THE EMPLOYMENT EFFECTS OF JOB RETENTION SCHEMES DURING THE COVID-19 PANDEMIC

Alexandre Ounnas

CEPS IN-DEPTH ANALYSIS

December, 2024-17

SUMMARY

Job retention schemes were one of the main policy instruments implemented across EU Member States to weather the negative economic effects of the Covid-19 pandemic. These schemes, including furlough programmes and wage subsidies, were introduced to maintain an attachment between employers and employees during the severe economic downturn. By supporting workers' incomes and allowing firms to reduce working hours rather than resorting to layoffs, job retention schemes aimed to minimise job losses and hasten labour market recovery.

More than four years later, this CEPS' In-Depth Analysis report evaluates the employment effects of job retention schemes during the pandemic, relying on data from the EU Labour Force Survey and advanced statistical techniques. The report estimates that job retention schemes significantly supported employment in 2020. They reduced job losses, enhancing employment growth by an average of five percentage points in the short term and preserving an estimated 13.9 million jobs across the EU. In some scenarios, this figure could rise to 20.3 million. However, the research also suggests that the rapid post-pandemic economic recovery and the broad eligibility criteria for job retention schemes might have led to their overuse, where fewer jobs might have been at risk than originally anticipated.

The analysis underscores the effectiveness of job retention schemes to dampen the negative effects of economic shocks on the labour market but also highlights the need for further research to understand how the design of the schemes affects the balance between economic support and efficient take-up. It provides important insights for shaping social and employment policies in the EU, emphasising the benefits from adaptable mechanisms to protect jobs.

These findings contribute to a deeper understanding of labour market interventions for future crisis-response strategies to ensure resilience and fairness in the labour market across Europe.

Alexandre Ounnas is a Research Fellow in the Jobs & Skills unit at CEPS.

This CEPS In-Depth Analysis paper is based on the Annex to an external study prepared for the Committee on Budgetary Control of the European Parliament under the framework contract IP/D/ALL/FWC/2020-001/C5. The views expressed herein are solely those of the author and the European Parliament assumes no responsibility for any consequences arising from the reuse of this publication.

CEPS ▪ Place du Congrès 1 ▪ B-1000 Brussels ▪ Tel: (32.2) 229.39.1

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INTRODUCTION

In response to the Covid-19 pandemic, EU Member States put in place various job retention (JR) schemes to support employment. These measures included short-time work schemes, furlough schemes and new income support schemes in the form of wage subsidies (Corti et al., 2023). The take-up of JR schemes was significant: by the beginning of May 2020, close to 42 million workers in the EU had applied for access – a far greater number than during the Great Recession (Müller & Schulten, 2020).

JR schemes enable the employer-employee attachment to continue during economic downturns by incentivising employment adjustments along the intensive (down to zero hours worked) rather than through the extensive margin (i.e. layoffs). These schemes support workers' income and should hasten labour market recovery as neither firms nor workers need to go through the resource-consuming process of looking for a new worker/job once the economy recovers. However, JR schemes could also distort the labour market as they limit the reallocations of workers from less to more productive firms/sectors (Cahuc, 2019), leading to uncertain labour market outcomes in the medium to long run.

This report analyses the employment effects of JR schemes during the Covid-19 recession using data from the EU Labour Force Survey (EU-LFS). Studies on the topic usually rely on the specification of dynamic versions of Okun's law estimated on country-level panels (Hijzen & Martin, 2013; Brey & Hertweck, 2020; Eurofound, 2024), or analyse employment effects for specific Member States (Cahuc et al. 2021; Lafuente Martinez & Ruland, 2022; Christl et al. 2023). Hence, the individual-level nature of EU-LFS data, its availability for all Member States and its representativeness are noteworthy advantages for providing complementary evidence on the labour market impacts of JR schemes. Moreover, the EU-LFS ensures that data on employment and JR schemes is collected from the same source following the same methodology.

Yet, the EU-LFS does not provide for the direct identification of workers on JR schemes. Corti et al. (2023) construct a proxy variable using information on workers absent from work or on reduced hours due to economic conditions. They show that the proxy variable leads to JR scheme numbers that are generally aligned with other (administrative) sources (Corti et al. 2023; Kiss-Galfavi et al. 2024). This proxy variable is therefore used to identify observations of workers who maintained an attachment with their employers during the recession, most likely through JR schemes.

The methodology employs the difference-in-difference (DID) estimator proposed by de Chaisemartin & D'Haultfoeuille, (2024). This estimator can handle staggered and continuous treatment designs, which are features of the sample. The treatment corresponds to the take-up of JR schemes by groups defined at the sector and occupation levels through NACE 1-digit sector and ISCO 3-digit occupation codes. The outcome variable is the employment stock in the same sector-occupation groups, expressed in logarithm. Thus, the DID estimator can be interpreted (approximately) as the average difference in employment growth rates between the treated and control groups, with the latter defined by sector-occupations that did not make use of JR schemes during the Covid-19 pandemic.

The analysis is restricted to periods from the fourth quarter of 2019 (2019Q4) to the fourth quarter of 2020 (2020Q4) due to changes in the EU-LFS introduced in 2021 (i.e. the new framework regulation on Integrated European Social Statistics). The new regulation led to the discontinuation of key variables necessary to compute the proxy variable for JR schemes. Furthermore, the Covid-19 pandemic caused important disruptions in data collection, particularly during the first semester of 2020 (European Commission, 2022). As a result, the analysis in this report has been performed at the EU level alone (excluding Germany for which data is not available).

The findings suggest that JR schemes had significant effects on supporting employment in 2020. On impact, JR schemes boosted (i.e. limited the decrease in) employment growth by around 5 percentage points (pp) on average. The effects one period after the treatment are of the same order of magnitude and even increase to 10 pp for some specifications considered in the analysis. The estimated effects two and three periods after the initial treatment should be viewed with care, given the characteristics of the sample, but these effects are never found to be significant. This could indicate that the positive effects of JR schemes on employment were rather short-lived. These results hold when several alternative specifications are considered.

The report further makes two methodological contributions that are potentially relevant for the evaluation of JR schemes (and beyond). First, it is shown that the point estimates obtained from the DID estimation are likely to be affected by the differences in the groups used to compute estimates at different leads (and lags). This can affect the interpretation of the results, so a slightly modified estimator is proposed (in the Annex) to account for this discrepancy. Second, I convert the DID estimates in terms of jobs saved and generate a job-saving ratio (i.e. the number of jobs saved divided by the stock of workers on JR schemes). Due to technical constraints, the methodology could only be applied to a subset of the samples and specifications considered in the report.

In general, the job-saving ratio is found to be less than 1 with a value of around 0.75. Bearing in mind that 18.5 million workers in the EU (excluding Germany) benefitted from JR schemes in 2020 (according to EU-LFS data, see Kiss-Galfavi et al. 2024), a job-saving ratio of 0.75 would imply that JR schemes contributed to preserving around 13.9 million jobs in 2020. In the most positive case, the job-saving ratio reaches a value of 1.1 – implying that JR schemes contributed to preserving 20.3 million jobs in 2020, a number similar to the results reported in a recent Eurofound study (Eurofound, 2024)[1].

Overall, the analysis confirms that JR schemes significantly contributed to supporting employment during the Covid-19 pandemic. Still, a job-saving ratio of less than one would also suggest a potential over-utilisation of JR schemes given the evolution of employment in the control group. These results could be explained by different factors, including the use of the more conservative DID estimates to compute the job-saving ratio.

Amid these factors, it is also worth mentioning the loosening of eligibility criteria to access the schemes (e.g. the self-employed), which may have contributed to increasing the take-up (Corti et al., 2023)[2] and the very specific nature of the Covid-19 economic shock, in particular the rapid recovery that ensued after the lifting of restrictions[3]. This V-shaped recovery is likely to have limited the positive effects of JR schemes owing to the faster labour market recovery.

[1] When Germany is filtered out from the results, the study reports that JR schemes preserved around 20.1 million jobs in 2020 (Table 7 in the report).

[2] And may have led to heterogenous effects of the schemes across different forms of employment (e.g. regular employee, temporary worker, self-employed).

[3] As exemplified by the evolution of employment in the control group.

1. DATA, METHODOLOGY AND TREATMENT DESIGN

1.1. DATA AND VARIABLES OF INTEREST

This exercise relies on the proxy variable constructed to identify workers on JR schemes (Corti et al., 2023; Kiss-Galfavi et al., 2024). It is important to remember that the EU-LFS does not provide a direct way to identify workers on JR schemes. Instead, the proxy variable makes use of information on workers absent from work or on reduced hours due to economic conditions/slack work, and who are still (partially) remunerated by their employers. Corti et al. (2023) show that this proxy variable results in JR stocks that are close to numbers obtained via other sources. Thus, to be more precise, the exercise in this section evaluates the employment effects of maintaining an employer-employee attachment during economic downturns. The attachment is likely to be preserved through a JR scheme, though the EU-LFS does not allow us to claim this with certainty.

A second limitation from the EU-LFS data regards the break in 2021 originating from the introduction of a new Integrated European Social Statistics Framework Regulation. The new regulation resulted in the modification (or discontinuation) of some of the variables used to compute the JR proxy. As a result, the analysis is limited to 2020 quarter 4 (2020Q4), although it should be possible to perform some adjustments on the data post-2020 to recover the proxy. This adjustment would allow for the estimation of medium-term effects up to 2022, but for this analysis, only short-term effects will be estimated. Finally, the disruption in data collection during the early phase of the pandemic implies that EU-LFS data for this period should be used with care (European Commission, 2022).

This exercise can nevertheless offer relevant insights into the employment effects of JR schemes. The micro-level nature of the EU-LFS enables the use of different methodologies to estimate the causal impact of a treatment (the take-up of JR schemes) on an outcome (employment). This constitutes an advantage in comparison with most of the evidence currently available on the topic, which usually relies on the estimation of dynamic versions of Okun's law using country-level macro-panels (Hijzen & Martin, 2013; Brey & Hertweck, 2020; Eurofound, 2024). Furthermore, the EU-LFS is the official source of statistics on labour markets in the EU. As such, it is of interest to estimate the impact of treatments on outcomes as measured through this dataset. The dataset further ensures greater harmonisation and comparability of data across Member States. Overall, this evaluation exercise should be seen as complementary to the already available evidence, providing new insights based on a data source and a methodology rarely used in the JR schemes literature.

The dataset for this analysis consists of quarterly EU-LFS data for the period 2019Q4 to 2020Q4. In addition to the proxy variable, the outcome of interest is the quarterly stock of employment computed from the EU-LFS and restricted to workers in employment (i.e. employees or the self-employed), aged 15-64, who only hold one job[4]. Family workers or individuals doing their military training or working for the armed forces are dropped from the sample. Due to the possible effects of the pandemic on data collection, this preliminary analysis is performed at the EU level by summing Member State EU-LFS files, except for Germany, which does not provide quarterly data for 2020. Exploring the potential for heterogenous effects of JR schemes across Member States is left for future work. It should also be noted that the data has not been seasonally adjusted since only one year of data is used. Seasonality should be addressed if the analysis is to be extended beyond 2020.

1.2. METHODOLOGY AND TREATMENT DESIGN

The methodology used to estimate the employment effects of JR schemes relies on the estimator developed by de Chaisemartin & D'Haultfoeuille, (2024). They propose a DID estimator, which is valid under standard assumptions found in the potential outcome literature (Rubin, 2005), and which can accommodate a wide range of treatment designs[5], while ensuring valid comparisons between the outcome of interest and the constructed counterfactual. The sample consists of T time periods and G groups. In our set-up, $\mathrm{T} = 5$ from 2019Q4 to 2020Q4 and the group level unit g is defined at the sector and occupation levels, through NACE 1-digit and ISCO 3-digit codes. This is a relevant level for this study as evidence indicates that the use of JR schemes varied significantly across sectors and occupations (Corti et al., 2023). Among the Member States, BG, MT, PL, and SI provide only 2-digit ISCO codes in their EU-LFS files and these countries are therefore not included in the current analysis. Moreover, some Member States, namely, CZ, DK, EE, ES, IE, LV, NL, and SK, provide information at less than 3-digit ISCO codes for a small share of their samples (usually less than 2 % of observations). These observations are not considered in this analysis either. The NACE 1-digit codes are also slightly adjusted. Workers in sectors T and U[6] are dropped from the sample, and sectors B, D and E[7] are aggregated together (i.e. the industry sector less manufacturing and construction). The treatment is defined as the weighted stocks (using sample weights) of workers on JR schemes in group g (i.e. in sector i and occupation j), obtained through our proxy variable. Likewise, the outcome of interest is defined by the employment level in group g

[4] This implies that workers on layoff are not considered in this analysis given that their labour market status is either unemployed or inactive. Corti et al. (2023) include these workers in their proxy.

[5] In our set-up, the fact that the treatment (the number of workers on JR schemes) is staggered and continuous is a constraint, which is accommodated by the estimator proposed by de Chaisemartin & D'Haultfoeuille, (2024).

[6] Respectively, activities of households as employers; activities of extraterritorial organisations and bodies.

[7] Respectively, mining and quarrying; electricity, gas, steam, and air conditioning supply; water supply, sewerage, waste management and remediation activities.

computed as the weighted sum of individual observations belonging to group g. Groups g with less than 1 (non-weighted) observation available on average over the sample period are dropped from the sample.

The DID estimator proposed by de Chaisemartin & D'Haultfoeuille, (2024) imposes some restrictions on the design of the treatment and hence on the sample. Using their notations, let us define $\mathrm{D}_{\mathrm{g,t}}$ as the value of the treatment at time t in group g (i.e. the stock of workers on JR schemes), F_{g} the first period t for which the treatment increases ($\Delta \mathrm{D}_{\mathrm{g,t}} > 0$), and T_{g} the maximum time period for which there is at least one non-treated group in the set defined by $\mathrm{D}_{\mathrm{g,1}} = \mathrm{D}_{\mathrm{g',1}}$. The DID estimator can then be obtained from groups g with the same treatment starting value, $\mathrm{D}_{\mathrm{g,1}} = \mathrm{D}_{\mathrm{g',1}}$, but a different first treatment period, $\mathrm{F}_{\mathrm{g,t}} \neq \mathrm{F}_{\mathrm{g',t}}$.

In the current set-up, the treatment is continuous and the stock of workers on JR schemes (based on our proxy variable) can be positive before 2020[8]. This implies that $\mathrm{D}_{\mathrm{g,1}} = \mathrm{D}_{\mathrm{g',1}}$ is unlikely to be met except when $\mathrm{D}_{\mathrm{g,1}} = 0$. As a result, the analysis starts with the imposition of the latter restriction, which further presents the advantage of ensuring that $\mathrm{D}_{\mathrm{g,t}} - \mathrm{D}_{\mathrm{g,1}} \geq 0$ and hence that the no-crossing condition of de Chaisemartin & D'Haultfoeuille, (2024) is met[9]. Therefore, the restriction $\mathrm{D}_{\mathrm{g,1}} = 0$ significantly simplifies the set-up at the cost of reducing the sample size (as discussed below). This restriction can be relaxed and an extension exists for estimating causal effects when the condition $\mathrm{D}_{\mathrm{g,1}} = \mathrm{D}_{\mathrm{g',1}}$ fails. This extension is explored at a later point of this analysis. Finally, it is worth mentioning that there are groups g in the sample that are never treated, implying that $\mathrm{T}_{\mathrm{g}} = 5$ for all g and effects up to four periods after the treatment can be estimated. The never-treated group is assigned $\mathrm{F}_{\mathrm{g}} = 6$.

Given that our set-up (initially) imposes $\mathrm{D}_{\mathrm{g,1}} = 0$ and that the no-crossing condition is satisfied, the estimator can be written with references only to F_{g}. This simplifies the expressions displayed below. The DID estimator takes the following form:

$$\mathrm{DID}^{\mathrm{L}}_{\mathrm{g,l}} = \mathrm{Y}_{\mathrm{g,F_g+l-1}} - \mathrm{Y}_{\mathrm{g,F_g-1}} - \left(\frac{1}{\mathrm{N}^{\mathrm{g}}_{\mathrm{F_g+l-1}}} \sum_{\mathrm{g':F'_g>F_g+l-1}} \mathrm{Y}_{\mathrm{g',F_g+l-1}} - \mathrm{Y}_{\mathrm{g',F_g-1}} \right), \tag{1}$$

$$\mathrm{DID}^{\mathrm{L}}_{\mathrm{l}} = \frac{1}{\mathrm{N_l}} \sum_{\mathrm{g:T_g \geq F_g+l-1}} \mathrm{DID}^{\mathrm{L}}_{\mathrm{g,l}}, \tag{2}$$

[8] JR schemes existed in several Member States before the Covid-19 pandemic (e.g. BE, DE, and FR).

[9] Since the stock of workers on JR schemes can never be negative.

where N_{F_g+l-1} is the number of observations not yet treated, which are used to construct the counterfactuals, and N_l is the number of groups satisfying $T_g \geq F_g - 1 + l$ and which serve to estimate the effects l periods after the treatment. It should be noted that the estimators above are expressed in terms of the relative time index $l \in \{1, \dots, T_g - F_g + 1\}$, and $l = 0$ for the time period just before the first treatment. Hence, the time period t associated with $l = 0$ will vary with the first treatment date F_g (e.g. for group g with $F_g = 2,\ l = 0$ when $t = 1$ (2019Q4); for group g with $F_g = 3,\ l = 0$ when $t = 2$). This definition further implies that $l = 1$ corresponds to the effect on impact when groups g are first treated. Thus, referring to DID_l^L as the effect l periods after the treatment is not fully correct, though this characterisation is often used below when discussing the results. Working with the relative time index l is justified by the staggered design of the treatment and ensures that the effects are analysed consistently across groups being treated at different points in time.

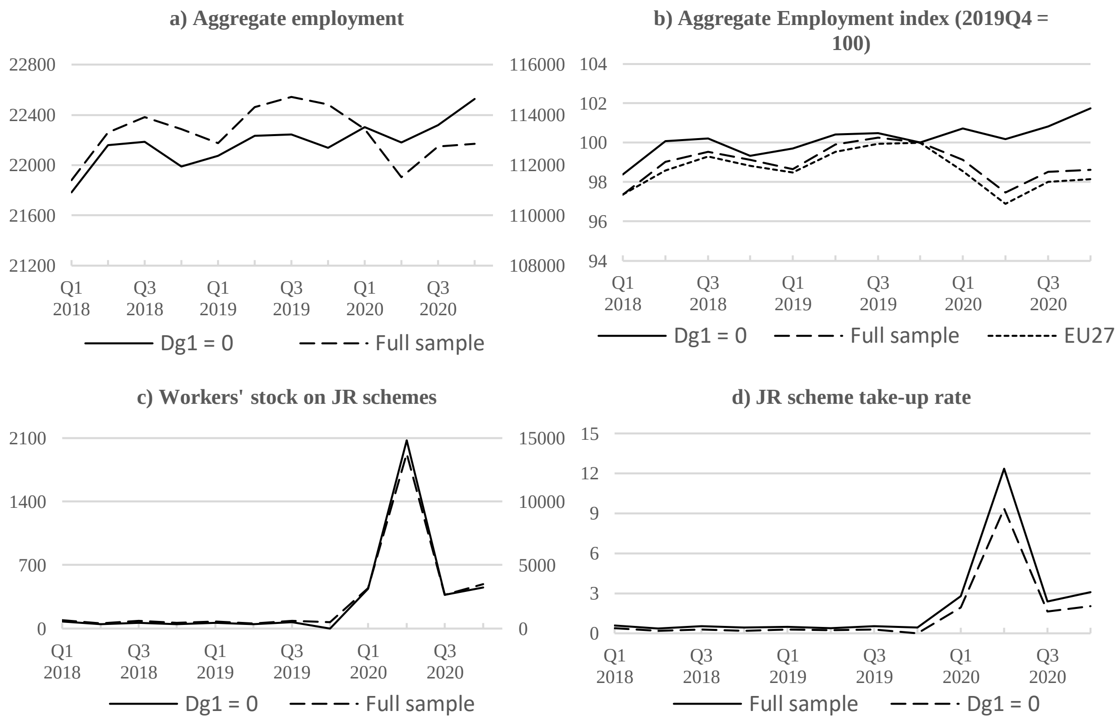
From equations (1) and (2), the DID estimator takes a rather intuitive form. For each g, equation (1) computes the difference between the realised outcome l period(s) after the last period before treatment, $Y_{g,F_g+l-1} - Y_{g,F_g-1}$, and a counterfactual (in parentheses in equation (1)) constructed by averaging the differences in outcomes across the same l period(s) for the groups g' not yet treated (i.e. $g' \colon F'_g > F_g + l - 1$). It is assumed that in the absence of treatment, group g's employment would have evolved similarly to employment in the not-yet treated group. These quantities can be computed for all groups g with $T_g \geq F_g - 1 + l$ and are then averaged to obtain an estimated value for DID_l^L. It should be clear that this effect does not account for the treatment intensity (i.e. variations in take-up rates across g) but is only concerned with whether groups g have been treated.

Furthermore, the average duration of treatment and its profile (i.e. if treatment increases with time) will affect the estimated values for DID_l^L. The size of each group g (i.e. the employment level) can also matter for the estimated DID_l^L since the outcome enters equation (1) in differences. This can be addressed by defining the outcome in $\log$ such that $Y_{g,F_g+l-1} - Y_{g,F_g-1}$ now corresponds (approximately) to the percentage variation in the outcome l period(s) after the last period before treatment.

Finally, it is shown in Annex B Section B1 that the estimates for DID_l^L across the l periods are affected by differences in the sets of groups g and g' used to compute $DID_{g,l}^L$ and DID_l^L. These differences can create inconsistencies when comparing point estimates for DID_l^L for different values of l (see Figure 15), though this is unlikely to matter from a statistical point of view if the assumptions underlying the estimator are met.

Before discussing estimation results, Figure 1, Figure 2, and Figure 3 present some evidence on the sample used for the estimation of $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$. These figures mainly aim at assessing the impact of the restriction $\mathrm{D}_{\mathrm{g},1} = 0$, to better understand the series for the outcome and treatment by the first treatment date F_{g}.

Figure 1: Outcome and treatment in the restricted and full sample – $\mathrm{D}_{\mathrm{g},1} = 0$



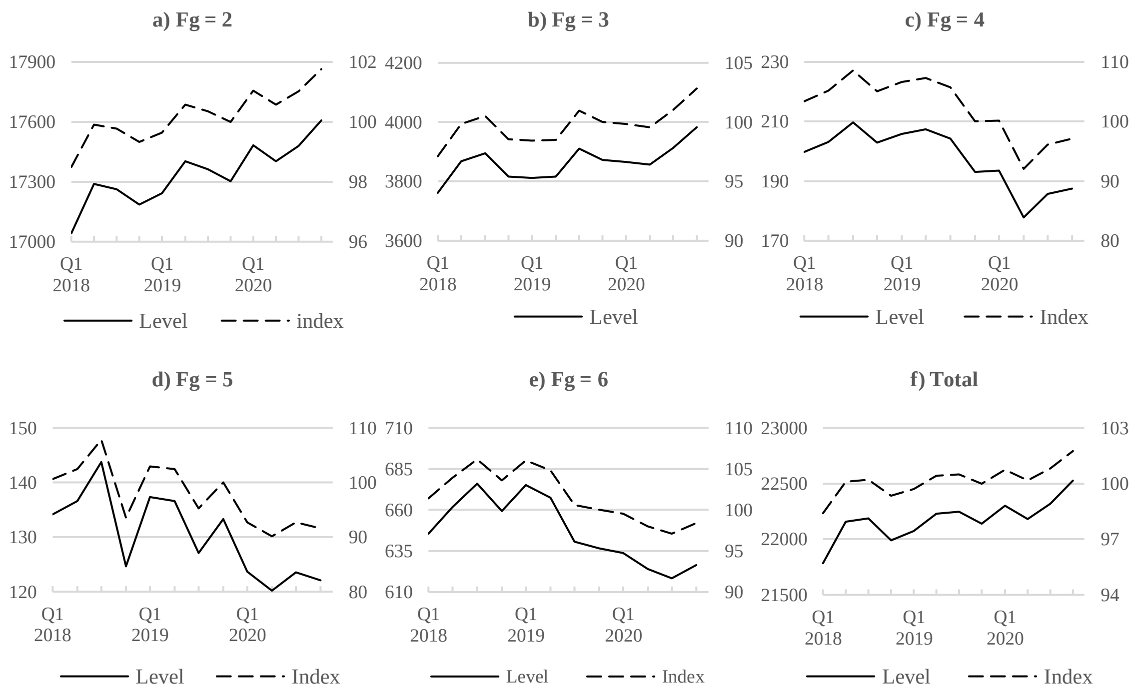
Source: Author's own elaboration

Note: In panels a) and c), the restricted sample ($\mathrm{D}_{\mathrm{g},1} = 0$) is displayed on the left axis and the full sample on the right axis. Data for the EU27 in panel b) is taken from Eurostat [lfsq_egan]. Stocks are displayed in thousands in panels a) and c) and in percentages for the take-up rate in panel d).

From Figure 1, it is first worth noting that the employment series obtained from the EU-LFS, which excludes some Member States (e.g. DE and PL) and imposes other restrictions, evolves in a similar way to the employment series at the EU27 level (panel b). From panel a), the restricted sample corresponds to a little bit less than 20 % of the original sample. More importantly, the restricted employment series does not decrease during 2020 (when compared with the 2019Q4 level, employment does decrease between 2020Q1 and 2020Q2). This suggests that the restriction $\mathrm{D}_{\mathrm{g},1} = 0$ leads to a sample of sectors-occupations g which did not suffer significantly from the Covid-19 pandemic. This could influence the estimation results, and it will therefore be important to perform robustness checks adjusting the sample size to increase its representativeness.

Regarding the treatment, panels c) and d) in Figure 1 show that the stock of workers on JR schemes follows a similar evolution in the restricted and full EU-LFS samples, with a first increase in 2020Q1, a maximum reached in 2020Q2 and a small rebound in 2020Q4. The main difference across samples regards the take-up rate, which is smaller in the restricted sample. Given that the size of the treatment does not impact $\mathrm{DID}_1^{\mathrm{L}}$, the difference in take-up should not have a major impact on the estimation results.

Figure 2: Employment levels and indices by first treatment period – $\mathrm{D}_{\mathrm{g},1} = 0$



Source: Author's own elaboration

Note: These series are obtained under the restriction $\mathrm{D}_{\mathrm{g},1} = 0$. Series are displayed in levels in thousands on the left axis and as indices on the right axis.

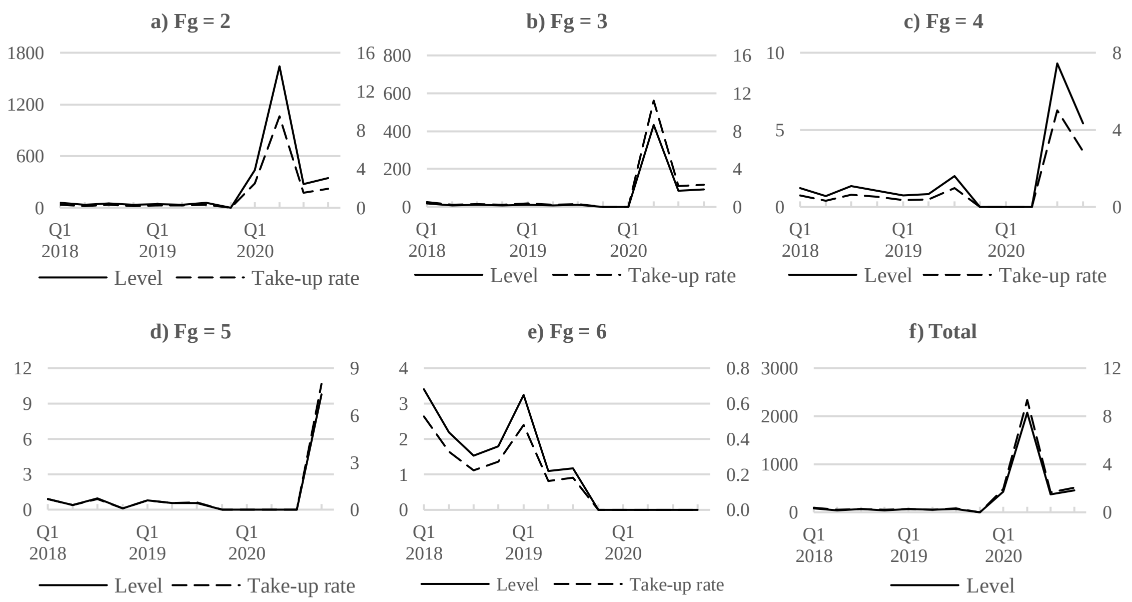
Figure 2 and Figure 3 display evidence on the outcome and treatment by the first treatment date. The main takeaway from these figures is that almost the entirety of the groups g belong to the set of industries-occupations treated in either periods two (2020Q1) or three (2020Q2). The employment stock treated at later periods represents a small share of total employment, though this does not mean that the number of groups g with $\mathrm{F}_{\mathrm{g}} > 3$ is small. In fact, there are more than 220 industry-occupations that are part of the never-treated group (see Table 1). This evidence further motivates specifying the outcome in $\log$ to account, at least to some extent, for the heterogeneity in outcomes size by groups.

Regarding the evolution of the outcome during 2020, we note that employment evolved more 'positively' in sectors-occupations treated the earliest. Employment for sectors-occupations treated in periods two and three (2020Q1 and 2020Q2) was around 2-3 percentage points (pp) above the 2019Q4 levels one year later. In contrast, employment was 1.5 pp below its 2019Q4 level for the never-treated groups (i.e. with $F_g = 6$).

Evidence from Figure 8, which is based on a more representative sample, seems to indicate that this is partly the result of the restriction $D_{g,1} = 0$ since panel a) shows that when (almost) the entirety of the sample is considered, employment for groups g with $F_g = 2$ did decrease during the first part of 2020 and had not recovered to its 2019Q4 level by the end of 2020.

This descriptive evidence tends to indicate that the restrictions $D_{g,1} = 0$ have a substantial impact on the representativity of the sample used for estimation. For this reason, Section 2 presents results for the restricted sample and the extension to continuous treatment starting values proposed by de Chaisemartin & D'Haultfoeuille, (2024). This extension allows the retention of more than 90 % of the original EU-LFS data and thus improves the representativity of the sample used for estimation (see Figure 7 to Figure 9).

Figure 3: JR stocks and take-up rates by first treatment period – $D_{g,1} = 0$



Source: Author's own elaboration

Note: These series are obtained under the restriction $D_{g,1} = 0$. Series are displayed in levels in thousands on the left axis and in percentages as rates on the right axis.

2. RESULTS

The estimation results for the restricted sample (i.e. $D_{g,1} = 0$) are displayed in Table 1 and Figure 4. Coefficients for each possible lead $l \in \{1, \ldots T_g - F_g + 1\}$ are reported together with two placebo effects $l \in \{-2, -1\}$. The latter enable insights into the validity of the parallel trend (no anticipation) assumption and the consistency of the estimates. This is the maximum number of placebo effects that can be estimated given our set-up[10], meaning that effects for $l > 2$ should be considered with de Chaisemartin & D'Haultfoeuille, (2024). In addition to the baseline specification, we estimate specifications with control variables. The controls include sectoral value added aggregated in 10 broad sectors and its first lag values[11], 1-digit NACE and ISCO fixed effects and interaction variables between sectoral value added and the sector fixed effects. Estimates obtained under two nonparametric trend specifications, which construct counterfactuals for the computation of $DID^{L}_{g,l}$ in equation (1) at the level of pre-specified variable(s), are also displayed in Table 1 (and Figure 4). The two variables considered are NACE 1-digit and ISCO 1-digit codes. The interaction of these two variables is used to cluster standard errors.

In Table 1, the placebo effect at lag 1 (i.e. one period prior to the last period before treatment) is never significant. At lag 2, the estimated coefficient is significant, at least at the 10 % level, across all specifications. This could indicate a potential problem with the model specification, although the small number of groups available for this estimation (35 relevant not-yet treated observations) also results in imprecise estimates and large standard errors. Jointly, the placebo effects are never significant except for the NACE nonparametric trend specification (see the bottom part of Table 1). Hence, the results at lag 2 should not be seen as too worrisome but reiterate the need to consider the effects for $l > 2$ (and perhaps even $l > 1$) with care.

Regarding the estimated coefficients for all leads, we first note that the effect on impact of being treated, $l = 1$, is positive and significant in all specifications. Given that our outcome variable is expressed in $\log$, the estimated effects indicate that the average growth rate of employment in treated sectors-occupations was between 4.6 and 6.7 pp greater than in non-treated sectors-occupations. This suggests a relatively large and

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[10] To estimate additional placebo effects, one could make use of data before the first treatment since the EU-LFS offers longer time series. However, the sample would have to be further restricted since, for instance, $D_{g,0} = D_{g,1} = 0$ would be required in our current set-up to use 2019Q3 data. Annex A3 presents estimation results when this restrictions is imposed and shows no particular issues with the parallel trend assumption except for the coefficients at lag 3, which can be significant. Jointly, placebo effects are never significant and estimation results for effects after the treatment occurs are generally not very different from the results reported in Table 1.

[11] Obtained from Eurostat [namq_10_a10].

positive effect of JR schemes, even though the take-up of the schemes is required to provide a more definitive judgement on the size of the effect (see Section 3). Following the impact effect of the first treatment, the estimated effect decreases in the baseline and ISCO nonparametric trend specifications and increases for the remaining ones. In general, these effects are less precisely estimated than the first lead coefficients (Figure 4).

Table 1: Estimation results – restricted sample ($D_{g,1} = 0$)

		Baseline	Controls				Nonparam. trend	
			(1)	(2)	(3)	(4)	NACE 1d	ISCO 1d
DID_{-2}	coeff.	-0.100*	-0.092*	-0.103*	-0.097*	-0.106‡	-0.200†	-0.088*
	(switcher; total)	(35; 258)	(35; 258)	(35; 258)	(35; 258)	(35; 258)	(35; 219)	(35; 257)
DID_{-1}	coeff.	-0.043	-0.051	-0.054	-0.046	-0.045	-0.041	-0.041
	(switcher; total)	(457; 1221)	(457; 1221)	(457; 1221)	(457; 1221)	(457; 1221)	(457; 1117)	(457; 1209)
DID_0		0	0	0	0	0	0	0
DID_1	coeff.	0.048†	0.065†	0.055†	0.067†	0.062†	0.064†	0.046†
	(switcher; total)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2235)	(895; 2327)
DID_2	coeff.	0.039	0.072‡	0.059*	0.076‡	0.071‡	0.071*	0.043
	(switcher; total)	(865; 1629)	(865; 1629)	(865; 1629)	(865; 1629)	(865; 1629)	(865; 1590)	(865; 1627)
DID_3	coeff.	0.009	0.052	0.044	0.059	0.063	0.034	0.018
	(switcher; total)	(830; 1306)	(830; 1306)	(830; 1306)	(830; 1306)	(830; 1306)	(830; 1306)	(830; 1305)
DID_4	coeff.	0.010	0.074	0.057	0.077	0.076	0.052	0.015
	(switcher; total)	(438; 661)	(438; 661)	(438; 661)	(438; 661)	(438; 661)	(438; 661)	(438; 661)
Controls								
	Sect. VA	N	Y	N	Y	Y	N	N
	Sect. FE	N	N	Y	Y	Y	N	N
	Occup. FE	N	N	Y	Y	Y	N	N
	Sect.VA X Sect. FE	N	N	N	N	Y	N	N

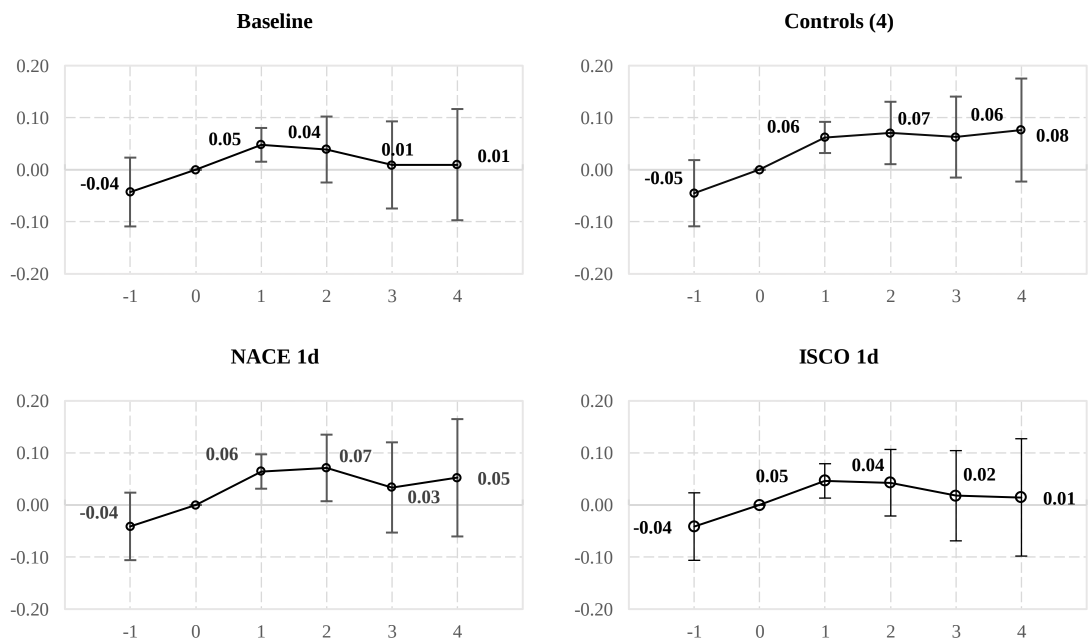
Joint test - Placebo	0.15	0.17	0.13	0.18	0.11	0.02	0.21

† p<0.01, ‡ p<0.05, * p<0.1

Source: Author's own elaboration

Note: Switchers correspond to groups being or having been treated already, relevant for the estimation of $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$. Controls include sectoral value added [namq_10_a10], 1-digit sectors and occupation fixed effects and interaction variables between sectoral value added and sectoral fixed effects. Standard errors are clustered at the NACE 1-digit and ISCO 1-digit level. Results have been generated using the STATA routine 'DID_multiplegt_dyn' developed by de Chaisemartin et al. (2024a).

Figure 4: Estimated effects – restricted sample ($\mathrm{D}_{\mathrm{g},1} = 0$)



Source: Author's own elaboration

Note: Estimates for $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$ at all leads and lags except for placebo coefficients at lag 2, which are imprecisely estimated. $\mathrm{l} = 0$ for the last period before the first treatment takes place.

Some specifications therefore suggest a possible ramping-up effect of JR schemes, which could increase after its effect on impact. However, it is not possible to draw such conclusions based on the estimates in Table 1 since the take-up of JR schemes reached its peak level in 2020Q2 also for those groups first treated in 2020Q1 (with $\mathrm{F}_{\mathrm{g}} = 2$, see Figure 3). Moreover, the point estimates for $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$ could be affected by the issue discussed in Section B1 in Annex B. Nevertheless, the significant effects in Table 1 indicate that the average growth of employment two periods after treatment was between 7.1 and 7.6 pp above the employment growth for not-yet treated sectors-occupations. For the remaining leads, $\mathrm{l} = 3$ and $\mathrm{l} = 4$, the confidence bounds are large around the estimated effects and the coefficients are never significant. Given the uncertainty surrounding these estimates we do not discuss these results further at this stage of the analysis.

As explained at the end of Section 1.2, restricting the sample to groups with $D_{g,1} = 0$ could lead to a dataset that is not representative of the employment evolution during 2020. Ultimately, this issue could be affecting the estimated results reported in Table 1. Two robustness checks are considered to enhance the confidence in our estimated effects. The first one consists of applying the extension proposed by de Chaisemartin & D'Haultfoeuille, (2024) to tackle design, in which $D_{g,1}$ is continuous. Results are reported below in Table 2 and Figure 5[12]. The second check applies an ad-hoc adjustment to the data before estimation, whereby the treatment values $D_{g,t}$ for groups g and period t with take-up rates below 0.5 % are set to 0. Results can be found in Table 3 and Figure 13. These two adjustments considerably increase the representativity of the sample (see Figure 7 and Figure 10), especially in the continuous starting treatment extension.

[12] de Chaisemartin & D'Haultfoeuille, (2024) recommend bootstrapping standard errors when using their continuous starting treatment extension, but this could not be implemented for this version of the report.

Table 2: Estimation results – continuous starting treatment

		Baseline	Controls				Nonparam. trend	
			(1)	(2)	(3)	(4)	NACE 1d	ISCO 1d
DID_{-2}	coeff.	-0.100	-0.092	-0.103	-0.097	-0.106	-0.200	-0.088*
	(switcher; total)	(35; 258)	(35; 258)	(35; 258)	(35; 258)	(35; 258)	(35; 219)	(35; 257)
DID_{-1}	coeff.	-0.043	-0.051	-0.054	-0.046	-0.045	-0.041	-0.041
	(switcher; total)	(457; 1221)	(457; 1221)	(457; 1221)	(457; 1221)	(457; 1221)	(457; 1117)	(457; 1209)
DID_{0}		0	0	0	0	0	0	0
DID_{1}	coeff.	0.039‡	0.055†	0.049‡	0.055†	0.055†	0.054†	0.046†
	(switcher; total)	(1157; 2601)	(1157; 2601)	(1157; 2601)	(1157; 2601)	(1157; 2601)	(1157; 2497)	(895; 2327)
DID_{2}	coeff.	0.045	0.090‡	0.072*	0.097‡	0.099†	0.108‡	0.043
	(switcher; total)	(1127; 1891)	(1127; 1891)	(1127; 1891)	(1127; 1891)	(1127; 1891)	(1127; 1852)	(865; 1627)
DID_{3}	coeff.	0.011	0.058	0.055	0.069*	0.072*	0.046	0.018
	(switcher; total)	(1092; 1568)	(1092; 1568)	(1092; 1568)	(1092; 1568)	(1092; 1568)	(1092; 1568)	(830; 1305)
DID_{4}	coeff.	0.000	0.077	0.070	0.093*	0.082	0.059	0.015
	(switcher; total)	(700; 923)	(700; 923)	(700; 923)	(700; 923)	(700; 923)	(700; 923)	(438; 661)
Controls	-							
	Sect. VA	N	Y	N	Y	Y	N	N
	Sect. FE	N	N	Y	Y	Y	N	N
	Occup. FE	N	N	Y	Y	Y	N	N
	Sect. VA X Sect. FE	N	N	N	N	Y	N	N
Joint test - Placebo		0.38	0.28	0.24	0.33	0.33	0.30	0.21

† p<0.01, ‡ p<0.05, * p<0.1

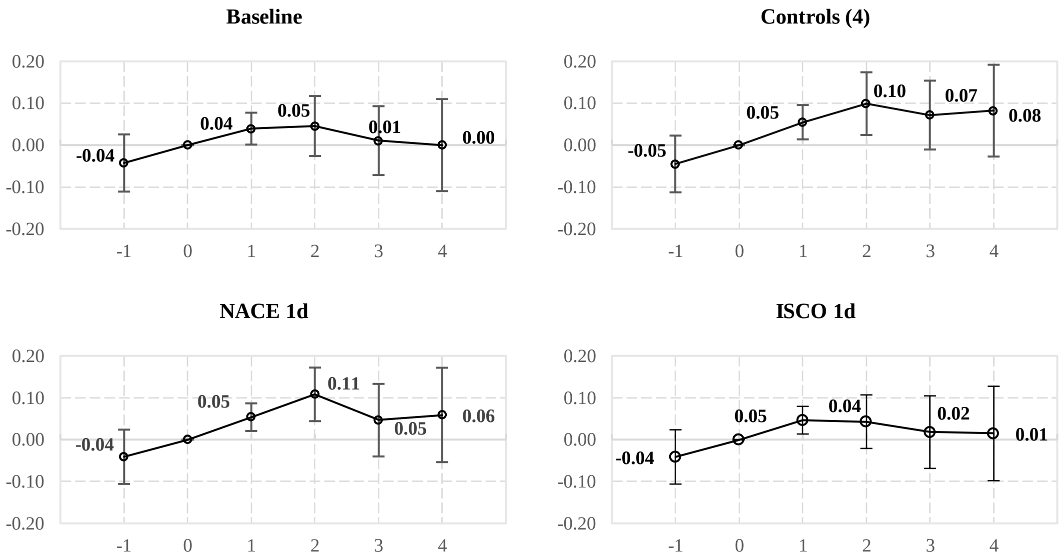
Source: Author's own elaboration

Note: Switchers correspond to groups being or having been treated already, relevant for the estimation of $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$. Controls include sectoral value added [namq_10_a10], 1-digit sectors and occupation fixed effects and interaction variables between sectoral value added and sectoral fixed effects. Standard errors are clustered at the NACE 1-digit and ISCO 1-digit level. Results have been generated using the STATA routine 'DID_multiplegt_dyn' developed by de Chaisemartin et al. (2024a).

From Table 2, the estimated coefficients under the continuous extension are very similar in size and significance to those reported in Table 1 for the restricted sample. The effect on impact ($l = 1$) is generally smaller but the second period effects can also be greater with an average positive effect of JR schemes on employment growth reaching values of around 10 pp for certain specifications. Coefficients at leads 3 and 4 can also be greater but are still not very precisely estimated. This can be understood from the fact that the large majority of additional observations now included in the analysis is treated in period two or three. As a result, the groups available to construct the counterfactual for longer leads (in particular, the never-treated group) are the same across samples. This is confirmed by estimation results for the placebo coefficients, which are the same in both tables. The statistical significance of the placebo test at the lag 2 effect differs, though this effect is again estimated on a very small number of groups.

The estimated effects when the ad-hoc adjustment (based on take-up rates smaller than 0.5 %, see Table 3) is performed are also very much in line with those reported in Table 1 and Table 2. These results seem to converge to indicate a positive impact of JR schemes on employment in the short run up to two periods after the first treatment. In certain specifications, the effects could reach close to an extra 10 pp in employment growth on average in treated industries-occupations, but generally the short-run effects are estimated to lie between 4 and 7 pp of additional employment growth. Effects for the subsequent periods could be positive (in specifications including controls in particular) but the coefficients are imprecisely estimated to conclude that the positive effects of JR schemes go beyond two quarters.

Figure 5: Estimated effects – continuous starting treatment



Source: Author's own elaboration

Note: Estimates for $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$ at all leads and lags except for the placebo coefficients at lag 2, which are imprecisely estimated. $\mathrm{l} = 0$ for the last period before the first treatment.

3. HOW MANY JOBS DID JR SCHEMES SAVE IN 2020?

The results in Section 2 tend to confirm that the take-up of JR schemes is associated with a positive impact on employment, at least in the short run. These estimated effects are expressed in terms of the relative time index l, which can complicate the interpretation of the coefficients and does not directly inform us of the number of jobs saved through the use of these schemes. Using the normalised estimator of de Chaisemartin & D'Haultfoeuille, (2024) can address these issues to a certain extent, but this estimator is not very well suited to our current set-up.

This section computes an estimate of the number of jobs saved for each quarter of 2020 in terms of the time index t. Before delving into the results, it is important to keep in mind that these derivations are preliminary and have not been checked by anyone else other than the author of this report.

For our purpose of obtaining estimated effects by the time index t, it is possible to define the following estimators:

$$\mathrm{DID}^{T}_{g,t} = Y_{g,t} - Y_{g,t-1} - \left(\frac{1}{N^{g}_{t}} \sum_{g':\, F_{g'} > t} Y_{g',t} - Y_{g',t-1} \right), \quad (3)$$

$$\mathrm{DID}^{T}_{t} = \frac{1}{N_t} \sum_{g:\, F_g \leq t} \mathrm{DID}^{T}_{g,t}, \quad (4)$$

where N^{g}_{t} is the number of groups with $g: F_g > t$ used to compute the counterfactuals and N_t is the number groups relevant for the computation of DID^{T}_{t} (i.e. with $g: F_g \leq t$). DID^{T}_{t} corresponds to the average per-period change (or first difference) between the outcome of interest and the relevant counterfactuals[13]. Moreover, the estimators given by equations (3) and (4) are of similar form to $\mathrm{DID}^{L}_{g,l}$ and DID^{L}_{l} obtained from (1) and (2) and as such, it should be possible to obtain analytical standard errors using a similar approach to de Chaisemartin & D'Haultfoeuille, (2024).

$\mathrm{DID}^{T}_{g,t}$ can be used to generate a counterfactual evolution of employment and obtain the number of jobs saved for each quarter t. Note that because the outcome variable is defined in $\log$, equation (3) can be interpreted as the difference between the actual and counterfactual employment growth rates for group g at time t:

$$\mathrm{DID}^{T}_{g,t} = \gamma_{g,t} - \hat{\gamma}_{t}, \quad (5)$$

$$\gamma_{g,t} = Y_{g,t} - Y_{g,t-1}, \quad (6)$$

[13] It is possible to compute a cumulative effect, but it is less relevant to do so when working with the time index t, since a cumulative effect implies that DID^{T}_{t} would be computed on periods for which some groups g are not yet treated.

$$\hat{\gamma}_t \quad = \quad \frac{1}{N_t^g} \sum_{g': F_{g'} > t} Y_{g',t} - Y_{g',t-1} \tag{7}$$

An estimate for the number of jobs saved can be obtained using employment levels for each group g, $E_{g,t}$, and its counterfactual, $\widehat{E}_{g,t}$:

$$\tau_{g,t} \quad = \quad (1 + \gamma_{g,t})E_{g,t-1} \; - \; (1 + \hat{\gamma}_t)\widehat{E}_{g,t-1}, \tag{8}$$

$$\tau_t \quad = \quad \sum_{g: F_g \leq t} \tau_{g,t} \tag{9}$$

Equation (8) can be simplified by noting that $E_{g,t} \approx (1 + \gamma_{g,t})E_{g,t-1}$[14]. Regarding the counterfactual level of employment $\widehat{E}_{g,t}$, its value is set to $E_{g,t}$ for $g: F_g > t$ and can then be obtained recursively using the counterfactual growth rate, $\hat{\gamma}_t$:

$$\widehat{E}_{g,t} \quad = \quad \begin{cases} E_{g,t}, & \text{for } g: F_g > t \\ (1 + \hat{\gamma}_t)\widehat{E}_{g,t-1} & \text{otherwise} \end{cases} \tag{10}$$

Computing $\widehat{E}_{g,t}$ recursively from the period of the first treatment is required to obtain the level of employment that would have prevailed if group g had never have been treated.

It should be noted that the computations described above constitute only one approach to obtaining the counterfactual level of aggregate employment and the number of jobs saved. Instead of computing the counterfactual for each group g, one could use aggregate treated employment and the average growth rates, γ_t and $\hat{\gamma}_t$, obtained from DID_t^T in equation (4), or compute growth rates and DID_t^T by the first treatment date F_g. Only one approach is pursued in this section, although it would be of interest to verify the extent to which the results differ under alternative computations.

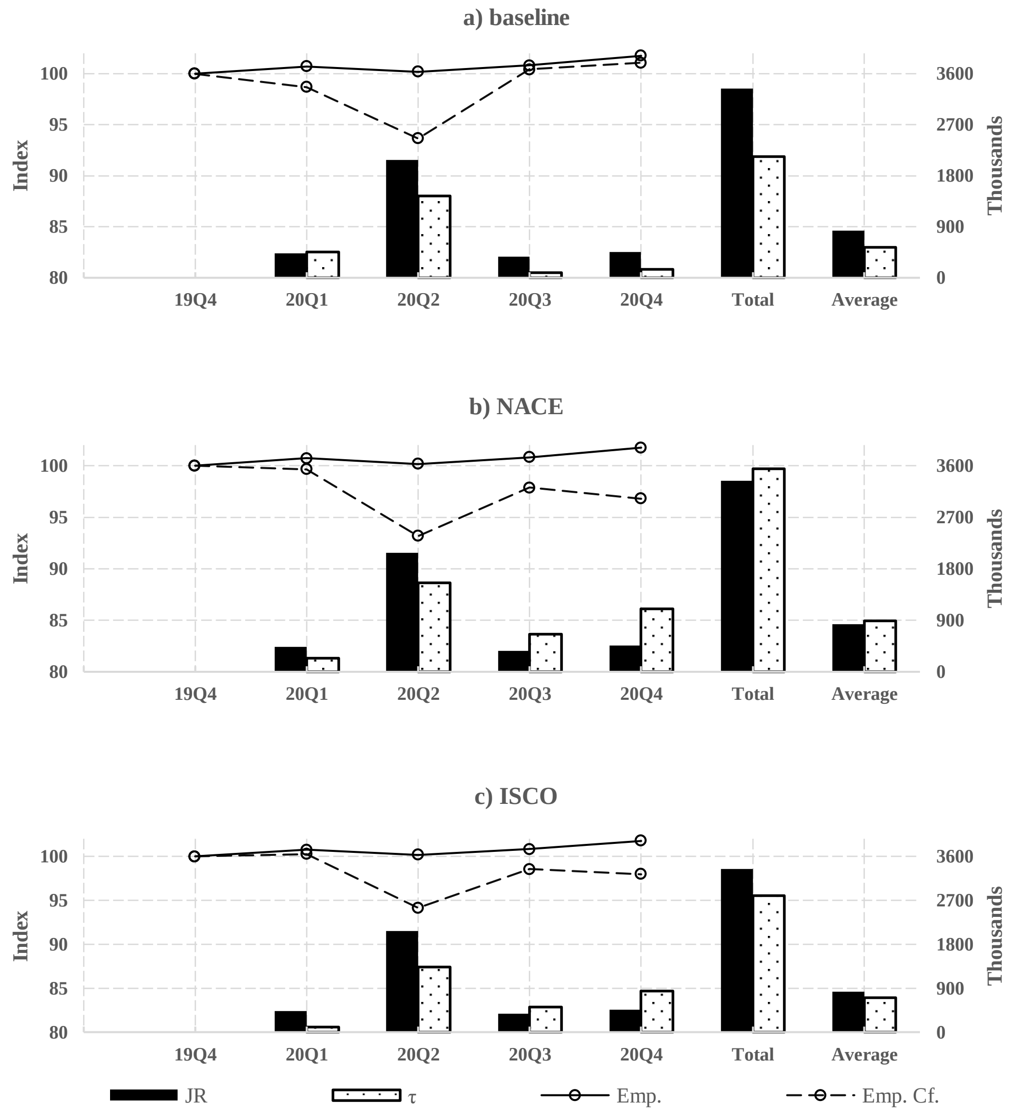
The procedure is applied to the baseline and the two nonparametric trend specifications for the sample restricted to $D_{g,1} = 0$ (Table 1)[15]. Figure 6 displays the results and shows that the counterfactual evolution of employment is relatively similar across specifications in the early part of 2020. The three specifications suggest that without JR schemes, employment would have decreased by about 5 to 7 pp in 2020Q2 (compared with the 2019Q4 levels). During the second part of 2020, the baseline specification suggests a rapid recovery of employment to its observed level. This rebound is much less

[14] Note that using $E_{g,t}$ instead of $(1 + \gamma_{g,t})E_{g,t-1}$ generally leads to slightly greater numbers of jobs saved.

[15] The main reason for this restriction is practical as the procedure cannot be directly implemented using the routine 'DID_multiplegt_dyn' of de Chaisemartin et al. (2024a) and requires writing scripts for each specification.

pronounced when nonparametric trends are considered and the counterfactual employment level decreases in 2020Q4[16].

Figure 6: Counterfactual aggregate employment



Source: Author's own elaboration

Note: Results obtained under the restriction $D_{g,1} = 0$. The actual employment series ('Emp.') and its counterfactual ('Emp. Cf.') are displayed as indices (2019Q4 = 100) on the left axis. The stocks of workers on JR schemes and the number of jobs saved, τ_t, are expressed in thousands on the right axis.

[16] This decrease would be in line with the new wave of Covid-19 that affected economic activity during this period. The stock of workers on JR schemes also increased during this quarter.

The differences in counterfactual employment rates are reflected in the number of jobs saved, which is the smallest in the baseline specification with a total of 2.1 million jobs over 2020[17]. The number reaches 2.8 and 3.5 million for the ISCO and NACE nonparametric trend specifications. Considering that the stock of workers on JR schemes reached a total of 3.3 million over 2020 in the restricted sample, these estimates imply that 1 JR scheme contributed to saving between 0.64 and 1.1 jobs[18].

The discrepancies between these numbers originate mostly from the evolution of the counterfactuals in the recovery phase after 2020Q2. The estimates under the two nonparametric trend specifications suggest a much larger number of jobs saved, consistent with the idea that JR schemes also contribute to speeding up labour market recovery by avoiding the resource-consuming process of finding a job/worker. However, it also worth noting that the effects for 2020Q3 and 2020Q4 are precisely those depending on the more long-term effects of JR schemes. Section 2 has shown that these effects should be treated with care as they tend to be more imprecisely estimated.

When focusing only on 2020Q1 and 2020Q2, the three specifications yield much more similar numbers of jobs saved, with respectively 1.9, 1.8 and 1.4 million for the baseline, NACE and ISCO specifications. The ratio of the number of jobs saved to the stock of workers on JR schemes is then always less than 1, lying between 0.6 and 0.75.

Keeping in mind the limitations of the procedure described above, the estimates tend to indicate that 1 JR scheme contributed to saving less than 1 job, at least in the early phase of the pandemic. Thus, even if JR schemes were successful in dampening the effects of the Covid-19 shock on the labour market (Section 2), their take-up might have been greater than required given the strong recovery that followed the initial shock. The potential over-utilisation of JR schemes should also be considered in light of the loosening of eligibility criteria to other forms of employment, which likely contributed to increasing the take-up of the schemes (Corti et al. 2023).

[17] These numbers should be interpreted having in mind that imposing $\mathrm{D}_{\mathrm{g},1} = 0$ restricts the sample to around 20 % of its original size.

[18] Note that in our set-up, it is sensible to compute such a ratio given that employment and JR stocks are computed in the same way (e.g. based on the activities of respondents during a reference week), ensuring no issues with double counting workers on the schemes (as would be the case with administrative data).

CONCLUDING REMARKS

This analysis proposes an estimation of the employment effects of JR schemes during 2020 and the Covid-19 crisis. This (pseudo) evaluation relies on the EU-LFS and a proxy variable constructed to identify potential workers benefitting from JR schemes. Effects are estimated using the DID estimator proposed by de Chaisemartin & D'Haultfoeuille, (2024).

Whilst additional work is required to enhance confidence in the results, the estimated effects of JR schemes on employment are positive and significant in the short run up to two periods after the first treatment. On impact, JR schemes boosted (i.e. limited the decrease in) employment growth by around 5 pp on average. The effects two periods after the treatment are of the same order of magnitude and even increase to 10 pp for some specifications considered.

The analysis then develops an approach to convert these estimates into the number of jobs saved. The proposed methodology could only be applied to a subset of the samples and specifications considered in the analysis, and results should therefore be considered with care. In the most positive case, the approach reveals that 1 JR scheme contributed to preserving around 1.1 jobs over 2020. However, the ratio is less than 1 for most specifications, with a value of around 0.75 and a minimum of 0.6. Based on EU-LFS data, 18.5 million workers in the EU (excluding Germany) benefitted from JR schemes in 2020 (see Kiss-Galfavi et al. 2024). With a job-saving ratio of 0.75, JR schemes could have contributed to preserving 13.9 million jobs in total in 2020. The same number reaches 20.3 million in the most optimistic case with a job-saving ratio of 1.1.

Thus, this analysis tends to confirm that JR schemes contributed to supporting employment, at least in the short run during the Covid-19 pandemic. At the same time, the preliminary evidence also suggests a potential over-utilisation of JR schemes, given the depth of the economic shock and the rapid recovery that ensued.

Although a more conservative approach has been taken to obtain these results, additional work is required to further analyse the effects over a longer time horizon and test the robustness of the results obtained in this exercise. In particular, testing heterogenous effects (e.g. by Member States) and considering alternative estimators potentially more suited to the treatment design (e.g. the Heterogenous Adoption Design of de Chaisemartin et al., 2024b) would be important extensions to further improve our understanding of JR schemes' effects on the labour market.

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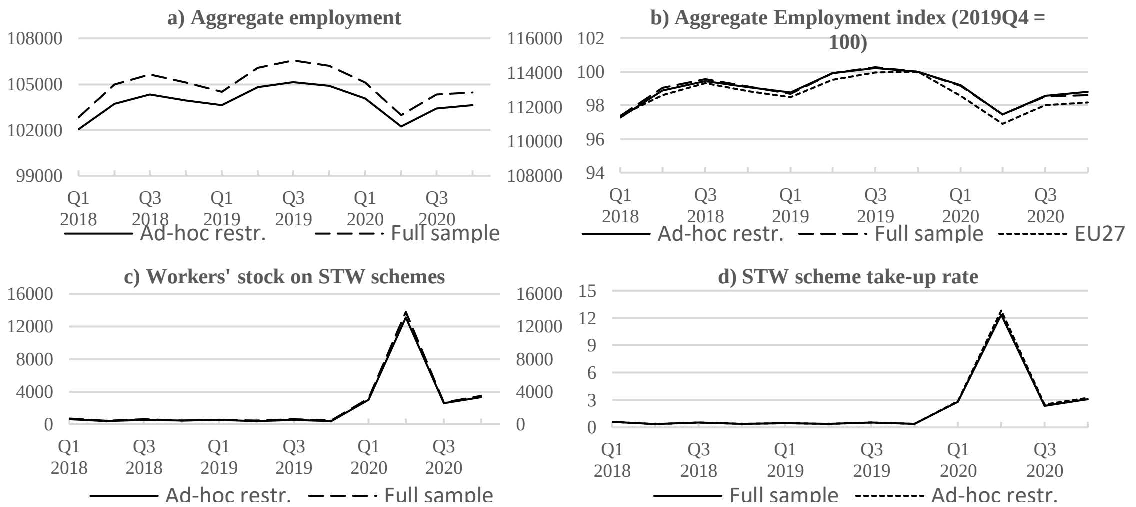
ANNEX

Annex A. ADDITIONAL EVIDENCE

A1. CONTINUOUS STARTING TREATMENT VALUES

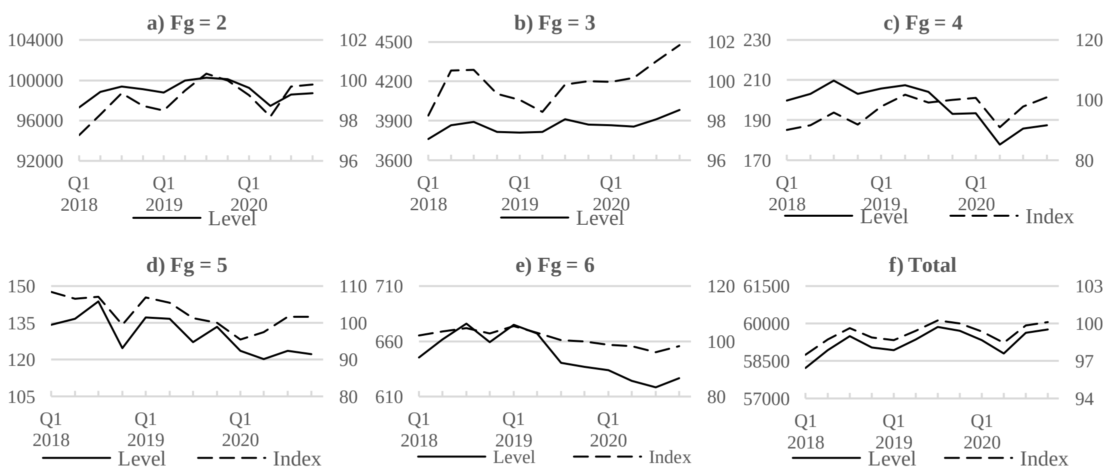
Figure 7: Outcome and treatment in the restricted and full sample – continuous starting treatment



Source: Author's own elaboration

Note: In panels a) and c), the restricted sample is displayed on the left axis and the full sample on the right. Data for the EU27 in panel b) is taken from Eurostat [lfsq_egan]. Stocks are displayed in thousands in panels a) and c) and in % in panel d).

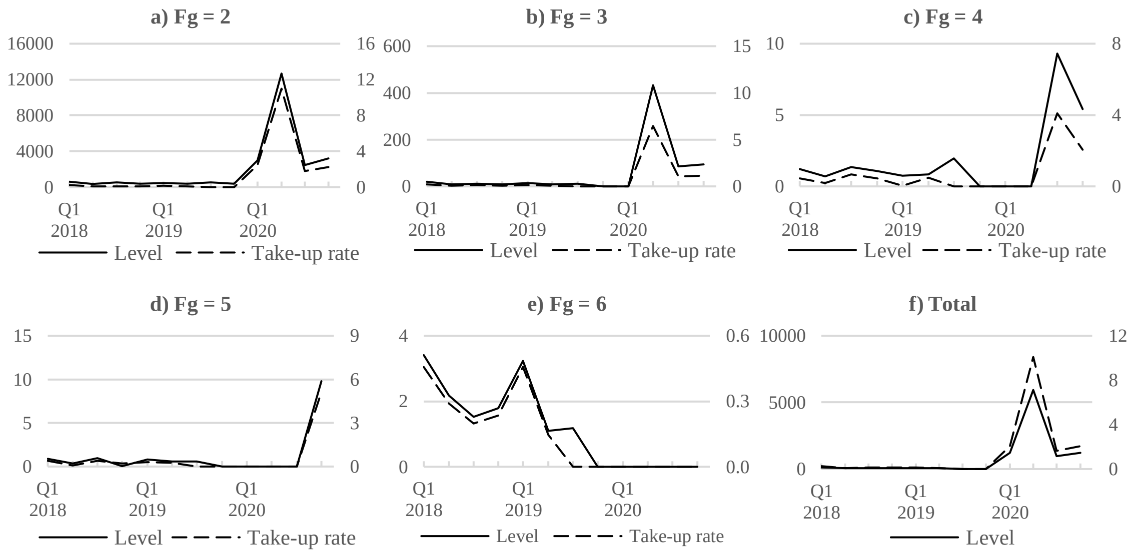
Figure 8: Employment levels and indices by first treatment period – continuous starting treatment



Source: Author's own elaboration

Note: These series are obtained under the continuous starting treatment extension of de Chaisemartin & D'Haultfoeuille, (2024). The series in levels are displayed in thousands on the left axis and as indices on the right axis.

Figure 9: JR stocks and take-up by first treatment period – continuous starting treatment



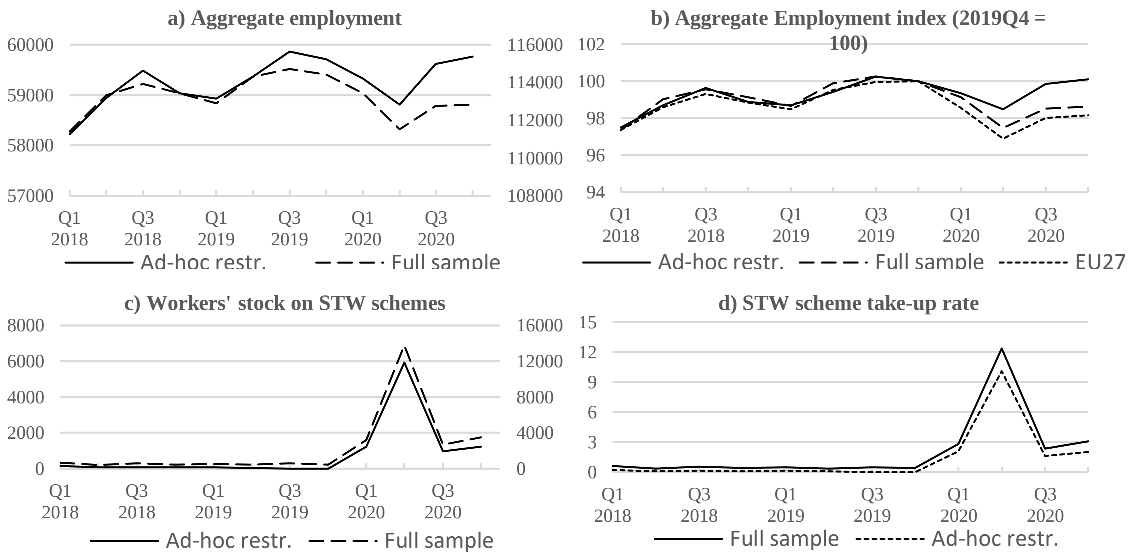
Source: Author's own elaboration

Note: These series are obtained under the continuous starting treatment extension of de Chaisemartin & D'Haultfoeuille, (2024). The series in levels are displayed in thousands on the left axis and as indices on the right axis.

A2. AD-HOC RESTRICTION

Evidence on the sample

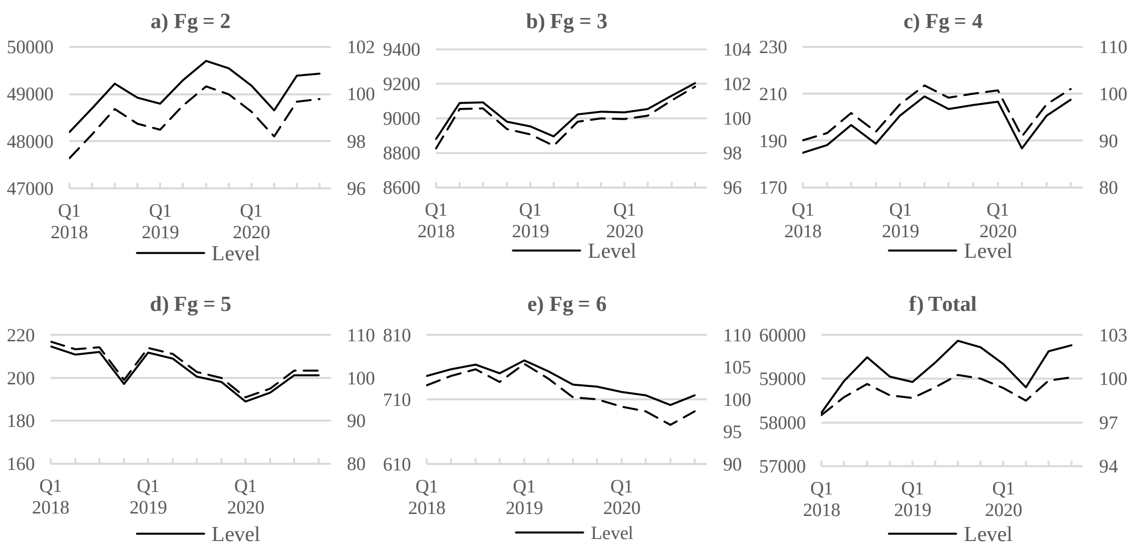
Figure 10: Outcome and treatment in the restricted and full sample – *ad-hoc* restriction



Source: Author's own elaboration

Note: In panels a) and c), the restricted sample is displayed on the left axis and the full sample on the right. Data for the EU27 in panel b) is taken from Eurostat [lfsq_egan]. Stocks are displayed in thousands in panels a) and c) and in % in panel d).

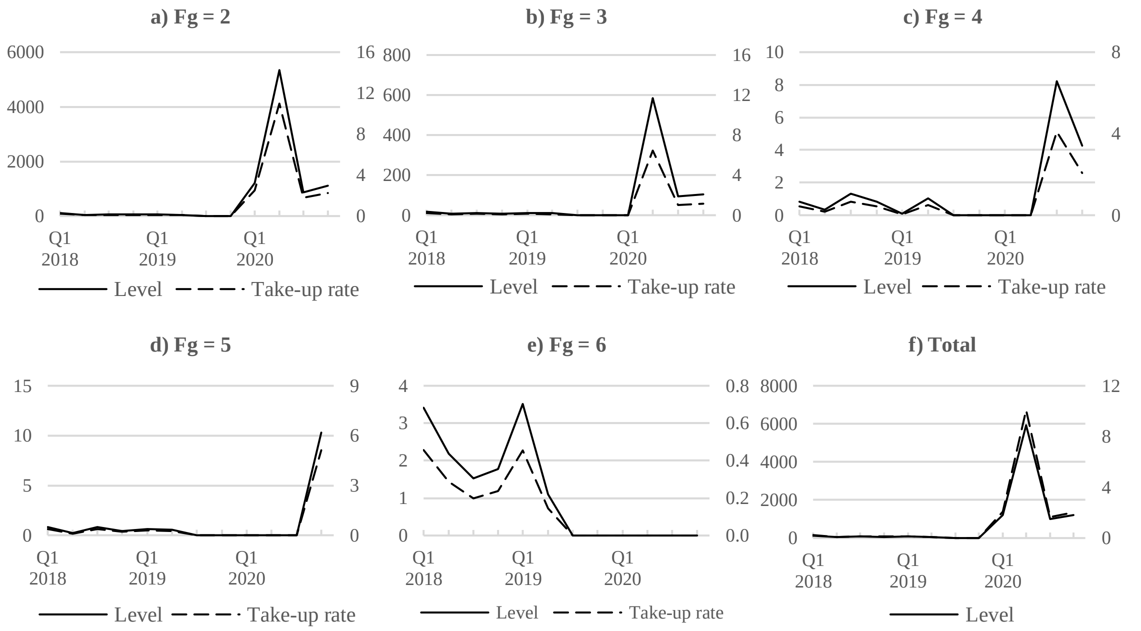
Figure 11: Employment levels and indices by first treatment period – ad-hoc restriction



Source: Author's own elaboration

Note: These series are obtained under the ad-hoc restriction. The series in levels are displayed in thousands on the left axis and as indices on the right axis.

Figure 12: JR stocks and take-up by first treatment period – ad-hoc restriction



Source: Author's own elaboration

Note: These series are obtained under the ad-hoc restriction. The series in levels are displayed in thousands on the left axis and as indices on the right axis.

Estimation results

Table 3: Estimation results – ad-hoc restriction

		Baseline	Controls				Nonparam. trend	
			(1)	(2)	(3)	(4)	NACE 1d	ISCO 1d
DID_{-2}	coeff.	-0.080	-0.070	-0.103*	-0.097*	-0.103‡	-0.197†	-0.083
	(switch er; total)	(32; 255)	(32; 255)	(32; 255)	(32; 255)	(32; 255)	(32; 203)	(32; 254)
DID_{-1}	coeff.	-0.048	-0.056*	-0.059*	-0.051	-0.049	-0.046	-0.048
	(switch er; total)	(445; 1208)	(445; 1208)	(445; 1208)	(445; 1208)	(445; 1208)	(445; 1126)	(445; 1197)
DID_0		0	0	0	0 -	0	0	0
DID_1	coeff.	0.042‡	0.059†	0.050†	0.060†	0.058†	0.056†	0.041‡
	(switch er; total)	(893; 2324)	(893; 2324)	(893; 2324)	(893; 2324)	(893; 2324)	(893; 2242)	(893; 2313)
DID_2	coeff.	0.032	0.069‡	0.055	0.073‡	0.076‡	0.074*	0.046

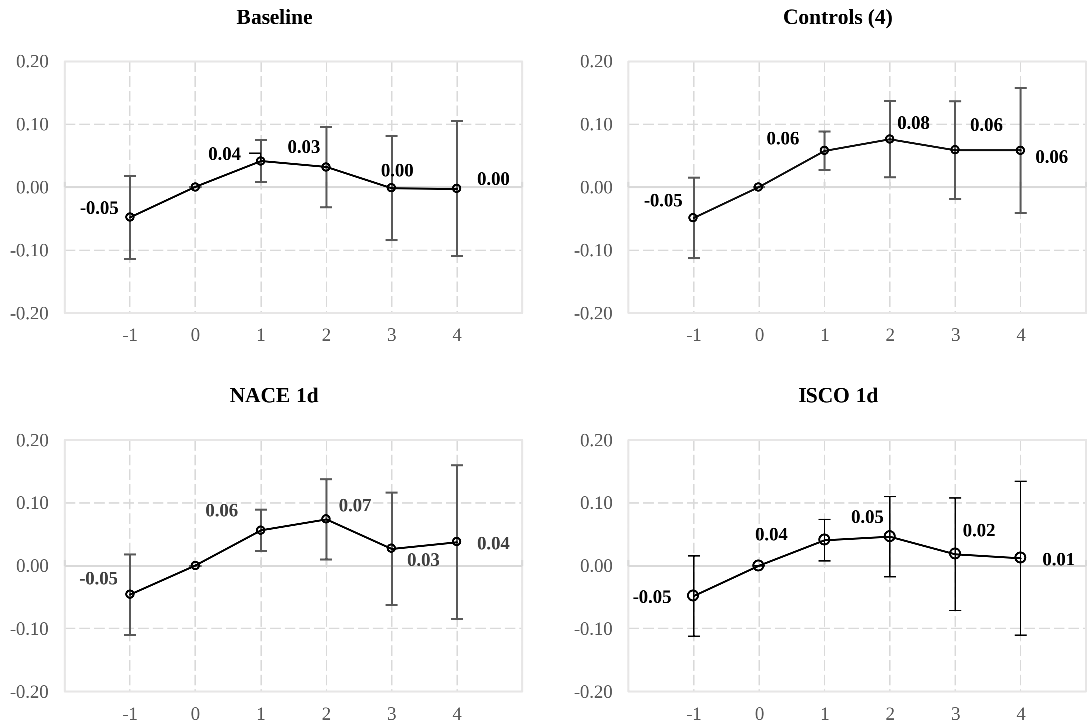
	(switcher; total)	(862; 1625)	(862; 1625)	(862; 1625)	(862; 1625)	(862; 1625)	(862; 1573)	(862; 1623)
DID_3	coeff.	-0.001	0.044	0.038	0.052	0.059	0.027	0.018
	(switcher; total)	(830; 1307)	(830; 1307)	(830; 1307)	(830; 1307)	(830; 1307)	(830; 1307)	(830; 1306)
DID_4	coeff.	-0.002	0.064	0.053	0.068	0.058	0.037	0.012
	(switcher; total)	(448; 671)	(448; 671)	(448; 671)	(448; 671)	(448; 671)	(448; 671)	(448; 671)
Controls								
	Sect. VA	N	Y	N	Y	Y	N	N
	Sect. FE	N	N	Y	Y	Y	N	N
	Occup. FE	N	N	Y	Y	Y	N	N
	Sect. VA X Sect. FE	N	N	N	N	Y	N	N
Joint test - Placebo		0.22	0.20	0.11	0.16	0.11	0.01	0.19

† p<0.01, ‡ p<0.05, * p<0.1

Source: Author's own elaboration

Note: Switchers correspond to groups being or having been treated already, relevant for the estimation of $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$. Controls include sectoral value added [namq_10_a10], 1-digit sectors and occupations fixed effects and interaction variables between sectoral value added and sectoral fixed effects. Standard errors are clustered at the NACE 1-digit and ISCO 1-digit level. Results have been generated using the STATA routine 'DID_multiplegt_dyn' developed by de Chaisemartin et al., (2024a).

Figure 13: Estimated effects – ad-hoc restriction



Source: Author's own elaboration

Note: Estimates for $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$ at all leads and lags except for the placebo coefficient at lag 2, which is imprecisely estimated. $\mathrm{l} = 0$ for the last period before the first treatment.

A3. PLACEBO TESTS – $D_{g,1} = 0$

The results presented below in Table 4 and Figure 14 are obtained by setting all values for the treatment before 2019Q4 to zero. Data from 2019Q1 is used for estimation. This allows for the computation of two additional placebo effects at lags 3 and 4. The placebo effect at lag 3 is often significative at the 5% critical level (except for the NACE non-parametric trend specification) but placebo effects are never jointly significant. Estimation results for periods after the treatment occurs are generally similar to those reported in Table 1, though estimates are often less precisely estimated and some coefficients can become non-significant (e.g. $\delta(2)$).

Table 4: Estimation results – additional placebos ($\mathrm{D}_{\mathrm{g,1}} = 0$)

		Baseline	Controls				Non param.trend	
			(1)	(2)	(3)	(4)	NACE 1d	ISCO 1d
δ(-4)	coeff. (switcher; total)	-0.020 (438; 881)	-0.018 (438; 881)	-0.029 (438; 881)	-0.014 (438; 881)	-0.020 (438; 881)	0.030 (438; 881)	-0.041 (438; 881)
δ(-3)	coeff. (switcher; total)	-0.082‡ (830; 1308)	-0.089‡ (830; 1308)	-0.088‡ (830; 1308)	-0.084‡ (830; 1308)	-0.083‡ (830; 1308)	-0.060 (830; 1308)	-0.079* (830; 1305)
δ(-2)	coeff.	-0.018	-0.022	-0.022	-0.020	-0.018	-0.003	-0.008

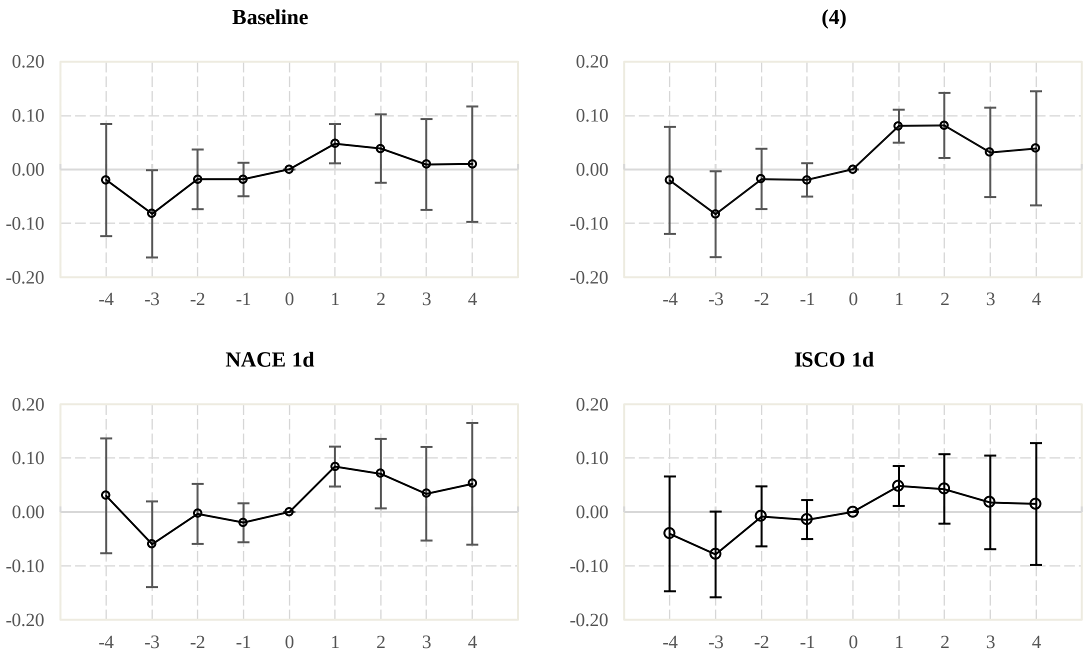
	(switcher; total)	(885; 1829)	(885; 1829)	(885; 1829)	(885; 1829)	(885; 1829)	(885; 1590)	(885; 1827)
δ(-1)	coeff.	-0.019	-0.022	-0.020	-0.022	-0.019	-0.020	-0.014
	(switcher; total)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2235)	(895; 2327)
δ(0)		0	0	0	0 -	0	0	0
δ(1)	coeff.	0.048†	0.084†	0.049†	0.085†	0.080†	0.084†	0.048‡
	(switcher; total)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2339)	(895; 2235)	(895; 2327)
δ(2)	coeff.	0.039	0.090†	0.043	0.072‡	0.082†	0.071*	0.043
	(switcher; total)	(885; 1829)	(885; 1829)	(885; 1829)	(885; 1829)	(885; 1829)	(885; 1590)	(885; 1827)
δ(3)	coeff.	0.009	0.046	0.015	0.049	0.032	0.034	0.018
	(switcher; total)	(830; 1308)	(830; 1308)	(830; 1308)	(830; 1308)	(830; 1308)	(830; 1308)	(830; 1305)
δ(4)	coeff.	0.010	0.085	0.019	0.089*	0.039	0.052	0.015
	(switcher; total)	(438; 881)	(438; 881)	(438; 881)	(438; 881)	(438; 881)	(438; 881)	(438; 881)
Controls								
Sect.VA		N	Y	N	Y	Y	N	N
Sect. FE		N	N	Y	Y	Y	N	N
Occup. FE		N	N	Y	Y	Y	N	N
Sect.VA X Sect.FE		N	N	N	N	Y	N	N
Joint test - Placebo		0.13	0.10	0.11	0.10	0.11	0.18	0.13

† p<0.01, ‡ p<0.05, * p<0.1

Source: Author's own elaboration

Note: Switchers correspond to groups being or having been treated already, relevant for the estimation of DID_{l}^{L}. Controls include sectoral value added [namq_10_a10], 1-digit sectors and occupations fixed effects and interaction variables between sectoral value added and sectoral fixed effects. Standard errors are clustered at the NACE 1-digit and ISCO 1-digit level. Results have been generated using the STATA routine 'DID_multiplegt_dyn' developed by de Chaisemartin et al., (2024a).

Figure 14: Estimated effects – additional placebos ($\mathrm{D}_{g,1} = 0$)



Note: Estimates for DID_{l}^{L} at all possible leads and lags. $l = 0$ for the last period before the first treatment.

Annex B. DID ESTIMATORS

B1. $DID_{g,l}^{L}$ AND DID_{l}^{L}

This section shows that the point estimates obtained from equations (1) and (2) are affected by the different sets of groups g (for the computation of $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$) and g' (for the counterfactual in $\mathrm{DID}_{\mathrm{g,l}}^{\mathrm{L}}$) used to compute $\mathrm{DID}_{\mathrm{g,l}}^{\mathrm{L}}$and $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$ for different values of l. As in Section 3, the derivations in Section B1 (and 0) have not been reviewed by anyone other than the author.

To understand the source of the potential problem, it should be noted that $\mathrm{DID}_{\mathrm{g,l}}^{\mathrm{L}}$ and $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$ are cumulative effects and as such, they should be equal to the sum of the first-differences estimates, $\mathrm{DID}_{\mathrm{g,l-1,l}}^{\mathrm{L}}$ and $\mathrm{DID}_{\mathrm{l-1,l}}^{\mathrm{L}}$. An estimator for the first differences can be obtained by slightly rewriting equations (1) and (2):

$$\mathrm{DID}_{\mathrm{g,l-1,l}}^{\mathrm{L}} = \mathrm{Y}_{\mathrm{g,F_g+l-1}} - \mathrm{Y}_{\mathrm{g,F_g+l-2}} - \left(\frac{1}{\mathrm{N}_{\mathrm{F_g+l-1}}^{\mathrm{g}}} \sum_{\mathrm{g':F_g'>F_g+l-1}} \mathrm{Y}_{\mathrm{g',F_g+l-1}} - \mathrm{Y}_{\mathrm{g',F_g+l-2}}\right) \tag{11}$$

$$\mathrm{DID}_{\mathrm{l-1,l}}^{\mathrm{L}} = \frac{1}{\mathrm{N_l}} \sum_{\mathrm{g:T_g \geq F_g+l-1}} \mathrm{DID}_{\mathrm{g,l-1,l}}^{\mathrm{L}} \tag{12}$$

and these estimators are linked to $\mathrm{DID}_{\mathrm{g,l}}^{\mathrm{L}}$ and $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$:

$$\mathrm{DID}_{\mathrm{g,l}}^{\mathrm{L}} = \sum_{\mathrm{k=1}}^{\mathrm{l}} \mathrm{DID}_{\mathrm{g,k-1,k}}^{\mathrm{L}} \tag{13}$$

$$\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}} = \sum_{\mathrm{k=1}}^{\mathrm{l}} \mathrm{DID}_{\mathrm{k-1,k}}^{\mathrm{L}} \tag{14}$$

In theory, computing $\mathrm{DID}_{\mathrm{g,l}}^{\mathrm{L}}$ and $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$ through equations (11), (12) and (14) or via (1) and (2) should lead to the same results. For $\mathrm{l} = 1$, $\mathrm{DID}_{\mathrm{g,0,1}}^{\mathrm{L}} = \mathrm{DID}_{\mathrm{g,1}}^{\mathrm{L}}$ by definition and it is for $\mathrm{l} \geq 2$ that some differences can arise. To see this, we can write the first-difference estimator obtained from (1) as:

$$\begin{aligned} \mathrm{DID}_{\mathrm{g,l-1,l}}^{\mathrm{L}} &= \mathrm{DID}_{\mathrm{g,l}}^{\mathrm{L}} - \mathrm{DID}_{\mathrm{g,l-1}}^{\mathrm{L}} \\ &= \mathrm{Y}_{\mathrm{g,F_g+l-1}} - \mathrm{Y}_{\mathrm{g,F_g-1}} - \left(\frac{1}{\mathrm{N}_{\mathrm{F_g+l-1}}^{\mathrm{g}}} \sum_{\mathrm{g':F_g'>F_g+l-1}} \mathrm{Y}_{\mathrm{g',F_g+l-1}} - \mathrm{Y}_{\mathrm{g',F_g-1}}\right) \end{aligned} \tag{15}$$

$$
\begin{aligned}
&- \Bigg(Y_{g,F_g+l-2} - Y_{g,F_g-1} \\
&\qquad - \Bigg(\frac{1}{N^g_{F_g+l-2}} \sum_{g':F'_g>F_g+l-2} Y_{g',F_g+l-2} - Y_{g',F_g-1} \Bigg) \Bigg) && (16)
\end{aligned}
$$

$$
\begin{aligned}
= \;& Y_{g,F_g+l-1} - Y_{g,F_g+l-2} \\
&\qquad - \Bigg(\frac{1}{N^g_{F_g+l-1}} \sum_{g':F'_g>F_g+l-1} Y_{g',F_g+l-1} - Y_{g,F_g+l-2} \Bigg) \\
&- \Bigg(\frac{1}{N^g_{F_g+l-1}} \sum_{g':F'_g>F_g+l-1} Y_{g',F_g+l-2} - Y_{g,F_g-1} \Bigg) \\
&+ \Bigg(\frac{1}{N^g_{F_g+l-2}} \sum_{g':F'_g>F_g+l-2} Y_{g',F_g+l-2} - Y_{g',F_g-1} \Bigg) && (17) \\
= \;& DID^L_{g,l-1,l} - \Bigg(\frac{1}{N^g_{F_g+l-1}} \sum_{g':F'_g>F_g+l-1} Y_{g',F_g+l-2} - Y_{g,F_g-1} \Bigg) \\
&+ \Bigg(\frac{1}{N^g_{F_g+l-2}} \sum_{g':F'_g>F_g+l-2} Y_{g',F_g+l-2} - Y_{g',F_g-1} \Bigg) && (18)
\end{aligned}
$$

where equation (17) is obtained by adding and subtracting $\frac{1}{N^g_{F_g+l-1}} \sum_{g':F'_g>F_g+l-1} Y_{g',F_g+l-2}$. Hence, equation (18) shows that taking the first difference of $DID^L_{g,l}$ as defined in equation (1) leads to the first-difference estimator $DID^L_{g,l-1,l}$ to which a non-zero quantity is added. This quantity originates from the different groups g' used to compute the counterfactuals in $DID^L_{g,l-1}$ and $DID^L_{g,l}$. To take a concrete example, if one considers $DID^L_{g,2}$, the cumulative effects should be equal to the sum of $DID^L_{g,0,1}$ and $DID^L_{g,1,2}$ with $DID^L_{g,0,1}$ equal to DID^L_1. However, $DID^L_{g,0,1} \neq DID^L_1$ when using (1), since the counterfactual in $DID^L_{g,0,1}$ is computed on groups $g': F'_g > F_g + 1$ whilst $DID^L_{g,1}$ is computed on $g': F'_g > F_g$. This implies that when comparing $DID^L_{g,l}$ obtained for different values of l, one should be aware that the point estimates also reflect differences in the computations of the counterfactuals. Note that the quantity in equation (18) can be computed in order to gain an idea of the impact of these differences in counterfactuals.

A second issue, similar in nature, arises with the computation of DID^L_l from equation (2). The discrepancy emerges again from the fact that the cumulative changes in the outcome variable should equal the sum of the first differences. Yet, the set of groups $g: T_g \geq F_g +$

$\mathrm{l}-1$ will differ across l for the computation of the same first-difference effects that constitute each cumulative effect.

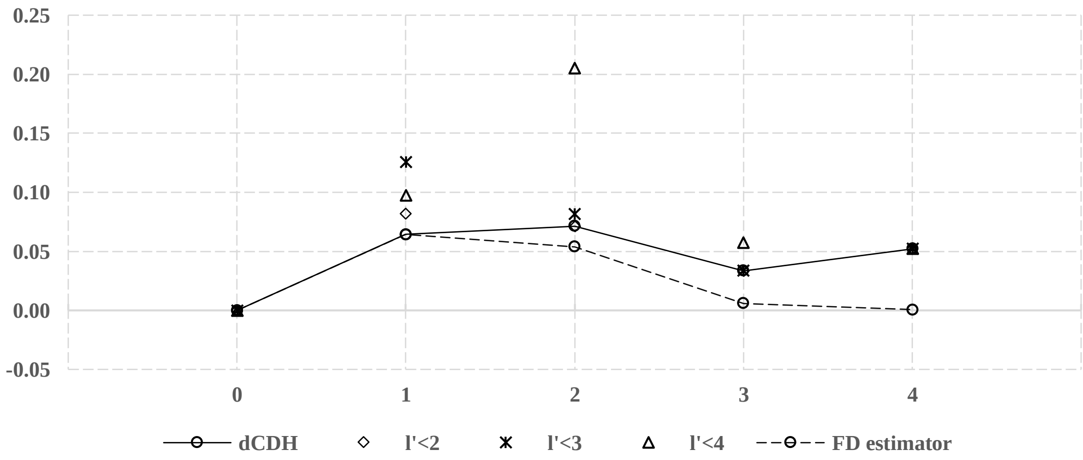
It turns out that the dataset used in this analysis provides a concrete example of why these differences can matter for the interpretation of the results. Figure 15 plots the results from an exercise where I use the fact that the estimation of $\mathrm{DID}_{\mathrm{l}}^{\mathrm{L}}$ for $\mathrm{l}>1$ can be utilised to provide estimates for effects $\mathrm{l}'<\mathrm{l}$ using again the relation between cumulative and first differences. For instance, the estimation of $\mathrm{DID}_{3}^{\mathrm{L}}$ can be used to obtain estimates for $\mathrm{DID}_{1}^{\mathrm{L}}$ and $\mathrm{DID}_{2}^{\mathrm{L}}$ for which the set of groups g and g' are the same. The $\mathrm{DID}_{\mathrm{l}'}^{\mathrm{L}}$ for $0<\mathrm{l}'<\mathrm{l}$[19] can then be compared with estimates obtained from equations (1) and (2) ('dCDH' in Figure 15). For $\mathrm{l}'\geq \mathrm{l}$, the estimates are the same as those developed by de Chaisemartin & D'Haultfoeuille, (2024). The figure also displays the estimator proposed at the end of this section, which should be immune to the issue discussed above ('FD estimator').

These results are based on the NACE nonparametric specification for the restricted sample $\mathrm{D}_{\mathrm{g},1}=0$. This specification is interesting because it suggests a possible ramping-up effect of JR schemes on employment (see Table 1 and Section 2). Figure 15 shows that the (small) increase between $\mathrm{l}=1$ and $\mathrm{l}=2$ is actually an artifact coming from the different sets of groups used for the computations of the counterfactuals and average effects. Using equations (1) and (2), one obtains $\mathrm{DID}_{1}^{\mathrm{L}}=0.064$ and $\mathrm{DID}_{2}^{\mathrm{L}}=0.071$, whilst computing $\mathrm{DID}_{1}^{\mathrm{L}}$ using the set of groups g and g^′ relevant for the computation of $\mathrm{DID}_{2}^{\mathrm{L}}$ leads to $\mathrm{DID}_{1}^{\mathrm{L}}=0.082$. Thus, the effect between periods 1 and 2 is always decreasing[20] but this decrease is actually hidden when using (1) and (2).

Figure 15: Estimated effects using different sets of groups g and g'

[19] Whilst not discussed in this section, the impact on placebo effects, computed as cumulative effects as well, should be the same.

[20] The only exception is for the case $\mathrm{l}'<4$ but these effects tend to be estimated on a smaller number of groups and are therefore likely to be more imprecisely estimated.



Source: Author's own elaboration

Note: Results obtained under the restriction $D_{g,1} = 0$ for the NACE nonparametric specification. 'dCDh' corresponds to results displayed in Table 1, '$l' < l$' are cumulative effects obtained from the computation of DID_l^L for periods before l and 'FD estimator' is the first-difference estimator obtained through equations (19) and (20).

Therefore, the evidence presented in this section implies that the comparison of DID_l^L at various leads (and lags) should be considered with care as estimates could be contaminated by differences in the set of groups used to compute counterfactuals and the average treatment effect. It is unclear at this stage whether this difference matters from a statistical point of view, particularly if the parallel trend assumption holds. Nevertheless, researchers using these methods should be aware of this possible effect.

This issue cannot be addressed by working only with the never-treated groups, as this would resolve the issue arising with the counterfactuals but not the one related to the different sets of groups g used to compute the average effect DID_l^L. It is further important to reflect on whether these issues would affect other estimators, in particular the two-way fixed effect event-study estimators.

Relying on the evidence developed by de Chaisemartin & D'Haultfoeuille, (2024), an estimator immune to these differences can be obtained by working with first differences and generating cumulative effects by summing the first-difference estimates:

$$DID_{g,l-1,l}^L = Y_{g,F_g+l-1} - Y_{g,F_g+l-2} - \left(\frac{1}{N_{F_g+l-1}^g} \sum_{g':F_g'>F_g+l-1} Y_{g',F_g+l-1} - Y_{g',F_g+l-2} \right) \quad (19)$$

$$DID_l^L = \sum_{k=1}^{l} \frac{1}{N_k} \sum_{g:T_g \geq F_g+k-1} DID_{g,k-1,k}^L \quad (20)$$

Hence, the cumulative effects DID_l^L can be obtained recursively starting from DID_1^L, computing the following first-difference estimate (i.e. $\mathrm{DID}_{1,2}^L$ for DID_2^L) and adding this quantity to the $l-1$ estimate (i.e. $\mathrm{DID}_2^L = \mathrm{DID}_1^L + \mathrm{DID}_{1,2}^L$; $\mathrm{DID}_3^L = \mathrm{DID}_2^L + \mathrm{DID}_{2,3}^L$).

Additional work is required to verify whether analytical standard errors could be computed in a similar way to de Chaisemartin & D'Haultfoeuille, (2024). Alternatively, bootstrapping could be used as a solution to obtain these standard errors.

DID_l^L and DID_t^T

Using the adjusted estimator presented in equations (19) and (20), it can be shown that DID_t^T can be written in terms of l index effects. First, we can define $\mathrm{DID}_l^{L,F}$ as the effect of the treatment by the first treatment date computed from groups g with $F_g = F$,

$$\mathrm{DID}_l^{L,F} = \frac{1}{N_l^F} \sum_{g:F_g=F;\, T_g \geq F_g+l-1} \mathrm{DID}_{g,l}^L \qquad (21)$$

and DID_l^L being a weighted average of the different $\mathrm{DID}_l^{L,F}$. The starting date is important to properly define the effects in terms of the time index t. Then, DID_t^T can be written as:

$$\mathrm{DID}_t^T = \sum_{k=1}^{t-1} \mathrm{DID}_{t-k}^{L,k+1} - \mathrm{DID}_{t-k-1}^{L,k+1} \qquad (22)$$

This expression implies that DID_t^T can be obtained through a linear combination of the relevant $\mathrm{DID}_l^{L,F}$ at time t. The above expression does not hold exactly when the $\mathrm{DID}_l^{L,F}$ are computed from (1) and (2).

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PLACE DU CONGRES 1
B-1000 BRUSSELS