THE PORTUGUESE ACTIVE LABOUR MARKET POLICY

A COMPREHENSIVE CONDITIONAL DIFFERENCE-IN-DIFFERENCES APPLICATION

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ABSTRACT: In most studies in the literature only the participation in a single programme versus non-participation is evaluated. This approach, however, does not address the needs of a comprehensive evaluation of an active public intervention in the labour market. Active labour market programmes, like the Portuguese, are not restricted to a particular measure. Rather, in most cases, the public employment service offers a wide variety of programmes to the universe of potential participants. In this context, the issue is participation in one programme versus participation in an alternative programme. In particular, it is appropriate to investigate which programme presents a higher causal effect. Imbens (2000) and Lechner (2001) extended the traditional matching methodology to a context of multiple programme participation. The approach followed in this paper intends to go even further. Indeed, since the traditional matching methodology, which considers the conditional independence assumption, is not appropriate in the context of the Portuguese labour market analysis, we will adopt the assumption of the bias stability. Taking into consideration the selection on unobservables, the matching methodology, combined with the difference-in-differences methodology, will be then our selected evaluation approach. The paper presents the estimation of the average treatment effects on the treated in six distinct states (the non-participation state, plus five "active" programmes). The results, in terms of employability, are not identical across the different states in the short-run (i.e. in the first six to twelve months after participation), but they do seem to converge in the long-run (i.e. after two and a half years).

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1. INTRODUCTION

The focus of the evaluation literature of Active Labour Market Policies (ALMPs), which is vast and increasingly sophisticated, is typically the evaluation of a single programme. However, the work of Imbens (2000) and Lechner (2001), who extended the matching methodology for a single treatment – under the Conditional Independence Assumption (CIA) – to the case of multiple treatments, stimulated advances in the econometric literature. The evaluation literature of labour market programmes is being extended to the evaluation of multiple programmes. That is, to programmes that are running simultaneous is a particular labour market.

The Portuguese labour market is an example of an institutional framework in which several ongoing active labour market programmes are available for the unemployed who are registered in the public employment service. An evaluation exercise that does not take into account the possibility of multiple treatments may not be sufficient to fully assess the impact of active labour market programmes. A comprehensive microeconometric evaluation of the Portuguese ALMPs seems therefore worthwhile. On top of that we do not know any work which addresses a comprehensive evaluation of active labour market policy in a multiple treatment context and even in the international literature empirical studies are not very common.

Our study follows recent empirical applications of the matching estimator to a multiple treatment context originally proposed by Imbens (2000) and Lechner (2001). A particularly interesting piece is the work done by Gerfin and Lechner (2002). The authors have evaluated the impact of active labour market policy in Switzerland, using an administrative dataset similar to ours. Other contributions can also be referred to. Brodaty et al. (2001) evaluated, for the period 1986-1988 and using administrative data, the effects of youth employment programmes that were set up in France to improve the labour market prospects of disadvantaged, unskilled young workers. Larsson (2003) evaluated, jointly, the effects on the employment of two Swedish active programmes. Finally, Dorsett (2001) evaluated the relative effectiveness of the New Deal's option in reducing the male youth unemployment in the United Kingdom.

These four references have one thing in common: they all use administrative data as we do. Our study uses the administrative records of *Instituto de Emprego e Formação Profissional* (IEFP) to assess the effectiveness of the Portuguese Active Labour market Policy to the improvement of the employability of participants. The raw dataset contains the individual records collected by all local offices of IEFP. It includes a substantial number of individual labour market characteristics and, in particular, very detailed information on participation in ALMPs over a period of six years (1998-2003).

Our empirical implementation also implements the propensity score matching methodology but, in contrast, we do not rely on the Conditional Independence Assumption. Since we admit the existence of some selection on unobservables, our maintained hypothesis is the Bias Stability Assumption. This means therefore that we have extended the econometric multiple treatment evaluation framework to apply a nonparametric conditional difference-in-differences methodology. This approach combines propensity score matching techniques with the conventional difference-in-differences estimation, to construct the relevant counterfactual under the hypothesis of selection on observables and unobservables. The treated and comparison individuals are followed for a period of five semesters before and after 2001, our reference period.

The paper is organized as follows. In the next section, we describe the Portuguese institutional context for the active labour market policy and the programmes we will evaluate. Section 3 presents the microeconometric framework to a multiple treatment evaluation. The dataset and the modelling strategy are described in Section 4 followed by Section 5 where the empirical analysis of participation on one of the selected treatment states is discussed and Section 6 where the matching procedure is presented. Results from the selected econometric conditional difference-in-differences methodology are reported in Section 7.

2. - PORTUGUESE ACTIVE LABOUR MARKET POLICY

2.1 – GENERAL INSTITUTIONAL CONTEXT

The Portuguese ALMP framework uses a wide variety of programmes here aggregated into five major groups of intervention: 1) Direct Placement; 2) Job Counselling; 3) Employment Programmes; 4) Training Programmes; and 5) Professional Rehabilitation Programmes (designed, specifically, for the disabled)[1]. These programmes, in most cases, run continuously over time. They are also potentially available for any registered unemployed. Moreover, the individuals can participate repeatedly (and the data show they actually do it) over their observed unemployment spell.

This institutional framework does not fit into a pure (experimental) evaluation process, according to which a programme is administered at a fixed point in time with participants and non-participants randomly selected. But it does not represent any national idiosyncrasy. It is a typical institutional framework that can be found in any European country (Sianesi, 2004) where one can find a range of ongoing programmes and any unemployed individual can potentially become a participant.

2.2 – ALTERNATIVE TREATMENT STATES

The group of Employment Programmes (group 3 above), can be divided in: a) Training/Employment Programmes; b) Private Employment Incentives (for those who want to create their own employment); and c) the programmes involving the so-called Social Employment Market, which includes, as a key group, Public Employment Programmes. The sub-group Training/Employment Programmes contains two main divisions: (i) vocational training; and (ii) professional training programmes (or basic training, in the international literature). In our

[1] Some of these major groups of intervention (e.g. Employment Programmes or Training Programmes) can in turn be sub-divided into sub-programmes comprised of an ample set of heterogeneous programmes.

analysis we will not consider the Private Employment Incentives, given their specificity. The reduced number of participants creates serious problems of comparability. Programmes of vocational training will also be ignored since they present particular characteristics: programme duration can be much longer (up to three years) and the goals are clearly distinct.

We will consider six different states of participation (including non-participation), which we will call treatment states: 1) No participation (NP); 2) Direct Placement (DP); 3) Job counselling (JC); 4) Training/Employment (TE); 5) Public Employment Programmes (PEP); and 6) Basic training (BT). We note that PEP is selected on the basis that it covers almost 100% of individuals in group c) above.

The NP treatment state will be defined as the treatment state where no participation is observed in any of the programmes offered by the public employment service. They are of course registered unemployed individuals.

The DP treatment state is considered, in this particular analysis, as a treatment state. It is one of the biggest groups identified by the Portuguese public employment service and, although it does not fit the traditional definition of an active labour market programme, individuals in this group benefit from the effort of the public employment service. It eases the match between supply and demand of labour. As a matter of fact, in the Portuguese institutional context even the non-participants are, in some way, "treated", because they do take advantage of services provided by the public employment service (e.g. counselling, guidance and direct job placement). To participate in the DP treatment state only requires the register in the public employment service as the participation in the JC treatment state.

The JC treatment state is allocated to individuals that have benefited from technical services offered by the public employment service. These technical services are designed to promote the acquisition of effective individual's ability to find labour market opportunities, to present an appealing CV or to conduct a job interview.

TE programmes are characterized by the offer of some type of training to a registered unemployed (looking for a first employment or with some job experience). These programmes involve a real labour market experience. The ultimate goal of the TE treatment state is to increase the opportunities of labour market integration. The training programmes include *Estágios Profissionais* for individuals with the highest levels of formal education.

PEP programmes (or *Programas Ocupacionais*) are mainly targeted to unemployed individuals in families with a per capita monthly income lower than the national minimum wage and to unemployment beneficiaries. Participants in these programmes are required to perform non market-oriented activities (i.e. activities which do not directly compete with existing labour market vacancies). Participation is not intended to exceed a maximum of twelve months. Any job or vocational training offered by the public employment service prevails over participation in *Programas Ocupacionais*. A refusal ends immediately entitlement to unemployment benefits and other income support schemes. In addition to participation in PEP programmes, participants must be involved in monitored job searching.

The BT programmes contain a wide range of training programmes but with certain common characteristics: participants are disadvantaged unemployed[2] and the programme duration never exceeds one year.

The selected treatment states cover quite different and not comparable individuals. But we will argue that information on individual characteristics, once they are taken fully into account, allow us to evaluate the impact of the different treatments/policies. Our main assumptions are the following: (i) all the treatment states are potentially available for all the registered unemployed; (ii) all of the selected treatment states (except of course the NP treatment state) involve a participation period which does not exceed one year of duration; (iii) the characteristics that might decide the entry on a particular treatment state according to the

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[2] Some of the basic training courses have also employed individuals as beneficiaries but they are not considered in the dataset used in the empirical application.

legislation regulating the programmes are observable characteristics captured by the administrative data; and (iv) the aim of all the treatment states is to improve the employability of the unemployed participants.

Our aim is to offer a comprehensive empirical evaluation of the Portuguese active labour market policy by comparing, within a multiple treatment econometric framework, the treatment effects across the selected six treatment states.

3. - CAUSAL EVALUATION MODEL WITH MULTIPLE TREATMENTS

To evaluate the Portuguese active labour market policy, under heterogeneous multiple treatments, we will apply the extension of Imbens (2000) and Lechner (2001) to the Rubin (1974) model of causality with a binary treatment framework.

Following the notation of Lechner (2001), let us assume that a random individual i can participate in $(M+1)$ mutually exclusive treatments, denoted by $0,1,...,M$[3]. The participation in treatment m is indicated by $D=\{0,1,...,M\}$. The potential results, associated with these $(M+1)$ possible treatments is defined by $\{Y^0, Y^1,..., Y^M\}$. The number of observations in the population is N, with $N=\sum_{m=0}^{M} N^m$, where N^m is the number of participants in treatment m. As usual, for each participating individual only one outcome is observed, the outcome associated with his/her specific treatment. However, under certain assumptions, that limitation does not preclude estimation of the average causal effect of the treatment, even in a multiple treatment context.

In the framework of multiple treatments, Lechner (2001) defines several interest parameters, by presenting the necessary adjustments to the definition of average treatment effects used in the binary treatment case. In particular, he defines the expected effect of treatment m relatively to treatment l for a participant drawn randomly from the population N, the average effect for a participant randomly selected from the group of participants in either m or l, and the average effect for an individual randomly drawn from the population of participants in treatment m, only. For the multi-treatment version, the average treatment on the treated (the parameter that receives more attention in the binary evaluation literature), can be presented as a pairwise comparison of the effects of the treatments m and l for the participants in treatment m, this is:

[3] Without loss of generality, treatment 0 denotes the absence of participation in any kind of policy (treatment).

$$ATT^{m,l} = E\left(Y^m - Y^l \middle| D = m\right) = E\left(Y^m \middle| D = m\right) - E\left(Y^l \middle| D = m\right), \tag{1}$$

where $ATT^{m,l}$ is the expected treatment effect for an individual randomly drawn from the population of participants in treatment m, in comparison with treatment l. It is important to note that the average treatment effects on the treated are not symmetric (i.e. $ATT^{m,l} \neq -ATT^{l,m}$) if the participants in treatments m and l differ in a non-random fashion[4].

The issue at stake is that the traditional model of causality (Rubin, 1974) assumes that in a non-experimental evaluation process it is not possible to identify the average causal effect of a treatment and therefore, the identification of that effect must rely on strong (non-testable) assumptions, which plausibility should be argued on a case-by-case basis depending on the underlying economic problem and data availability. The extension of the traditional model of causality to the case of a multiple treatment context takes on the same problem and makes the same assumption: the conditional independence assumption (CIA), or "strong uncounfoudness" (Imbens, 2000). In the multiple treatment context the CIA can be formalised as follows:

$$\left\{Y^0, Y^1, ..., Y^M\right\} \perp D \middle| X = x, \forall x \in \chi, \tag{2}$$

that is, all potential treatment outcomes are independent of the selection mechanism for any given value of a vector of characteristics, X, in a characteristics space, χ (Lechner, 2002a). This means that the researcher observes all relevant characteristics which jointly influence the participation on a particular treatment and the subsequent potential outcome.

Additionally, the identification of the average causal effect requires that all individuals actually have the possibility of participation in all the alternative states of treatment, this is, it is required a support condition:

$$0 < P\left(D = m \middle| X = x, \forall m = 0, ..., M, \forall x \in \chi\right) < 1 \tag{3}$$

Since conditioning on all relevant observable characteristics may cause a problem of dimensionality. Imbens (2000) and Lechner (2001) show that the properties of the particular

[4] We also note that, for $m = l$, $ATT^{m,l} = ATT^{m,m} = 0$.

balancing score, the propensity score, suggested by Rosenbaum and Rubin (1983) to overcome the "curse of dimensionality" also hold for the multiple treatment case. So, using the probability of participation in a treatment conditional on the observable characteristics, the $ATT^{m,l}$ can be presented as:

$$ATT^{m,l} = E\left(Y^m \middle| D = m\right) - \underset{P^{l|ml}}{E}\left\{E\left[Y^l \middle| P^{l|ml}(X), D = l\right]\middle| D = m\right\}, \tag{4}$$

$P^{l,ml}(x)$ is the conditional choice probability of a treatment, given either treatment m or l, this is:

$$P^{l|ml}(x) = P^{l|ml}\left(D = l \middle| D \in \{l, m\}, X = x\right) = \frac{P^l(x)}{P^l(x) + P^m(x)} \tag{5}$$

The $ATT^{m,l}$ parameter can be then identified from an infinitely large random sample because all participation probabilities, as well as $E\left(Y^m \middle| D = m\right)$ and $E\left(Y^l \middle| P^{l|ml}(X), D = l\right)$, are identified (Lechner, 2002a and Lechner, 2002b).

These results allow us to apply in the multiple-treatment context the appealing nonparametric propensity score matching methodology. A methodology not dependent of any functional form assumption and that allow us to correct two of the three important evaluation biases identified by Heckman et al. (1997, 1998). Indeed, the matching methodology eliminates the bias due to a different support of the vector of characteristics X (that is, the violation of the common support condition resulting from having a different range of X for treated and non-treated individuals) and the bias due to a different distribution of characteristics X over the region of common support. Although, it does not eliminate the third source of selectivity bias: the "selection on unobservables", or the bias arising from unobserved heterogeneity among potential participants. The acceptance of the CIA is therefore very dependent on the nature of the data sources.

The assumption that selection is driven only by observable characteristics is highly restrictive. For instance, some unobservable characteristics such as motivational differences across registered individuals, while known by public employment officers, are likely not to be observed by a researcher with no full access to the raw information. The implication is that the available administrative data is likely to be insufficiently informative to make the CIA an acceptable assumption. We decided therefore to extend the work of Imbens (2000) and Lechner (2001) a little further and apply, in the multi-treatment context, the Heckman et al (1997) proposal to eliminate the selection on unobservables – the so-called conditional difference-in-differences (CDiD) methodology.

The CDiD estimator assumes the Bias Stability Assumption (BSA) (Heckman et al., 1997). That is, that selection on unobservables is constant over time. It assumes, in particular, that the treatment has no impact in pre-treatment outcomes and therefore any observed difference in the pre-treatment period between participants and non-participants can be used to correct the observed differences in post-treatment outcomes. Under BSA, and denoting t and t' as the time periods after and before the programme, respectively, the effect of treatment on the treated is then given by:

$$\Delta_{CDiD}^{ATT} = \Delta_{Mt}^{ATT} - \Delta_{Mt'}^{ATT}\ , \tag{6}$$

where Δ_{Mt}^{ATT} is the matching estimator for the effect of participation at time t and $\Delta_{Mt'}^{ATT}$ is the matching estimator at time t'. Since we assume that everything not observable is constant over time, by differentiating twice over treated and non-treated individuals and before and after the event, one gets rid of the unobservable component presented in both groups.

Less restrictive in terms of identification – one does not have to assume that the unobservable characteristics are identical across participants and non-participants – we believe that the CDiD estimator is preferred to the original propensity score matching estimator.

4 - DATA

With the empirical evaluation carried out by this paper we pretend to assess the impact of the Portuguese Active Labour Market Policy on the participants in the main ongoing programmes considering a multi-treatment framework. For that purpose, the paper's empirical evaluation relies on the dataset containing secondary information built from *SIGAE* the information system of the *IEFP*. It consists on an administrative dataset containing relevant information, as individual and labour market characteristics, related to all the individuals who had been registered by the public employment service. These records allow us to follow the registered labour history, including the participation on each ALMP and all (de)registration dates on a monthly basis[5].

The sample population considered on this particular paper corresponds to all the individuals registered as unemployed in the beginning of January 2001 and who never participated in an ALMP before that period or will never participate in another one after the analysed participation in one of the interest programmes. These restrictions to the sample construction try to avoid the contamination of the results for previous or subsequent participation in some kind of public employment programmes[6], who could lead to questions of sequential treatments which are not address by the present work.

The interested unemployed population is divided in different treatment sub-samples – the treatment states – according to the participation on a particular active programme, between

[5] The knowledge of all the registration and de-registration dates is an important issue because they allow us to understand the participation's path during the time period recorded by the public employment service. For instance, a registered individual recorded by *IEFP* as "openly" unemployed, can change his labour market status due to the participation on an ALMP and became again "openly" unemployed before permanently, or just temporarily, de-register due to a transition to a labour market status characterised by a regular employment.

[6] Obviously, because we do not have information prior to January 1998 it is not possible to guarantee that the individual participated, or not, in previous ALMPs before that date.

January and December 2001, or the non-participation in any of the considered programmes[7]. Thus the treated individuals cover all individuals that participated in one of the possible considered treatment states between the period $t^{'}$ and t. These denote points corresponding to periods of time before and after a particular treatment state participation, respectively.

The unemployment register at a specific period, t, after participation will still be assumed as the outcome variable within our evaluation process. So, a positive average treatment effect on the treated will represent the maintenance of the unemployment register and a failure of the official aim of the ALMPs. Their aim is to help the unemployed individuals to find regular employment and leave the unemployment registers. The average treatment effects were computed comparing the effects of participation in a particular programme with the participation in each of the other programmes and the non-participation case. This is, the outcome resulting from a participation in a treatment state will be compared with the outcome obtained by the alternative participation in each of the other treatment states.

After deciding which were the interested evaluation sample and sub-samples, it was necessary to transform the original data in suitable empirical data. Figure 1 helps to explain the process.

First of all, after dividing the interest unemployment population in the different treatment states, was necessary to aggregate in each $t^{'}$ and t points in time all the selected individuals. All of these individuals, except the ones in the non-participation state, started the participation at different months along 2001, not uniformly distributed. However for the non-participants the start dates are not clearly defined becoming necessary the definition of a "virtual" starting date. As was discussed before, rapidly changing labour market conditions could become a methodological problem. Since we decided to consider the participation over only a single year

[7] A non-participant is defined as a register unemployed individual who has never been enrolled in any ALMP, however since we considered the non-participation as another treatment state, we will refer to all individuals as treated individuals.

the time changing in some variables is not significant so we will adopt an "inflated approach" (Lechner, 1999) that generates a well defined start date for all the non-participant individuals. Empirically that means to consider, each month, as a non-participant the individual who does not participate in one of the six selected treatment states.

Since were considered initially twelve possible months of entry in a treatment state twelve groups of participants were also constructed for each one of the six treatment states. Then, for each group, the correspondent months for programme evaluation were identified. Connected each group to the time points in time where will be estimated the causal effects of the treatment states, the groups were pooled in six single treatment groups.

Actually, another important choice in our empirical process of evaluation was the choice of the pre-treatment (t') and post-treatment (t) points in time used to estimate the average treatment effect on the treated. It will choose the relevant comparison time periods (t' and t) as close as possible in order to make the social and economic contexts also as similar as possible. The matching empirical analysis must ensure that the treatment group is compared with a control group in the same economic environment.

Another option which is maintained consists in comparing the outcome variable immediately after the beginning of participation. This option allows to consider the non-treatment state as a different non-employment labour market state (Gerfin and Lechner, 2002 and Sianesi, 2004). However the results of this selected approach should be seen with care. Participants in some programmes do not have the same amount of time to search for a new employment as non-participants. Therefore locking-in effects may occur and consequently the initial effect from participation on the programme could be negative (Ours, 2004). To exclude these potential negative effects we will start the evaluation of the outcome six months after the beginning of participation and we will extend the evaluation period during two and a half years.

5. EMPIRICAL ANALYSIS OF PARTICIPATION ON A TREATMENT STATE

5.1 – OBSERVABLE PRE-TREATMENT CHARACTERISTICS

The observable factors that could be potentially important to influence the decision of participation in one of the selected treatment states as well as the future potential outcomes are: (i) socio-demographic variables like sex, age, regional location or the responsibility for others; (ii) qualification variables like the educational level, the previous occupational group, the qualification rank; and, (iii) labour market variables like the reason for being unemployed, the unemployment category or a previous register in a public employment office.

Details about the variables used in this paper, as well as their distribution between the treatment states are presented in Table 1.

The predominant treatment state is by far the NP state with approximately 86% of the whole sample. Consequently only 14% of the selected unemployed population participated in a particular active programme, during the year 2001. Among those who effectively participated it is important to note the participants in JC programmes – almost 8% of the whole sample – and the individuals directly placed in a job by the public employment service, which represent 3% of the sample. The remaining selected treatment states present a very similar size concerning the number of participants. The whole sample is composed, in a higher percentage, by women, non-qualified individuals or with no previous occupation, with lower levels of education and under the age of 40.

Table 3.1 also shows there are differences related with gender, age, geographic location, educational levels, reasons for the unemployment register, number of registers *per* individual and previous occupational groups among the individuals distributed by the six treatment states. For example, the majority of women is less obvious in the NP and JC states, where the presence of relatively older individuals is also visible. The former treatment states are also the ones who bring together a major number of unemployed who were dismissal or ended a temporary occupation.

These groups have also participants with lower educational levels. PEP is the treatment state with a bigger percentage of non-qualified workers and the TE treatment state is the group with individuals who present higher educational levels and the group with more individuals looking for a first employment. Individuals with more than one register at the public service are more frequent in the DP, BT and PEP treatment states, respectively.

The above mentioned differences are not a surprise since the programmes present particular institutional features. However an issue remains. Could unobservable variables as motivation, ability or some sort of administrative selection missing for the analysis be important? The answer to the question will rely on the application of the conditional difference-in-differences estimator to try to capture the effect of hypothetical unobservable characteristics on the participant's outcomes.

5.2 – Probability of Treatment State Participation

This sub-section describes the results of the estimation of ${(M+1)M}/{2}$, with $(M+1)$ the number of treatment states, binomial *logit* models for the probability of individual participation in the selected treatment states. The results can be found in Table 2a), Table 2b) and Table 2c).

Lechner (2001) discusses if the conditional participation probabilities should be estimated for each combination of states separately as binary choices or whether the process should be modelled simultaneously with a discrete choice model including all relevant states. Both alternatives present advantages, namely in a practical level[8]. Choosing to estimate the binomial logit models, as did Larsson (2003) or Dorsett (2001), could be preferable since it avoids the restrictions associated with simultaneous models, namely the IIA assumption associated with the

[8] Lechner (2001) argues that if $P^{l|ml}$ is modelled directly no information from sub-samples other than the containing participants in m and l is needed for the identification of equation (4) and we are basically back to the context of a single treatment. If all values of m and l are of interest, the whole sample is needed for identification. In that case either the binomial conditional probabilities could be estimated or a structural approach, where a complete choice problem is formulated in one model and estimated on the full sample, could be used.

multinomial logit model. At a practical level, such an option could be more robust to error since a misspecification is one model will have fewer consequences than in the simultaneous model in which case all results will be compromised. Arguments in favour of a multinomial option (using, for example, a multinomial probit model as Gerfin and Lechner (2002) and Frolich (2004) could hold up at a practical level since there is less output to consider.

The results of binomial logit models estimation show the probability of participation in one treatment state, compared to the remaining ones. For example, Table 2a) shows the results of the probability of being in the DP treatment state compared to each one of the other options – NP, JC, TE, PEP and BT[9], respectively. Given the large number of models – fifteen binomial logit models – and variables the results are extensive and will not be discussed.

Table 3 presents the number of observations in the treatment (in row) and control (in columns) groups, for each binomial logit model, and several tests related to the estimation of these models. With the more common tests, as the Pseudo-R^2, the F-test ($LR\chi^2$, with degrees of freedom in brackets) and the value of the log-likelihood, we present also the correction prediction rate for participants in the treatment state ($\mathrm{CPR}_{\mathrm{TG}}$). Still since the dataset provides a full range of individual characteristics, we looked mainly at two aspects to obtain the preferred logit specifications: i) minimization of classification error[10]; and (ii) statistical significance of the included regressors.

The observation of Table 2a), Table 2b) and Table 2c) allow us to verify that the majority of variables are statistical significant in each logit model. To illustrate, variables like sex, age,

[9] The probability of being in NP, JC, TE, PEP or BT compared to DP is equal to one minus the probability of being in DP related to each other treatment state, respectively. So with six treatment states to be considered $(5+1)5/2 = 15$ binomial logit models were estimated.

[10] Minimization of classification error was suggested by Heckman et al. (1998) and Heckman et al. (1999), who, assuming that the costs for the misclassification are equal for both groups, chose to maximize the within-sample correct prediction rates using the fraction of participants as the "cutoff" to predict someone to be a participant. In practice, $\hat{P}(X) > P_C$ is used to predict $D = 1$, and $\hat{P}(X) \le P_C$ to predict $D = 0$, with $P_C = E(D)$.

educational levels and the reasons for the unemployment register perform particularly well in all models. In table 3 we can also verify that the variables in each model are jointly statistically significant. These results stress the findings that there are differences in the composition of the treatment states and represent a good indication that a matching procedure could produce effective results.

Concerning the minimization of the classification error (Table 3) it is possible to find a within-sample correct prediction rate for participants in the treatment state in the 63-78% range. The values indicate that the estimated probability of treatment participation is generally well classified in 63 to 78% of the cases.

6. THE MATCHING PROCEDURE

Given the participation probability (propensity score), in other words, the probability of a given individual being in a particular treatment state and not in one of the alternative ones, the next step is to perform matching on the propensity score.

For computational reasons, associated with the dimension of the dataset, we selected the nearest neighbour matching estimator, with replacement (within a common support region) of non-participant observations. This is a highly intuitive procedure which requires finding a pairwise matching for every treated individual, obtained by choosing the closest non-treated individual given its propensity score. The replacement option allows us to use the same non-participant individual more than once if it happens to be a good match for participants.

Before the matching procedure, it is necessary to guarantee the common support condition. That is, to ensure that for a given propensity score, the two possible treatment states can be observed. In non-experimental studies if one wants to obtain the counterfactual for a given individual in the treatment group, someone similar in the non-treatment state has to be found. This is exactly what the common support region is supposed to replicate. Therefore we will only use values of the propensity score for which both the density of the treatment group and the comparisons groups are positive.

In practice, this implies that some of the observations at the tails of the propensity score distributions will be eliminated if they do not cover the exact same interval. Since we estimate pairwise effects between each of the different six treatment states the requirement is that all observations in the treatment state m for which there are no comparison observation in treatment state l ($m, l \in \{0,1,...M\}, m \neq l$) are removed from the sub-sample.

Table 4 shows the number of observations lost across the different treatment states due to the imposition of the common support requirement. This loss is between 0 and nearly 5%, with the biggest percentage found in the treatment states with fewer observations (in absolute terms though, NP is the group with the largest number of observations lost).

The next step is to check the quality of the implemented matching procedure, that is, whether our matching produced balanced characteristics across the treatment and non-treatment groups. In other words, the variables included in the propensity score model should guarantee that, for a given propensity score, the exposure to treatment is random. Table 5 shows the results of our testing.

The standardized bias suggested by Rosenbaum and Rubin (1985) is often used in the evaluation literature (e.g. Gerfin and Lechner, 2002; Larsson, 2003; and Dorsett, 2001). Recalling, this indicator is defined as the difference in the mean of the treated and comparison samples as a percentage of the square root of the average of the sample variances in both groups. Given the number of states and variables, we will not comment on every single result. However it seems clear that our matching generated a substantial reduction of the standardized differences among the variables, as can be seen in Table 5. Indeed, we found mean standardized differences larger than 20% (and never lower than 10%) before matching, while after matching the bias lies between 1.14 and 8.55%. Clearly there is plenty of evidence that the matching procedure was able to balance the characteristics in the treatment and the matched comparison groups.

We applied also other balancing tests. The t-test on differences in means between the treated and comparison groups, before and after matching, for each variable included in the matching procedure. This test yielded statistically significant differences before matching but not after matching, which is a further indication that matching has been effective. Moreover, after matching there should be no systematic differences in the distribution of the covariates between the two groups (participants and matched non-participants). The pseudo-R^2 after matching should be fairly low. As Table 5 shows, this is true in our case. Finally, the log-likelihood ratio points in the same direction, indicating a joint significance of all variables before but not after matching for some of the estimated models.

7. ESTIMATION RESULTS

Our goal is to measure the causal effects of participation in each one of the selected treatments states in a multiple treatment framework with a view to evaluation of employability of participants, both in the short and long-run. Registration at the public employment service (yes/no) is our outcome measure.

Table 6 organizes the results in two main columns ($\Delta_{Mt'}^{ATT}$ and Δ_{Mt}^{ATT}), while Table 7 presents the results of the CDiD estimator (Δ_{CDiD}^{ATT}). In particular, Table 6 (column $\Delta_{Mt'}^{ATT}$) presents the difference, in terms of unemployment register, between the matched participant and non-participants before treatment. The column Δ_{Mt}^{ATT} of the table, in turn, shows the same difference but after treatment.

In the context of the econometric methodology presented, we will assume that the true effect of a treatment state before the beginning of participation is null. Thus the changes in the registered unemployment rates of each group are a good estimator of the unobserved differences among treated and comparison matched individuals. This assumption allows us to estimate the potentially bias in Δ_{Mt}^{ATT} (which only assumes the conditional independence assumption). In other words, if we further assume that this bias is on average identical to the t and t' points in time chosen – the bias stability assumption – we can use the bias computed in t' to correct the estimate of the average effect of the treatment on the treated we get for t.

In Table 6 each 6x6 matrix includes all selected measures, from semester 1 to semester 5. The programme effects, column Δ_{Mt}^{ATT}, are presented off the main diagonal. A positive number indicates that the effect on the participants in the programme, compared to the participating individuals in the comparison groups, is negative in terms of employability. For example, six months after the beginning of participation (case $t=1$) in a PEP, the probability of a participant being a registered unemployed is 22.3 percentage points higher than a non-participant (NP). The corresponding effect is of 50 percentage points compared to those in the DP state, to almost 53% compared to the BT state and to 66% compared to the TE state. The bigger percentage of unemployment registers for PEPs participants it is only reduced if we compare the PEP participation with the JC state – the percentage of unemployment rates is still higher for the PEP state but now the difference is of 5.6%, only.

After two and a half years, the participation on a PEP programme still compares relatively badly except when compared to the NP treatment state. In semester 5 participants in this programme will have 3% less probability of being registered as unemployed than a NP participant. This long-run effect is also present in the case of the other comparison groups.

In contrast, the TE participants present lower probabilities of having an unemployment register than the participants in other treatment options, six months after the beginning of participation. The results, although remaining generally positive in terms of the employability of TE participants, are reduced in the long-run. Compared to the DP state, for instance, the participants in the TE treatment present a higher probability of being unemployed.

In general, we might say that in the short-run PEP and JC treatment states seem to perform poorly when compared to DP, TE, BT and even NP treatment states. The programmes which seem to perform better, in the short–run, are BT and TE. Performing even better than DP. We think, however, that these short-run findings are not due directly to the performance of the programmes themselves but to administrative reasons: Participation in BT and TE implies an immediate unemployment de-registration. In this case, only the long-run effect measures the impact of programme participation. Another explanation concerns the locking-in effects due to a lower amount of free time to look for a regular job.

It should be pointed out, at this stage, that all programmes seemed to produce better results than the non-participation treatment state, as is supposed to happen to any active labour market programme. After 4 or 5 semesters the probability of being unemployed is lower for DP, JC, TE, PEP and BT treatments than for non-participants (NP). However, among the effective participation in a particular active programme, the PEP state is the one that presents the worst results. These are followed by JC programmes. After 5 semesters, the programme that performs better is DP.

The above results are not net of the unobserved heterogeneity bias. It is possible to observe before participation (column $\Delta_{Mt'}^{ATT}$ in Table 6) differences in the unemployment's register rates among the state's participants. This indicates the existence of some unobserved heterogeneity. As mentioned above, in order to estimate an unbiased average treatment effect on the treated we implemented a conditional difference-in-differences estimator, Δ_{CDiD}^{ATT}. Our implementation uses two approaches. The first approach assumes t' symmetric to t, which means that, given t_0 (the month where the program begins), the outcome variable is evaluated 1, 2, …, 5 semesters before and after t_0. The acronym $(t' = -t)$ denotes this case, for t = 1, 2,…, 5. The second approach considers t' fixed at one semester before t_0 and then t equal to 1, 2, …, and 5 semesters, respectively. This case is denoted by the acronym $(t' = -1)$. The results for both approaches are presented in Table 7 and Figures 2-7.

Figure 2, for example, shows the evolution of the average treatment effects for participants in each treatment state. We note that the zero axis line corresponds to the reference treatment state (NP). Any point above zero indicates registered unemployment rates larger than the ones found for the reference treatment group. Any point below zero, the opposite. In the Figure is possible to observe that the two mentioned approaches present different results for longer time periods but in the short-run the patterns are identical. Having as reference the NP treatment state, all the programmes present better results except the JC and PEP states. The explanation for the worse results of the PEPs, for example, could rest in a probable reduction of job search activities during participation, which can last for twelve months. The better results of DP, TE and BT could rest in administrative reasons. Their participant individuals leave the unemployment register at the beginning of participation. Over time, however, the effects of all treatment states tend to converge. In the long run all the treatment states seem to perform better than the non-participation state.

Figures 3-7 give more information about the specific active labour market programmes. In these figures each reference active programme is compared to the others and to the state of non-participation.

The DP reference treatment group can be observed in Figure 3. In the short-run only TE and BT perform better. A reason is probably the duration of these programmes. They can last twelve months and their participants must leave the unemployment register during the participation period. Indeed if we observe the twelve months period we find worse results for the BT and TE treatment states than for the DP treatment state. The better results of the DP treatment state remains in the long-run. A possible explanation is that the individuals directly placed by the public employment service in a regular job could be better adapted to the needs of the labour market. Thus, it is easier to match their job demand with the available job offers.

The results of the selected treatment states having as reference the JC programmes can be observed in Figure 4. Only the PEP treatment state participants perform worse. The relative position of the JC programmes remains over time.

Figure 5 shows the results of the average treatment effect on the treated (compared to the TE treatment state). The TE treatment outperforms all other programmes both in the short- and the long-run, although the gap is clearly lower in the long-run.

The training programmes seem to give participants some persistent effect in terms of labour market opportunities as we can see in Figure 6, which presents the BT treatment state as the reference group. In fact, when using the differences in registered unemployment rates for $t = -1$ it is clear that a participation on BT produces better results than a participation in other types of active labour market programmes. When using the differences in the unemployment register rates for $t = -t$ the absolute better results of the BT treatment state are not so obvious and are quite similar to the results obtained for the TE treatment state. Since TE programmes have an important training component, the conclusion that training generates greater employability is reinforced.

Finally, we can observe that the PEP-type programme, the reference treatment state in Figure 7, has the worst results among all active labour market programmes. In the short-run, participation is worse than non-participation (NP). Only after five semesters is this finding reversed. Similar results were reported by Gerfin and Lechner (2002), who admit that the additional amount of human capital obtained in PEP-type programmes is too small to compensate for the initial (negative) effects due to a reduced job search.

8 - CONCLUSIONS

The unemployed in the public employment service can participate in a wide variety of active labour market programmes. To fully evaluate the impact of each selected programme we decided to extend the work of Imbens (1999) and Lechner (2001) and apply, in a multi-treatment context, the Heckman et al. (1997) difference-in-differences approach to eliminate the selection on unobservables. We have therefore reinforced the rejection of the assumption that selection into participation is exclusively driven by observable characteristics.

Assuming six different treatment states (including the non-participation state), our findings suggest that ALMPs have an impact in long-run. In the short-run, however, there is a lot to improve. PEP-type programmes in particular perform very poorly, while programmes in which there is some training component seem to have a greater impact on employability. Given the estimated long-run effects, the major lesson drawn from this comprehensive empirical evaluation exercise is that programme evaluation restricted to the short run impact may not be totally informative.

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ANNEX: FIGURES AND TABLES

Figure 1: Participation and Non-Participation Groups and Pre and Post-treatment Points

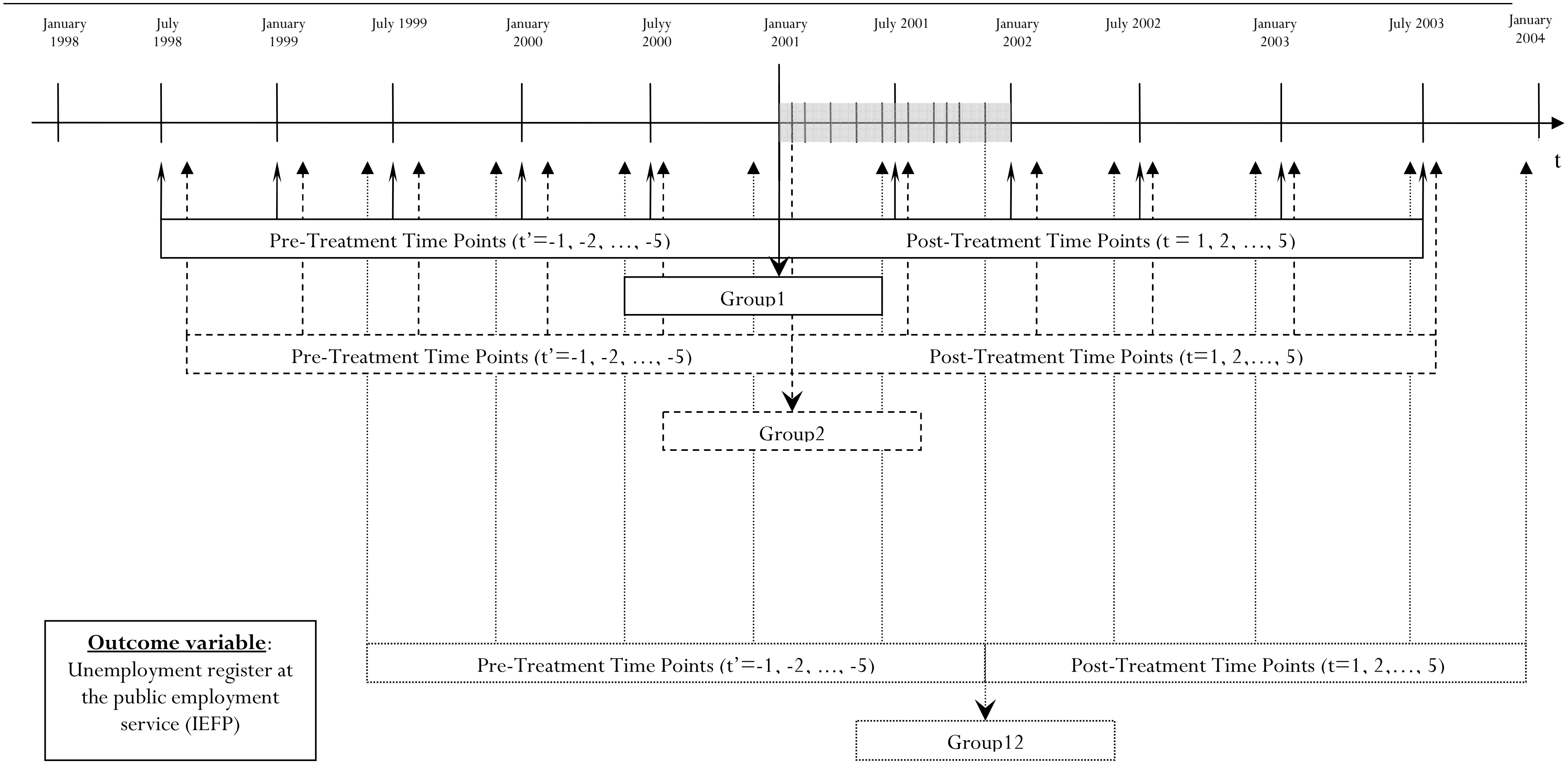

Table 1: Number of Observations and Pre-Treatment Characteristics

	NP	DP	JC	TE	PEP	BT
Number of individuals (in %)	147548 (85.65)	5414 (3.14)	13581 (7.88)	1686 (0.98)	2550 (1.48)	1484 (0.86)
Variables :						
Sex (Men) (in %)	40.08	34.97	41.67	21.00	22.94	23.99
Age (in years)	37.44	30.59	40.79	28.66	36.99	32.13
Persons at charge (in %)	47.58	42.93	49.91	33.63	60.00	52.63
Geographic location (in %)						
Norte	40.12	21.33	60.56	36.60	30.28	22.71
Centro	11.50	35.30	9.82	26.99	20.94	15.50
Lisboa e Vale do Tejo	40.04	23.68	26.03	23.07	27.41	43.26
Alentejo	5.29	4.71	1.84	7.59	17.37	16.11
Algarve	3.05	14.98	1.75	5.75	4.00	2.43
Educational level (in %)						
None	6.43	3.86	7.08	0.83	9.10	2.16
Primary (4 years)	34.07	23.68	41.30	17.97	36.16	19.95
Compulsory Secondary (9 years)	34.70	44.79	31.44	27.34	35.77	54.72
Secondary (12 years)	16.09	21.70	13.08	21.83	14.90	20.01
Superior (15 or more years)	8.72	5.97	7.11	32.03	4.08	3.17
Previous occupational group (in %)						
- None	11.24	17.64	8.67	45.02	8.98	13.34
- Management	1.43	0.35	1.23	0.53	0.39	0.20
- Scientific specialist	3.79	2.07	4.15	3.20	2.35	2.02
- Technical worker	6.78	4.17	7.63	2.85	3.80	4.25
- Administrative worker	13.22	10.79	13.70	8.96	14.00	13.88
- Seller	15.52	20.04	12.96	12.34	16.63	21.63
- Farmer	4.60	3.36	3.70	3.74	8.63	5.26
- Manufacturer's worker	14.87	11.95	18.28	6.94	10.71	11.12
- Machine's operator	9.64	8.52	11.43	2.37	6.71	6.13
- No-qualified worker	18.92	21.11	18.25	14.06	27.80	22.17
First employment (in %)	11.24	17.66	8.70	45.02	8.98	13.48
Re-application at IEFP (in %)	48.95	62.10	40.00	54.09	60.12	62.00
Reasons for unemployment (in %)						
- End of formal education	9.95	15.87	7.14	38.14	5.96	12.33
- Dismissal	38.39	25.38	48.47	16.07	32.04	26.48
- End of temporary occupation	34.74	41.98	31.23	19.87	39.73	36.93
- Re-application	2.81	5.84	2.49	10.14	5.77	7.35
- Other	14.11	10.94	10.68	15.78	16.51	16.91

Table 2a): Determinants of Participation on DP Programmes

Variables	DP (compared with)				
	NP	JC	TE	PEP	BT
Sex	-0.058 (***) (0.032)	0.106 (**) (0.045)	0.548 (*) (0.076)	0.685 (*) (0.065)	0.511 (*) (0.076)
Age	-0.051 (*) (0.002)	-0.083 (*) (0.002)	-0.017 (*) (0.005)	-0.053 (*) (0.003)	-0.026 (*) (0.004)
Persons at charge	0.046 (0.033)	0.032 (0.045)	-0.239 (*) (0.082)	-0.238 (*) (0.059)	-0.250 (*) (0.073)
Geographic location					
Norte	-2.202 (*) (0.052)	-3.409 (*) (0.092)	-1.284 (*) (0.134)	-2.022 (*) (0.126)	-1.932 (*) (0.188)
Centro	-0.477 (*) (0.048)	-0.966 (*) (0.093)	-0.301 (**) (0.134)	-1.052 (*) (0.124)	-1.032 (*) (0.189)
Lisboa e Vale do Tejo	-1.992 (*) (0.050)	-2.207 (*) (0.091)	-0.786 (*) (0.135)	-1.609 (*) (0.123)	-2.438 (*) (0.181)
Alentejo	-1.744 (*) (0.077)	-1.355 (*) (0.133)	-1.203 (*) (0.171)	-2.860 (*) (0.143)	-3.147 (*) (0.200)
Algarve	(a)	(a)	(a)	(a)	(a)
Educational level					
None	0.579 (*) (0.105)	0.670 (*) (0.143)	3.590 (*) (0.310)	-0.382 (***) (0.204)	0.481 (0.297)
Primary (4 years)	0.549 (*) (0.080)	0.627 (*) (0.113)	2.172 (*) (0.142)	-0.243 (0.177)	-0.280 (0.223)
Compulsory Secondary (9 years)	0.520 (*) (0.073)	0.460 (*) (0.102)	2.138 (*) (0.115)	-0.207 (0.167)	-1.007 (*) (0.207)
Secondary (12 years)	0.552 (*) (0.072)	0.497 (*) (0.101)	1.726 (*) (0.108)	-0.132 (0.165)	-0.744 (*) (0.206)
Superior (15 or more years)	(a)	(a)	(a)	(a)	(a)
Previous occupational group					
- None	-0.031 (0.565)	1.330 (***) (0.784)	-0.206 (0.877)	-0.314 (1.429)	1.224 (0.970)
- Management	-0.886 (*) (0.238)	-1.231 (*) (0.280)	0.114 (0.474)	-0.251 (0.440)	0.597 (0.683)
- Scientific specialist	-0.491 (*) (0.116)	-1.136 (*) (0.152)	0.907 (*) (0.219)	-0.529 (**) (0.229)	-0.539 (**) (0.280)
- Technical worker	-0.275 (*) (0.080)	-0.586 (*) (0.106)	0.545 (*) (0.201)	-0.011 (0.152)	-0.004 (0.176)
- Administrative worker	-0.285 (*) (0.057)	-0.561 (*) (0.079)	0.230 (***) (0.137)	-0.364 (*) (0.103)	-0.168 (0.120)
- Seller	-0.143 (*) (0.046)	-0.159 (**) (0.068)	0.155 (0.112)	0.211 (**) (0.084)	0.056 (0.099)
- Farmer	-0.436 (*) (0.084)	-0.486 (*) (0.115)	-0.414 (**) (0.177)	0.074 (0.131)	0.045) (0.174)
- Manufacturer's worker	0.013 (0.053)	-0.161 (**) (0.071)	0.181 (0.130)	0.360 (*) (0.096)	0.142 (0.116)
- Machine's operator	0.044 (0.059)	-0.115 (0.081)	0.783 (*) (0.187)	0.400 (*) (0.113)	0.256 (**) (0.141)
- No-qualified worker	(a)	(a)	(a)	(a)	(a)
First employment	-0.131 (0.565)	-1.785 (**) (0.783)	-0.058 (0.874)	0.230 (1.427)	-1.032 (0.967)
Re-application at IEFP	0.342 (*) (0.031)	0.641 (*) (0.043)	0.117 (***) (0.069)	0.165 (*) (0.058)	0.121 (**) (0.068)
Reasons for unemployment register					
- End of formal education	(a)	(a)	(a)	(a)	(a)
- Dismissal	-0.128 (0.080)	-0.372 (*) (0.115)	0.511 (*) (0.143)	-0.739 (*) (0.151)	0.049 (0.158)
- End of temporary occupation	-0.058 (0.077)	-0.298 (*) (0.113)	0.729 (*) (0.138)	-0.726 (*) (0.148)	0.095 (0.154)
- Re-application	0.354 (*) (0.087)	0.336 (**) (0.132)	-0.050 (0.142)	-0.613 (*) (0.166)	-0.283 (0.174)
- Other	-0.073 (0.077)	0.085 (0.111)	0.005 (0.130)	-0.838 (*) (0.143)	-0.242 (0.152)
Constant	-0.523 (*) (0.122)	3.902 (*) (0.185)	0.145 (0.246)	4.728 (*) (0.262)	4.502 (*) (0.333)

Notes: (a) denotes the reference variable. *, **, and *** denote statistical significance at 0.01, 0.05, and 0.1. Standard errors are in parentheses.

Table 2b): Determinants of Participation on JC Programmes

Variables	JC (compared with)			
	NP	JC	TE	PEP
Sex	-0.073 (*) 0.020	0.447 (*) 0.074	0.583 (*) 0.057	0.464 (*) 0.074
Age	0.024 (*) 0.001	0.062 (*) 0.004	0.031 (*) 0.003	0.051 (*) 0.004
Persons at charge	-0.015 0.019	-0.225 (*) 0.077	-0.320 (*) 0.051	-0.232 (*) 0.068
Geographic location				
Norte	0.948 (*) 0.069	1.635 (*) 0.153	1.575 (*) 0.132	1.328 (*) 0.200
Centro	0.392 (*) 0.073	0.276 (***) 0.160	0.085 0.137	-0.160 0.206
Lisboa e Vale do Tejo	0.050 0.070	0.936 (*) 0.157	0.718 (*) 0.133	-0.326 (**) 0.197
Alentejo	-0.475 (*) 0.094	-0.056 0.196	-1.289 (*) 0.152	-1.777 (*) 0.219
Algarve	(a)	(a)	(a)	(a)
Educational level				
None	-0.053 0.063	2.866 (*) 0.300	-0.672 (*) 0.178	0.061 0.280
Primary (4 years)	0.090 (***) 0.052	1.431 (*) 0.131	-0.472 (*) 0.158	-0.552 (*) 0.208
Compulsory Secondary (9 years)	0.155 (*) 0.048	1.455 (*) 0.106	-0.399 (*) 0.148	-1.261 (*) 0.192
Secondary (12 years)	0.119 (**) 0.048	1.090 (*) 0.098	-0.466 (*) 0.148	-1.120 (*) 0.191
Superior (15 or more years)	(a)	(a)	(a)	(a)
Previous occupational group				
- None	-0.551 0.464	-0.283 1.122	-17.188 .	0.381 0.957
- Management	0.021 0.088	1.022 (*) 0.377	1.224 (*) 0.346	1.877 (*) 0.599
- Scientific specialist	0.450 (*) 0.062	1.709 (*) 0.193	0.858 (*) 0.190	0.531 (**) 0.242
- Technical worker	0.229 (*) 0.043	0.988 (*) 0.186	0.687 (*) 0.130	0.630 (*) 0.161
- Administrative worker	0.163 (*) 0.035	0.640 (*) 0.128	0.321 (*) 0.086	0.449 (*) 0.111
- Seller	0.017 0.034	0.325 (*) 0.112	0.387 (*) 0.078	0.202 (**) 0.098
- Farmer	0.073 0.052	-0.004 0.169	0.356 (*) 0.111	0.534 (*) 0.168
- Manufacturer's worker	0.113 (*) 0.031	0.384 (*) 0.125	0.593 (*) 0.084	0.365 (*) 0.112
- Machine's operator	0.042 0.035	0.872 (*) 0.183	0.538 (*) 0.100	0.500 (*) 0.137
- No-qualified worker	(a)	(a)	(a)	(a)
First employment	0.892 (**) 0.464	0.423 1.119	17.567 (*) 0.131	0.382 0.949
Re-application at IEFP	-0.230 (*) 0.020	-0.545 (*) 0.067	-0.388 (*) 0.051	-0.456 (*) 0.066
Reasons for unemployment register				
- End of formal education	(a)	(a)	(a)	(a)
- Dismissal	0.300 (*) 0.067	0.779 (*) 0.143	-0.476 (*) 0.151	0.550 (*) 0.168
- End of temporary occupation	0.328 (*) 0.067	0.884 (*) 0.139	-0.567 (*) 0.149	0.479 (*) 0.164
- Re-application	0.252 (*) 0.081	-0.540 (*) 0.146	-0.896 (*) 0.172	-0.537 (*) 0.189
- Other	-0.113 (**) 0.064	-0.305 (**) 0.130	-1.001 (*) 0.144	-0.273 (***) 0.162
Constant	-4.158 (*) 0.107	-2.855 (*) 0.245	0.435 (***) 0.240	0.508 0.321

Notes: (a) denotes the reference variable. *, **, and *** denote statistical significance at 0.01, 0.05, and 0.1. Standard errors are in parentheses.

Table 2c): Determinants of Participation on TE, PEP and BT Programmes

	TE (compared with)			PEP (compared with)		BT (compared with)
Variables	NP	PEP	BT	NP	BT	NP
Sex	-0.558 (*) 0.063	-0.103 0.100	-0.300 (*) 0.107	-0.643 (*) 0.050	-0.163 (***) 0.088	-0.590 (*) 0.065
Age	-0.039 (*) 0.004	-0.037 (*) 0.005	-0.014 (**) 0.006	-0.009 (*) 0.002	0.031 (*) 0.004	-0.029 (*) 0.003
Persons at charge	0.212 (*) 0.067	-0.129 0.088	-0.060 0.100	0.314 (*) 0.044	-0.010 0.077	0.307 (*) 0.059
Geographic location	(a)	(a)	(a)	(a)	(a)	(a)
Norte	-0.935 (*) 0.116	-0.221 0.185	-0.419 (***) 0.229	-0.483 (*) 0.110	-0.092 0.217	-0.273 0.180
Centro	-0.186 0.118	-0.485 (**) 0.191	-0.664 (*) 0.235	0.378 (*) 0.112	-0.106 0.221	0.536 (*) 0.183
Lisboa e Vale do Tejo	-1.300 (*) 0.119	-0.400 (**) 0.187	-1.304 (*) 0.226	-0.528 (*) 0.109	-0.922 (*) 0.212	0.433 (**) 0.174
Alentejo	-0.598 (*) 0.141	-1.423 (*) 0.212	-1.814 (*) 0.250	0.924 (*) 0.114	-0.341 0.223	1.331 (*) 0.183
Algarve	(a)	(a)	(a)	(a)	(a)	(a)
Educational level						
None	-2.876 (*) 0.284	-3.805 (*) 0.335	-2.985 (*) 0.402	0.754 (*) 0.152	0.745 (**) 0.312	0.103 0.255
Primary (4 years)	-1.472 (*) 0.105	-2.030 (*) 0.196	-2.072 (*) 0.239	0.604 (*) 0.136	-0.107 0.251	0.682 (*) 0.188
Compulsory Secondary (9 years)	-1.491 (*) 0.081	-1.887 (*) 0.176	-2.717 (*) 0.217	0.555 (*) 0.129	-0.836 (*) 0.238	1.370 (*) 0.174
Secondary (12 years)	-1.075 (*) 0.074	-1.685 (*) 0.171	-2.270 (*) 0.212	0.563 (*) 0.128	-0.629 (*) 0.239	1.166 (*) 0.173
Superior (15 or more years)	(a)	(a)	(a)	(a)	(a)	(a)
Previous occupational group						
- None	0.016 0.741	-0.220 2.122	2.004 1.350	-0.023 1.013	18.361 .	-1.570 (**) 0.712
- Management	-1.148 (*) 0.349	0.128 0.571	0.746 0.805	-0.962 (*) 0.325	0.637 0.683	-1.611 (*) 0.585
- Scientific specialist	-1.253 (*) 0.169	-1.137 (*) 0.267	-1.207 (*) 0.330	-0.283 (***) 0.166	-0.288 0.306	-0.220 0.219
- Technical worker	-0.818 (*) 0.168	-0.169 0.223	-0.038 0.244	-0.455 (*) 0.117	0.032 0.192	-0.461 (*) 0.146
- Administrative worker	-0.470 (*) 0.114	-0.349 (**) 0.148	-0.146 0.161	-0.111 0.074	0.168 0.126	-0.293 (*) 0.096
- Seller	-0.296 (*) 0.099	0.001 0.124	-0.116 0.134	-0.339 (*) 0.064	-0.147 0.109	-0.178 (**) 0.081
- Farmer	0.143 0.146	0.252 0.179	0.361 (**) 0.210	-0.225 (*) 0.084	-0.030 0.166	-0.260 (**) 0.132
- Manufacturer's worker	-0.186 0.116	0.153 0.143	0.036 0.158	-0.392 (*) 0.074	-0.153 0.129	-0.128 0.099
- Machine's operator	-0.813 (*) 0.173	-0.312 0.206	-0.425 (***) 0.224	-0.425 (*) 0.088	-0.060 0.158	-0.303 (**) 0.121
- No-qualified worker	(a)	(a)	(a)	(a)	(a)	(a)
First employment	0.171 0.738	0.422 2.121	-1.516 1.343	-0.118 1.012	-18.057 (*) 0.175	1.105 0.709
Re-application at IEFP	0.231 (*) 0.055	0.008 0.083	0.052 0.090	0.149 (*) 0.044	0.054 0.076	0.184 (*) 0.058
Reasons for unemployment register						
- End of formal education	(a)	(a)	(a)	(a)	(a)	(a)
- Dismissal	-0.502 (*) 0.118	-1.337 (*) 0.182	-0.488 (*) 0.190	0.610 (*) 0.126	0.977 (*) 0.185	-0.309 (**) 0.140
- End of temporary occupation	-0.612 (*) 0.114	-1.454 (*) 0.176	-0.628 (*) 0.184	0.628 (*) 0.124	0.932 (*) 0.179	-0.269 (**) 0.136
- Re-application	0.774 (*) 0.110	-0.575 (*) 0.190	-0.311 0.201	1.010 (*) 0.139	0.422 (**) 0.203	0.530 (*) 0.152
- Other	0.157 0.101	-0.764 (*) 0.166	-0.230 0.176	0.793 (*) 0.120	0.700 (*) 0.176	0.092 0.134
Constant	-0.781 (*) 0.198	4.036 (*) 0.325	3.960 (*) 0.385	-4.532 (*) 0.207	-0.405 0.378	-4.609 (*) 0.288

Notes: (a) denotes the reference variable. *, **, and *** denote statistical significance at 0.01, 0.05, and 0.1. Standard errors are in parentheses.

Table 3: Tests for the Binomial *Logit* Model

	DP (compared with)					JC (compared with)				TE (compared with)			PEP (compared with)		BT (compared with)
	NP	JC	TE	PEP	BT	NP	TE	PEP	BT	NP	PEP	BT	NP	BT	PEP
Observations (N)	152962	18995	7100	7964	6898	161129	15267	16131	15065	149234	4236	3170	150098	4034	149032
TG N	5414	5414	5414	5414	5414	13581	13581	13581	13581	1686	1686	1686	2550	2550	1484
TG %	3.53	28.5	76.25	67.98	78.49	8.43	88.96	84.19	90.15	1.13	39.8	53.19	1.70	63.21	1.0
CG N	147548	13581	1686	2550	1484	147548	1686	2550	1484	147548	2550	1484	147548	1484	147548
CG %	96.46	71.5	23.75	32.02	21.51	91.57	11.04	15.81	9.85	98.87	60.2	46.81	98.3	36.79	99.0
Pseudo-R^2 (%)	12.39	30.82	18.15	16.17	13.4	4.09	29.08	16.87	23.65	14.17	25.42	21.16	5.49	9.5	7.04
$LR\chi^2(26)$	5800.53 (0.000)	6996.3 (0.000)	1412.82 (0.000)	1615.23 (0.000)	962.57 (0.000)	3812.3 (0.000)	3085.14 (0.000)	2375.29 (0.000)	2293.2 (0.000)	2617.47 (0.000)	1447.43 (0.000)	927.19 (0.000)	1418.12 (0.000)	504.02 (0.000)	1170.63 (0.000)
Log-Likelihood	-20506.034	-7853.9152	-3185.3842	-4185.9202	-3110.3723	-44678.769	-3761.527	-5853.1116	-3701.149	-7926.3089	-2123.7195	-1727.2436	-12210.904	-2401.6096	-7731.6506
CPR_{TG} (%)	67.53	75.66	72.63	69.04	66.01	64.44	78.28	73.85	76.57	65.3	65.54	63.76	63.84	62.78	70.15

Notes: Subscripts TG and CG denote treatment and control groups, respectively. CPR is the correction prediction rate for participants

Table 4: Observations Lost Due to the Common Support Condition

			Treatment Group					
			NP 147548	DP 5414	JC 13581	TE 1686	PEP 2550	BT 1484
Comparison Group	NP	Observations lost		1	1	0	0	0
		(in percentage)		0.02	0.01	0.00	0.00	0.00
		Observations after matching		5413	13580	1686	2550	1484
	DP	Observations lost	2550		566	0	1	1
		(in percentage)	1.73		4.17	0.00	0.04	0.07
		Observations after matching	144998		13015	1686	2549	1483
	JC	Observations lost	33	16		3	2	6
		(in percentage)	0.02	0.30		0.18	0.08	0.40
		Observations after matching	147515	5398		1683	2548	1478
	TE	Observations lost	3459	22	333		119	3
		(in percentage)	2.34	0.41	2.45		4.67	0.20
		Observations after matching	144089	5392	13248		2431	1481
	PEP	Observations lost	286	104	12	74		5
		(in percentage)	0.19	1.92	0.09	4.39		0.34
		Observations after matching	147262	5310	13569	1612		1479
	BT	Observations lost	685	9	1539	24	2	
		(in percentage)	0.46	0.17	11.33	1.42	0.08	
		Observations after matching	146863	5405	12042	1662	2548	

Table 5: Matching Quality

Comparison Group			Treatment Group: NP		DP		JC		TE		PEP		BT	
			Before	After	Before	After	Before	After	Before	After	Before	After	Before	After
	NP	MSAB			18.91	1.64	10.02	1.14	30.29	1.27	13.95	1.64	17.03	2.79
		Pseudo-R2			12.5	0.2	4.1	0.1	14.0	0.2	5.5	0.2	7.1	0.5
		Log-Like			5866.21	30.06	3817.92	52.84	2580.09	8.95	1432.46	15.79	1182.54	18.52
		P>chi			(0.000)	(0.265)	(0.000)	(0.001)	(0.000)	(0.999)	(0.000)	(0.921)	(0.000)	(0.856)
	DP	MSAB	18.91	5.34			25.44	4.63	23.75	2.48	18.41	2.97	13.41	2.78
		Pseudo-R2	12.5	1.3			31.0	1.5	29.1	0.5	16.9	0.7	23.8	0.7
		Log-Like	5866.21	5174.68			7036.10	549.71	3091.85	25.32	2384.7	48.11	2303.35	27.22
		P>chi	(0.000)	(0.000)			(0.000)	(0.000)	(0.000)	(0.501)	(0.000)	(0.004)	(0.000)	(0.398)
	JC	MSAB	10.02	1.34	25.44	3.06			36.44	2.71	18.93	3.33	26.04	3.38
		Pseudo-R2	4,1	0.1	31.0	0.6			18.2	0.7	16.1	0.6	13.3	0.8
		Log-Like	3817.92	460.32	7036.10	90.06			1419.8	31.8	1611.15	42.82	957.78	31.28
		P>chi	(0.000)	(0.000)	(0.000)	(0.000)			(0.000)	(0.200)	(0.000)	(0.000)	(0.000)	(0.218)
	TE	MSAB	30.29	6.24	23.75	5.02	36.44	8.43			30.63	4.41	27.23	2.22
		Pseudo-R2	14.0	2.6	18.2	1.4	29.1	4.4			25.5	1.4	21.1	0.3
		Log-Like	2580.09	10511.49	1419.8	208.14	3091.85	1619.22			1449.89	92.19	926.4	12.83
		P>chi	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)			(0.000)	(0.000)	(0.000)	(0.985)
	PEP	MSAB	13.95	2.42	18.41	3.81	18.93	3.78	30.63	4.67			13.24	3.74
		Pseudo-R2	5,51	0.4	16.1	0.9	16.9	0.8	25.5	1.4			9.5	0.8
		Log-Like	1432.46	1715.4	1611.15	134.35	2384.7	301.17	1449.89	64.17			505.01	31.53
		P>chi	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)			(0.000)	(0.172)
	BT	MSAB	17.03	6.94	13.41	3.75	26.04	8.55	27.23	6.01	13.24	4.73		
		Pseudo-R2	7.1	1.8	13.3	1.0	23.8	2.3	21.1	2.2	9.5	1.4		
		Log-Like	1182.54	7282.18	957.78	152.92	2303.35	774.77	926.4	101.25	505.01	102.08		
		P>chi	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		

Table 6: Average Registered Unemployment, Before and After Treatment

Time Period	$\Delta^{\acute{ATT}}_{Mt'}$	Time Period	$\Delta^{\acute{ATT}}_{Mt}$

Time Period - 1

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		6.2%	-1.1%	5.9%	7.6%	17.5%
DP	-7.6%		0.7%	0.7%	-1.4%	5.7%
JC	-9.6%	-10.9%		-6.2%	-2.8%	9.9%
TE	-11.2%	-5.9%	-8.4%		-4.6%	7.2%
PEP	-7.5%	-1.6%	0.8%	2.2%		9.3%
BT	-15.1%	-10.9%	-6.4%	-9.0%	-12.2%	

Time Period 1

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-24.0%	14.2%	-39.1%	21.5%	-28.2%
DP	28.4%		48.9%	-18.6%	51.0%	-3.7%
JC	-15.2%	-39.5%		-51.6%	4.3%	-43.8%
TE	44.9%	13.5%	67.4%		69.4%	13.8%
PEP	-22.3%	-49.8%	-5.6%	-65.8%		-52.7%
BT	26.6%	2.1%	43.1%	-14.5%	54.9%	

Time Period - 2

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-1.5%	-7.5%	0.2%	-1.1%	12.8%
DP	-2.1%		2.5%	1.3%	-4.4%	8.9%
JC	-2.6%	2.7%		7.7%	-3.5%	13.7%
TE	-16.5%	-9.1%	-13.1%		-15.1%	4.1%
PEP	-0.8%	0.4%	6.8%	-0.1%		11.0%
BT	-11.6%	-8.7%	-7.7%	-3.9%	-13.9%	

Time Period 2

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-12.7%	15.1%	-7.0%	18.5%	2.4%
DP	17.2%		36.2%	0.8%	35.2%	14.0%
JC	-13.3%	-20.3%		-16.5%	4.2%	-12.2%
TE	11.7%	-4.6%	33.3%		29.5%	5.7%
PEP	-20.0%	-29.2%	-3.7%	-27.3%		-19.0%
BT	5.9%	-11.1%	24.5%	-7.5%	21.2%	

Time Period - 3

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-5.1%	-39.4%	-4.2%	-12.3%	-2.3%
DP	3.2%		-21.3%	-0.2%	-6.6%	0.3%
JC	25.1%	14.6%		14.1%	13.1%	20.4%
TE	3.5%	-2.4%	-33.3%		-6.7%	-0.1%
PEP	11.6%	4.2%	-10.2%	1.4%		3.9%
BT	2.1%	-2.9%	-25.9%	1.0%	-10.2%	

Time Period 3

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-6.4%	10.5%	-2.3%	12.9%	1.5%
DP	12.8%		24.5%	0.8%	23.8%	5.2%
JC	-6.8%	-10.7%		-6.2%	3.8%	-4.1%
TE	3.7%	-4.5%	22.1%		14.8%	0.2%
PEP	-12.5%	-16.4%	-3.1%	-12.3%		-13.4%
BT	7.1%	-7.6%	14.9%	-5.4%	16.0%	

Time Period - 4

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-5.7%	-26.3%	-6.7%	-11.7%	-5.1%
DP	2.2%		-14.3%	-2.0%	-3.3%	-1.1%
JC	17.0%	8.3%		6.2%	5.8%	9.9%
TE	6.0%	-0.3%	-14.2%		-3.0%	-1.5%
PEP	7.9%	2.3%	-5.2%	-3.0%		-1.2%
BT	5.6%	-0.1%	-13.5%	1.2%	-3.8%	

Time Period 4

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-4.6%	17.1%	1.7%	13.1%	7.0%
DP	9.1%		25.4%	4.2%	21.5%	9.0%
JC	-11.5%	-8.6%		-4.1%	3.4%	-2.6%
TE	0.1%	-5.3%	23.8%		12.1%	2.8%
PEP	-17.0%	-16.5%	2.0%	-12.9%		-10.5%
BT	0.8%	-8.0%	15.0%	-2.6%	9.0%	

Time Period - 5

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-1.2%	-12.4%	-4.1%	-3.5%	-3.6%
DP	-1.7%		-7.8%	-2.0%	0.0%	0.7%
JC	6.2%	2.4%		1.7%	3.8%	3.3%
TE	1.1%	-2.5%	-5.3%		-0.8%	2.1%
PEP	1.0%	-1.3%	-3.1%	-4.2%		-3.2%
BT	0.5%	-2.7%	-6.7%	-1.3%	-3.3%	

Time Period 5

Comparison Group \ Treatment Group	NP	DP	JC	TE	PEP	BT
NP		-6.6%	-8.4%	-8.0%	-2.5%	-6.6%
DP	13.8%		5.6%	2.3%	9.8%	3.1%
JC	8.1%	-2.9%		-0.6%	5.0%	0.3%
TE	10.4%	0.4%	7.0%		6.5%	0.9%
PEP	1.4%	-7.3%	-5.2%	-7.1%		-3.2%
BT	8.7%	-1.7%	0.5%	-3.9%	6.0%	

Table 7: Results of the CDiD Estimator - ATT in Terms of Registered Unemployment

$\Delta^{\acute{ATT}}_{Mt} - \Delta^{\acute{ATT}}_{Mt'}$ (t'=t)

Time Period	Comparison Group	NP	DP	JC	TE	PEP	BT
		Treatment Group					
1	NP		-30.2%	15.4%	-45.0%	13.9%	-45.7%
	DP	36.0%		48.1%	-19.3%	52.4%	-9.4%
	JC	-5.6%	-28.5%		-45.4%	7.1%	-53.8%
	TE	56.1%	19.3%	75.8%		74.0%	6.6%
	PEP	-14.8%	-48.1%	-6.4%	-68.0%		-62.0%
	BT	41.8%	13.0%	49.6%	-5.5%	67.2%	
2	NP		-11.2%	22.6%	-7.2%	19.6%	-10.4%
	DP	19.2%		33.7%	-0.5%	39.5%	5.1%
	JC	-10.7%	-23.0%		-24.1%	7.6%	-25.8%
	TE	28.3%	4.5%	46.4%		44.5%	1.7%
	PEP	-19.2%	-29.6%	-10.5%	-27.2%		-30.0%
	BT	17.6%	-2.3%	32.2%	-3.7%	35.1%	
3	NP		-1.3%	49.9%	1.9%	25.2%	3.8%
	DP	9.5%		45.9%	1.1%	30.4%	4.9%
	JC	-32.0%	-25.3%		-20.3%	-9.3%	-24.5%
	TE	0.2%	-2.2%	55.5%		21.6%	0.3%
	PEP	-24.1%	-20.6%	7.1%	-13.7%		-17.2%
	BT	5.0%	-4.8%	40.8%	-6.4%	26.1%	
4	NP		1.1%	43.4%	8.4%	24.8%	12.1%
	DP	7.0%		39.7%	6.2%	24.8%	10.0%
	JC	-28.5%	-16.9%		-10.3%	-2.4%	-12.5%
	TE	-5.9%	-5.0%	38.0%		15.0%	4.3%
	PEP	-24.9%	-18.8%	7.1%	-9.9%		-9.3%
	BT	-4.8%	-7.9%	28.4%	-3.8%	12.8%	
5	NP		-5.4%	3.9%	-3.9%	0.9%	-3.0%
	DP	15.5%		13.5%	4.3%	9.8%	2.4%
	JC	1.9%	-5.3%		-2.3%	1.2%	-3.0%
	TE	9.3%	2.8%	12.3%		7.3%	-1.2%
	PEP	0.5%	-6.1%	-2.1%	-2.9%		0.1%
	BT	8.3%	1.0%	7.2%	-2.5%	9.3%	

$\Delta^{\acute{ATT}}_{Mt} - \Delta^{\acute{ATT}}_{Mt'}$ (t'=-1)

Time Period	Comparison Group	NP	DP	JC	TE	PEP	BT
		Treatment Group					
1	NP		-30.2%	15.4%	-45.0%	13.9%	-45.7%
	DP	36.0%		48.1%	-19.3%	52.4%	-9.4%
	JC	-5.6%	-28.5%		-45.4%	7.1%	-53.8%
	TE	56.1%	19.3%	75.8%		74.0%	6.6%
	PEP	-14.8%	-48.1%	-6.4%	-68.0%		-62.0%
	BT	41.8%	13.0%	49.6%	-5.5%	67.2%	
2	NP		-18.9%	16.3%	-12.9%	10.9%	-15.1%
	DP	24.7%		35.5%	0.2%	36.6%	8.3%
	JC	-3.6%	-9.4%		-10.2%	6.9%	-22.1%
	TE	22.9%	1.3%	41.7%		34.0%	-1.5%
	PEP	-12.5%	-27.6%	-4.5%	-29.5%		-28.3%
	BT	21.1%	-0.2%	31.0%	1.4%	33.4%	
3	NP		-12.7%	11.6%	-8.2%	5.2%	-16.0%
	DP	20.3%		23.8%	0.2%	25.2%	-0.5%
	JC	2.8%	0.3%		0.0%	6.6%	-14.0%
	TE	14.9%	1.3%	30.5%		19.4%	-7.0%
	PEP	-5.0%	-14.8%	-3.9%	-14.6%		-22.7%
	BT	22.3%	3.3%	21.3%	3.6%	28.2%	
4	NP		-10.8%	18.2%	-4.2%	5.4%	-10.5%
	DP	16.7%		24.7%	3.5%	22.9%	3.3%
	JC	-1.8%	2.3%		2.1%	6.2%	-12.6%
	TE	11.3%	0.6%	32.2%		16.6%	-4.5%
	PEP	-9.5%	-14.9%	1.2%	-15.1%		-19.8%
	BT	15.9%	2.8%	21.4%	6.4%	21.3%	
5	NP		-12.9%	-7.3%	-13.9%	-10.2%	-24.1%
	DP	21.3%		4.9%	1.6%	11.2%	-2.6%
	JC	17.7%	8.0%		5.6%	7.8%	-9.6%
	TE	21.6%	6.2%	15.4%		11.0%	-6.3%
	PEP	8.9%	-5.7%	-5.9%	-9.3%		-12.5%
	BT	23.9%	9.2%	6.9%	5.1%	18.3%	

Figure 2: ATT Compared to the NP Treatment State

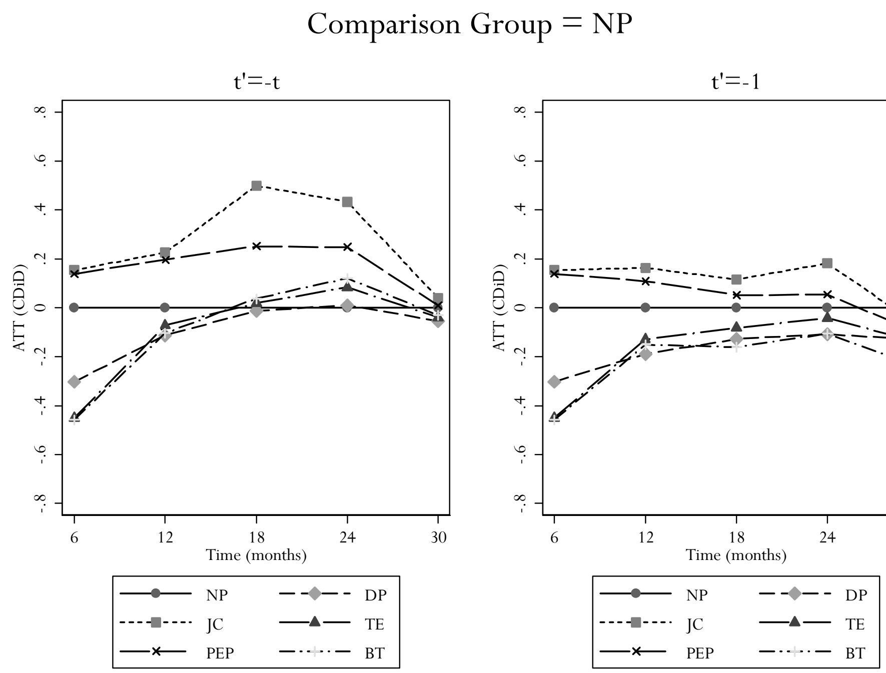

Figure 3: ATT Compared to the DP Treatment State

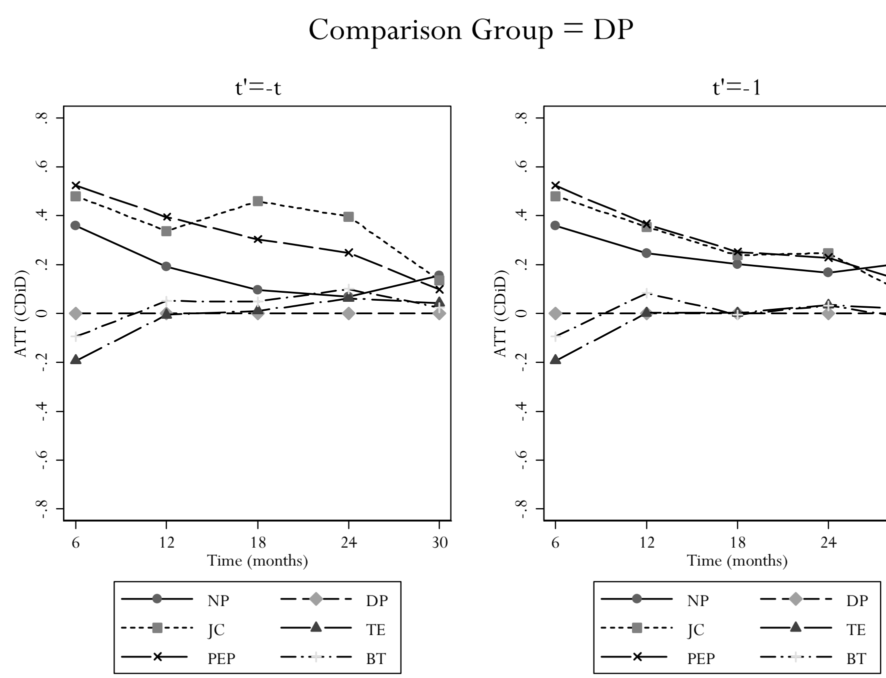

Figure 4: ATT Compared to the JC Treatment State

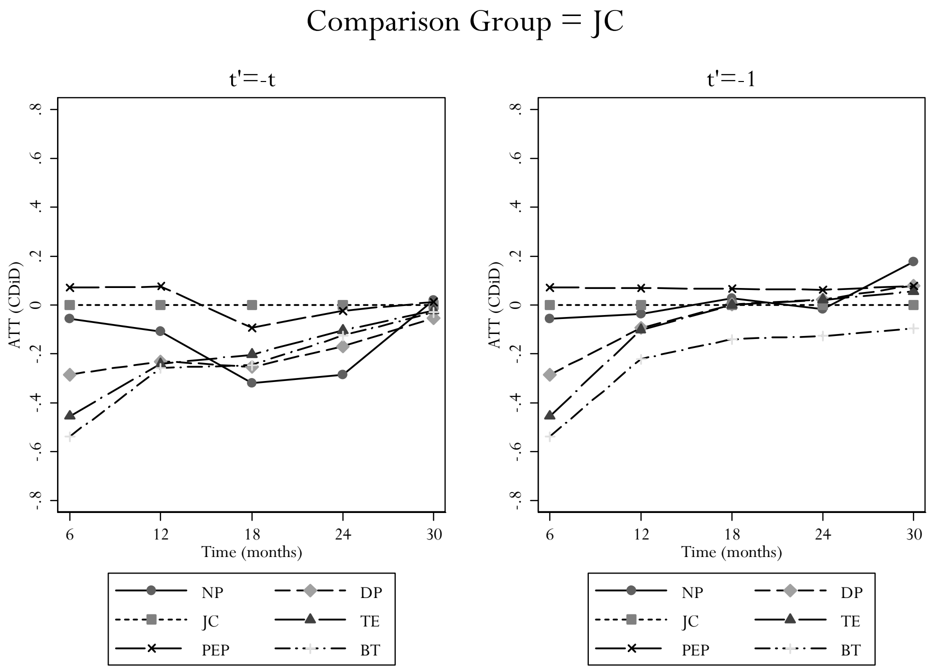

Figure 5: ATT Compared to the TE Treatment State

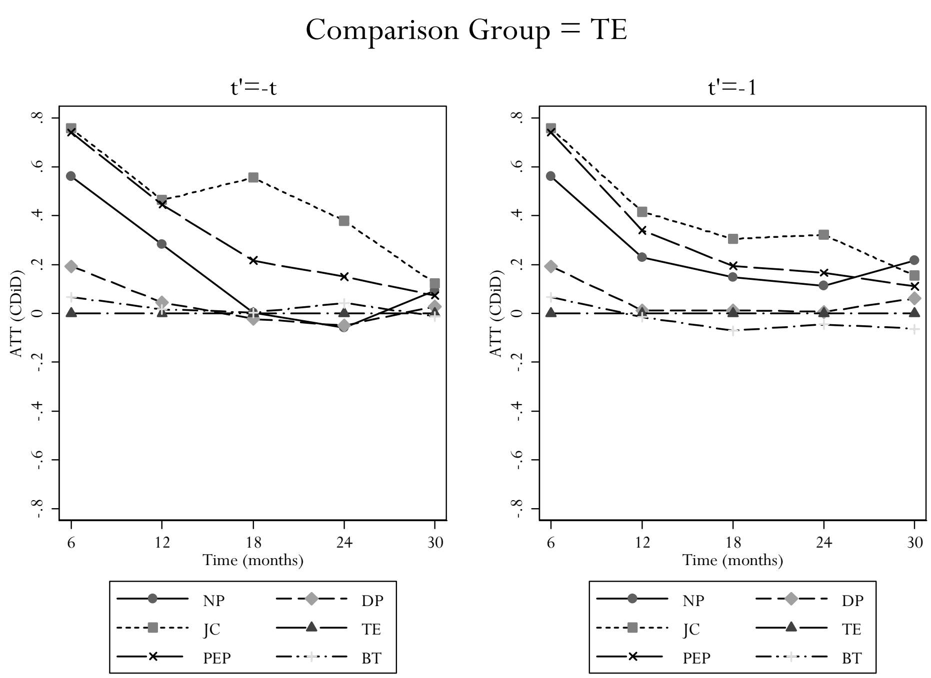

Figure 6: ATT Compared to the BT Treatment State

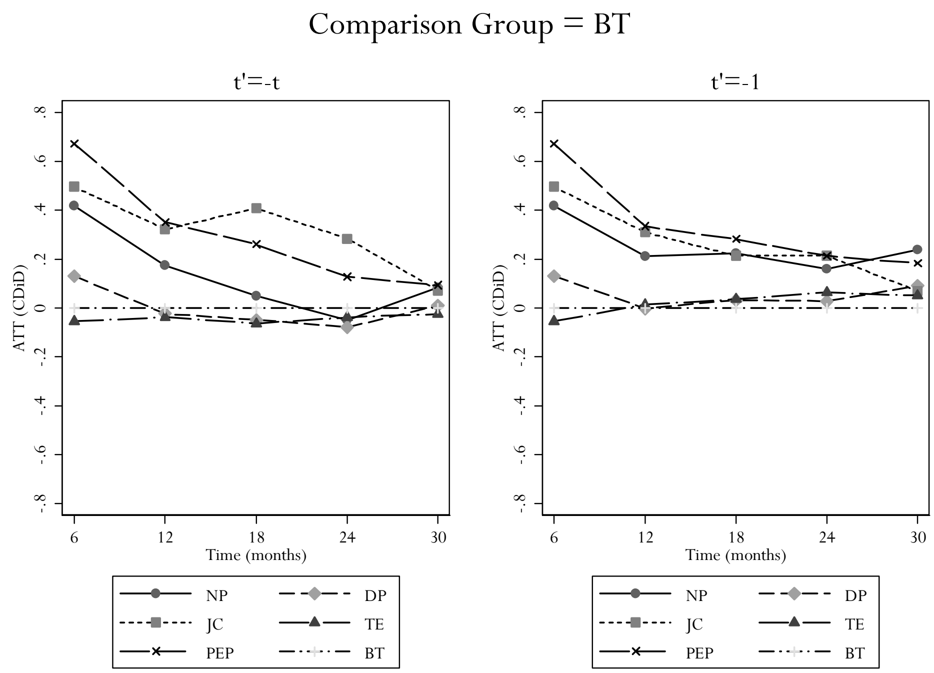

Figure 7: ATT Compared to the PEP Treatment State