# Evaluating the effects of short and medium-term temporary work reduction schemes: the case of Spain's ERTEs during the COVID-19 outbreak

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# Evaluating the effects of short and medium-term temporary work reduction schemes: the case of Spain's ERTEs during the COVID-19 outbreak *

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**Abstract**

This paper presents an average treatment effect analysis of the Spain's furlough program during the onset of the COVID-19 pandemic, using propensity score matching techniques. Merging 2020 labor force quarterly microdata, we find that the probability to be re-employed after treatment was significantly higher among the treated (furlough granted group) than in the control group (comparable non-furloughed individuals who lost their job). These results seem to be robust across models, having tested a wide range of matching specifications. Furthermore, we also explore if the Spanish furlough scheme had an uniform impact across regions with different industrial structures, concluding that the furlough participation was positively related to the probability of re-employment across any region and economic activity. Nevertheless, a different time arrangement did affect the magnitude of the effect, suggesting that it may decrease with the furlough duration. This finding advises against long lasting schemes under persistent recessions, notwithstanding, short time work schemes still stand as useful strategies to face essentially transitory adverse shocks.

- **JEL:** J08, J38, J65, J68.
- **Keywords:** Furlough, short-time work, ERTE, propensity score matching, Covid-19, Spain.

## Highlights

- We present a novel contribution that assesses the causal effects of furlough programs using updated microdata from the pandemic period in Spain.
- As a key finding we found evidence of a robust and positive average effect of furlough schemes in the probability of being re-employed at the short-term. In addition, this effect was widespread and close in magnitude across both regions and industrial sectors.
- However, this positive effect was time dependent and lessened when the furlough scheme was held in time for two consecutive quarters. This result suggests effectiveness losses in the medium-term and advises against long-lasting schemes.

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# Declarations

## Availability of data and materials

The datasets generated and analysed during the current study will be available soon in the corresponding author´s website `https://javier-garcia-clemente.weebly.com/materials.html`, or alternatively, upon reasonable request.

## Competing interests

The authors declare that they have no competing interests.

## Funding

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## Authors’ contributions

EC devised the project, the conceptual ideas and proof outline. JGC managed the data, performed the computations and worked on the manuscript. NR aided in developing the analysis and interpreting the results, and drafted the core of the manuscript. Finally, all authors discussed the results and contributed to the final manuscript.

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# 1. Introduction

The Covid-19 outbreak caused an unprecedented sanitary crisis all over the world, forcing the governments to implement restrictive measures as mandatory lockdown and social distancing. Concerned about a boost in unemployment digits, most countries devised portfolios of Coronavirus job retention schemes as a way to temporarily protect the employees' positions meanwhile the labor market were adjusting to the shock. Although we can find differences in the eligibility requirements, the degree of coverage, and the duration, these national temporary workforce reduction programs share a considerable number of characteristics.[1]

In this paper, we evaluate the impacts of the Spanish furlough program, the so-called ERTEs (temporary employment adjustment schemes). Spain, is presented as a suitable case of study due to the kind of labor adjustment suffered during the previous Great Recession. In this country, the labor adjustment has predominantly had an extensive nature, where collective layoffs were the usual. As a result, no euro country, with the Greece exception, destroyed more jobs than Spain then, which reached an unemployment rate of 26.94% in the first quarter of 2013. Even though the ERTE mechanism already exists by that time (*art. 47, Estatuto de Trabajadores, 1980*), it was only during the pandemic that such policy tool saw wider application, covering around 3 million workers (more than 20% of the affiliated workers) in the second quarter of 2020.[2] The following quarters, it covered around 5% of the affiliated workers, which is still a significantly higher proportion than it was during the previous recession. It happened because in mid-March 2020, convinced about the transitory nature of the sanitary crisis, the Spanish government quickly entrusted the labor adjustment to fast and wide-coverage Covid-19 related ERTEs (*RD-Ley 8/2020, del 17 de marzo*), encouraging the use of these schemes and imposing penalties to companies which after being granted were dismissing employees within the next 6 month. This policy, essentially consists in a temporary suspension of the labor relationship between the employer and the employee, or alternatively, a reduction of working hours, justified by a major cause. This cause must be related to economic, technical, organizational or production issues, including Covid related consequences from March, 2020. During this period of suspension, the employee is getting a social security allowance while the employer only has to assume a social contribution, which is a minor part of the employee's wage that sometimes might be even relieved or discharged. As a result, it works as a transitory mechanism of flexibility to adjust the labor market, whose cost is essentially assumed by the public administration. We may say that the main purpose of these furlough schemes is to maintain the employees' position despite not being working, avoiding a sharp boost of unemployment during the shock.

Since its first approval on March 17th, 2020, the expiration date has been postponed several times, remaining in the current legislation and expecting to be redesigned as a permanent employment strategy in the next labor reform. For this reason, some evaluation of the impact of this policy in all dimensions is urgently needed to improve the design of these programs for the next future. In addition, we cannot think of a better testing ground for these schemes than the current pandemic scenario, where the shock is strictly exogenous and eminently transitory, and the furlough take-up rate is unprecedented.

To sum up, with the aim to tackle that task, this analysis assess the question of what has been the effect of the Spain´s ERTE program in the employees follow-up labor outcomes, turning, as far as we know, into the first causal evaluation of these schemes using updated microdata from the pandemic at the individual level. Therefore, our contribution comes from the novelty of the context we have considered, finding evidence of positive effects for furloughed employees on their re-employment prospects at the short-term, but strongly conditioned by the duration of the furlough spell.

[1] In this context, furlough schemes and short-time work are the most common terms to refer to these job retention programs but we will use any term equally along this paper. Similar programs in other countries are called *Furlough schemes* in the UK, *Kurzabeit* in Germany, *Activité partielle* in France, and *Cassa Integrazione Guadagni* in Italy. A detailed description about these schemes and their use across Europe can be found in Drahokoupil and Müller (2021).

[2] Social Security official registers (click to visit website).

The rest of the paper is organized as follows: section 2 introduces the reader to the main related literature; section 3 contains a detailed description of the data and the sample selection procedure; section 4 focus on the methodological and technical aspects of the analysis; in section 5 we present our main results for the furlough treatment effects together with multiple robustness checks; while section 6, offers additional results for a more dis-aggregated level, accounting for Spanish regions and sectors; and finally, our concluding remarks are summarized in section 7.

## 2. Related literature

Overall, most of the reviewed literature consider that temporary work reduction schemes –also short-time work (STW) or furloughs– may have a positive impact on labor outcomes, but this impact is often conditioned by the nature of the shock, the labor market features and the characteristics of the scheme itself. Furthermore, there is a general concern about the implications of these programs on the labor market efficiency.

The debate about the introduction of some sort of furlough programs as an alternative to layoffs dates back to the 80s (Burdett and Wright, 1989; Fitzroy and Hart, 1985) and long ago, authors like Abraham and Houseman (1994) remarked the more equitable nature of STW arrangements since they are able to spread the costs of labor adjustment across the workforce, rather than on a small number of workers, as a classical layoff strategy would do.

From a theoretical perspective, STW may complement the Unemployment Insurance programs (UI), but as UI, they are not free from introducing distortions in the labor market, mainly via moral hazard issues and hindering reallocation. However, a excess of layoffs during an adverse shock might be inefficient too, but this unwanted effect could be attenuated by using these schemes to maintain valuable labor matches during temporary recessions Giupponi et al. (2022).

Another matter of concern is the stabilization power of STW on the aggregate demand. In this regard, using a New Keynesian macroeconomic model, Dengler and Gehrke (2021) have found that, by reducing the unemployment risk of workers, the precautionary savings motive is mitigated, and so do the aggregate demand fall during recessions.

So far, most empirical literature have focused on the effects of STW during the Great Recession, with many authors using this period to conduct research from specially a country level, but also from a micro approach. Probably, one of the most comprehensive assessments comes from the work of Hijzen and Venn (2011), who made use of data from 19 OECD countries to identify causal effects via a differences-in-differences approach. Their findings suggest the program effectiveness on job preservation, specially for Germany and Japan, but heterogeneous effects were found across countries and type of contracts. Afterward, Hijzen and Martin (2013) points out that timing might be crucial as the positive net effect of furloughs might be nonlinear with respect to subsequent re-employment and job creation, so much so that the positive causal relationship might turn, in due time, to negative. With some external validity granted by the methodological approach used by the authors, who create a panel of 23 OECD countries ranging from the first quarter of 2004 to the last of 2010, our article tackles the timing issue by considering a robustness check which extends the ERTE period of analysis, considering individuals who have been furloughed during two consecutive quarters too. Other remarkable contribution can be found in Cahuc and Carcillo (2011), where the authors instrument the STW take-up to evaluate their potential benefits in the onset of the Great Recession using a macro approach with a large sample of OECD countries. Boeri and Bruecker (2011), follow a similar strategy for the main analysis, but include a firm level approach using German data. As a result, both studies agree in the potential benefits of the furlough schemes, but warn about the inefficiencies which may appear with a deficient design.

However, Kruppe and Scholz (2014) did not find significant employment preserving effect of the German STW using establishment data from the 2008-2010 period. Again from the firm perspective, Biancardi et al. (2022) have recently explored the Italian STW effects on firm performance during the financial crisis, paying special attention to the role of unions in this regard. They show that a more intensive use of STW reduced labor costs and productivity per employee, with no effects on hourly productivity and negative but small effects on firm´s profits in the short-term, finding heterogeneous effects by the union power from firm to firm. Likewise, other analysis at the micro level emphasize on the characteristics of companies which take-up STW and the impact on their layoff strategies using data from different countries, covering once again the Great Recession period: see Lydon et al. (2019) for EU countries, Crimmann et al. (2012) for Germany, Kato and Kodama (2019) for Japan, Kopp and Siegenthaler (2021) for Switzerland, Calavrezo and Lodin (2012) for France and Pavlopoulos and Chkalova (2019) for the Netherlands.

On the other hand, Osuna and García-Pérez (2015) focused, as we do, on the Spanish case, but find out that the intensity of subsidies to payroll taxes, which ultimately fall on the workers as a direct salary reduction, might amplify or dampen re-employment and job creation/destruction. In line with previous literature findings, the authors confirm the crucial role of the policy design in order to generate positive effects on labor market outcomes and document a reduction of the degree of segmentation of labor markets in Spain. Following the basis of their previous work, Osuna and Pérez (2021) have recently reexamined the efficiency of short-time work schemes during the COVID-19 crisis, pointing again at the policy design as the key to success. However, a trade-off between maximizing the job preservation and minimizing the deadweight costs and fiscal deficit arise when moving from generous to moderate schemes. The work of Arranz et al. (2018), focus on the STW participation rate extending the analysis to one more GIIPS country other than Spain, Italy. As they establish a comparison between the short-time work scheme take-up rates of the 1990s crises compared to the 2000s crises, the authors look at the workers' profiles to establish which categories are more willing to accept being furloughed. They show that, everything else equal, if the labor markets in the 2000s had the same workforce composition of those same markets in the 1990s, we would have assisted to a higher take-up rate due to the higher weight of older, less educated workers, suggesting that policy making should indeed focus on adjusting new short term work policies with a very close eye on the evolution of workforce composition.

Recently, Cahuc et al. (2021) has proved that the impact of Short time work in France has indeed been necessary to save jobs and balance-out strong asymmetric shocks suffered by the firms more exposed to the previous crisis, furthermore showing that, although hampered by the existence of windfall effects, STW prove to be still more cost-efficient at saving jobs than any kind of subsidy. Working on the same period, Giupponi and Landais (2020) assess the effects of Italian STW schemes in a comprehensive analysis from both firm and worker level approaches. They found large and positive effects on headcount employment, arguing in favor of welfare enhancing effects specially under temporary shocks, when liquidity constrains and labor market rigidities generate an inefficient excess of layoffs in the firms. This time dependent and non-linear effects have been described in Gehrke and Hochmuth (2021) too, despite using a different macro approach with German data.

In general, whatever the case of the context, the current general agreement on short terms work schemes and policy intervention is that furloughs, under certain conditions, might have a positive net effect on the economy (Cahuc, 2019) and be of great help in the immediate proximity of the current Covid-19 crisis not just in strict market terms but also in terms of potential welfare gains, given the appropriate reforms (Müller and Schulten, 2020). An exception to the rule can be found in a recent contribution of Arranz et al. (2020). Similarly as we do, they use propensity score matching techniques to evaluate the impact of Spain´s furloughs on the subsequent labor status of workers. Nevertheless, the data and period covered is not the same. For the analysis, the authors use longitudinal administrative data for the 2008 recession period and focus on the worker specifically within-firm persistence, unexpectedly finding that treated individuals were less likely to remain working with the same employer later on.

To the best of our knowledge, most recent contributions in literature from the Covid-19 period appear to have much focused on the health related effects of layoffs and other cut off measures, leaving the matter of deciding what consequential effect such policy tools would have on immediate future market outcomes untouched. However, the presumably exogenous and transitory nature of this adverse shock turns the current context into the best case scenario to test the validity of the work reduction programs. Our study fills up this gap by carrying out a national and regional analysis of the effect of Spain´s ERTEs on re-employment probability across 2020. In order to do so, Spanish Labor Force Survey microdata have been filtered to derive a database of salaried workers who have been matched in order to calculate the average effect of being on ERTE on follow-up labor market outcome to prove whether or not being furloughed increases or decreases the likelihood of successful employment. To complete the analysis, we match the propensity score tests with a weighted regional level analysis to look for the marginal effect of the ERTE on employment probability in a logistic model.

## 3. Data

Traditionally, administrative data is the usual source to carry out this type of analysis, nonetheless, since there is an important delay in its provision, we decided to take advantage of the quarterly flow microdata 2020/q1 to 2020/q4 of the Spanish Labor Force Survey (henceforth SLFS) to perform our analysis. This survey is conducted by the National Statistical Institute and is a large household sample survey providing results on labor participation of people aged 16 and over as well as people outside the labor force in which each sampled individual remaining in the survey for a period of six quarters at a time, with no resampling after individuals are rotated out of the sample. The Survey is targeted at a rotating sample of around 60,000 households throughout the national territory. For every household member, both socioeconomic and labor information is collected in order to summarize the main characteristics of the Spanish workforce each quarter. As mentioned, individuals in the sample are interviewed for six consecutive quarters, thus we have information on quarterly labor transitions for a maximum period of 18 months for each individual in the sample.[3]

As we look for a way to rearrange our data for our initial matching analysis we partly followed the intuition of Izquierdo et al. (2021), choosing to identify observations and participants by selecting those that would consecutively appear in the first three quarters of 2020, have been getting paid work during the first quarter of 2020, and can be identified and divided for treatment along the second quarter, considering those who had lost their job during such period and those who were full-time furloughed.[4] A binary outcome was finally generated to identify the objective variable for the third quarter of 2020, pointing at those who would have been working during that period non-conditional on the fact that the furlough period might have ended up with the individual reincorporated in its former job or finally getting a new one. The flowchart displayed in figure 1 illustrates the data selection procedure employed for the matching analysis. Therefore, in our final database for this analysis each observation represents an individual who stay at least the 3 initial quarters of 2020 at the sample, was employed in the 1st quarter, was either furloughed (treatment group) or displaced/jobless (control group) in the 2nd quarter, and whose employment status was observed in the 3rd quarter (outcome=1 if he/she have been re-employed, outcome=0 otherwise, e.g. still jobless or furloughed). Then, we have an identifier for each individual, a treatment dummy indicating the furloughed in 2nd quarter, an outcome dummy indicating the re-employment status in the 3rd quarter, and the observable pre-treatment characteristics of each individual, thus, taken their values in the 1st quarter. Note the database keep a cross-section structure because each observation represents a single individual with their 1st quarter characteristics and the time dimension was only used for the treatment assignment and the outcome generation.

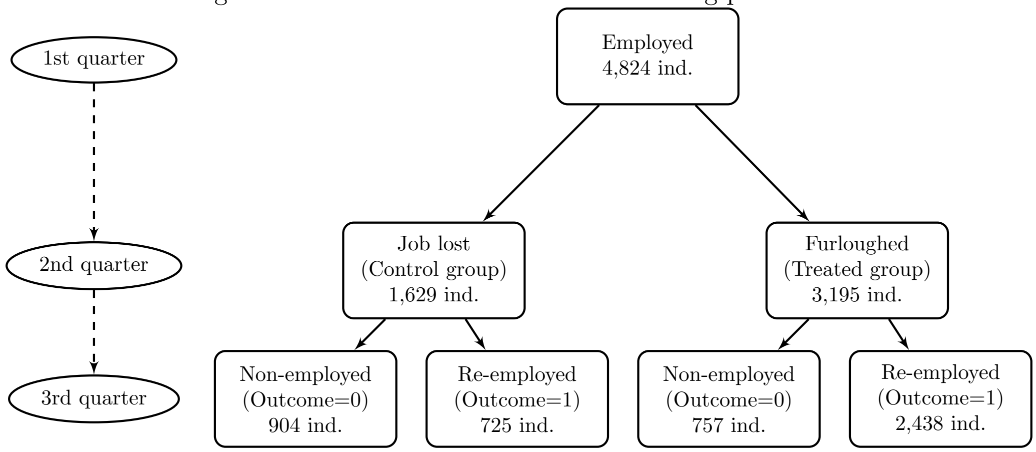
Finally, population weights included in the SLFS for the representative interview were not included

[3] For further methodological information of the survey or data access visit INE website.

[4] Note in the control group we are considering individuals who stopped working in the 2nd quarter, no matter whether they were unemployed, discouraged or potential workforce that quarter. Since lockdown and containment measures may have significantly hampered the employment active seeking process we made no distinction between these labor states, as suggested by the Spanish Central Bank aforecited report (Izquierdo et al., 2021).

in the matching analysis, due to obvious limitation in processing power. In the second part of the analysis, as we focus on a single step logistic regression, weights were included at the regional level but did not change the marginal effects of the probability results in any significant way.

Figure 1: Flowchart for the database filtering procedure.




# 4. Methodology

## 4.1. Potential outcomes framework

As mentioned, our treatment variable will be defined as follows:

$$D = \begin{cases} 1 \text{ if furloughed (treated) in the 2nd quarter} \\ 0 \text{ if non-furloughed (untreated) in the 2nd quarter} \end{cases} \tag{1}$$

And the outcome variable [5]:

$$y = \begin{cases} 1 \text{ if re-employed in the 3rd quarter} \\ 0 \text{ otherwise} \end{cases} \tag{2}$$

Now, our first step entails potential outcome creation, as we define:

$$\begin{aligned} y_{\alpha i} &= \text{potential value of the outcome for individual } i \text{ when furloughed} \\ y_{\omega i} &= \text{potential value of the outcome for individual } i \text{ when non-furloughed} \end{aligned} \tag{3}$$

Note that for a given individual $i$ their individual treatment effect would be the difference $ITE = y_{\alpha i} - y_{\omega i}$. However, both potential outcomes cannot be observed for the same individual as no one can be treated and untreated simultaneously.

In terms of expectations, the average treatment effect of the treated and the untreated can be respectively defined as:

$$ATT = E(y_\alpha - y_\omega | D = 1) = E(y_\alpha | D = 1) - E(y_\omega | D = 1) \tag{4}$$

$$ATU = E(y_\alpha - y_\omega | D = 0) = E(y_\alpha | D = 0) - E(y\omega | D = 0) \tag{5}$$

[5]As our outcome variable has a binary nature, we will be obtaining the potential outcome means in terms of the fraction of individuals who get re-employed (outcome=1) in both the furloughed (treated) and the non-furloughed (untreated) group. As a result, once the $ATE$ has been obtained, it can be interpreted as an increment in the probability of re-employment caused by the furlough scheme.

And the average treatment effect for the whole sample would be:

$$ATE = E[y_\alpha] - E[y_\omega] = \pi ATT + (1-\pi)ATU \tag{6}$$

being $\pi$ the proportion of treated individuals in the sample. This $ATE$ accounts for the hypothetical average effect of the treatment had everyone in the sample been furloughed.

Since the treatment assignment is not random the observed sample means difference ($E[y_\alpha|D = 1] - E[y_\omega|D = 0]$) would be biased and specific methods are needed to isolate causal effects. From now on these estimands ($ATT, ATU, ATE$) will be the target of our analysis.

## 4.2. Propensity score matching

The methodology we employ to look for an average treating effect is propensity score matching (PSM), based on the seminal contribution of Rosenbaum and Rubin (1983). This technique uses as identifying tools, granting the ceteris paribus condition, a number of observable and measurable control variables capable of capturing all the relevant differences between groups. [6]

Back to our matching procedure, it was executed based on the individual pre-treatment characteristics, hence using the 1st quarter values. The set of observable $X$ controls selected for the propensity score calculation can be classified in two categories: social-demographics and labor conditions. For the social-demographic dimension we considered a set of standard controls as sex, age, region and education. On the other hand, the labor economic dimension comprise the industry or economic activity where the individual was employed (by the 1-digit Spanish National Classification of Economic Activities), the type of contract (either temporary or permanent) and the type of working day (either part or full-time).

Our exogeneity pre-assumption will be based on the concept of conditional independence: once a set of observable variables able to capture all possible forms of heterogeneity has been identified, our identifying assumption, imply that once the covariate vector $X$ is fixed, the results of the potential outcome under no treatment would be the same for both the treated and the untreated individuals:

$$E[y_\omega|X, D = 1] = E[y_\omega|X, D = 0] \tag{7}$$

Similarly, and assuming once more the matrix of covariates $X$ has been identified correctly, we would want to see that the values of the potential outcome under treatment, once conditioned for the

[6] As a methodological digression we shall mention that there were alternative methods to control for the selection bias generated in the treatment assignment when it is not random. Typically, these methods which have been widely used with microdata in economic policy evaluations are Regression Discontinuity Design (RDD), Differences in Differences (DiD) and Instrumental Variables (IV). Indeed, RDD and DiD are more powerful techniques than PSM since they are able to control for unobserved factors. However, these methods cannot be used for this analysis because the nature of our sample and our treatment made it impossible: RDD needs a threshold rule in the continuous range of a certain variable which assign individuals either to the treated or untreated group (e.g. a grant which is given to the students when their incomes are lower than a threshold value); and DiD needs the outcome variable to be observed before and after treatment (e.g. analyzing wages before and after applying a minimum wage policy for similar pre-treatment individuals), which is not possible this time because, as we explained before, our outcome variable is generated after treatment by definition, this is, considering whether the individual have been re-employed after being furloughed or not. On the other hand the IV method makes use of instruments in a first step regression to estimate the treatment variable, leading to a second regression where the outcome is estimated considering the previous step. Hence, this technique does not substantially differ from PSM since both focus on modelling the treatment through a previous step, using observed controls that may affect the treatment assignment. Since the scheme was widespread during the pandemic, it is hard to find a credible exogenous source of variation to use as instrument for the scheme take-up. In addition, classical approaches as OLS regressions are not suitable for our outcome variable since it is binary, therefore a discrete choice regression as the logit estimation we performed at the end of the paper would fit better our data. All in all, we consider that PSM is good enough to infer causality in this particular situation because it fits perfectly the nature of our data and most importantly because the treatment assignment should not be affected by unobserved factors since it depends on eligibility criteria, satisfying the main theoretical assumption of the method. To add more to this, it is worth mentioning that PSM has been already used in the literature to control for the selection bias when evaluating the same policy in the past (see the example of Arranz et al. (2020)).

control matrix $X$, are indeed the same for both the furloughed and non-furloughed workers:

$$E[y_\alpha|X, D = 1] = E[y_\alpha|X, D = 0] \tag{8}$$

As we mentioned before, the furlough take-up decision is based on the circumstances of the companies and employees right before the take-up, and they are subject to eligibility criteria, we assume that there are no latent factors associated to the decision. Thus, the selection on observables can be sustained for our analysis.

One last preliminary condition, necessary for the comparison and average impact estimates to make sense, is the existence of a set of homogeneous counterfactual/individuals couples. Any matching procedure essentially requires that the ceteris paribus condition is respected by associating though different techniques individuals with their almost identical counterfactuals in a given neighborhood of values. An essential way to define the concept of common support has to do with the non existence of a full probability set for any given characteristic inside matrix $X$. In other words, for the $ATT$, the common support requirement implies that for any observable $X_i$ the proportion of furloughed individuals with a specific value of that characteristic should always be less than 1. The absence of such condition would imply an empty set for the untreated, and as such the absence of counterfactual for that specific characteristic $X_i$ that would immediately bias the estimated values.

$$ATT : P(D = 1|X_{i=1,2,3,n}) = P(D = 1|X) < 1 \tag{9}$$

In a similar fashion, but considering that we need a lower bound for the treated to avoid all workers from being untreated, the common support condition for the average treatment effect for the non furloughed will be:

$$ATU : P(D = 1|X_{i=1,2,3,n}) = P(D = 0|X) > 0 \tag{10}$$

Joining the two conditions, and considering the average treatment effect, the overall common support condition will thus imply:

$$ATE : 0 < P(D = 1|X) < 1 \tag{11}$$

The last step required is the propensity score matching calculation. The propensity score represents the probability that an individual might be part of the furloughed group given he shows given values of an observable $X_i$. The values thus represents the proportion of the treated with that given value of the defining characteristic $X_i \implies p(X) = P(D = 1|X = X_i)$. The propensity score is thus calculated through a logit regression with the full set of $X_i$ as controls:

$$P(D = 1|X) = \frac{\exp(\delta' X)}{1 + \exp(\delta' X)} \tag{12}$$

Similar values of the propensity score, according to discretional proximity criteria, are then used to match furloughed workers from the treatment group with unfurloughed workers from the control group. The matching finally allow to calculate a comparable result, that is an estimated outcome of those untreated neighbors which have been coupled with the $i^{th}$ treated person:

$$\widehat{y}_i = \sum_{j \in C^0(p_i)} w_{ij} y_j \tag{13}$$

the comparable result $\widehat{y}_i$ is thus defined as the geometric sum of real outcome values of the $j^{th}$ neighbor in the $C^0(p_i)$ set of untreated neighbors of individual $i$. The set will be bounded, as

$\sum_{j \in C^0(p_i)} w_{ij} = 1$ and the different matching methods we have employed offered different definitions of the neighbor set given the way weights have been defined. Once the estimated value of the outcome variable for the counterfactual have been estimated, the only step left to calculate the $ATT$ will thus be the simple average of the difference between actual outcome values and the aforementioned estimated outcome values inside the common support bounds:

$$ATT = \frac{1}{\sum_{k=1}^{n}(D_k = 1 \cap C^0(p_i))} \sum_{i \cap \{D_i = 1 \in C^0(p_i)\}} (y_i - \widehat{y}_i) \tag{14}$$

We are looking, in other words, for an average calculated on a difference embodying the treatment effect. To define it disjointedly between the treatment and control groups:

$$ATT = E(Y|\textit{treated in common support}) - E(Y|\textit{non-matched treated/weighted}) \tag{15}$$

The $ATU$ will instead be calculated as:

$$ATU = \frac{1}{\sum_{k=1}^{n}(D_k = 0 \cap C^0(p_i))} \sum_{i \cap \{D_i = 0 \in C^0(p_i)\}} (y_i - \widehat{y}_i) \tag{16}$$

Finally, the $ATE$ can be easily obtained from the $ATT$ and $ATU$ results, as shows equation 6. In the following Sections, we shall offer a complete overview of our results, together with the necessary post-estimation checks and inference.

# 5. National propensity score matching results

## 5.1. Benchmark results for single quarter furloughs

In this section, results from the matching procedure will be shown to illustrate how the average treatment effect stands out as being statistically significant, favoring ERTE as a mean of re-employment. In this exercise, we evaluate the transition from a state of furlough to a state of employment on the basis of the first three quarters of 2020, as explained in the data section. Our first preliminary look at the data starts with a simple smoothing baseline approach as we choose to pair each treated individual with his/her nearest neighbor. In this section we use no caliper and we also allow for replacement, options that will be tested later in the robustness sections.

After the logit estimation for the propensity score[7] we take a look to its distribution over the furloughed and non-furloughed samples (see Figure 2). This graph evidences the existence of overlapped individuals in the sample, a necessary condition to carry out the subsequent analysis.

Then, Table 1 shows the so-called benchmark estimates for the average effect on the treated/furloughed (ATT), the average effect on the untreated/unfurloughed (ATU), and most importantly the average treatment effect accounting for the whole sample (ATE). Both bootstrapped and Abadie-Imbens standard errors[8] with respective z-stats are displayed together as it is not fairly clear in the literature which one should be used preferably.

The naive difference between groups leads to an average net effect of 0.317. Despite lower than this unmatched estimation, our average treatment effect (ATE) has been estimated as a positive quantity near 0.28, which can be interpreted as a bonus of 28 percentage points (hereafter p.p.) in the probability to be re-employed thanks to the furlough scheme for any randomly selected individual in the sample. This effect was even higher when accounting only for the average effect on furloughed individuals (ATT), who increased their re-employability chances around 30 p.p. compared with a counterfactual

[7] Logit output available in appendix (see Table A1).

[8] According to the methods derived by Abadie and Imbens (2006, 2011, 2012)

Figure 2: Bar plot of the overall distribution of the propensity score for the treatment and the control groups.

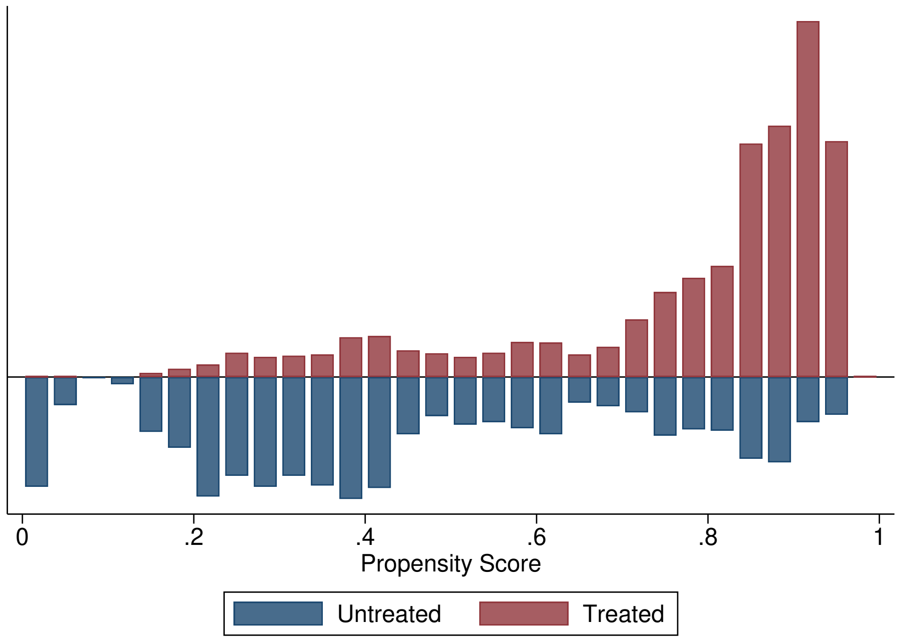



Table 1: Propensity score matching 1nn, benchmark estimates.

| *Estimand* | *Coefficient* | *BS S.E. (z-stat)* | *AI S.E. (z-stat)* |
|---|---|---|---|
| (I) | (II) | (III) | (IV) |
| *Unmatched* | 0.317 | | |
| *ATT* | 0.303 | 0.030 (10.12) | 0.032 (9.39) |
| *ATU* | 0.228 | 0.035 (6.43) | 0.024 (9.38) |
| *ATE* | 0.278 | 0.024 (11.44) | 0.025 (11.06) |

(I) Estimands, (II) estimated values, (III) 500 bootstrapped standard error with associated z-stat and (IV) Abadie-Imbens standard error with associated z-stat.

non-furloughed scenario. To a lesser extent, the ATU coefficient suggests that untreated individuals also could have been benefited from this scheme had they been furloughed.

To infer on the robustness of the matching and the (joint) validity of the instruments, Figures 3 and 4 show the score densities before and after matching. Equivalently, Figure 5, shows a box and whisker plot of the distribution of the propensity score before and after completion of the matching procedure, highlighting quartiles. Most strikingly, the two distributions appear to be almost identical after the procedure. This last result is further enhanced by the bias reduction plot in Figure 6: after matching, the average bias is clearly reduced and less dispersed around 0.[9]

As expected, the matching procedure succeeded in balancing the treated and untreated, leading to a very similar distribution in terms of propensity score. Summary stats for this procedure are available in Table 2.

[9]The cautious reader might have understood that an average bias reduction does not necessarily imply that every single control in every single cross section led to a bias reduction. As a further indication of this, the Likelihood Ratio test in Table 2 for the matched analysis did not reject the joint null any better than in the umatched analysis. This, however, a calculated risk entailed by having to resort to a high number of covariates to check for any sort of observable heterogeneity, exposing the analysis to the risk of "overcontrolling" it.

Figure 3: Densities of the propensity score before matching.

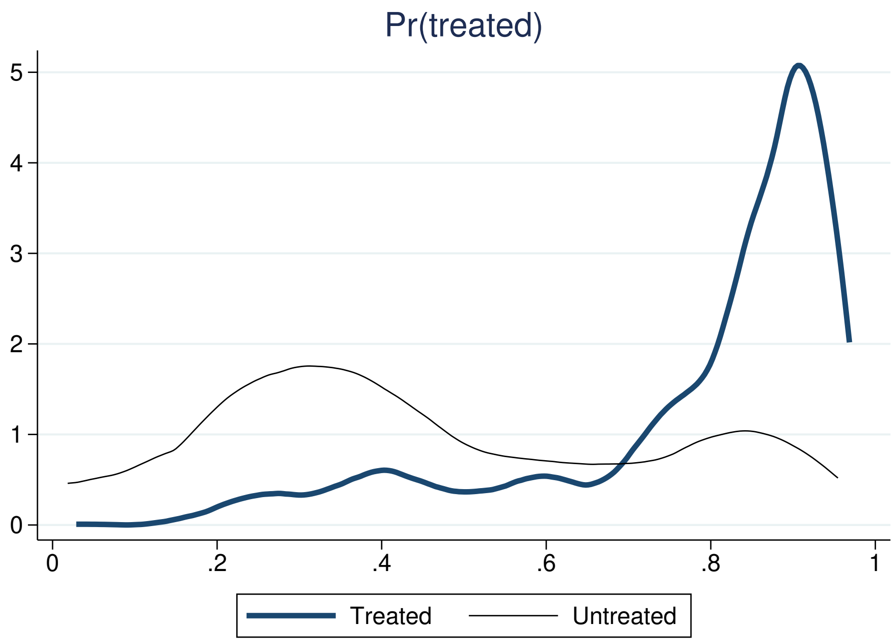



Figure 4: Densities of the propensity score after 1nn matching.

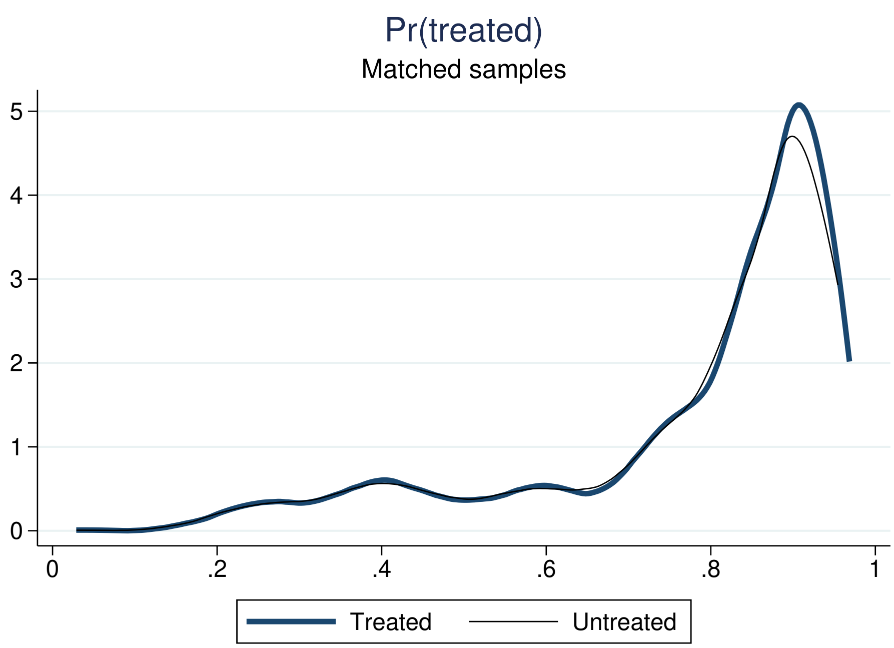


Figure 5: Box and Whiskers plot, distribution of the propensity score across treatment and control groups before-after 1nn matching.

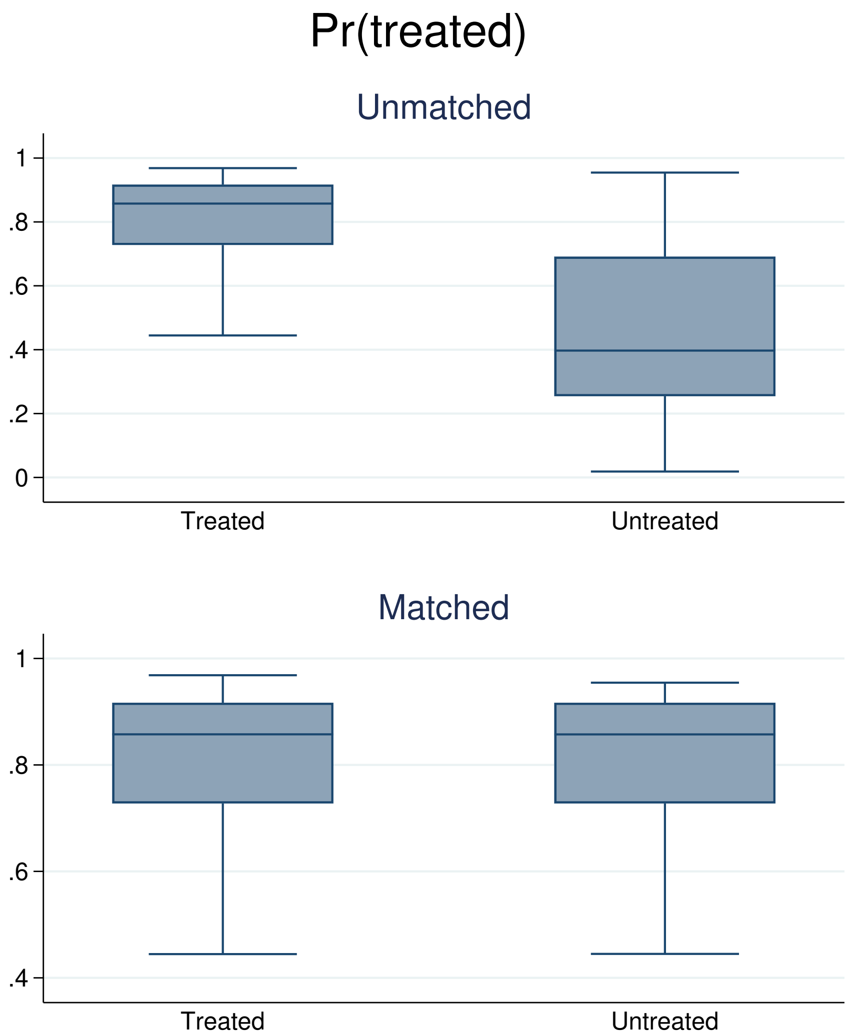


Figure 6: Standardized percentage bias across covariates before-after 1nn matching.

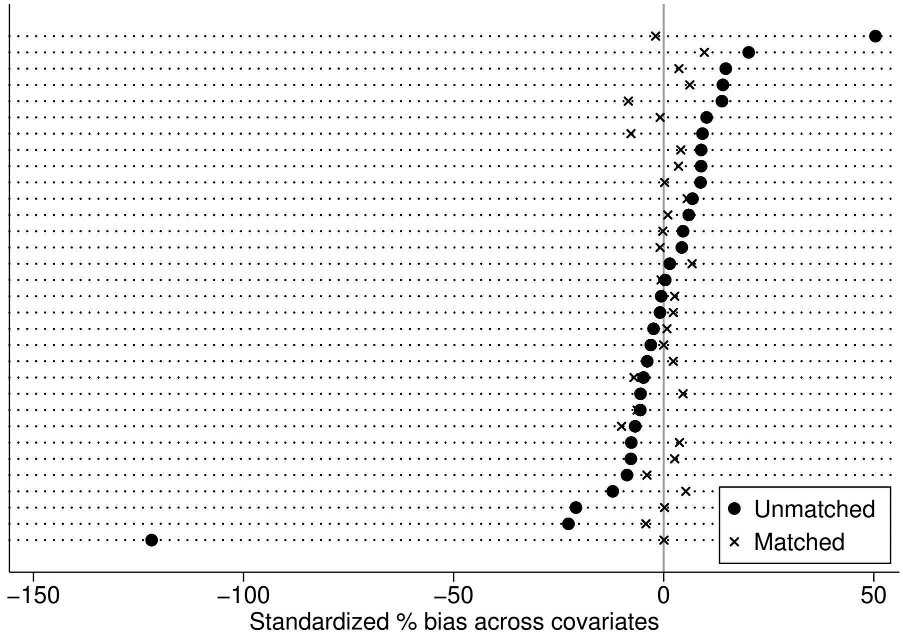



Table 2: Propensity score matching 1nn, quality check.

| *Sample* | *Pseudo* $R^2$ | *LR* $\chi^2$ | $p > \chi^2$ | *Mean bias* | *Median bias* | *B* | *R* |
|---|---|---|---|---|---|---|---|
| (I) | (II) | (III) | (IV) | (V) | (VI) | (VII) | (VII) |
| *Unmatched* | 0.267 | 1555.68 | 0.000 | 13.1 | 7.8 | 136.1 | 0.56 |
| *Matched* | 0.013 | 115.12 | 0.000 | 3.7 | 3.6 | 27.4 | 1.17 |

Values of the pseudo correlation coefficient and related tests on the joint hypothesis of non-significance of the control variables. Mean and median bias changes in column (V) and (VI). Rubin's B and R in Columns (VII) and (VIII). Suggested values for B and R: B less than 25, R between 0.5 and 2.

## 5.2. Robustness I: caliper, more neighbors and no replacement

In order to prove the robustness of the results previously seen, this section presents and compares an alternative matching choice to the benchmark we already established with the k=1, no caliper approach with replacement. We now will produce results based on different discretionary choices of the caliper. We remind that the reason why a caliper can be imposed on this kind of discretionary procedures is that some treated individuals could be very far away from the closest untreated individual. That would imply a reduction in the matching precision as treated individuals might be paired with dissimilar untreated ones. Having a caliper is a way to ensure the existence of a common support interval, but the lower the value of it, the higher the chances to leave some individuals outside of the estimates. While, in the benchmark case, the loss in terms of comparable individuals was 0, when imposing a caliper we may lose some individuals who might be off support, this means, out of the maximum score distance we have established with the caliper option for any comparable units to be so. In exchange, we will be ensuring any paired individuals to be as similar as we desire in terms of their propensity score.

Equivalently, a trade off exists when matching techniques with replacement are compared, everything else equal (width of the caliper, etc), to their no-replacement counterpart. Although replacement ensures lower bias and higher matching quality, since the distance between propensity scores is minimal as the best optimal choices from the control group are not systematically ruled out of the matching procedure, variances (and thus standard error) end up being comparatively higher with respect to the no-replacement option. That naturally happens since the information set is smaller (as "far away individuals" are never selected given the replacement mechanism), leading us to smaller precision and higher uncertainty for close to zero results in the estimates of the average effects. On the other side of the coin, avoiding replacement reduces variances as all the available information is being employed, but naturally worsen the quality of the matching as less likely individuals are paired by their less similar propensity scores. All in all, we expect the loss in terms of unmatched individuals to be non-negligible when no-replacement is present, but hope results stay robust, if not in terms in magnitude, at least in terms of sign when compared to the benchmark case.

One last discretionary choice comes from the number of nearest comparable neighbors used for the matching procedure (k). So far, our basic approach have taken only the nearest individual (k=1), but the selection of a higher number is equally possible. This option, combined with a caliper, may rise the precision of the estimates.

As these decisions might cast a shadow on the (internal) validity for the results, this section presents some alternative robustness exercises where a 1% caliper and a more restrictive caliper of 0.1%[10] is used alongside the usual k=1 and k=10 choice and replacement and no replacement alternatives to test the robustness of the ATT, ATU and ATE results under these conditions. A first glimpse at the outcome of the matching procedure can be seen in Table 3, which offers the estimates for: (i) 1 nearest neighbor, 1% caliper, replacement model; (ii) 1 nearest neighbor, 0.1% caliper, replacement model; (iii) 10 nearest neighbors, 1% caliper, replacement model; and (iv) 1 nearest neighbor, 1% caliper, no replacement model.

In any case, the results are quite similar in magnitude and significantly positive, with the average treatment effect not moving far from the 28 p.p. previously established by the benchmark estimation despite some individuals have been thrown out from the common support when imposing the caliper and no replacement options. Table 4 shows the off and on support individuals for each model[11]. The 1% caliper imposition only left out 21 individuals compared to the benchmark case, which is a insignificant fraction of the whole sample. It evidences again the clear existence of overlapped individuals in both groups which ensures the common support assumption. Going a step further, we imposed a strict 0.1% caliper. This time, although the sample lost was higher, it was still a minor portion. Finally, the no replacement option, together with the 1% caliper, leaves out of the pairing procedure many Furloughed workers from upper regions of the propensity score, too many when compared to their comparable equivalent in the Unfurloughed group. This phenomenon can be explained due to the larger number of treated individuals in the sample. Anyway, besides the significant loss (2,195 unavailable treated individuals), the left sample seems to be enough to reduce the variance of the estimate, even if slightly, while preserving the magnitude of the effect.

Table 5 reports the post-estimation checks. All values related to the goodness of the matching appear to suggest the stability of the procedure, with a very low matched $R^2$, a strong reduction of the Log-likelihood ratio $\chi^2$, a significant bias reduction and at least borderline values for the parameters B and R in most models.

All in all, estimates of the average effect remained close to the benchmark case in every single attempted estimation, therefore the idea that Furloughs might have a positive impact on re-employment opportunities still stands.

[10]Results for estimates based on lower than 0.01% calipers are available upon request.
[11]Graphically displayed in the appendix (see Figures A1, A2, A3 and A4)

Table 3: Propensity score matching, robustness I: caliper, 10 neighbors and no replacement estimates.

| *Model* | *Estimand* | *Coefficient* | *BS S.E. (z-stat)* |
|---|---|---|---|
| (I) | (II) | (III) | (IV) |
| *k=1, cal=0.01, repl.* | *ATT* | 0.303 | 0.030 (10.13) |
| | *ATU* | 0.232 | 0.030 (7.83) |
| | *ATE* | 0.280 | 0.023 (11.96) |
| *k=1, cal=0.001, repl.* | *ATT* | 0.303 | 0.029 (10.54) |
| | *ATU* | 0.262 | 0.026 (10.19) |
| | *ATE* | 0.291 | 0.023 (12.63) |
| *k=10, cal=0.01, repl.* | *ATT* | 0.305 | 0.026 (11.57) |
| | *ATU* | 0.232 | 0.026 (8.98) |
| | *ATE* | 0.281 | 0.022 (12.64) |
| *k=1, cal=0.01, norepl.* | *ATT* | 0.248 | 0.021 (12.08) |
| | *ATU* | 0.257 | 0.021 (12.00) |
| | *ATE* | 0.253 | 0.018 (13.98) |

(I) Model specification (number of nearest neighbours, caliper and replacement options), (II) estimands, (III) estimated values, (IV) 500 bootstrapped standard error with associated z-stat.
Note: Abadie-Imbens standard error not available in the software using these specifications.

Table 4: Common support for robustness I models.

| *Model* | *Group* | *Off support* | *On support* |
|---|---|---|---|
| *k=1, cal=0.01, repl.* | *Untreated* | 18 | 1,493 |
| | *Treated* | 3 | 3,099 |
| *k=1, cal=0.001, repl.* | *Untreated* | 263 | 1,248 |
| | *Treated* | 175 | 2,927 |
| *k=10, cal=0.01, repl.* | *Untreated* | 18 | 1,493 |
| | *Treated* | 3 | 3,099 |
| *k=1, cal=0.01, norepl.* | *Untreated* | 574 | 937 |
| | *Treated* | 2,195 | 907 |

Table 5: Propensity score matching robustness I models, quality check.

| *Sample* | *Pseudo* $R^2$ | *LR* $\chi^2$ | $p>\chi^2$ | *Mean bias* | *Median bias* | *B* | *R* |
|---|---|---|---|---|---|---|---|
| (I) | (II) | (III) | (IV) | (V) | (VI) | (VII) | (VII) |
| *Unmatched* | 0.267 | 1555.68 | 0.000 | 13.1 | 7.8 | 136.1 | 0.56 |
| *k=1, cal=0.01, repl.* | 0.013 | 114.53 | 0.000 | 3.7 | 3.6 | 27.3 | 1.18 |
| *k=1, cal=0.001, repl.* | 0.013 | 109.09 | 0.000 | 3.6 | 3.3 | 27.4 | 1.17 |
| *k=10, cal=0.01, repl.* | 0.012 | 100.25 | 0.000 | 3.3 | 2.0 | 25.6 | 1.13 |
| *k=1, cal=0.01, norepl.* | 0.021 | 47.65 | 0.037 | 3.7 | 2.6 | 34.2 | 1.26 |

Values of the pseudo correlation coefficient and related tests on the joint hypothesis of non-significance of the control variables. Mean and median bias changes in column (V) and (VI). Rubin's B and R in Columns (VII) and (VIII). Suggested values for B and R: B less than 25, R between 0.5 and 2.

## 5.3. Robustness II: kernel estimates

One last step is perhaps missing in our robustness check: the possibility to allow for heterogeneous weights in the matching procedure. That is, instead of averaging out the k propensity scores from the matched untreated, we want to resort to a method that creates a comparable result as a weighted average based on some function. Kernel densities can thus be used so that the comparable result (the propensity score associated with the i$^{th}$ treated) is the weighted averaged of the propensities of the untreated neighbors with weights negatively proportional to the distance between propensity score of the i$^{th}$ treated and the j$^{th}$ matched untreated individual. The more far away the untreated, the less its contribution to the result. In mathematical terms, the calculated comparable result $\widehat{y_i}$ , given p$_i$, p$_j$ and $h$, (the propensity score of the i$^{th}$ treated, the propensity score of the j$^{th}$ treated, and the bandwidth of the kernel respectively) will thus be:

$$\widehat{y_i} = \frac{\sum_{j\in\{d=0\}} K(\frac{p_i - p_j}{h})y_j}{\sum_{j\in\{d=0\}} K(\frac{p_i - p_j}{h})} \tag{17}$$

with $\widehat{w}_{ij} = \frac{K(\frac{p_i - p_j}{h})y_j}{\sum_{j\in\{d=0\}} K(\frac{p_i - p_j}{h})}$ representing the contribution of the weight of the j$^{th}$ untreated on the comparable result associated to the i$^{th}$ treated. We anticipate to the reader that the outcome of the matching remained basically unchanged, when compared to the benchmark case regardless of the approximated shape of our kernel density (normal, biweight, tricubic, epanechnikov), given a chosen bandwidth of 0.6 and 0.1, the only difference being the fact that the off support treatment group could not be considered an empty set anymore. The last discretional choice we did put to test was the bandwidth length. As larger values of h imply a more platicurtic distribution while lower values imply a narrower one, we tried to experiment on the results given by the normal kernel density on a similar way we did with the simple smoothing: that is trying with different values and gradually reducing it.

Table 6 contains the results for two different kernel functions (epanechnikov and normal), combined with two bandwidth specifications (0.06 and 0.01) and the common support imposition for every model. Our results stayed absolutely in line with the baseline case.[12] In addition, note that the imposition of the common support condition did not dramatically affected the remaining sample, dropping a minor number of individuals in any case (see Table 7).

[12]For the sake of simplicity, results for other kernel and bandwidth are available upon request.

Table 6: Propensity score matching, robustness II: kernel estimates.

| *Model* | *Estimand* | *Coefficient* | *BS S.E. (z-stat)* |
|---|---|---|---|
| (I) | (II) | (III) | (IV) |
| *epanechnikov, bw=0.06, cs.* | *ATT* | 0.290 | 0.024 (12.27) |
| | *ATU* | 0.247 | 0.020 (12.21) |
| | *ATE* | 0.277 | 0.020 (13.94) |
| *epanechnikov, bw=0.01, cs.* | *ATT* | 0.304 | 0.025 (12.39) |
| | *ATU* | 0.250 | 0.020 (12.26) |
| | *ATE* | 0.287 | 0.020 (14.32) |
| *normal, bw=0.06, cs.* | *ATT* | 0.292 | 0.023 (12.83) |
| | *ATU* | 0.255 | 0.020 (13.03) |
| | *ATE* | 0.281 | 0.020 (14.50) |
| *normal, bw=0.01, cs.* | *ATT* | 0.302 | 0.024 (12.67) |
| | *ATU* | 0.244 | 0.021 (11.58) |
| | *ATE* | 0.284 | 0.020 (14.22) |

(I) Model specification (type of kernel, bandwidth and common support), (II) estimands, (III) estimated values, (IV) 100 bootstrapped standard error with associated z-stat.

Table 7: Common support for robustness II, kernel models.

| *Model* | *Group* | *Off support* | *On support* |
|---|---|---|---|
| *epanechnikov, bw=0.06, cs.* | Untreated | 78 | 1,433 |
| | Treated | 29 | 3,073 |
| *epanechnikov, bw=0.01, cs.* | Untreated | 94 | 1,417 |
| | Treated | 29 | 3,073 |
| *normal, bw=0.06, cs.* | Untreated | 78 | 1,433 |
| | Treated | 29 | 3,073 |
| *normal, bw=0.01, cs.* | Untreated | 78 | 1,433 |
| | Treated | 29 | 3,073 |

Table 8: Propensity score matching robustness II: kernel models, quality check.

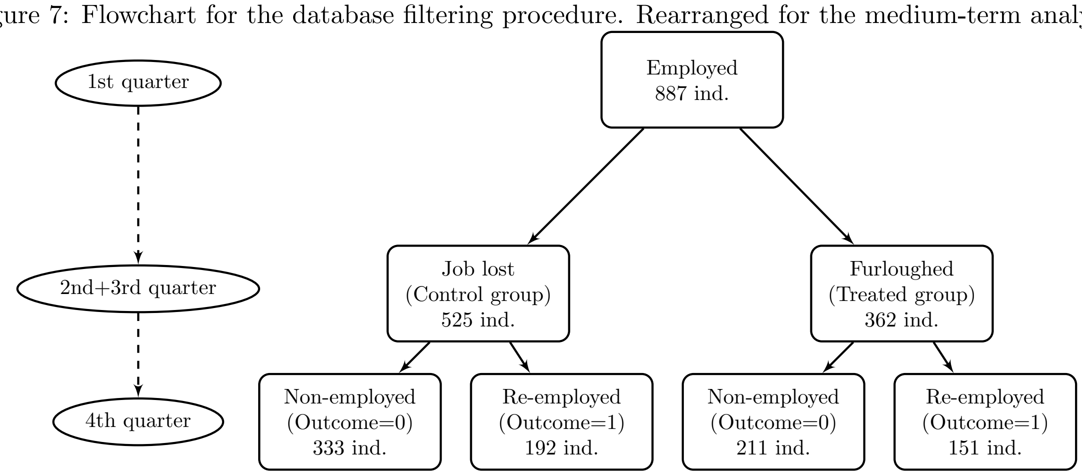
| *Sample* | *Pseudo* $R^2$ | *LR* $\chi^2$ | $p > \chi^2$ | *Mean bias* | *Median bias* | *B* | *R* |
|---|---|---|---|---|---|---|---|
| (I) | (II) | (III) | (IV) | (V) | (VI) | (VII) | (VII) |
| *Unmatched* | 0.267 | 1555.68 | 0.000 | 13.1 | 7.8 | 136.1 | 0.56 |
| *epanech., bw=0.06, cs.* | 0.015 | 125.79 | 0.000 | 4.1 | 3.3 | 28.8 | 1.13 |
| *epanech., bw=0.01, cs.* | 0.012 | 98.31 | 0.000 | 3.4 | 2.8 | 25.4 | 1.12 |
| *normal, bw=0.06, cs.* | 0.020 | 169.60 | 0.000 | 5.0 | 4.3 | 33.5 | 1.23 |
| *normal, bw=0.01, cs.* | 0.012 | 101.50 | 0.000 | 3.4 | 3.1 | 25.8 | 1.14 |

Values of the pseudo correlation coefficient and related tests on the joint hypothesis of non-significance of the control variables. Mean and median bias changes in column (V) and (VI). Rubin's B and R in Columns (VII) and (VIII). Suggested values for B and R: B less than 25, R between 0.5 and 2.

## 5.4. Extended schemes: average effect of two-consecutive quarter furloughs

We now ask what would happen if we were to aggregate those workers who have been Furloughed during both the second and third quarter of 2020 in order to verify the causal consistency of the impact of the Spanish ERTE on employment in the last quarter of 2020. In this definition of medium term, we expect the positive causal effect to persist as the ERTE gives more time to both the worker and the entrepreneurs to adjust to new market conditions. This time, the filtering procedure for the data is analogous. However, now the individuals considered for both control and treatment group must necessarily stay in the same situation during the second and third quarter consecutively, as illustrates Figure 7. Despite reducing dramatically the sample size, this medium term analysis still preserves significance as we will see afterward.

Figure 7: Flowchart for the database filtering procedure. Rearranged for the medium-term analysis.




The point of this section is to compare the average treatment effect in the previous quarter to quarter analysis with an estimate coming from a treatment group that has spent relatively more time being furloughed. We are thus mainly interested in: 1) confirming causality in the medium term too; 2) establishing a relative comparison in terms of magnitude between the previous "short term" exercise and the current "medium term" one. For these purposes, we will stick to the same modelling procedure: making use of the k=1, no caliper and replacement model for the benchmark estimate and testing the robustness of those results after imposing multiple caliper options, increasing the number of nearest neighbors, not allowing for replacement and using different kernel specifications.

Benchmark estimates and stability tests for this medium term analysis are visible in Tables 9 and 10, while other model specifications are available in the appendix (see Table A2, A3 and A4). The estimates of the average treatment effect lay from 0.091 to 0.169 across the different models but seems to be positive in any case. Similarly, the ATT point estimation for our models is between 0.123 and 0.190, being statistically significant in any case.

These results suggest that the effect on re-employment is smaller in magnitude when compared to the previous short term exercise, which favors the idea of effectiveness losses in the furloughs schemes when they are held in time. Remember that a single quarter ERTE had a average treatment effect which was around 10 p.p. above the estimated effects for the two-consecutive quarter one. However it may be, the positive causal effect of Furloughs on employment appears to have helped workers in Spain across the whole year, regardless of the duration of the Furlough policy.

Table 9: Propensity score matching 1nn, benchmark estimates for the medium term analysis.

| *Estimand* | *Coefficient* | *BS S.E. (z-stat)* | *AI S.E. (z-stat)* |
|---|---|---|---|
| (I) | (II) | (III) | (IV) |
| *Unmatched* | 0.044 | | |
| *ATT* | 0.190 | 0.060 (3.16) | 0.046 (4.17) |
| *ATU* | 0.101 | 0.091 (1.11) | 0.052 (1.93) |
| *ATE* | 0.138 | 0.061 (2.26) | 0.038 (3.62) |

(I) Estimands, (II) estimated values, (III) 500 bootstrapped standard error with associated z-stat and (IV) Abadie-Imbens standard error with associated z-stat.

Table 10: Propensity score matching 1nn: medium term analysis, quality check.

| *Sample* | *Pseudo* $R^2$ | *LR* $\chi^2$ | $p > \chi^2$ | *Mean bias* | *Median bias* | *B* | *R* |
|---|---|---|---|---|---|---|---|
| (I) | (II) | (III) | (IV) | (V) | (VI) | (VII) | (VII) |
| *Unmatched* | 0.353 | 408.45 | 0.000 | 16.7 | 8.9 | 167.6 | 0.62 |
| *Matched* | 0.033 | 32.94 | 0.325 | 6.4 | 5.3 | 43.1 | 1.37 |

Values of the pseudo correlation coefficient and related tests on the joint hypothesis of non-significance of the control variables. Mean and median bias changes in column (V) and (VI). Rubin's B and R in Columns (VII) and (VIII). Suggested values for B and R: B less than 25, R between 0.5 and 2.

# 6. Regional and sectoral effects for single and two-consecutive quarter furloughs

The last section proved how the analysis of the Furlough measures at the national aggregated level led us to conclude, ceteris paribus, that people Furloughed showed a higher probability of reemployment with respect to unfurloughed workers during the most relentless, initial phase of the coronavirus outbreak. We might now wonder whether or not any residual heterogeneity is left at a more disaggregated territorial level. Since the SLFS is currently available at different aggregation levels, this section present a logit regression of the binary outcome variable (employment state after being furloughed) on the treatment dummy and the controls we have already made use of in the matching exercise, that is considering the pre-treatment characteristics of the individuals.[13] This exercise was carried out for both the short-term and medium-term data arrangement that was used before, hence, for the single quarter furloughs and the two-consecutive quarter furloughs, as it was done with the matching methods for the national level analysis.

As the logit output[14] cannot be directly interpreted and we are particularly interested in the marginal effects of the treatment dummy variable, we will go straightaway to its computed average marginal effects and its interactions with the regional and sectoral dummies to uncover the heterogeneity of the treatment effect. These Marginal effects to analyze the direct impact of Furlough in each autonomous region and sector are presented in Table 11. To give a more solid idea of the precision and consistency of the results (distance from the null of the coefficients and heterogeneity) we also report marginal effects and their confidence intervals in the appendix Figures A5 and A6.

[13]The low representativeness of the sample and the lack of enough common support at the regional and sectoral level when using matching techniques forced us to change our strategy from matching to regression analysis in this particular section, which allowed the use of sample weights.

[14]Available in the appendix, see Table A5.

By a raw and direct glimpse at the marginal effects of the single quarter (short-term) Furloughs on Re-employment, conditional on the region and the economic activity, we are left with a very statistically precise and heterogeneous sequence of coefficients, but in any case, close in magnitude. Additionally, the average effect is close but slightly below our matching estimates for the ATE. Regarding the regional comparison, an ERTE take-up in the 2nd quarter was associated with an increase in the probability of re-employment in the third quarter of 2020 in a range of values comprised between 20 p.p. (for C. F. Navarra) and 27 p.p. (for Canarias). On a similar note, conditional on each sector of activity, the same situation would lead to an increase in probability within a range of 22.2 (for construction) and 27 p.p. (for primary sector). On the other hand, the medium-term furlough margins lay around an estimation of 8 p.p. aprox., reducing their magnitude when compared to the short-term ones, but remaining significant. Note that every single computed marginal effect falls comfortably into a confidence interval well above 0.

Table 11: Average marginal effects of the furlough treatment in the probability of re-employment, interactions with sectors and regions

| *Variables* | *Short-term regress.* $\delta y/\delta x$ *(z-stat)* | *Medium-term regress.* $\delta y/\delta x$ *(z-stat)* |
|---|---|---|
| Furloughed | 0.245 (314.46)*** | 0.087 (39.76)*** |
| | | |
| Furloughed at sector: | | |
| Primary sector | 0.270 (314.00)*** | 0.091 (39.04)*** |
| General manufacturing | 0.256 (289.56)*** | 0.095 (39.85)*** |
| Extract, supply and other industr. | 0.241 (246.46)*** | 0.095 (39.84)*** |
| Machinery, install and repair | 0.251 (270.70)*** | 0.069 (35.37)*** |
| Construction | 0.222 (252.67)*** | 0.094 (39.61)*** |
| Trade, acommodation, food serv. | 0.245 (303.37)*** | 0.087 (40.18)*** |
| Transp. and store/ info. and comm. | 0.251 (294.45)*** | 0.075 (38.11)*** |
| FI./ r.estate/ prof-sci-tech | 0.257 (304.29)*** | 0.094 (39.71)*** |
| Public admin/ educ and health | 0.265 (315.48)*** | 0.093 (39.43)*** |
| Other services | 0.243 (291.25)*** | 0.067 (38.36)*** |
| | | |
| Furloughed at region: | | |
| Andalucía | 0.256 (313.99)*** | 0.091 (39.57)*** |
| Aragón | 0.243 (227.70)*** | 0.095 (39.84)*** |
| Asturias | 0.246 (208.43)*** | 0.094 (39.39)*** |
| Baleares | 0.250 (249.32)*** | 0.069 (35.82)*** |
| Canarias | 0.270 (315.09)*** | 0.076 (40.15)*** |
| Cantabria | 0.204 (115.81)*** | 0.081 (30.74)*** |
| Castilla-León | 0.237 (254.27)*** | 0.094 (39.80)*** |
| Castilla-La Mancha | 0.245 (253.06)*** | 0.094 (39.84)*** |
| Cataluña | 0.249 (300.41)*** | 0.083 (39.92)*** |
| C. Valenciana | 0.216 (265.82)*** | 0.093 (39.68)*** |
| Extremadura | 0.244 (208.01)*** | 0.085 (37.53)*** |
| Galicia | 0.228 (244.07)*** | 0.074 (37.41)*** |
| C. Madrid | 0.252 (310.28)*** | 0.092 (39.36)*** |
| R. Murcia | 0.243 (237.33)*** | 0.086 (38.65)*** |
| C. F. Navarra | 0.200 (138.25)*** | 0.039 (22.20)*** |
| País Vasco | 0.254 (271.61)*** | 0.088 (38.95)*** |
| La Rioja | 0.218 (104.96)*** | 0.071 (27.29)*** |
| Observations † | 1,575,212 | 309,893 |

$z$ statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

† Note: sample weights have been applied

# 7. Concluding remarks

We used the 2020 quarterly waves of the Spanish Labor Force Survey in order to collect a sample of workers who were furloughed at the initial phase of the pandemic, together with comparable non-furloughed individuals who lost their job at that time and joined any source of potential workforce, using the latter to construct the counterfactual. Then, carrying out propensity score matching techniques, we provide evidence on how the probability to be re-employed was significantly higher in the treated (furlough granted group) than in the control group, leading to a positive net effect on after-ERTE re-employability. Additionally, this analysis has led us to results which appear to be robust to a variety of alternative specifications, may that be a time-wise different data arrangement or a series of tweaks to the selection procedure related to the matching method. Nonetheless, the magnitude of the treatment effect reduced significantly when two-consecutive quarter schemes were considered in comparison to the single quarter one, which supports the idea of furloughs effectiveness losses when they are held in time[15]. As a result, although these job retention programs seem to be a useful strategy to face transitory adverse shocks, one might expect that when a shock is of a more permanent nature, these schemes only delay the destruction of jobs. Extending the analysis to more enduring schemes in order to test whether their effects keep shrinking and vanish with time would be interesting, but data nonavailability prevent us from doing so. Likewise, analyzing the long-term effects of the programs is an unresolved matter for further research.

To add more to our analysis, we have also shown the existence of the aforementioned positive re-employment effect at a lower level of aggregation (autonomous regions) everywhere in the country and in every sector despite the existing heterogeneity at those levels. Every marginal effect, calculated for each of the Spanish regions and sectors, appeared statistically significant and close in magnitude to the other, reinforcing the results of the previous national level analysis for both period arrangements: single and two-consecutive quarter furlough schemes.

To conclude, intuition tells us that when many jobs were paralyzed due to lockdown and social distancing measures at the beginning of the pandemic, these short-time work schemes did a great job on preserving the workers' position while favoring the labor market adjustment at the least cost for the economic agents. Conversely, jobs affected by more structural changes probably will be captured in a furlough program for a long time, wasting public resources and generating deadweight losses while hindering the workforce reallocation process. Therefore, in line with most previous literature, we precise that as an implication to policy-making STW public schemes appear to be once more a very relevant policy tool both at the national and the regional level when labor market stability is the target as long as the shock is expected to be transitory, but any public choice related to this kind of tool should be considered keeping in mind that the duration and timing of the maneuver is essential for it to reduce social costs and achieve the highest possible effect on re-employability, given the conditions of the labor market in the Covid era.

In future research we will continue exploring this topic with new data, trying to overcome some of the limitations that we have faced, looking for long-term and dynamic effects, using other identification strategies and identifying some of the heterogeneity sources.

[15] Considering the reviewed literature, such similar results for analogous schemes in other countries during the previous recession suggest potential external validity for our findings

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# Appendix

Figure A1: Bar plot of the overall distribution of the propensity score for the treatment and the control groups. Common support after k=1, cal=0.01 and replacement matching.

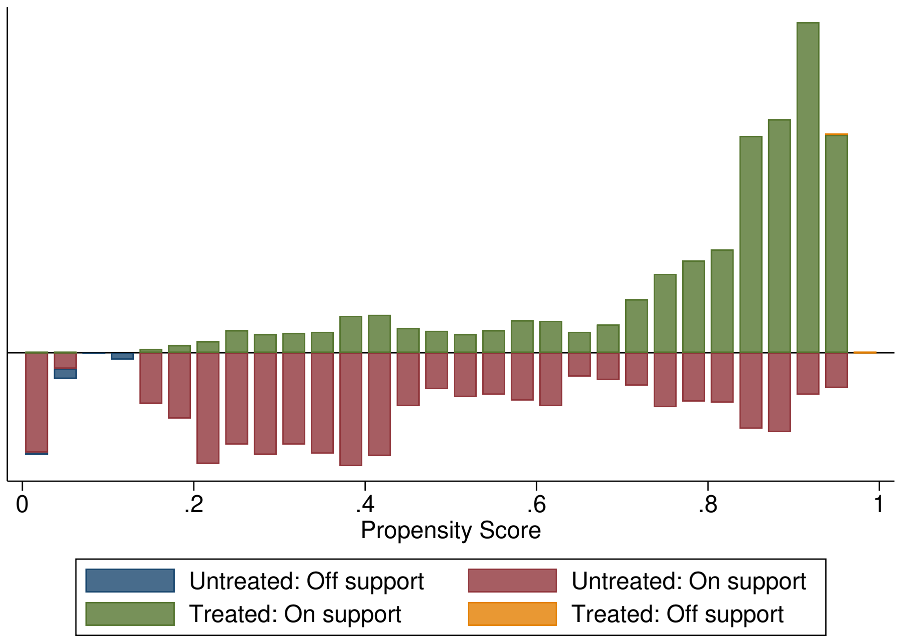

Table A1: Logit output for the propensity score calculation (probability of treatment).

| Covariates | Coefficient (z-stat) | |
|---|---|---|
| Male | | . |
| Female | -0.140 | (-1.65) |
| Andalucía | | . |
| Aragón | 0.723 | (3.36)*** |
| Asturias | 1.282 | (4.39)*** |
| Baleares | 1.005 | (3.60)*** |
| Canarias | 0.957 | (4.71)*** |
| Cantabria | 0.376 | (1.34) |
| Castilla-León | 0.633 | (4.05)*** |
| Castilla-La Mancha | 0.317 | (1.71) |
| Cataluña | 0.833 | (5.45)*** |
| C. Valenciana | 0.310 | (2.02)* |
| Extremadura | -0.189 | (-0.72) |
| Galicia | 0.729 | (5.13)*** |
| C. Madrid | 0.120 | (0.69) |
| R. Murcia | 0.161 | (0.68) |
| C. F. Navarra | 0.561 | (2.24)* |
| País Vasco | 0.988 | (4.48)*** |
| La Rioja | 0.419 | (1.34) |
| Ceuta | 0.847 | (1.03) |
| Melilla | 0.424 | (0.49) |
| Age | -0.003 | (-0.93) |
| Non-higher education | | . |
| Higher education | 0.197 | (2.28)* |
| Primary sector | | . |
| General manufacturing | 2.020 | (4.91)*** |
| Extract, supply and other industr. | 2.425 | (5.90)*** |
| Machinery, install and repair | 3.162 | (7.73)*** |
| Construction | 1.920 | (4.79)*** |
| Trade, acommodation, food serv. | 3.203 | (8.40)*** |
| Transp. and store/ info. and comm. | 2.309 | (5.77)*** |
| FI./ r.estate/ prof-sci-tech | 2.306 | (5.77)*** |
| Public admin/ educ and health | 2.288 | (5.80)*** |
| Other services | 2.411 | (6.12)*** |
| Indefinite | | . |
| Temporary | -2.254 | (-27.23)*** |
| Full-time | | . |
| Part-time | 0.105 | (1.15) |
| Constant | -1.250 | (-3.06)** |
| Observations | 4,613 | |
| Pseudo $R^2$ | 0.267 | |
| $\chi^2$ | 1555.4 | |
| p > $\chi^2$ | 0.000 | |

$z$ statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A2: Bar plot of the overall distribution of the propensity score for the treatment and the control groups. Common support after k=1, cal=0.001 and replacement matching.

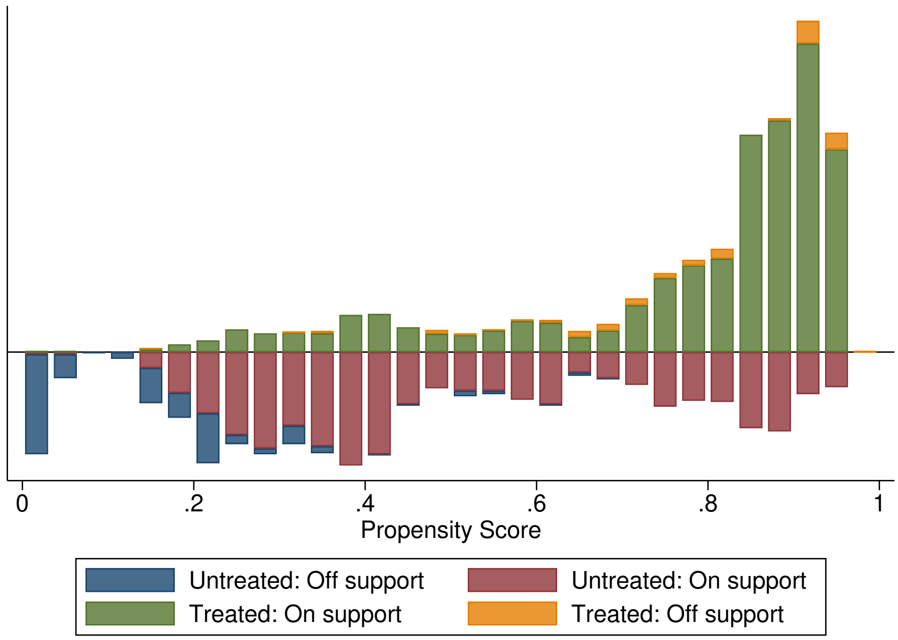


Figure A3: Bar plot of the overall distribution of the propensity score for the treatment and the control groups. Common support after k=10, cal=0.01 and replacement matching.

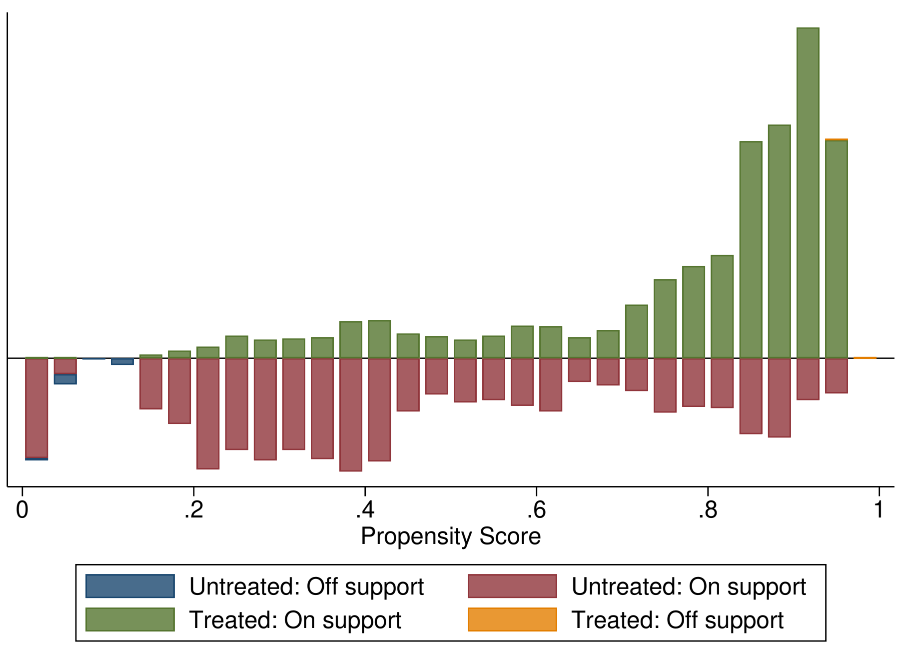

Figure A4: Bar plot of the overall distribution of the propensity score for the treatment and the control groups. Common support after k=1, cal=0.01 and no replacement matching.

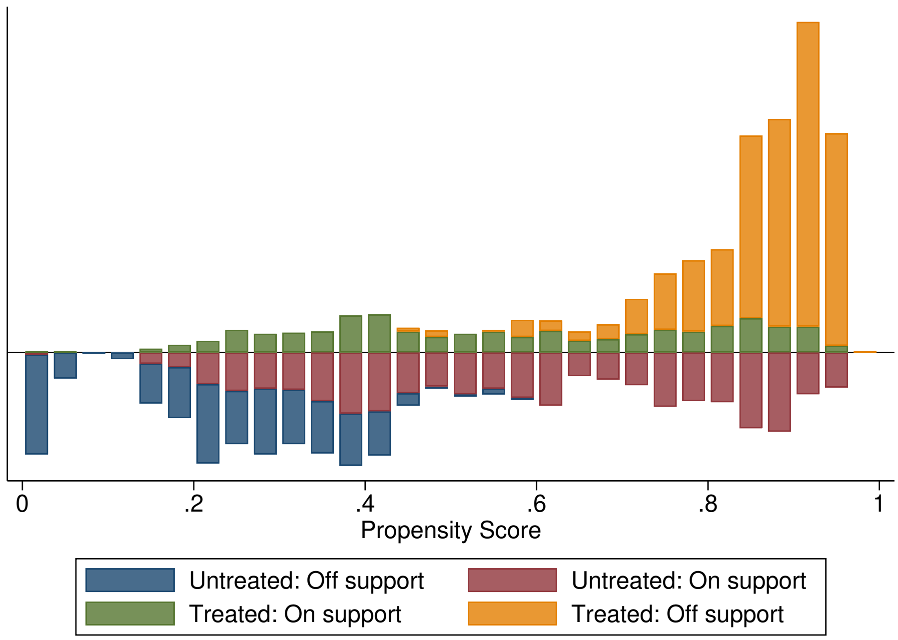


Figure A5: Average Marginal Effect of short and medium term furlough schemes by sector with 95% C.I.s.

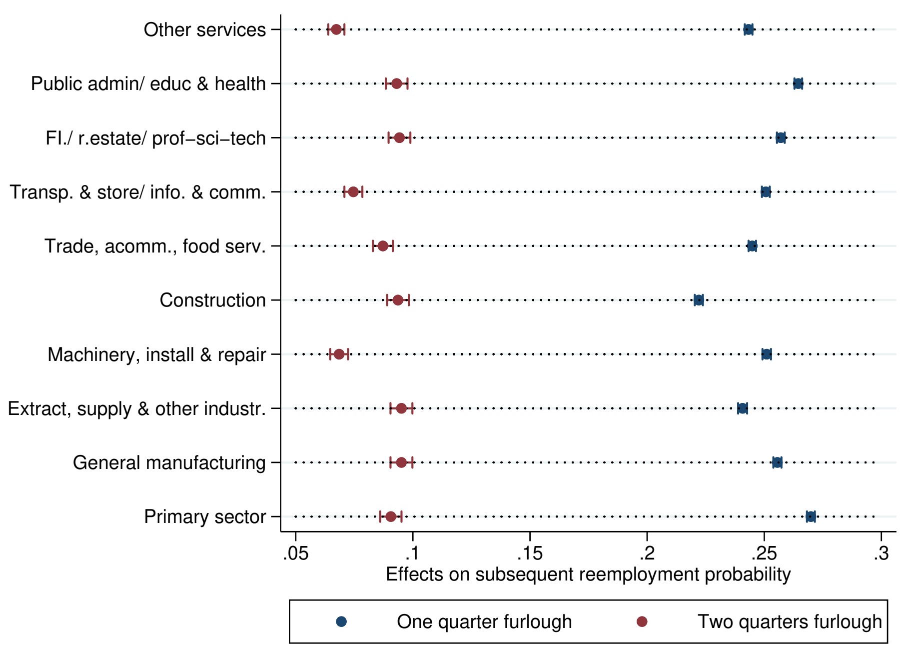

Table A2: Propensity score matching, robustness estimates for the medium term analysis.

| *Model* | *Estimand* | *Coefficient* | *BS S.E. (z-stat)* |
|---|---|---|---|
| (I) | (II) | (III) | (IV) |
| *k=1, cal=0.01, repl.* | *ATT* | 0.181 | 0.052 (3.46) |
| | *ATU* | 0.117 | 0.074 (1.58) |
| | *ATE* | 0.144 | 0.052 (2.78) |
| *k=1, cal=0.001, repl.* | *ATT* | 0.158 | 0.084 (1.88) |
| | *ATU* | 0.178 | 0.092 (1.94) |
| | *ATE* | 0.169 | 0.077 (2.20) |
| *k=10, cal=0.01, repl.* | *ATT* | 0.143 | 0.050 (2.87) |
| | *ATU* | 0.079 | 0.073 (1.08) |
| | *ATE* | 0.107 | 0.052 (2.04) |
| *k=1, cal=0.01, norepl.* | *ATT* | 0.152 | 0.058 (2.59) |
| | *ATU* | 0.135 | 0.049 (2.75) |
| | *ATE* | 0.143 | 0.046 (3.10) |
| *epanechnikov, bw=0.06, cs.* | *ATT* | 0.130 | 0.046 (2.81) |
| | *ATU* | 0.089 | 0.063 (1.43) |
| | *ATE* | 0.106 | 0.047 (2.24) |
| *epanechnikov, bw=0.01, cs.* | *ATT* | 0.137 | 0.046 (2.98) |
| | *ATU* | 0.073 | 0.067 (1.09) |
| | *ATE* | 0.100 | 0.049 (2.04) |
| *normal, bw=0.06, cs.* | *ATT* | 0.123 | 0.045 (2.75) |
| | *ATU* | 0.068 | 0.062 (1.10) |
| | *ATE* | 0.091 | 0.047 (1.95) |
| *normal, bw=0.01, cs.* | *ATT* | 0.141 | 0.043 (3.28) |
| | *ATU* | 0.082 | 0.063 (1.30) |
| | *ATE* | 0.106 | 0.045 (2.34) |

(I) Model specification (number of nearest neighbours, caliper and replacement options), (II) estimands, (III) estimated values, (IV) bootstrapped standard error with associated z-stat (500 reps for nearest neighbors, 100 reps for kernel options).

Table A3: Propensity score matching, robustness for medium term analysis, quality check.

| *Sample* | *Pseudo* $R^2$ | *LR* $\chi^2$ | $p > \chi^2$ | *Mean bias* | *Median bias* | *B* | *R* |
|---|---|---|---|---|---|---|---|
| (I) | (II) | (III) | (IV) | (V) | (VI) | (VII) | (VII) |
| *Unmatched* | 0.353 | 408.45 | 0.000 | 16.7 | 8.9 | 167.6 | 0.62 |
| *k=1, cal=0.01, repl.* | 0.030 | 27.93 | 0.574 | 6.1 | 4.9 | 40.8 | 1.20 |
| *k=1, cal=0.001, repl.* | 0.059 | 22.73 | 0.826 | 7.5 | 6.3 | 57.7 | 0.57 |
| *k=10, cal=0.01, repl.* | 0.026 | 24.67 | 0.741 | 4.7 | 3.4 | 38.3 | 1.76 |
| *k=1, cal=0.01, norepl.* | 0.085 | 30.62 | 0.334 | 8.8 | 6.7 | 70.4 | 0.98 |
| *epanech., bw=0.06, cs.* | 0.020 | 18.61 | 0.948 | 4.3 | 4.0 | 33.1 | 1.80 |
| *epanech., bw=0.01, cs.* | 0.024 | 21.72 | 0.864 | 4.7 | 3.4 | 36.6 | 1.70 |
| *normal, bw=0.06, cs.* | 0.021 | 19.83 | 0.921 | 4.0 | 3.3 | 34.2 | 1.98 |
| *normal, bw=0.01, cs.* | 0.022 | 20.85 | 0.893 | 4.5 | 4.8 | 35.2 | 1.66 |

Values of the pseudo correlation coefficient and related tests on the joint hypothesis of non-significance of the control variables. Mean and median bias changes in column (V) and (VI). Rubin's B and R in Columns (VII) and (VIII). Suggested values for B and R: B less than 25, R between 0.5 and 2.

Table A4: Common support for robustness models in the medium term analysis.

| *Model* | *Group* | *Off support* | *On support* |
|---|---|---|---|
| *k=1, cal=0.01, repl.* | *Untreated* | 34 | 455 |
| | *Treated* | 21 | 339 |
| *k=1, cal=0.001, repl.* | *Untreated* | 325 | 164 |
| | *Treated* | 222 | 138 |
| *k=10, cal=0.01, repl.* | *Untreated* | 34 | 455 |
| | *Treated* | 21 | 339 |
| *k=1, cal=0.01, norepl.* | *Untreated* | 325 | 164 |
| | *Treated* | 206 | 154 |
| *epanechnikov, bw=0.06, cs.* | Untreated | 4 | 485 |
| | Treated | 19 | 341 |
| *epanechnikov, bw=0.01, cs.* | Untreated | 38 | 451 |
| | Treated | 32 | 328 |
| *normal, bw=0.06, cs.* | Untreated | 4 | 485 |
| | Treated | 19 | 341 |
| *normal, bw=0.01, cs.* | Untreated | 4 | 485 |
| | Treated | 19 | 341 |

Figure A6: Average Marginal Effect of short and medium term furlough schemes by region with 95% C.I.s.

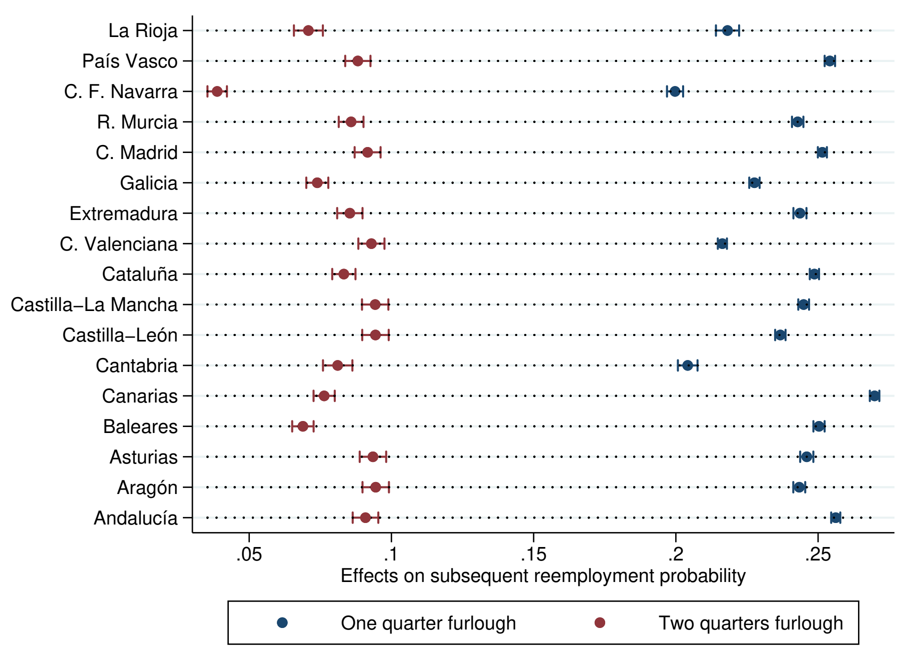

Table A5: Logit output on the outcome Pr(reemployed=1) for the regional and sectoral analysis.

| *Variables* | *Short-term regress.* *Coefficient (z-stat)* | *Medium-term regress.* *Coefficient (z-stat)* |
|---|---|---|
| Furloughed | 1.206 (283.48)*** | 0.397 (39.40)*** |
| Male | . | . |
| Female | -0.097 (-24.87)*** | -0.131 (-14.93)*** |
| Andalucía | . | . |
| Aragón | 0.227 (18.09)*** | 0.481 (17.02)*** |
| Asturias | 0.184 (12.47)*** | 0.648 (18.34)*** |
| Baleares | 0.111 (9.45)*** | -0.807 (-28.43)*** |
| Canarias | -0.638 (-83.16)*** | -0.578 (-34.65)*** |
| Cantabria | 0.741 (37.57)*** | -0.420 (-7.39)*** |
| Castilla-León | 0.326 (35.36)*** | 0.339 (17.19)*** |
| Castilla-La Mancha | 0.202 (19.14)*** | 0.315 (15.24)*** |
| Cataluña | 0.140 (22.70)*** | -0.343 (-26.04)*** |
| C. Valenciana | 0.595 (84.07)*** | 0.148 (9.77)*** |
| Extremadura | 0.223 (15.03)*** | 1.107 (38.27)*** |
| Galicia | 0.451 (51.44)*** | -0.654 (-29.12)*** |
| C. Madrid | 0.090 (14.14)*** | 0.0464 (3.53)*** |
| R. Murcia | 0.235 (20.11)*** | -0.243 (-9.30)*** |
| C. F. Navarra | 0.793 (50.58)*** | -1.737 (-33.74)*** |
| País Vasco | 0.041 (4.07)*** | -0.140 (-5.60)*** |
| La Rioja | 0.572 (23.12)*** | -0.749 (-13.24)*** |
| Ceuta | 1.507 (15.87)*** | . |
| Melilla | -0.037 (-0.56) | . |
| Age | -0.003 (-21.26)*** | -0.006 (-15.95)*** |
| Non-higher education | . | . |
| Higher education | -0.015 (-3.73)*** | -0.003 (-0.29) |
| Primary sector | . | . |
| General manufacturing | 0.612 (43.73)*** | 0.496 (20.18)*** |
| Extract, supply and other industr. | 0.866 (60.17)*** | 0.491 (18.59)*** |
| Machinery, install and repair | 0.698 (50.59)*** | 1.711 (57.64)*** |
| Construction | 1.119 (87.41)*** | 0.203 (9.21)*** |
| Trade, acommodation, food serv. | 0.802 (70.41)*** | -0.159 (-9.03)*** |
| Transp. and store/ info. and comm. | 0.705 (55.41)*** | -0.594 (-26.39)*** |
| FI./ r.estate/ prof-sci-tech | 0.582 (46.16)*** | 0.269 (13.21)*** |
| Public admin/ educ and health | 0.394 (31.79)*** | 0.157 (7.94)*** |
| Other services | 0.827 (66.59)*** | -0.808 (-37.41)*** |
| Indefinite | . | . |
| Temporary | -0.173 (-40.69)*** | 0.063 (6.45)*** |
| Full-time | . | . |
| Part-time | -0.262 (-60.28)*** | 0.357 (36.99)*** |
| Constant | -0.739 (-56.65)*** | -0.370 (-16.39)*** |
| Observations † | 1,575,212 | 309,893 |
| Pseudo $R^2$ | 0.093 | 0.055 |
| $\chi^2$ | 192,184.1 | 22,646.1 |
| p $> \chi^2$ | 0.000 | 0.000 |

$z$ statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

† Note: sample weights have been applied