
</tr>
<tr>
<td>( \hat{\gamma} )</td>
<td>* -0.563</td>
<td>0.199</td>
<td>* -0.551</td>
<td>0.160</td>
</tr>
<tr>
<td></td>
<td>(0.116)</td>
<td>(0.158)</td>
<td>(0.169)</td>
<td>(0.293)</td>
</tr>
<tr>
<td>( \hat{\delta} )</td>
<td>0.002</td>
<td>0.001</td>
<td>0.051</td>
<td>0.103</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.033)</td>
<td>(0.028)</td>
<td>(0.060)</td>
</tr>
<tr>
<td>( \hat{\beta} )</td>
<td>0.259**</td>
<td>0.190***</td>
<td>0.002</td>
<td>-0.144</td>
</tr>
<tr>
<td></td>
<td>(0.115)</td>
<td>(0.109)</td>
<td>(0.166)</td>
<td>(0.231)</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.1593</td>
<td>0.0925</td>
<td>0.1809</td>
<td>0.0049</td>
</tr>
</tbody>
</table>

Notes: The standard error is indicated in parenthesis. * corresponds to 1%, ** to 5% and *** to 10% level of significance.
<table>
<thead>
<tr>
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<th>1st treat-1st control</th>
<th>1st treat-2nd control</th>
<th>2nd treat-1st control</th>
<th>2nd treat-2nd control</th>
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</thead>
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<td><strong>DID</strong></td>
<td>0.222</td>
<td>0.436***</td>
<td>0.199</td>
<td>0.041</td>
</tr>
<tr>
<td></td>
<td>(0.230)</td>
<td>(0.339)</td>
<td>(0.328)</td>
<td>(1.123)</td>
</tr>
<tr>
<td>N.Obs treat.</td>
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<td>281</td>
<td>57</td>
<td>60</td>
</tr>
<tr>
<td>N.Obs control</td>
<td>190</td>
<td>100</td>
<td>163</td>
<td>8</td>
</tr>
<tr>
<td><strong>DID</strong></td>
<td>0.383***</td>
<td>0.136</td>
<td>0.349</td>
<td>0.527</td>
</tr>
<tr>
<td></td>
<td>(0.223)</td>
<td>(0.470)</td>
<td>(0.400)</td>
<td>(0.977)</td>
</tr>
<tr>
<td>N.Obs. treat.</td>
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<td>234</td>
<td>77</td>
<td>51</td>
</tr>
<tr>
<td>N.Obs. control</td>
<td>172</td>
<td>97</td>
<td>171</td>
<td>6</td>
</tr>
<tr>
<td><strong>DID</strong></td>
<td>0.435***</td>
<td>–</td>
<td>–0.124</td>
<td>0.835</td>
</tr>
<tr>
<td></td>
<td>(0.244)</td>
<td>–</td>
<td>(0.279)</td>
<td>(0.988)</td>
</tr>
<tr>
<td>N.Obs. treat.</td>
<td>254</td>
<td>–</td>
<td>84</td>
<td>49</td>
</tr>
<tr>
<td>N.Obs. control</td>
<td>137</td>
<td>–</td>
<td>182</td>
<td>18</td>
</tr>
<tr>
<td><strong>DID</strong></td>
<td>0.359**</td>
<td>0.412***</td>
<td>0.448</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.232)</td>
<td>(0.327)</td>
<td>0.394</td>
<td>–</td>
</tr>
<tr>
<td>N.Obs. treat.</td>
<td>176</td>
<td>268</td>
<td>45</td>
<td>–</td>
</tr>
<tr>
<td>N.Obs. control</td>
<td>191</td>
<td>92</td>
<td>130</td>
<td>–</td>
</tr>
<tr>
<td><strong>DID</strong></td>
<td>0.520**</td>
<td>0.112</td>
<td>0.598***</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.224)</td>
<td>(0.451)</td>
<td>(0.466)</td>
<td>–</td>
</tr>
<tr>
<td>N.Obs. treat.</td>
<td>162</td>
<td>121</td>
<td>65</td>
<td>–</td>
</tr>
<tr>
<td>N.Obs. control</td>
<td>173</td>
<td>89</td>
<td>138</td>
<td>–</td>
</tr>
<tr>
<td><strong>DID</strong></td>
<td>0.572**</td>
<td>–</td>
<td>0.373</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.246)</td>
<td>–</td>
<td>(0.338)</td>
<td>–</td>
</tr>
<tr>
<td>N.Obs. treat.</td>
<td>258</td>
<td>–</td>
<td>72</td>
<td>–</td>
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<tr>
<td>N.Obs. control</td>
<td>164</td>
<td>–</td>
<td>149</td>
<td>–</td>
</tr>
</tbody>
</table>

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