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Did Job Retention Schemes Save Jobs during the Covid-19 Pandemic? Firm-level Evidence from Latvia

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Abstract

This paper studies the employment effect of the job retention scheme implemented during the Covid-19 pandemic. Using firm-level data from Latvia, we investigate whether a change in the number of employees in firms that received support from the job retention programme has been different from that of similar firms which did not receive such support, and whether these differences have disappeared over time. We find strong evidence that job retention scheme participants in Latvia were less likely to cut employment and that this effect persisted for several months after receiving support. Participation in the job retention scheme affected both the likelihood of a firm's survival and the rate at which employees were laid off. Our results also suggest that the participation effect was not uniform across firms, with the effect being less pronounced in service sectors with a higher level of contact intensity and more pronounced in sectors with a higher proportion of highly skilled employees.

Keywords: Job retention schemes, idle-time allowance, Covid-19, employment

JEL Codes: E24, H12, J62, J68

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1 Introduction

Although job retention schemes (JRS) existed long before the Covid-19 outbreak, it was during the pandemic that most advanced economies scaled up existing schemes and introduced new ones to cushion the economic impact of the crisis. Just two months after the Covid-19 outbreak was declared a pandemic, almost all OECD members had already implemented some form of employment protection mechanism, supporting millions of jobs.[1]

JRS received widespread acclaim for maintaining existing employment contracts and therefore reducing the economic cost of the pandemic. For employers, the scheme preserved the talent and experience of their employees and spared them the costly process of hiring and training new workers. For employees, JRS helped to support income and reduce overall uncertainty. The benefits of JRS are especially significant in a crisis caused by an external shock such as a pandemic, as firms suffering the most are not necessarily financially less sound. This is particularly important against the backdrop of the frequently voiced criticism that JRS provide disproportionate support for inefficient firms and therefore hinder productive reallocation of resources. This criticism is less relevant in an environment where the firms seeking help are not necessarily less productive, but are subject to some external constraints, such as those imposed by the government to contain the spread of the Covid-19 virus.

Despite the widespread use of JRS during the Covid-19 pandemic, their impact on employment remains a relatively under-researched topic. Previous literature has mainly focused on measuring the overall macroeconomic impact of fiscal measures during the pandemic. While it largely confirms that without the active use of JRS, the unemployment rate would be significantly higher (Aiyar and Dao 2021, Albertini et al. 2022), relatively little is known about the persistence of the JRS effect on employment. Against this background, using firm-level data from Latvia, we aim at examining whether employment dynamics were different in firms that received JRS subsidies compared to similar firms that did not.

Answering these questions is not trivial for several reasons. First, JRS can be used to save jobs that are not viable in the medium term. In particular, the Covid-19 pandemic could have led to structural changes in consumer preferences, working patterns, etc., resulting in some job cuts even after the crisis subsided. Supporting these jobs with JRS may have only delayed their liquidation and slowed down the transition to new and viable jobs. This may imply that employment effect

[1]The design of JRS varied across countries. For a detailed overview see OECD (2022).

of JRS is short-lived and that it would disappear when subsidies are removed. Alternatively, JRS can also be used to save jobs that firms would have kept anyway. In that case, the impact on employment may be rather negligible.

Studying the impact of JRS on employment may be complicated if firms applying for government assistance are not random. More specifically, firms that have experienced the largest drop in demand and the highest costs of freezing or reorganising production have more incentives to use JRS. Hence, the problem of self-selection arises. We overcome this problem by using an extensive firm-level dataset from Latvia that merges balance sheet data with JRS subsidy information on a monthly basis. This allows us to apply a matching algorithm to find a control group for those firms that received JRS subsidies during the first wave of the Covid-19 pandemic. Using the sample of JRS participants and matched non-participants, we employ the difference-in-difference (DiD) technique to assess whether JRS participation had any impact on employment dynamics of recipient firms.

The results obtained in this study indicate that JRS participants in Latvia were indeed statistically less likely to reduce employment and that this effect persisted for several months after the receipt of support. Participation affected both the likelihood of the firm's survival and the rate at which employees were fired. Our results also suggest that the effect of participation has not been homogeneous across firms. Companies that operated in service sectors with a higher level of interpersonal contact seem to have gained less, as the allowance was less protective against deteriorating conditions. Moreover, participation appears more beneficial in sectors with a larger proportion of highly skilled employees. The results of the assessment are robust to various definitions of participation and employment dynamics, as well as to alternative matching techniques.

The remainder of the study is structured as follows. Section 2 reviews the related literature. Section 3 outlines the details of the JRS in Latvia. Section 4 introduces the datasets used in the study as well as presents some aggregate statistics on the programme's take-up. Section 5 lays out the methodology used. Section 6 presents the estimation results and the results of the robustness exercise. Finally, Section 7 concludes.

2 Literature Review

Our study is one of a large body of literature investigating the economic impact of the Covid-19 pandemic (Baek et al. 2021, Brinke et al. 2022, Coibion et al. 2020b, Coibion et al. 2020a, Cox

et al. 2020, Horvath et al. 2021 and Maloney and Taskin 2020 are just a few examples). This literature highlighted the unprecedented disruption to economic activity caused by the pandemic and related public health measures as well as provided the evidence of the importance of government programmes to limit the negative consequences. Fiscal policy played a vital role in offsetting the decline in earnings (Larrimore et al. 2022), boosting confidence (Deb et al. 2021), reducing business failure rates (Gourinchas et al. 2021), and, as a result, in limiting the contraction of aggregate output (Chudik et al. 2021, Tervala and Watson 2022).

In addition, our study contributes to the literature that examines the macroeconomic effects of JRS programmes. A significant amount of research has been built based on the experience gained during the global financial crisis, particularly in Germany and France – countries which have a long tradition of JRS programmes and offer extensive microdata on the use of the scheme. For example, Balleer et al. (2016) distinguish between the "rules-based component", i.e. the core mechanism of the German JRS and any discretionary changes to the characteristics of the scheme, such as eligibility. They argue that only the former has a statistically significant effect on unemployment. In contrast, Gehrke and Hochmuth (2018) show that it is the discretionary element of JRS that has a positive impact on employment, but only when implemented during a recession. Other studies show that the macroeconomic effects of JRS are highly heterogeneous. For example, Cahuc et al. (2021) use the detailed information from French institutions and find that each additional worker participating in JRS saved 0.6 jobs during the global financial crisis, but only in the firms hardest hit by the crisis. The estimates of JRS macroeconomic effects are not unambiguously positive in the previous literature. For example, Calavrezo et al. (2010) find that the exit rate of French firms which received subsidies from JRS over the period from 2000 until 2005 is higher than for comparable firms which didn't receive JRS support.

Despite the limited data availability, some studies have tried to assess the success of JRS during the Covid-19 pandemic benefiting from the variation in cross-country or cross-region eligibility, exposure or programme take-up. For example, Aiyar and Dao (2021) exploit state-level variation in exposure to the pandemic to quantify the economic impact of the Kurzarbeit programme in Germany. They show that the programme was successful in mitigating unemployment (by around 3 pp vis-à-vis the scenario without the programme). Albertini et al. (2022) build a heterogeneous agents model with search frictions to study the effects of JRS in France where in certain months of the Covid-19 pandemic more than 40% of employees benefited from the scheme. They find that JRS

were successful in preserving jobs and limiting lost earnings, but not without some side-effects. They show that policy also saved some jobs that would have been maintained even in the absence of the scheme. Similar evidence of windfall effects which significantly increased the cost of the scheme has also been found by Cahuc et al. (2021). Christl et al. (2022) use EUROMOD microsimulation model complemented with a labour transition element and focus on income substituting properties of JRS. They find that JRS and other discretionary policy measures absorbed approximately 85% of the reduction in disposable income. The absorbing effect is particularly strong for households located in the bottom half of income distribution. Crossley et al. (2021) use household survey data from the UK to study the labour market shocks in the first wave of the Covid-19 pandemic and highlight the importance of JRS which proved to be crucial for limiting the earnings decline (nonetheless, 45% of individuals still experienced a decline in earnings of at least 10%). We contribute to this growing strand of literature by estimating the employment effect of JRS using firm-level microdata and investigating how it evolved over time.

An often voiced criticism of JRS is that it delays the restructuring of less productive firms and therefore stalls a productivity enhancing reallocation of resources. The empirical evidence of this side-effect is however inconclusive. For example, Merikül and Paulus (2022) find that a generous JRS introduced in Estonia during the pandemic exerted a negative impact on reallocation implying negative consequences for aggregate productivity. The impact was also found to be negative in Portugal (Kozeniauskas et al. 2022). Andrews et al. (2021) and Andrews et al. (2021) show that implications for reallocation varied by country due to differences in the design of JRS. Thus, productivity-enhancing reallocation of labour was strengthened in Australia and the UK but was weakened in New Zealand. However, the differences across these three countries have largely disappeared by the end of 2020. Finally, Aiyar and Dao (2021) find that the use of JRS is associated with resource misallocation only in those countries which already have a high initial level of misallocation.

3 JRS in Latvia

In contrast to several other advanced economies, JRS in Latvia is not a permanent government programme. It was introduced at the outset of the pandemic and was active only during a certain period of time when economic activity was hampered by the pandemic and the related restrictions imposed by the government. The first programme introduced as a part of JRS was the *idle-*

time allowance which was granted to employees suspended from working. It aimed at protecting employment and preventing lay-offs in firms which suffered from the pandemic driven revenue decline. The first instalment of the idle-time allowance was distributed during the period between 14 March 2020 and 30 June 2020, roughly covering the first wave of the Covid-19 outbreak.

The idle-time allowance was set at 75% of the average monthly remuneration of the respective employee, but not exceeding 700 EUR. Employees receiving the allowance were not permitted to work and were not allowed to be fired within a month after the application. In order to be entitled to the allowance firms had to prove that they suffered from a reduction in turnover by at least 30% (compared to the average monthly revenue in 2019 or the average revenue across active months from 1 January, 2019 until 1 March, 2020). The threshold for a decline in turnover was lowered to 20% if one of the following criteria was met: a) exports of goods and services in 2019 was at least 10% of the total turnover or were at least 500 000 EUR; b) the average monthly gross work remuneration in 2019 were at least 800 EUR; c) as on 31 December 2019 long-term investments in fixed assets were at least 500 000 EUR.

In addition, the JRS legislation outlined 14 reasons why firms were not eligible to participate in the scheme, including tax arrears of more than 1000 EUR, being in active bankruptcy proceedings, etc. Moreover, eligibility was pegged to the condition that the drop in income was caused by the pandemic. This, together with the temporary nature of the scheme, may have reduced the likelihood of JRS being abused by firms facing structural problems even before the pandemic and postponing layoffs (an issue raised by Calavrezo et al. 2010).

Against the background of high infection rates and government imposed restrictions, JRS was reactivated in late November 2020 with some changes to eligibility criteria (e.g. to the reference period for the decline in turnover) and to the size of the allowance (set now at 70% of the average monthly remuneration, but in the range between 500 EUR and 1000 EUR). Furthermore, the government introduced the second program – *wage subsidies*, aimed at covering costs of employees who worked shorter hours rather than being idle. Wage subsidy was set at 50% of the average salary of the respective employee in August, September and October 2020, but it could not exceed 500 EUR. The firm was obliged to pay the difference between the subsidy and the full-time wage. Also, in the case of this programme employer was obliged to retain an employee for at least a month after applying for the subsidy. Therefore, during the second wave of the Covid-19 pandemic firms in Latvia had a possibility to apply for two JRS programmes, one aimed at idle employees

and the other at employees with a reduced number of working hours. Both programmes remained operational until June 2021.[2]

4 Data

To analyse the impact of JRS on employment, we match several anonymised firm-level datasets provided by the State Revenue Service (SRS) and Central Statistical Bureau (CSB) of Latvia (monthly employer-employee data, annual balance sheet and profit-loss statements, business registry data, data on international trade) with the dataset on JRS benefit recipients provided by the SRS on a monthly basis. Although both the employment and the JRS data are available at the employer-employee level, for the purpose of this study they are aggregated to the firm level.

The SRS employer-employee data is an administrative dataset covering all legal employees in Latvia. However, employees from the government bodies are excluded from the analysis since we are not able to match them with corresponding balance sheet and profit-loss statement data. Moreover, we further narrow down the focus of our analysis by excluding commercial firms from financial service (K), public administration (O), education (P) and healthcare (Q) sectors. As illustrated in Figure A.1 in Appendix, this subset of employees is still fairly representative as we cover more than 75% of all employees in Latvia. The dynamics of employment in 2019–2020 are broadly in line with the one for aggregated employment (which also accounts for government sector employees), although the decline in employment was more pronounced in the private sector during the first half of 2020, with some rebound afterwards.

The employer-employee data is aggregated to the firm level and matched with other firm-level variables, mainly coming from the annual balance sheets, profit-loss statements and the business registry. The summary statistics of the main firm-level variables in 2019 and 2020 are provided by Table A.1 in Appendix. The dataset contains 92 550 commercial firms (excluding firms with the main activity registered in financial service, public administration, education and healthcare sectors in 2020), although not all data is available for all firms (the issue of non-reporting tends to be more severe for the small firms). All in all, the average Latvian firm appears to be rather small (7–8 employees) and about 10 years old. Slightly more than 5% of firms are involved into merchandise exports, while approximately 7% of firms have a substantial foreign ownership.

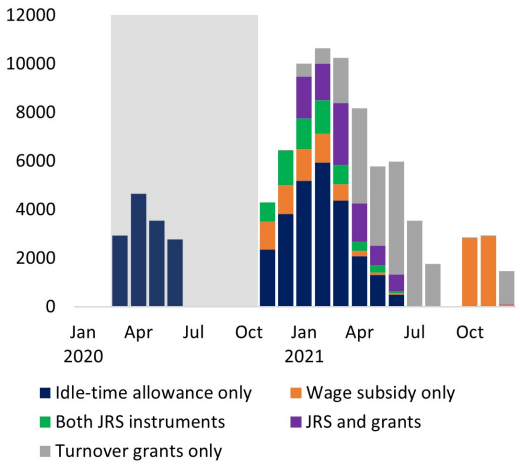
Total expenditure on Covid-related support for firms (excluding firms in financial service, public

[2]Wage subsidy programme was reactivated in October 2021 and lasted until February 2022.

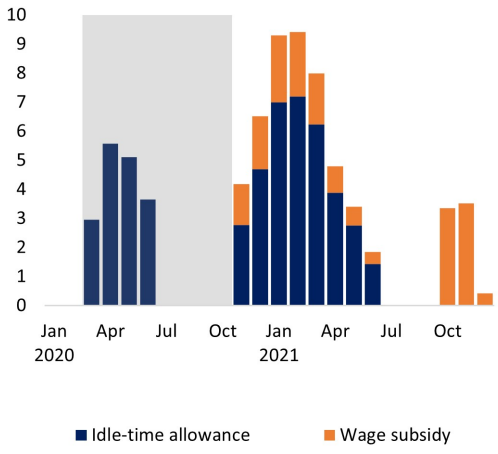
administration, education and healthcare sectors) in 2020-2021 was around 700 million EUR. The largest part of it came during the second outbreak of Covid-19 (in late 2020 and the first half of 2021), mostly in the form of turnover grants (as illustrated by Figure A.2 in Appendix). Almost 160 million EUR was spent on the initial JRS instrument – the idle-time allowance, of which one third was paid out during the first phase of the pandemic. On average, 3500 firms received the idle-time allowance each month during the first wave of the pandemic. The maximum of 4654 firms was reached in April 2020 (see Figure 1a). More firms were covered by the JRS support during the second wave (4700 firms per month on average, counting together both the idle-time allowance and the wage subsidy). The share of employees covered by JRS during the first phase reached the maximum of nearly 6% in April 2020, peaking at almost 10% in February 2021 during the second phase (see Figure 1b).

Figure 1: JRS coverage in Latvia, Jan'2020–Dec'2021

(a) The number of firms covered by JRS programmes and turnover grants



(b) The share of employees covered by JRS programmes, %



Source: SRS and CSB of Latvia, own calculations.
Note: Sample covers 92'550 firms. It excludes financial service (K), public administration (O), education (P) and healthcare sectors (Q). Gray shaded area shows the sample period for this study.

The largest take-up of the idle-time allowance was observed in those sectors which were affected the most by the lockdown. Thus, around a quarter of firms in the in-person service sectors, such as accommodation and food, and entertainment received JRS support during the first phase to keep their furloughed employees on payroll (see Table A.2 in Appendix). Participation rate was the lowest in sectors where employees could continue working remotely (e.g. ICT sector, professional services sector) or in sectors which operated critical infrastructure (e.g. electricity and gas, water

supply). A similar pattern was observed during the second phase of the JRS support, although more employees were covered.

In this paper, we focus on the first phase of JRS support, covering the period March–June 2020. There are three main reasons why we focus on the first phase despite the availability of the Covid-support data until the end of 2021. First, the initial phase was the result of an absolutely unforeseen exogenous shock. The Covid-19 pandemic was unexpected and there had been no examples of JRS in Latvia in the past. Firms did not expect it, and once the pandemic arrived there was little understanding of how such scheme might work. Firms could not have prepared for this policy in any way that would impact the outcome due to the anticipation effect. Second, the first phase of JRS support consisted of only one instrument – the idle-time allowance, whereas the second phase was a combination of the idle-time allowance and the wage subsidy. There are many instances when a single firm received both types of support (as illustrated in Figure 1a). Moreover, starting from January 2021 firms were eligible for turnover grants aimed at compensating a dramatic decline in economic activity due to the lockdown. Finally, most firms which participated in the second JRS phase had also participated in the first phase, hence, the effect of the second wave might be difficult to identify. Thus, our analysis covers the period between March 2020 (the first month when the idle-time allowance programme was introduced) and October 2020 (the last month before the second phase of the JRS) – as indicated by the grey area in Figure 1.

5 Methodology

5.1 Measurements of employment growth

We begin by elaborating on the measures of employment changes, which are used throughout the study: the rates of employment growth, job creation and destruction, as well as the rate of firm survival.

Employment growth rate in this paper is defined as follows:

$$g_{i,t} = \frac{L_{i,t} - L_{i,0}}{(L_{i,t} + L_{i,0})/2}, \tag{1}$$

where $L_{i,t}$ denotes total employment (in terms of the number of employees) of firm i in month t, while $L_{i,0}$ corresponds to the employment in the base period (February 2020). We follow the

conventional approach by Davis et al. (1996): the employment growth rate is defined as the change in firm-level employment divided by the average employment across the current and the base period (to avoid the situation when an entering firm implies the employment growth rate to be equal to infinity). The value of the above defined employment growth figure varies between –2 and 2.

The job creation rate is defined as

$$g^{+}_{i,t} = \frac{\sum_{\tau=1}^{t} \Delta L^{+}_{i,\tau}}{(L_{i,t} + L_{i,0})/2}, \tag{2}$$

where $\Delta L^{+}_{i,\tau}$ denotes the number of hired employees in month τ (running between the month following the base period and month t) in firm i. Note that we are able to observe $\Delta L^{+}_{i,\tau}$ every month due to the availability of employer-employee data (later aggregated to the firm level).

Similarly, the definition of *the job destruction rate* is

$$g^{-}_{i,t} = \frac{\sum_{\tau=1}^{t} \Delta L^{-}_{i,\tau}}{(L_{i,t} + L_{i,0})/2}, \tag{3}$$

where $\Delta L^{-}_{i,\tau}$ denotes the number of separated employees in month τ in firm i.

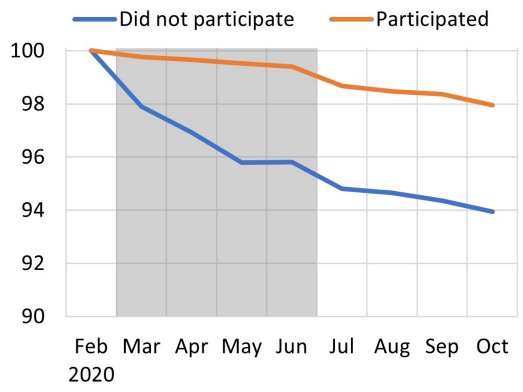
In order to account for the extensive margin of employment growth, we also analyse the *firm survival rate* – the share of firms that were active in the base month (February 2020) and still remained active in a month t that followed. We lack the explicit information on firms' closure therefore we assume that the firm is inactive when it reports zero employment in the respective month.

Figure 2 plots the evolution of the survival rate (panel 2a) and the employment growth rate (panel 2b). It suggests that by October 2020 the survival rate of firms that participated in JRS was higher by around 4 percentage points as compared to non-participating firms. There could be more than one explanation for such a difference in survival probability. One explanation is that JRS helped companies withstand the liquidity crisis without turning it into the solvency crisis. Another explanation is that only those companies which were sure about continuing activity applied for the subsidy, whereas companies facing the largest problems did not even consider applying for JRS and ceased their activity. This non-random selection motivates the use of matching framework in the next section. The difference in the evolution of the survival rate is similar across different types of industries, i.e. split by the intensity of interpersonal contact (see Figure A.3 in Appendix).[3]

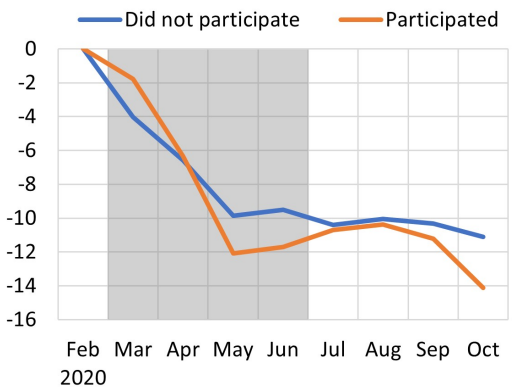
[3]We follow the recent analysis of the ECB and split sectors of the economy into service sectors with a higher level

Figure 2: Firm survival and employment growth rates

(a) Firm average survival rate (% since Feb'2020)



(b) Firm average employment growth rate (%, since Feb'2020)



Sources: SRS and CSB of Latvia, own calculations.
Notes: A firm is classified as "participated" if at least one of employees was covered by the idle-time allowance during March-June'2020 (gray shaded area). The sample includes only firms with non-zero employment in Feb'2020 and excludes financial service (K), public administration (O), education (P) and healthcare (Q) sectors. The firm average employment growth rate is calculated using equation (1) and is multiplied by 100. The sample consists of 65 948 non-participating firms and 4824 participating firms in February'2020.

The survival rate of both types of firms is somewhat smaller in contact-intensive services, however, implying that firms in these sectors were hit particularly hard by the pandemic, irrespective of JRS participation status. The firm-level change in employment was negative for both participants and non-participants since the start of the pandemic, and the degree of the decline in employment was fairly similar (see Figure A.4 in Appendix for sectoral differences).

5.2 Econometric framework

5.2.1 Propensity score matching

A simple comparison of employment dynamics across firms is merely the first step in the analysis, as firms participating in JRS are systematically different from non-participating firms along a number of dimensions. Most important, firms participating in JRS faced a more severe reduction in activity during the Covid-19 pandemic therefore differences in survival and employment growth rates captured in Figure 2 do not point to the effect of the idle-time allowance programme. Also, participating firms may differ in other respects (e.g. size and productivity) which may influence

of contact intensity (wholesale and retail trade (G), transport (H), accommodation and food services (I), arts and entertainment (R, S, T, U)), service sectors with a lower level of contact intensity (information and communication (J), real estate (L), professional, scientific and technical activities (M), and administrative and support service activities (N)), and industry (other sectors), see https://www.ecb.europa.eu/pub/economic-bulletin/focus/2021/html/ecb.ebbox202107_05~9d4efa40bc.en.html.

employment dynamics. In line with other related literature on the effects of participation in various public intervention programmes, we employ a non-experimental matching technique followed by the DiD estimation.

Non-experimental matching is performed based on a single index that measures the probability of a firm to receive the idle-time allowance conditional upon initial characteristics of a firm (see Rosenbaum and Rubin 1983). To identify this probability, we estimate the following probit model:

$$Pr(JRS_i = 1) = \Phi(g_i^{FTE}, X_i), \tag{4}$$

where JRS_i denotes a dummy variable for firm i participation in the JRS programme (receiving idle-time allowance), g_i^{FTE} stands for changes in full-time equivalent workloads (FTE) during the first wave of Covid-19, while X_i is a vector of other determinants of the probability to participate in JRS.

As for the dependent variable in equation (4), we consider participation in the idle-time allowance programme to have taken place ($JRS_i = 1$) when at least 25% of firm employees were covered by JRS during March-June 2020 on average. No participation is assumed ($JRS_i = 0$) if a firm was not part of the JRS scheme. Firms, where JRS participants constituted less than 25% of the workforce, are not considered in the estimation.[4]

As mentioned previously, to be eligible for JRS support a firm should have experienced a marked decline in turnover (see Section 3). It was the necessary albeit not the sufficient condition for eligibility (as the number of other requirements, which are unavailable in our dataset, such as the absence of even minor tax arrears, should have been satisfied as well), implying that there were firms, which experienced a similar decline in activity, but did not obtain JRS support. We assume that these other criteria, which are unobservable in our dataset, do not affect the outcome.[5] Although firm turnover data is not available in our dataset on a monthly basis, we make use of the availability of the monthly data on the total working hours by firm's employees. We assume that a decline in the number of FTE working hours consistently describes a firm's economic conditions and includes a change in FTE[6] in the probit regression (also interacted with a dummy indicating

[4]There were 2375 firms that satisfied the criteria of at least 25% employees covered by JRS during March–June 2020 on average, and 2991 firms that participated in JRS, but the degree of participation was not substantial. However, the firms satisfying the criteria dominate in terms of the JRS value (35.4 million EUR out of 46.8) and employees covered (85.4 thousand out of 114.9) during the first phase.

[5]This assumption is needed to correctly identify the casual effect.

[6]We use the growth rate of FTE in March–June 2020 on average compared with February 2020, as well as the

an exporting firm to capture different eligibility thresholds as explained in Section 3). The other determinants of the probability to participate in JRS are: a firm's relative productivity (measured by the value added per employee relative to the average productivity in a respective two-digit NACE industry), a firm's year of establishment, the relative capital-to-labour ratio, the number of employees, changes in employment in February 2020 relative to 2019 on average, and a few dummy variables: a) indicating whether a firm is an exporter; b) indicating whether a firm is owned by a foreign company; c) indicating whether it is a state-owned company; d) a set of industry dummies (defined at the two-digit NACE level). All additional determinants, except changes in employment, refer to 2019 to reduce problems associated with the reverse causality.

We denote the estimated participation probability (propensity score) for firm i in sector k as $Pr_{i,k}$. The control group is comprised of firms, which experienced a similar decline in total working hours and had similar characteristics before the Covid-19 pandemic as the participants. In technical terms the control group firms are the ones with the closest predicted probability $Pr_{i,k}$ to firms which participated in JRS. We employ the nearest-neighbour matching method with a caliper that requires the control firm j to be chosen within a certain probability distance:

$$\lambda > |Pr_{i,k} - Pr_{j,k}| = \min_{j \in \{JRS_i = 0\}} (|Pr_{i,k} - Pr_{j,k}|), \tag{5}$$

where λ is a caliper, i.e. a pre-specified scalar that determines the maximum allowed difference in the predicted propensity score. If no firm is found in λ proximity to match the treated firm, the treated firm is excluded from further analysis (it remains off support). Matching occurs only within a specified two-digit NACE sector to ensure comparability of variables between firms as some sectors were hit harder by the pandemic. We use a two nearest-neighbour matching technique, i.e. we search for two control firms with the closest estimated probability to receive JRS. The value of caliper applied is 0.001. We test the robustness of our results by employing other matching techniques and different values of caliper in Section 6.4.

Next, we employ the DiD estimator for the subset of firms with substantial participation in JRS (treated) and matched non-participating firms (control), and regress the employment performance indicators described in Section 5.1 on the dummy variable denoting participation in the first phase

growth rate of FTE in March–June 2020 on average compared with 2019 on average, to control also for changes in activity before the Covid-19 pandemic. The definition of the growth rate coincides with the one used for employment in equation (1) for consistency.

of the JRS, as well as other control variables:[7]

$$Y_{i,t} = \beta_0 + \beta_1 JRS_i + \beta_2 Z_i + \epsilon_{i,t}, \tag{6}$$

where $Y_{i,t}$ denotes an employment performance indicator for firm i in month t, JRS_i – a dummy variable which equals 1 for treated firms and 0 for matched control firms, and Z_i corresponds to the vector of various control variables (e.g. size, productivity, exporting activities, mostly referring to 2019). Employment performance indicators $Y_{i,t}$ used in this study are: a) a cumulative change in employment between February 2020 and the subsequent month t; b) a cumulative change in both hiring and separation rates between February 2020 and the subsequent month t; c) the survival rate in month t for firms that were active in February 2020. Since we are interested in the effect of the idle-time allowance support during the first phase, t runs between March 2020 and October 2020, so we estimate eight different DiD regressions for each employment performance indicator.

5.2.2 Unmatched regression analysis

Although propensity score matching allows forming a control group of firms similar to treated firms in terms of changes in activity during the first wave of pandemic, this comes at a cost: some treated firms do not have a corresponding match in the control group and are excluded from the DiD analysis (off support). To cover treated firms which remain off support, we also estimate the casual effect of JRS participation using data for all treated and control firms.[8] We add changes in FTE directly into the simple difference-in-difference regression to proxy for the changes in activity faced by firms during the Covid-19 pandemic. To this end, we regress the outcome variable (employment growth rate) on treatment dummies, changes in FTE and control for other factors:

$$Y_{i,t} = \beta_0 + \beta_1 JRS_{i,t}^{previous} + \beta_2 JRS_{i,t}^{other} + \beta_3 g_{i,t}^{FTE} + \beta_4 Z_i + \epsilon_{i,t}, \tag{7}$$

where $Y_{i,t}$ denotes firm's i employment growth rate in month t compared to February 2020. Unmatched regression allows more flexibility, so we use two JRS participation dummies instead of one. The first participation dummy $JRS_{i,t}^{previous}$ equals 1 if a firm obtained the idle-time allowance in

[7] In the baseline, we estimate the cross-industry average effect of JRS participation. However, in the robustness section, we distinguish between sectors with different levels of contact intensity and different shares of high-skilled labour and examine the heterogeneity of the effect of JRS participation.

[8] Thus, we include also firms that participated in the idle-time allowance programme, but did not satisfy the criteria of at least 25% employees covered on average.

the previous month. This dummy captures the legal provision of the programme, which required that employees cannot be laid off in the next month after receiving the idle-time allowance. Note that $JRS_{i,t}^{previous}$ equals 0 for all firms starting from August 2020 (as the idle-time allowance programme expired in June 2020). $JRS_{i,t}^{other}$ equals 1 if a firm participated in JRS in other months (e.g. $JRS_{i,t}^{other} = 1$ implies any participation in March–April 2020 for t corresponding to June 2020). The estimated coefficients β_1 and β_2 can be interpreted as estimates of the employment effect in the next month following the participation in the idle-time allowance programme, and in the months afterwards, respectively. $g_{i,t}^{FTE}$ is a vector of various FTE growth variables[9], included to control for changes in firm's economic activity and account for the endogeneity of JRS participation. Finally, Z_i stands for other control factors, similar to those in equation (6).

6 Empirical results

6.1 Unmatched regression analysis

We begin by reporting the results of the unmatched regression analysis, where the changes in activity are controlled by adding various FTE growth rates into the DiD regression. The estimation results for each month between April 2020 and October 2020[10] are reported in Table 1, while the evolution of the coefficients β_1 and β_2 is also reflected by Figure A.5a in Appendix.

Throughout the first few months during which the idle-time allowance was paid out, the effect was mainly coming from the legislative provision forbidding to lay off employees next month after the participation, while the effect of earlier participation was less pronounced. After the end of the first phase, as the lockdown eased participating firms showed greater ability to adapt to rapidly changing environment and kept the gap vis-à-vis non-participating firms. The effect appears to be persistent, at least in the short horizon of our analysis: JRS participating firms demonstrate more favourable employment performance (by approximately 15%) in October compared to February 2020 after controlling for changes in activity during the first wave of the pandemic.

[9] These are: growth rate in month $t-1$ compared with February 2020, growth rate in month $t-1$ compared with 2019 on average, growth rate comparing average FTE in other months between March–June 2020 compared with February 2020, growth rate comparing average FTE in other months between March–June 2020 compared with 2019 on average. In addition, the squares of all abovementioned FTE growth variables are added to equation (7).

[10] We do not estimate the effect for March 2020 as we only used lagged JRS participation dummies in unmatched regression.

Table 1: Unmatched regression estimation results for employment growth rates

Variable	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS participation dummies:							
$JRS^{previous}$	0.0482***	0.142***	0.109***	0.205***	-	-	-
JRS^{other}	-	0.0389***	0.0992***	0.0511***	0.175***	0.163***	0.152***
FTE growth rates:							
$g^{FTE}_{t-1,2019}$	0.0565***	0.0270	0.0303	0.0326	-	-	-
$(g^{FTE}_{t-1,2019})^2$	-0.0152***	0.00766	0.0230***	-0.00956	-	-	-
$g^{FTE}_{t-1,2020m2}$	0.0640***	0.154***	0.199***	0.211***	-	-	-
$(g^{FTE}_{t-1,2020m2})^2$	-0.0198***	-0.00692	-0.0232***	-0.0244***	-	-	-
$g^{FTE}_{other,2019}$	-	0.0455**	0.0686***	0.0845***	0.131***	0.130***	0.125***
$(g^{FTE}_{other,2019})^2$	-	-0.0200***	-0.0270***	-0.00594	-0.0135***	-0.0144***	-0.0288***
$g^{FTE}_{other,2020m2}$	-	-0.0483**	-0.0640***	-0.0528**	0.166******	0.162***	0.155***
$(g^{FTE}_{other,2020m2})^2$	-	-0.0299***	-0.0135*	-0.0171***	-0.0546***	-0.0550***	-0.0494***
Other control variables:							
Relative productivity (2019)	0.00920***	0.0111***	0.0124***	0.0125***	0.0166***	0.0188***	0.0208***
Relative K/L ratio (2019)	0.00154***	0.00199***	0.00235***	0.00223***	0.00395***	0.00458***	0.00535***
Log of average gross wage (2019)	-0.00445**	-0.00482*	-0.0105***	-0.000406	-0.00559**	-0.00421	-0.00494
Log of employment (2019)	-0.0154***	-0.0301***	-0.0351***	-0.0352***	-0.0297***	-0.0303***	-0.0333***
Birth year	-0.0008***	-0.0011***	-0.0013***	-0.0012***	-0.0009***	-0.0011***	-0.0015***
Exports of goods (2019, dummy)	0.00468	0.0103**	0.0167***	0.0113**	0.00956*	0.00904	0.0101
Foreign ownership (2019, dummy)	-0.00980**	-0.0122***	-0.0196***	-0.0252***	-0.0395***	-0.0445***	-0.0533***
State ownership (2019, dummy)	0.0224***	0.0339***	0.0529***	0.0650***	0.0354	0.0428**	0.0513***
Seasonal growth ($g_{t-12,2020m2}$)	0.0409***	0.0719***	0.0942***	0.121***	0.143***	0.162***	0.171***
R^2	0.0945	0.163	0.220	0.261	0.206	0.189	0.180
Number of observations	48'279	48'116	48'156	48'204	48'528	48'543	48'550

Sources: SRS and CSB of Latvia, own calculations.
Note: * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

Using this simple unmatched regression framework entails weaknesses associated with a potential imbalance in the distribution of confounding control factors across the two firm groups and a potential lack of overlap in the confounders (such that for a number of treated firms there are no empirical counterfactuals). As shown below, the latter is likely to be a problem, as for a large number of firms with a marked decline in economic activity we are not able to find similar firms, which experienced a similar decline in activity, but did not receive the support. To overcome this problem, we further proceed with the above described approach which entails matching and DiD estimation using the sample of participants and matched non-participants.

6.2 Propensity score matching

6.2.1 Matching results and quality

To perform matching, we need to calculate the conditional probabilities of firms to receive the idle-time allowance. For this purpose, we use the probit estimation, where we take into account a number of factors, primarily the decline in activity faced by firms during the first wave of the pandemic proxied by changes in FTE. Table 2 shows the estimation results of the JRS participation

probit regression.

Table 2: Factors affecting the probability of JRS participation

Determinants	Coefficients
FTE growth rate (March–June'2020 to 2019)	-0.549***
FTE growth rate (March–June'2020 to February'2020)	-0.615***
FTE growth rate (March–June'2020 to 2019) * Exports (2019, dummy)	0.320*
FTE growth rate (March–June'2020 to February'2020) * Exports (2019, dummy)	-1.02***
Relative labour productivity (2019)	0.0999***
Relative K/L ratio (2019)	0.0200***
Log of average gross wage (2019)	0.268***
Log of employment (2019)	0.259***
Changes in employment (February'2020 to 2019)	0.155***
Birth year	-0.00809***
Merchandise exports (2019, dummy)	-0.427***
Foreign ownership, $\geq$ 50% (2019, dummy)	-0.117**
State ownership, $\geq$ 50% (2019, dummy)	-1.88***
Sector fixed effect (two-digit NACE)	Yes
Number of observations	45 863

Sources: SRS and CSB of Latvia, own calculations.
Notes: Dependent variable =1 if on average at least 25% of workers covered by JRS over March–June'2020, =0 if no participation in JRS. Firms with participation in JRS below the abovementioned threshold were excluded from the sample. * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

By design of the idle-time allowance, firms that experienced a substantial decline in activity (proxied by a change in the number of employees' FTE workloads) were more likely to receive benefits, which is also captured by the negative coefficients before FTE growth rate variables.[11] Interestingly, the status of an exporter firm negatively affects the probability to participate in the idle-time allowance programme. However, exporting firms that experienced a comparable decline in FTE showed a higher probability to participate than non-exporting firms, which corresponds to the lower threshold of decline in turnover required for exporters (see Section 3). The state-owned firms were less likely to participate in the JRS, which may indicate that the idle-time allowance program was primarily designed for the private sector. The obtained coefficients of the probit model also indicate that the firm size (as well as a change in it), labour productivity and capital-to-labour ratio have a positive and statistically significant effect on the likelihood of receiving the JRS support, while firm age has a negative effect.

The estimated propensity scores (probabilities to receive JRS support) are used to perform matching for each treated firm. We implement matching using two nearest neighbour approach additionally requiring matching within the same two-digit sector and by applying a caliper with the value of 0.001. The caliper ensures that the propensity score of a treated firm is close to that of the

[11] We include FTE growth rates with respect to February 2020 as well as compared with 2019 on average. The purpose is threefold: keeping the model close to the legal requirements of the idle-time allowance, improving the fit of a probit model, and ensuring similar activity trends for matched firm before the pandemic.

matched control, thus helping to get rid of potentially bad matches. A drawback of applying a tight caliper is a relatively large number of observations with no empirical counterfactual (i.e. without similar control firms).[12]

Matching is performed assuming that there are no unobserved systematic differences between the treatment and control group (i.e. factors that can affect treatment assignment and the outcome). We believe this assumption to be valid against the background of a large number of additional rather bureaucratic requirements for the idle-time allowance applicants, which had to be fulfilled in order to be eligible for the support. These requirements (such as companies not having even minor tax arrears) during the relatively chaotic first phase could have entailed a sort of randomness when assigning support to otherwise very similar firms.

Table 3: Quality of matching

Variable	Unmatched			Matched		
	Treated	Control	p-value	Treated	Control	p-value
FTE growth rate (Mar–Jun'2020 to 2019)	-1.006	-0.187	0.000	-0.843	-0.860	0.518
FTE growth rate (Mar–Jun'2020 to Feb'2020)	-0.984	-0.109	0.000	-0.809	-0.816	0.789
Relative labour productivity (2019)	0.348	0.0605	0.000	0.265	0.200	0.084
Relative K/L ratio (2019)	1.493	0.766	0.000	1.225	1.125	0.517
Log of average gross wage (2019)	6.299	5.967	0.000	6.188	6.145	0.195
Log of employment (2019)	1.941	1.195	0.000	1.698	1.755	0.276
Changes in employment (February'2020 to 2019)	-0.0413	-0.0123	0.000	-0.0371	-0.0384	0.910
Birth year	2006.9	2007.8	0.000	2007.3	2006.9	0.378
Merchandise exports (2019, dummy)	0.109	0.0908	0.005	0.0763	0.0763	1.000
Foreign ownership, $\geq$ 50% (dummy)	0.0995	0.0750	0.000	0.0831	0.0910	0.494
State ownership, $\geq$ 50% (dummy)	0.00047	0.00467	0.005	0.000	0.00043	0.477

Sources: SRS and CSB of Latvia, own calculations.
Notes: t-test for mean values of treated vs control firms. 1180 on support, 951 off support, 1670 controls.

To proceed with the DiD estimation we need to be sure that the quality of matching is satisfactory, i.e. firms that received JRS support are comparable to those that did not receive it. It is evident from Table 3 that before matching the treated firms experienced a much more pronounced decline in activity (proxied by changes in FTE) than non-participants, but also tended to be larger, older and more productive. Although no formal requirement existed in the idle-time allowance programme with respect to the latter variables, such firms may have better ability to apply for the support, or have less tax arrears. After matching, on the contrary, the differences in means among the treated and matched control firms are statistically insignificant at 95% confidence level for all variables.

However, such a high matching quality is achieved at the cost of losing around 40% of treated firms, which remained off support. These observations refer to firms that experienced a very signif-

[12] We check the robustness of our results by applying a larger caliper in Section 6.4.

icant decline in economic activity during the first wave of pandemic, so that it was impossible to find a similar firm within the same sector without JRS participation (see Table A.3). The decline in employment was also more pronounced for treated firms remaining off support. These firms are also different in other dimensions: they are bigger, more productive and more capitalised than JRS recipients that stayed on support. Table A.4 also shows that these firms are more likely to be found in manufacturing and some services sectors, such as administrative services; arts, entertainment and recreation. This systematic difference between on support and off support treated firms should be kept in mind while interpreting the results of DiD estimations that use matched firms.

6.2.2 Difference-in-difference estimation

First, we look at descriptive evidence on the performance of matched treated and control firms. Figure A.6 in Appendix plots the evolution of the employment rate and the survival rate for matched firms. It illustrates that firms receiving the idle-time allowance are more likely to survive as well as they reduce employment to a much lower extent than matched control firms.[13] This outcome stands in contrast to that obtained before matching (see Figure 2), which highlights the role of matching when estimating the employment effect of JRS.

Now we can switch to the results of a more formal analysis by DiD. Table 4 documents the coefficients of equation (6), estimated for each month following the outset of the first pandemic wave until October 2020. The difference in the rate of employment growth between recipients and non-recipients of JRS support appears positive and statistically significant in all months, peaking in August 2020 and shows a very gradual decline afterwards. Relative productivity, the capital-to-labour ratio as well as firm age appear significant in most of months.

Figure 3a provides the visual illustration of the set of estimated β_1 coefficients of the casual effect of JRS participation on employment growth alongside their confidence intervals. It is clearly evident that the gap between the two types of firms was expanding rapidly throughout the very first few months after the onset of the pandemic, as economic and financial indicators of firms were worsening sharply. The difference was resilient throughout the months following the end of the first phase of the JRS support and stabilised around 30 percentage points. This suggests that as the economy was recovering in June firms that had received the idle-time allowance managed to retain

[13] We also highlight that the assumption of parallel trends holds after matching in Figure A.7 in Appendix. Both treated and control firms demonstrate very similar employment dynamics for more than a year before the start of pandemic.

employment more successfully compared to non-participants.

Table 4: DiD regression results for employment growth rates

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (dummy)	0.0671***	0.164***	0.275***	0.322***	0.326***	0.327***	0.300***	0.269***
Relative productivity (2019)	0.0175**	0.0253**	0.0444***	0.0595***	0.0611***	0.0618***	0.0607***	0.0605***
Relative K/L ratio (2019)	0.00119	0.0061***	0.0119***	0.0118***	0.0171***	0.0212***	0.0176***	0.0199***
Log of average gross wage (2019)	0.0127	0.0242*	0.0214	0.0110	0.0271	0.0108	0.0261	0.0249
Log of employment (2019)	0.00403	-0.00867	-0.0112	-0.0106	-0.00471	0.00418	0.00732	-0.00806
Birth year	-0.002***	-0.007***	-0.008***	-0.009***	-0.008***	-0.008***	-0.008***	-0.009***
Merchandise exports (2019)	0.00866	0.0576***	0.0788****	0.0427	0.0658**	0.0637**	0.0360	0.0598**
Foreign ownership, $\geq$ 50% (2019)	-0.0490**	-0.0362	-0.0373	-0.0230	-0.0461	-0.0647	-0.0732*	-0.0758*
State ownership, $\geq$ 50% (2019)	0.0494***	0.216***	0.332***	0.375***	0.296***	0.271***	0.234***	0.294***
R^2	0.0340	0.0836	0.136	0.140	0.130	0.125	0.112	0.105
Number of observations	2'842	2'849	2'850	2'849	2'848	2'850	2'848	2'849

Source: SRS and CSB of Latvia, own calculations
Note: * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

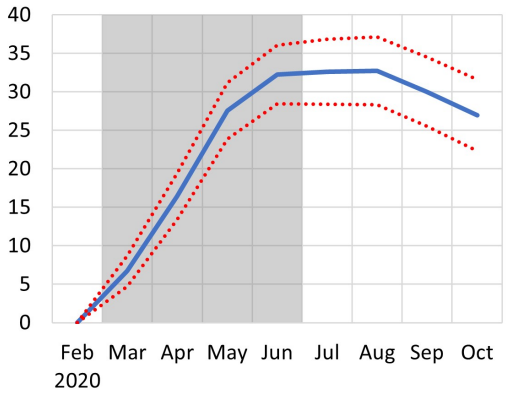
The overall effect on employment is likely to mask two effects: the declining probability of becoming inactive (having zero employment), and a positive effect on employment for firms that remained active. We separate these extensive and intensive effects by running a DiD estimate for the probability to survive, and re-running a DiD estimate for the employment growth using the subsample of firms with non-zero employment in month t (see Table A.5 and Table A.6 in Appendix respectively). Figure 3b decomposes the overall effect on employment into extensive and intensive margins, demonstrating that JRS recipient firms were both less likely to exit and more likely to hoard labour. Although the overall gap in firm-level change in employment remained stable after May 2020, the contribution of the extensive margin rose, while that of the intensive margin started to decline after July 2020. The former implies that many non-recipient firms could not benefit from the re-opening of the economy, or that economic recovery only partly improved their conditions, such that they were forced to cease their activity.

Further insight can be obtained by decomposing the treatment effect on employment growth into the effect of hiring and separation. This is implemented by re-estimating equation (6) for job creation (hiring) and job destruction (separation) rates, rather than employment growth rate. The estimation results are provided in Table A.7 and Table A.8 in Appendix. The estimated effects of the JRS participation on hiring and separation are drawn in Figure 4.

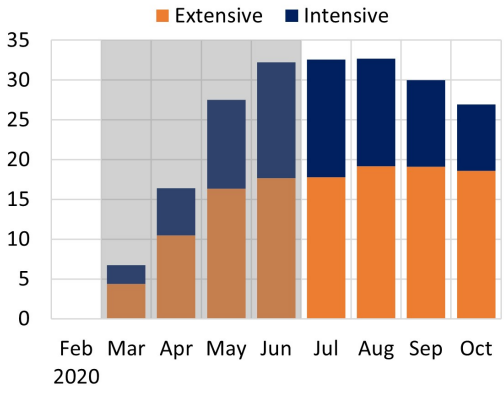
The JRS recipients were statistically less likely to both hire new employees and lay off the current ones. But the latter effect appears to be much stronger, so the positive effect on employment mainly came through the reduction of separation rate. For separation, there is a minor reversal in

Figure 3: The effect of JRS support on employment growth rate

(a) Difference in employment dynamics (vis-à-vis Feb–2020) between recipients and non-recipients of JRS support, percentage points



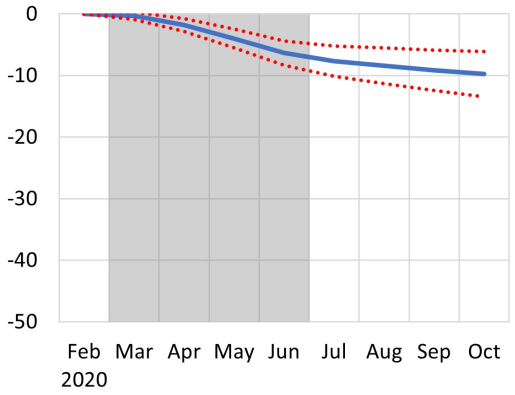
(b) Decomposition of the difference in employment dynamics into extensive and intensive margins, percentage points



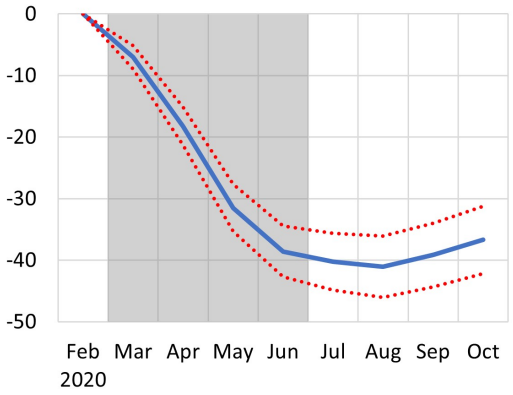
Sources: SRS and CSB of Latvia.
Notes: Dotted red lines denote the ±2 standard deviation confidence interval. Shaded grey area stands for the period when firms were receiving the first wave of the idle-time allowances.

Figure 4: The effect of JRS support on hiring and separation rate

(a) Differences in hiring rates between JRS recipients and non-recipients (%)



(b) Differences in separation rates between JRS recipients and non-recipients (%)



Sources: SRS and CSB of Latvia.
Notes: Dotted red lines denote the ±2 standard deviation confidence interval. Shaded grey area stands for the period when firms were receiving the first wave of the idle-time allowances.

their relative dynamics after August 2020. This could imply that some non-recipient firms, which survived the first wave of the pandemic, started to slow down the pace of laying off their workforce as the economy was reopening. Alternatively, some participating firms may have increased layoffs, a potential sign that at least partially JRS could have been used to support jobs which were not viable in medium term. Nevertheless, the reversal in dynamics is relatively minor and the aggregate

differences between the participants and non-participants in hiring and firing remain persistent.

6.3 Heterogeneity of the JRS participation effect

Now we analyse the heterogeneity of the above described positive effect of JRS on employment. In particular, we examine whether the difference in employment dynamics between JRS recipients and matched non-recipients is affected by some firm or industry level characteristics.

First, we distinguish between service sectors with different levels of contact intensity (using industry as the benchmark) and different shares of high-skilled labour.[14] The economic costs of the Covid-19 pandemic were particularly high in contact-intensive service sectors, such as accommodation and food, entertainment and recreation, tourism, etc. Economic activity in these sectors did not recover even when most of the public health related restrictions were lifted after the initial wave of infections, and remained subdued until the widespread availability of vaccines (see Brinke et al. 2022). This could entail less pronounced employment effect, as firms in these sectors had to adjust employment irrespective of their participation in JRS (see Figures A.3 and A.4 in Appendix). Similarly, high-skill sectors could exhibit larger employment effects as the costs of employment adjustment for non-participants might be increasing with the complexity of skills required.

Second, we examine the impact of firm productivity, size, exporting status and ownership on the JRS effect. In particular, this exercise may help getting some insights about the effect of JRS on the allocation of resources. We accomplish this task by introducing an interaction term in DiD regressions:

$$Y_{i,t} = \beta_0 + \beta_1 JRS_i + \beta_2 JRS_i * F_{i,k} + \beta_3 Z_i + \epsilon_{i,t}, \tag{8}$$

where $F_{i,k}$ corresponds to one of the above-explained factors at firm i or industry k level. The firm-level factors $F_{i,k}$ always refer to 2019 to ensure exogeneity.

The estimated coefficients of the JRS treatment effect and its interactions are presented in Table 5. The estimation results indicate that taking as the benchmark industry, the effect of JRS participation is lower in service sectors with a high level of contact intensity. Firms in these sectors were hit particularly hard including those that applied for and obtained the idle-time allowance. The magnitude of the interaction coefficient grows over time, implying that re-opening of the economy after the first wave of the pandemic, which took place in summer 2020, was less supportive for these

[14] We use the data from the LFS to calculate the share of high-skilled labour by broad NACE sectors. We classify the first three groups of ISCO-08 occupation classification as high-skilled labour: 1) Managers; 2) Professionals; 3) Technicians and Associate Professionals.

sectors. Although the employment effect of JRS for service sectors with a high level of contact intensity became lower over time, it was still positive and statistically significant. Regarding the share of high-skilled employees, its interaction coefficient is positive and statistically significant until July'2020.

Table 5: DiD regressions with interactions for employment growth rates

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (dummy)	0.0650***	0.174***	0.295***	0.335***	0.341***	0.362***	0.350***	0.349***
. x Services with low contact intensity (dummy)	0.0304**	0.0196	0.0211	0.0257	0.0234	-0.0291	-0.0607*	-0.0691*
. x Services with high contact intensity (dummy)	-0.00508	-0.0201**	-0.0341**	-0.0244	-0.0277	-0.0419*	-0.0544**	-0.095***
JRS (dummy)	0.0397***	0.128***	0.223***	0.270***	0.274***	0.303***	0.277***	0.221***
. x Share of high-skilled labour	0.0354	0.0605**	0.110***	0.111***	0.0985***	0.00593	0.00378	0.0876
JRS (dummy)	0.0749***	0.177***	0.301***	0.352***	0.352***	0.353***	0.328***	0.294***
. x Relative productivity (2019)	-0.033***	-0.057***	-0.109***	-0.124***	-0.111***	-0.109***	-0.116***	-0.103***
JRS (dummy)	0.101***	0.246***	0.422***	0.460***	0.463***	0.459***	0.410***	0.378***
. x Log of employment (2019)	-0.020***	-0.048***	-0.085***	-0.080***	-0.080***	-0.076***	-0.064***	-0.063***
JRS (dummy)	0.0726***	0.177***	0.293**	0.337***	0.343***	0.343***	0.314***	0.282***
. x Merchandise exports (2019)	-0.073***	-0.169***	-0.235***	-0.192***	-0.229***	-0.207***	-0.180***	-0.168***
JRS (dummy)	0.0618***	0.162***	0.278***	0.324***	0.326***	0.325***	0.295***	0.265***
. x Foreign ownership (2019)	0.0662*	0.0216	-0.0299	-0.0244	0.000856	0.0326	0.0579	0.0548
Number of observations	2'842	2'849	2'850	2'849	2'848	2'850	2'848	2'849

Sources: SRS and CSB of Latvia, own calculations.
Notes: We only report the coefficients related to the treated dummy and its interactions, the rest is available upon request. * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

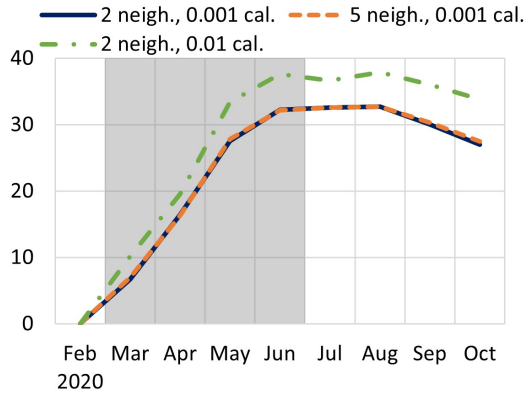
For firm level characteristics, we find that larger and more productive firms not only had a higher probability to receive the subsidy, but also once having received them gained somewhat less in terms of employment. This could probably be related to the fact that larger and productive firms, even if they did not participate in JRS may have had enough internal resources to keep their workers during the period of restrictions. Also, for exporters participation effect is significantly smaller, probably for similar reasons. These findings lead to contradicting conclusions about the effect of the idle-time allowance programme on the allocation of resources in Latvia. On the one hand, the fact that more productive firms gained less in terms of employment implies the negative effect on allocation of resources – similar to the one reported by Merikül and Paulus (2022) for Estonia. On the other hand, selection of more productive firms into the idle-time allowance programme (captured by the probit analysis in Table 2) together with the positive effect of JRS on employment suggests increasing allocation efficiency. The net effect is unclear and requires further analysis that goes beyond the scope of this paper.

6.4 Robustness exercise

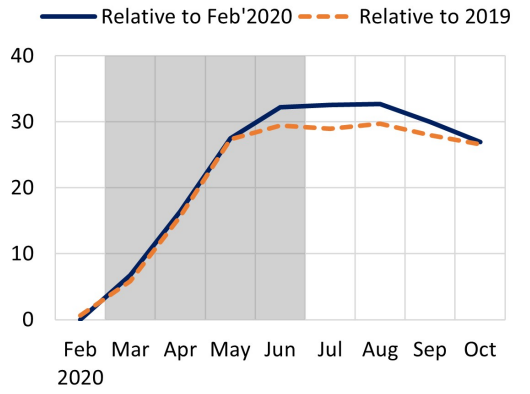
At last, we conduct a number of robustness checks. First, we repeat matching with five nearest neighbours (instead of two) and with a larger value of caliper (0.01 instead of 0.001). Matching quality is still high in the former exercise (Table A.9 in Appendix), but it gets worse in the latter (Table A.10 in Appendix) because less similar non-participating firms are now considered and used for matching. A larger number of treated firms are included into the analysis, however. Applying alternative matching parameters does not affect the estimates of the participation effect markedly (see Figure 5a). While applying five nearest firms instead of two results in the curve which is almost identical to the one obtained in the baseline, using less restrictive caliper slightly shifts the estimated effect upwards. The latter can be the sign that the effect of the JRS programme was actually higher for the off support firms and the results of the DiD regression in Section 6.2.2 point to the lower bound of the effect.

Figure 5: The impact of changing the base and alternative matching parameters on the participation coefficient estimates

(a) Participation effect on employment growth using alternative matching parameters, percentage points



(b) Participation effect on employment growth with a different base period, percentage points



Sources: SRS and CSB of Latvia.
Notes: The shaded grey area stands for the period when firms were receiving the first wave of the idle-time allowances.

Second, we recalculate differences in employment growth rates with respect to the 2019 average values (rather than to February 2020 values). Changing the base period against which employment growth is being calculated does not result in a notable difference as compared to the baseline casual effect (see Figure 5b). The difference in employment growth rates between recipients and non-recipients is getting only slightly lower from May 2020.

Next, we estimate equation (7) replacing the participation dummy on the right-hand side with a) the share of employees which obtained the idle-time allowance; b) the share of the wage bill paid out to those employees that obtained the idle-time allowance. Both estimation results are documented in Table A.11 and Table A.12 in Appendix respectively. As can be seen, using an alternative to the participation dummy does not qualitatively impact our conclusions.

We also apply a stricter criteria for treated firms in order to avoid the difference between firms that obtained the idle-time allowance only in April, and the firms that obtained the support only in June. Table A.13 in Appendix shows the effect of JRS on employment when only firms receiving a substantial amount of support in all months between April and June 2020 were included into the treated group. The more restrictive definition of the treated group (with fewer firms) does not affect our previous conclusions.

Finally, we replicate the simple regression analysis (see equation (7)) on the sample of firms that consists of matched participants and matched non-participants. This excludes off support participant firms and a large number of non-participants. The visual inspection of the coefficients of both JRS dummies (Figure A.5 in Appendix) leads to the conclusion that the results are qualitatively similar, albeit the effect of participation is somewhat more pronounced when matched observations are employed (see Figure A.5b).

7 Conclusions

This paper contributes to the literature on the macroeconomic impact of job retention schemes (JRS) by studying the effect of the idle-time allowance paid out to commercial firms in Latvia during the first wave of the Covid-19 pandemic. The study uses a rich firm-level dataset of Latvia's firms, merged with the data on allowance recipients.

The empirical results obtained in this paper imply the positive employment effect of JRS participation. More specifically, the change in the number of employees vis-à-vis the pre-pandemic levels in participating firms remained persistently higher compared with non-participating firms. This effect lasted for several months after the end of the first JRS wave. Participation affected both the likelihood of the firm's survival (positively) and the rate at which employees were laid off (negatively).

The results of the paper also suggest that the participation effect is not homogeneous across

firms. Companies that operated in sectors with a higher level of contact intensity seem to have gained less, as the allowance was less protective against deteriorating conditions in those industries. Furthermore, the participation effect appears stronger in sectors with a higher proportion of highly skilled employees.

The paper also provides some tentative indications of the negative allocation effects of JRS, however this remains the topic of a further investigation. In fact, Latvia's dataset is unique as it provides a very detailed information on JRS support recipients, which makes it possible to go beyond the previous allocation literature.

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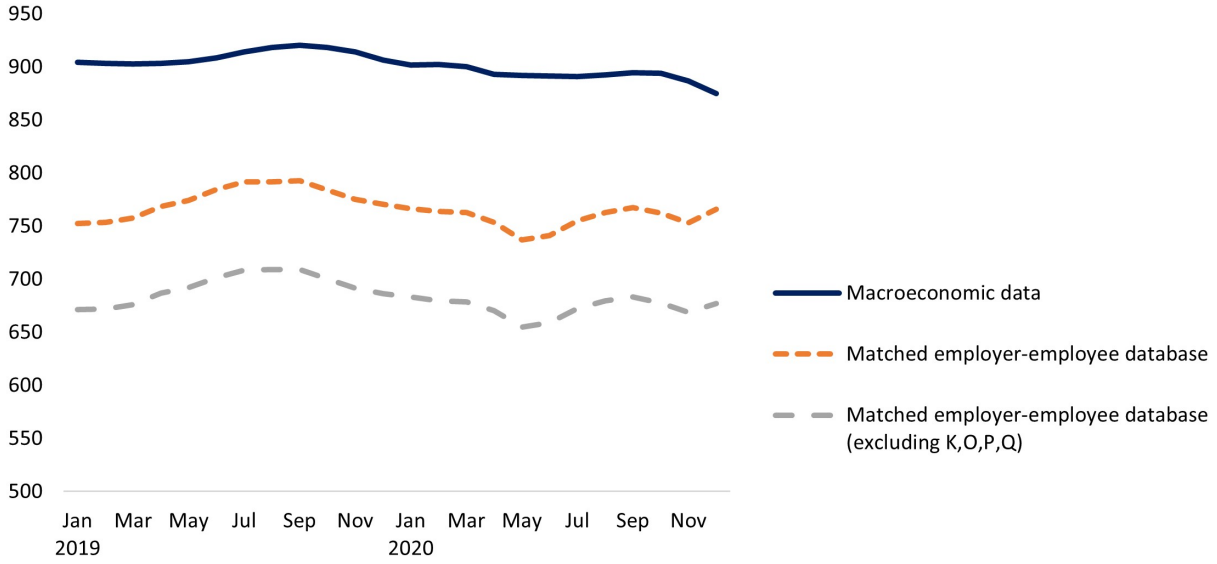
8 Appendix

Figure A.1: Aggregate number of employees in 2019–2020, thousands



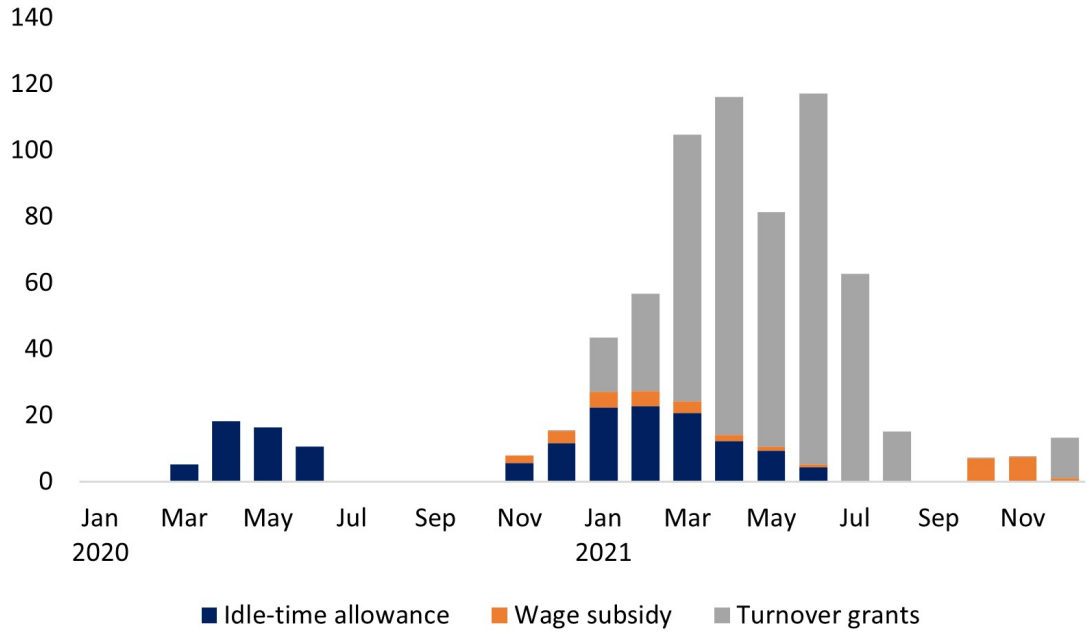
Sources: SRS and CSB of Latvia, own calculations.
Notes: Matched employer-employee dataset excludes the government sector. After excluding commercial firms with main activity in financial service (K), public administration (O), education (P) and healthcare (Q) sector in 2020, the dataset covers 92 550 firms.

Table A.1: Summary statistics of main firm-level variables

Variable	Mean	Median	St.dev	Min	Max	N.obs
		2019				
Number of employees	8.28	2	60.5	0	7'313	76'428
Capital per employee (EUR)	92'418.3	961.06	1'438'859	0	191'000'000	76'422
Value added per employee (EUR)	21'221.1	8'269.67	179'483.6	-15'100'000	17'100'000	74'692
Birth year	2009.05	2011	7.81	1991	2019	79'136
Merchandise exports (dummy)	0.0584	0	0.235	0	1	92'550
Foreign ownership, $\geq$ 50% (dummy)	0.0773	0	0.267	0	1	80'946
State ownership, $\geq$ 50% (dummy)	0.00413	0	0.0641	0	1	80'946
		2020				
Number of employees	7.67	2	56.5	0	7'096	80'756
Capital per employee (EUR)	88'006.12	793.33	1'396'910	0	175'000'000	80'541
Value added per employee (EUR)	20'884.0	8'241.33	116'128.7	-3'273'575	10'400'000	74'466
Birth year	2009.07	2011	8.32	1991	2020	92'533
Merchandise exports (dummy)	0.0557	0	0.229	0	1	92'550
Foreign ownership, $\geq$ 50% (dummy)	0.0706	0	0.256	0	1	86'449
State ownership, $\geq$ 50% (dummy)	0.00371	0	0.0608	0	1	86'449

Sources: SRS and CSB of Latvia, own calculations
Notes: Excludes firms with main activity in financial service (K), public administration (O), education (P) and healthcare (Q) sector in 2020. Covering 92 550 firms.

Figure A.2: Total expenditure of Covid-related support in Jan'2020–Nov'2021, million of EUR



Sources: SRS and CSB of Latvia, own calculations.
Notes: Sample covers 92 550 firms. It excludes financial service (K), public administration (O), education (P) and healthcare (Q) sectors.

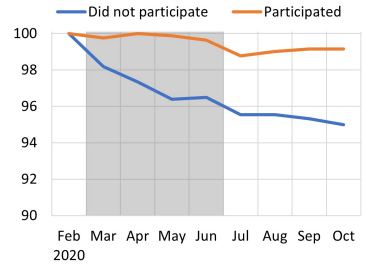
Table A.2: The share of employees covered by JRS programmes by industry, %

NACE	Mar'2020 - Jun'2020		Nov'2020 - Jun'2021	
	Idle-time allowance	Wage subsidy	Idle-time allowance	Wage subsidy
(A) Agriculture	0.5	-	0.7	0.2
(B) Mining	0.2	-	0.7	0.1
(C) Manufacturing	4.2	-	2.6	0.8
(D) Electricity and gas	0.0	-	0.1	0.1
(E) Water supply	0.0	-	0.4	0.2
(F) Construction	0.8	-	1.9	0.5
(G) Trade	2.8	-	8.6	1.8
(H) Transportation	2.8	-	1.8	0.6
(I) Accommodation and food	26.8	-	47.9	9.9
(J) Information and communication	1.6	-	1.2	0.2
(L) Real estate	1.2	-	2.3	0.7
(M) Professional services	2.8	-	3.3	1.1
(N) Administrative services	4.0	-	5.0	1.0
(R) Arts, entertainment and recreation	24.4	-	32.9	2.1
(S) Other services	5.3	-	16.0	3.3

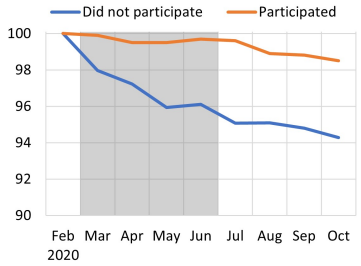
Sources: SRS and CSB of Latvia, own calculations.
Notes: The wage subsidy programme was not available during the first wave of the COVID-19 pandemic since it was introduced in November 2020. The sample covers 92 550 firms. It excludes financial service (K), public administration (O), education (P) and healthcare (Q) sectors.

Figure A.3: Survival rate of firms by the level of interpersonal contact (% since February'2020)

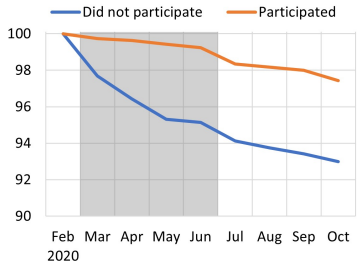
(a) Industry



(b) Less contact-intensive services



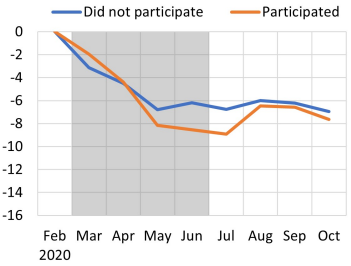
(c) More contact-intensive services



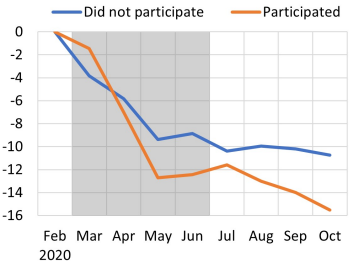
Sources: SRS and CSB of Latvia, own calculations.
Notes: A firm classified as "participated" if at least one of employees was covered by the idle-time allowance during March–June'2020 (gray shaded area). The sample includes only firms with non-zero employment in February'2020 and excludes financial service (K), public administration (O), education (P) and healthcare (Q) sectors. Sectors are split into more contact-intensive service sectors (wholesale and retail trade (G), transport (H), accommodation and food services (I), arts and entertainment (R, S, T, U)); less contact-intensive service sectors (information and communication (J), real estate (L), professional, scientific and technical activities (M), and administrative and support service activities (N)); industry (other sectors).

Figure A.4: Employment growth rate of firms by the level of interpersonal contact (% since February'2020)

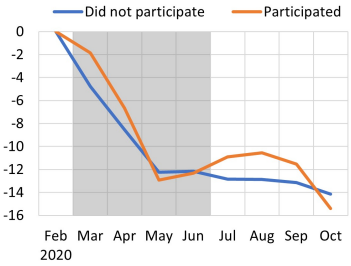
(a) Industry



(b) Less contact-intensive services



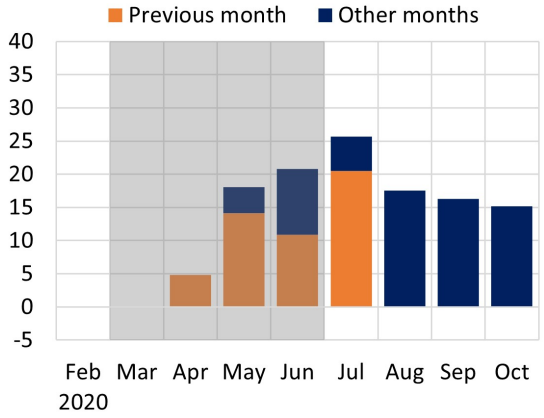
(c) More contact-intensive services



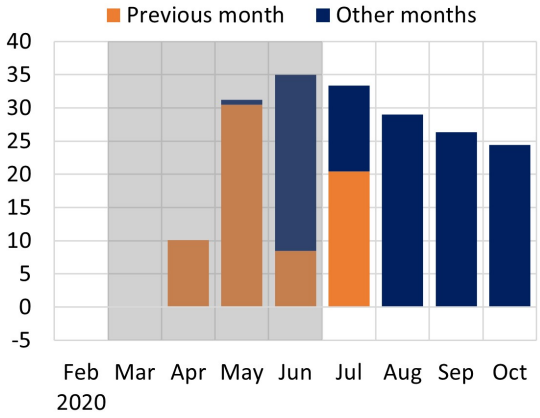
Sources: SRS and CSB of Latvia, own calculations.
Notes: A firm classified as "participated" if at least one of employees was covered by the idle-time allowance during March–June'2020 (gray shaded area). The sample includes only firms with non-zero employment in February'2020 and excludes financial service (K), public administration (O), education (P) and healthcare (Q) sectors. Sectors are split into more contact-intensive service sectors (wholesale and retail trade (G), transport (H), accommodation and food services (I), arts and entertainment (R, S, T, U)); less contact-intensive service sectors (information and communication (J), real estate (L), professional, scientific and technical activities (M), and administrative and support service activities (N)); industry (other sectors).

Figure A.5: Comparison of the JRS participation effect on employment growth rate: without matching vs with matching

(a) Difference in employment growth rates between participants and non-participants (equation (7) *without matching*, two JRS dummies), percentage points



(b) Difference in employment growth rates between participants and non-participants (equation (7) *with matching*, two JRS dummies), percentage points



Sources: SRS and CSB of Latvia, own calculations.
Notes: Result of unmatched and matched regression analysis (see equation (7)). The gray shaded area reflects the period when JRS were active.

Table A.3: Comparison of participating firms which are on and off support

NACE	On support (mean)	Off support (mean)	t-test (p-value)
Employment growth rate (April'2020 to February'2020)	-0.0370	-0.0649	0.000
Employment growth rate (June'2020 to February'2020)	-0.0577	-0.125	0.000
Employment growth rate (August'2020 to February'2020)	-0.0595	-0.125	0.000
Employment growth rate (October'2020 to February'2020)	-0.111	-0.186	0.000
FTE growth rate (March-June'2020 to 2019)	-0.843	-1.208	0.000
FTE growth rate (March-June'2020 to February'2020)	-0.809	-1.201	0.000
Relative labour productivity (2019)	0.265	0.451	0.000
Relative K/L ratio (2019)	1.225	1.824	0.000
Log of average gross wage (2019)	6.188	6.436	0.000
Log of employment (2019)	1.698	2.242	0.000
Changes in employment (February'2020 to 2019)	-0.0371	-0.0465	0.348
Birth year	2007.3	2006.4	0.020
Merchandise exports (2019, dummy)	0.0763	0.149	0.000
Foreign ownership, $\geq$ 50% (2019, dummy)	0.0822	0.120	0.004
State ownership, $\geq$ 50% (2019, dummy)	0.00105	0.000	0.265

Sources: SRS and CSB of Latvia, own calculations.
Note: 1180 firms are on support, 951 firms – off support.

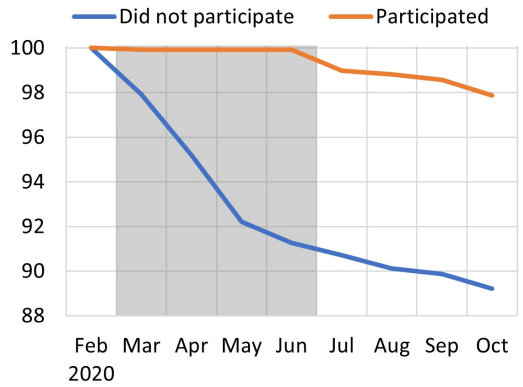
Table A.4: The number of treated firms by industry

NACE	On support	Off support
(A) Agriculture	13	3
(B) Mining	1	0
(C) Manufacturing	71	147
(D) Electricity and gas	0	1
(E) Water supply	0	0
(F) Construction	45	10
(G) Trade	382	66
(H) Transportation	86	66
(I) Accommodation and food	289	252
(J) Information and communication	30	29
(L) Real estate	35	6
(M) Professional services	88	76
(N) Administrative services	67	160
(R) Arts, entertainment and recreation	41	108
(S) Other services	32	27

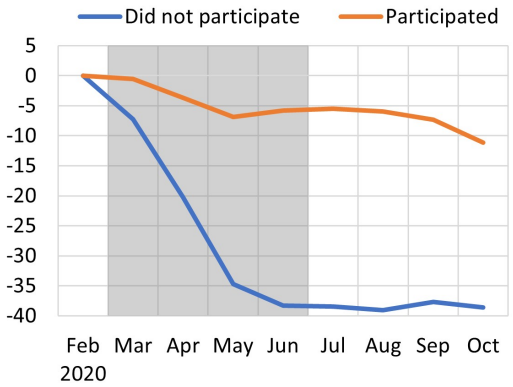
Sources: SRS and CSB of Latvia.
Note: 1180 firms are on support, 951 firms – off support.

Figure A.6: Firm survival and employment growth rates after matching

(a) Firm survival rate (% since February'2020)

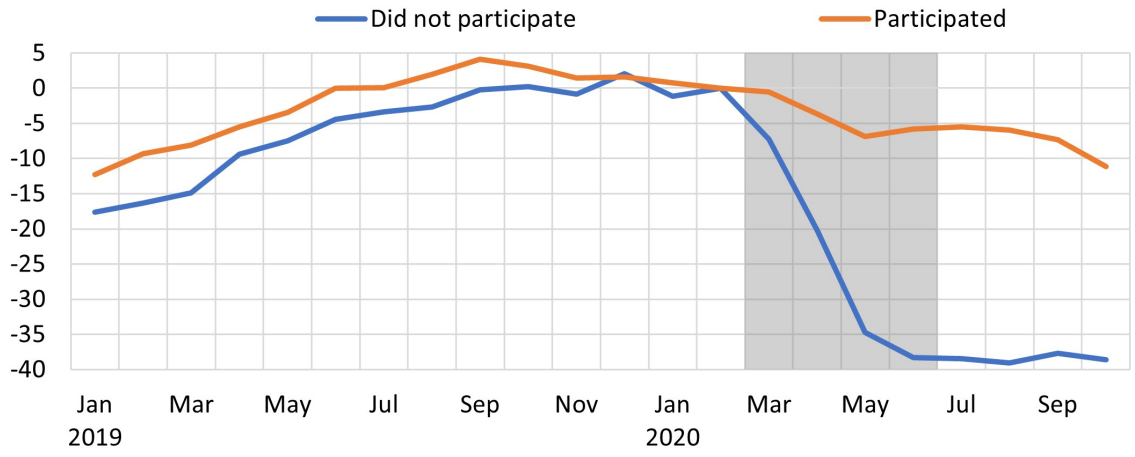


(b) Employment growth rate (% since February'2020)



Sources: SRS and CSB of Latvia, own calculations.
Note: A firm classified as "participated" if at least one of employees was covered by the idle-time allowance during March–June'2020 (gray shaded area). The sample includes only firms with non-zero employment in February'2020 and excludes financial service (K), public administration (O), education (P) and healthcare (Q) sectors. The employment growth rate is calculated using equation (1) and multiplied by 100. The matched sample consists of 1670 non-participating firms and 1180 participating firms in February'2020.

Figure A.7: Employment growth rates after matching, 2019–2020, %, comparing with February'2020



Sources: SRS and CSB of Latvia, own calculations.
Notes: Firm classified as "participated" if at least one of employees was covered by the idle-time allowance during March-June'2020 (gray shaded area). The sample includes only firms with non-zero employment in February'2020 and excludes financial service (K), public administration (O), education (P) and healthcare (Q) sectors. The employment growth rate is calculated using equation (1) and multiplied by 100. The matched sample consists of 1670 non-participating firms and 1180 participating firms in February'2020.

Table A.5: DiD regression results for survival rates (extensive margin)

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (dummy)	-0.022***	-0.055***	-0.090***	-0.099***	-0.100***	-0.107***	-0.106***	-0.103***
Relative productivity (2019)	-0.007*	-0.008	-0.012**	-0.012**	-0.020***	-0.021***	-0.017**	-0.021***
Relative K/L ratio (2019)	-0.0005	-0.003**	-0.006***	-0.005***	-0.007***	-0.008***	-0.007***	-0.007***
Log of average gross wage (2019)	-0.00181	-0.00868	-0.00487	-0.00275	-0.014***	-0.00829	-0.0146	-0.0155*
Log of employment (2019)	-0.005**	-0.006*	-0.011***	-0.013***	-0.012***	-0.013***	-0.014***	-0.011**
Birth year	0.0006**	0.0019***	0.0024***	0.0028***	0.0030***	0.0033***	0.0030***	0.0039***
Merchandise exports (2019)	-0.00267	-0.00938	-0.018***	0.00174	-0.00930	-0.00993	-0.00561	-0.00521
Foreign ownership, $\geq$ 50% (2019)	0.0108	0.0184	0.0296*	0.0296	0.0375*	0.0389*	0.0457**	0.0506**
State ownership, $\geq$ 50% (2019)	-0.00295	-0.0233**	-0.038***	-0.044***	-0.0037**	-0.051***	-0.041**	-0.054***
R^2	0.0248	0.0595	0.0967	0.101	0.107	0.111	0.0999	0.0993
Number of observations	2'842	2'849	2'850	2'849	2'848	2'850	2'848	2'849

Sources: SRS and CSB of Latvia, own calculations
Note: * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

Table A.6: DiD regression results for employment growth rates using the subsample of active firms (intensive margin)

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (dummy)	0.0230***	0.0595***	0.112***	0.146***	0.148***	0.135***	0.109***	0.0837***
Relative productivity (2019)	0.0034	0.0108**	0.0244***	0.0262***	0.0262***	0.0261***	0.0325***	0.0253**
Relative K/L ratio (2019)	0.0003	0.0006	0.0002	0.0032	0.0049**	0.0066***	0.0055**	0.0079***
Log of average gross wage (2019)	0.0093*	0.0080	0.0130	0.0060	-0.0026	-0.0058	-0.0018	-0.0049
Log of employment (2019)	-0.006***	-0.020***	-0.033***	-0.036***	-0.028***	-0.020***	-0.021***	-0.032***
Birth year	-0.001***	-0.0031*	-0.004***	-0.004***	-0.003***	-0.0017**	-0.0022**	-0.0020**
Merchandise exports (2019)	0.00343	0.0401***	0.0468***	0.0469***	0.0492***	0.0464***	0.0254	0.0522***
Foreign ownership, $\geq$ 50% (2019)	-0.028**	-0.0016	0.0167	0.0304	0.0221	0.00614	0.00967	0.0182
State ownership, $\geq$ 50% (2019)	0.0437***	0.174***	0.269***	0.304***	0.236***	0.0252***	0.0246***	0.206***
R^2	0.0205	0.0477	0.0801	0.0920	0.0772	0.0601	0.0447	0.0377
Number of observations	2'806	2'768	2'719	2'702	2'681	2'671	2'662	2'644

Source: SRS and CSB of Latvia, own calculations
Note: Only the firms with positive employment in the respective month were included in the regression.
* p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

Table A.7: DiD regression results for job creation (hiring) rates

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (dummy)	-0.00316	-0.019***	-0.040***	-0.064***	-0.077***	-0.084***	-0.091***	-0.098***
Relative productivity (2019)	-0.00072	-0.00334	-0.00113	0.00125	0.00210	-0.00292	-0.00062	-0.00346
Relative K/L ratio (2019)	0.00041	0.00140*	0.00221**	0.00311**	0.00444**	0.00521**	0.00486*	0.00372
Log of average gross wage (2019)	0.00485	0.00928*	0.0108	0.00835	0.00652	0.00933	0.00492	-0.00040
Log of employment (2019)	0.00722***	0.00471**	0.00721**	0.00938*	0.0203***	0.0268***	0.0352***	0.0429***
Birth year	0.0005**	0.0006*	0.0010**	0.0019***	0.0032***	0.0046***	0.0055***	0.0070***
Merchandise exports (2019)	-0.010**	-0.013*	-0.030***	-0.048***	-0.053***	-0.065***	-0.079***	-0.088***
Foreign ownership, $\geq$ 50% (2019)	-0.008*	-0.008	-0.021*	-0.024	-0.058***	-0.084***	-0.103***	-0.107***
State ownership, $\geq$ 50% (2019)	-0.045***	-0.048***	-0.098***	-0.148***	-0.245***	-0.321***	-0.394***	-0.456***
R^2	0.0202	0.0129	0.0189	0.0272	0.0333	0.0352	0.0382	0.0374
Number of observations	2'842	2'849	2'850	2'849	2'848	2'850	2'848	2'849

Sources: SRS and CSB of Latvia, own calculations
Note: * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

Table A.8: DiD regression results for job destruction (separation) rates

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (dummy)	-0.070***	-0.182***	-0.315***	-0.386***	-0.402***	-0.411***	-0.391***	-0.367***
Relative productivity (2019)	-0.018**	-0.029***	-0.046***	-0.058***	-0.059***	-0.065***	-0.061***	-0.064***
Relative K/L ratio (2019)	-0.0008	-0.005*	-0.010***	-0.009**	-0.013***	-0.016***	-0.013***	-0.016***
Log of average gross wage (2019)	-0.0080	-0.0150	-0.0106	-0.0027	-0.0207	-0.0017	-0.0213	-0.0256
Log of employment (2019)	0.00321	0.0134**	0.0184**	0.0199*	0.0250**	0.0224*	0.0277**	0.0507***
Birth year	0.0029***	0.0054***	0.0089***	0.0102***	0.0101***	0.0100***	0.0125***	0.0160***
Merchandise exports (2019)	-0.018**	-0.070***	-0.109***	-0.091***	-0.119***	-0.128***	-0.114***	-0.148***
Foreign ownership, $\geq$ 50% (2019)	0.0406*	0.0281	0.0158	-0.00057	-0.0117	-0.0192	-0.0295	-0.0311
State ownership, $\geq$ 50% (2019)	-0.094***	-0.264***	-0.430***	-0.523***	-0.541***	-0.591	-0.627	-0.749***
R^2	0.0386	0.0934	0.143	0.167	0.163	0.152	0.136	0.126
Number of observations	2'842	2'849	2'850	2'849	2'848	2'850	2'848	2'849

Sources: SRS and CSB of Latvia, own calculations
Note: * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

Table A.9: Quality of matching, five neighbours and 0.001 caliper

Variable	Unmatched			Matched		
	Treated	Control	p-value	Treated	Control	p-value
Changes in FTE (Mar-Jun'2020 to 2019)	-1.006	-0.186	0.000	-0.843	-0.868	0.342
Changes in FTE (Mar-Jun'2020 to Feb'2020)	-0.984	-0.109	0.000	-0.809	-0.820	0.661
Relative labour productivity (2019)	0.348	0.0605	0.000	0.265	0.204	0.105
Relative K/L ratio (2019)	1.493	0.766	0.000	1.225	1.151	0.632
Log of average gross wage (2019)	6.299	5.967	0.000	6.188	6.138	0.139
Log of employment (2019)	1.941	1.195	0.000	1.698	1.740	0.412
Changes in employment (Feb'2020 to 2019)	-0.0413	-0.0123	0.000	-0.0371	-0.0436	0.551
Birth year	2006.9	2007.8	0.000	2007.3	2007.1	0.595
Merchandise exports (2019, dummy)	0.109	0.0908	0.005	0.0763	0.0781	0.871
Foreign ownership, $\geq$ 50% (dummy)	0.0995	0.0750	0.000	0.0831	0.0893	0.591
State ownership, $\geq$ 50% (dummy)	0.00047	0.00464	0.005	0.00046	0.00067	0.463

Sources: SRS and CSB of Latvia, own calculations.
Notes: t-test for mean values of treated vs control firms. 1180 on support, 951 off support, 2878 controls.

Table A.10: Quality of matching, 0.01 caliper and two neighbours

Variable	Unmatched			Matched		
	Treated	Control	p-value	Treated	Control	p-value
Changes in FTE (Mar-Jun'2020 to 2019)	-1.006	-0.186	0.000	-0.977	-1.077	0.000
Changes in FTE (Mar-Jun'2020 to Feb'2020)	-0.984	-0.109	0.000	-0.954	-1.029	0.000
Relative labour productivity (2019)	0.348	0.0605	0.000	0.315	0.202	0.000
Relative K/L ratio (2019)	1.493	0.766	0.000	1.420	0.998	0.001
Log of average gross wage (2019)	6.299	5.967	0.000	6.254	6.151	0.000
Log of employment (2019)	1.941	1.195	0.000	1.844	1.678	0.000
Changes in employment (Feb'2020 to 2019)	-0.0413	-0.0123	0.000	-0.0391	-0.0456	0.451
Birth year	2006.9	2007.8	0.000	2007.2	2007.6	0.093
Merchandise exports (2019, dummy)	0.109	0.0908	0.005	0.0849	0.0744	0.232
Foreign ownership, $\geq$ 50% (dummy)	0.0995	0.0750	0.000	0.0875	0.0960	0.368
State ownership, $\geq$ 50% (dummy)	0.00047	0.00464	0.005	0.00052	0.00027	0.691

Sources: SRS and CSB of Latvia, own calculations.
Notes: t-test for mean values of treated vs control firms. 1908 on support, 223 off support, 2371 controls.

Table A.11: DiD regression results for employment growth rates, participation proxied by the share of participating employees

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (share of employees)	0.119***	0.303***	0.518***	0.596***	0.592***	0.583***	0.532***	0.458***
Relative productivity (2019)	0.0174**	0.0251**	0.0439***	0.0590***	0.0606***	0.0615***	0.0608***	0.0607***
Relative K/L ratio (2019)	0.00125	0.00626**	0.0121***	0.0120***	0.0174***	0.0215***	0.0178***	0.0201***
Log of average gross wage (2019)	0.0126	0.0236*	0.0204	0.00989	0.0261	0.00982	0.0252	0.0242
Log of employment (2019)	0.00614	-0.00318	-0.00173	0.000185	0.00593	0.0146	0.0168*	-0.0001
Birth year	-0.002***	-0.005***	-0.007***	-0.008***	-0.006***	-0.005***	-0.006***	-0.009***
Merchandise exports (2019)	0.0101	0.0613***	0.0850***	0.0498*	0.0728***	0.0706**	0.0423	0.0654**
Foreign ownership, $\geq$ 50% (2019)	-0.046**	-0.0292	-0.0254	-0.00930	-0.0325	-0.0513	-0.0609	-0.0653
State ownership, $\geq$ 50% (2019)	0.0385	0.192***	0.293***	0.327***	0.245***	0.218***	0.186***	0.247***
R^2	0.0350	0.0878	0.141	0.155	0.149	0.141	0.119	0.104
Number of observations	2'842	2'849	2'850	2'849	2'848	2'850	2'848	2'849

Sources: SRS and CSB of Latvia, own calculations
Notes: * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

Table A.12: DiD regression results for employment growth rates, participation proxied by the share of wage bill paid out to participating employees

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (share of wage)	0.0914***	0.286***	0.534***	0.606***	0.577***	0.567***	0.510***	0.419***
Relative productivity (2019)	0.0186**	0.0279**	0.0486***	0.0644***	0.0660***	0.0668***	0.0657***	0.0650***
Relative K/L ratio (2019)	0.00126	0.00630**	0.0122***	0.0121***	0.0175***	0.0216***	0.0179***	0.0202***
Log of average gross wage (2019)	0.0146	0.0293**	0.0308*	0.0218	0.0376**	0.0211	0.0354*	0.0328
Log of employment (2019)	0.00518	-0.00451	-0.00301	-0.00144	0.00377	0.0124	0.0146	-0.00235
Birth year	-0.002***	-0.007***	-0.009***	-0.009***	-0.009***	-0.008***	-0.008***	-0.009***
Merchandise exports (2019)	0.0108	0.0645***	0.0915***	0.0571**	0.0795***	0.0772***	0.0482	0.0699**
Foreign ownership, $\geq$ 50% (2019)	-0.046**	-0.0260	-0.0181	-0.00128	-0.0255	-0.0444	-0.0549	-0.0608
State ownership, $\geq$ 50% (2019)	0.0280*	0.171***	0.263***	0.293***	0.208***	0.181***	0.151***	0.215***
R^2	0.0263	0.0734	0.125	0.135	0.126	0.121	0.101	0.0884
Number of observations	2'842	2'849	2'850	2'849	2'848	2'850	2'848	2'849

Sources: SRS and CSB of Latvia, own calculations.
Note: * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.

Table A.13: DiD regression results for employment growth rates, fulfilling criteria for three months in a row

	Mar'2020	Apr'2020	May'2020	Jun'2020	Jul'2020	Aug'2020	Sep'2020	Oct'2020
JRS (dummy)	0.0989***	0.191***	0.315***	0.363***	0.363***	0.349***	0.311***	0.267***
Relative productivity (2019)	0.0000142	0.00519	0.0109	0.0357*	0.0259	0.0233	0.0349	0.0376
Relative K/L ratio (2019)	0.00714**	0.0133***	0.0250***	0.0223***	0.0262***	0.0288***	0.0190***	0.0225***
Log of average wage (2019)	0.0405**	0.0460**	0.0271	0.0173	0.0244	0.0154	0.00223	-0.00496
Log of employment (2019)	-0.00225	-0.00555	-0.00156	-0.000708	0.00898	0.0118	0.0305**	0.0110
Birth year	-0.004***	-0.007***	-0.008***	-0.010***	-0.008***	-0.008***	-0.008***	-0.010***
Merchandise exports (2019)	-0.0119	0.0602***	0.106***	0.0815**	0.110***	0.0937**	0.0941**	0.0997**
Foreign ownership, $\geq$ 50% (2019)	-0.0232	-0.0335	-0.0425	-0.0609	-0.0988*	-0.100*	-0.102*	-0.0852
R^2	0.0635	0.106	0.169	0.190	0.183	0.170	0.130	0.111
Number of observations	1'585	1'586	1'587	1'585	1'587	1'587	1'587	1'585

Sources: SRS and CSB of Latvia, own calculations
Notes: JRS=1 if on average at least 25% of workers covered by JRS in all months between April'2020 and June'2020, =0 if no participation in JRS. Firms with participation in JRS below the abovementioned threshold were excluded from the sample. * p-value< 0.1, ** p-value< 0.05, *** p-value< 0.01.