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Were jobs saved at the cost of productivity in the Covid-19 crisis?

Jaanika Meriküll and Alari Paulus*

Abstract

Economic recessions can boost the productivity-enhancing reallocation of jobs, yet the Covid-19 crisis has provided limited and mixed evidence of that. The paper studies the link between productivity and reallocation and investigates the role of job retention schemes in it, using a rich administrative dataset for Estonia that covers the whole population of firms from 2004 to 2020. We find persistent evidence for the reallocation of jobs towards more productive sectors and firms. However, the within-sector reallocation was surprisingly unresponsive to productivity in the Covid-19 crisis, in sharp contrast to the experience in the previous major crisis, the Great Recession. We show that a generous job retention scheme supressed the acceleration of within-industry reallocation towards more productive firms, which had negative consequences for aggregate productivity during Covid-19. These estimates appear sufficiently large to imply that there are negative overall welfare effects that offset the positive employment effect.

JEL Codes: J62, D24, J68, D61

Keywords: job reallocation, productivity, Covid-19, cleansing effect, firm exit and entry, job retention scheme

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Non-technical summary

The debate over whether economic recessions enhance opportunities for reallocating resources from less productive firms to more productive ones has attracted substantial attention since the writings of Schumpeter. A number of studies show that recessions speed up the reallocation of labour resources towards high-productivity firms, but there are also papers that find no evidence for this relationship. The outbreak of the Covid-19 pandemic started another chapter in the literature on productivity-enhancing reallocation, and one point that particularly stands out for the Covid-19 crisis is the much wider reliance of governments on job retention schemes to support the preservation of jobs.

We contribute to the literature by addressing the research questions of whether Covid-19 accelerated the reallocation of jobs from low-productivity firms to high-productivity firms, and whether generous job retention schemes may have restrained this reallocation. We also gauge the welfare consequences of job retention schemes with a simple evaluation exercise that brings together the effects on employment and productivity-enhancing reallocation.

The paper uses rich firm-level administrative data for Estonia in 2004–2020. We study job reallocation, structural changes and allocative efficiency at the aggregate and industry level, and the link between firm productivity and job growth at the level of the firm. We also estimate whether the generous job retention scheme that was introduced in 2020 muted the link between productivity and reallocation during the Covid-19 crisis, and what the effect of this policy was on productivity and jobs. While one job in five was supported by the job retention scheme in our sample country during the Covid-19 pandemic, there was no such support available in the previous major crisis, the Great Recession.

We find that structural changes towards high-productivity industries typically make a positive contribution to aggregate productivity, and notably so during major crisis episodes. However, the within-industry reallocation of jobs from low-productivity firms to high-productivity firms did not speed up during the Covid-19 crisis, unlike in the Great Recession. We find that the job retention scheme resulted in a weaker link between firm productivity and job growth during Covid-19. The more the industry relied on the job retention scheme, the less pronounced the productivity-enhancing reallocation was in the recession. Our counterfactual estimates indicate that if this link between productivity and reallocation had not been supressed, aggregate productivity would have been up to 11% higher in 2020.

It can be expected that the unemployment rate would also have been higher without this policy, at least partly counterbalancing the negative effect on productivity. To understand the welfare implications of the policy, we carry out a simple assessment of the average productivity of economically active individuals, including the unemployed with zero productivity. For this exercise, we also need to estimate the effect of the job retention scheme on jobs, and to do so we use matching techniques and create the control group from firms that were as severely hit by the crisis than the firms that received the support. Our finding is that the job retention scheme saved about 14,000–26,000 jobs in 2020, as roughly one worker in five who participated in the scheme would have lost their job without it, and the unemployment rate would have increased by 2–4 percentage points in 2020.

Weighing the negative effect on productivity against the positive effect on jobs reveals a delicate balancing act between saving jobs and fostering productivity. Our preferred approach suggests there were some welfare losses from the scheme as the negative effect on productivity exceeds the positive effect on jobs.

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1. Introduction

Whether economic crises create more opportunities to reallocate resources from less productive firms to more productive ones has been much discussed since Schumpeter first wrote about it. Caballero and Hammour (1994) demonstrated that job destruction is more cyclical than job creation, and as job destruction happens more in recessions and tends to be concentrated in low-productivity firms, recessions are typically productivity-enhancing and have a cleansing effect. The opportunity costs of long-term, productivity-enhancing activities are lower in recessions than those of short-term investment, and this makes long-term investment countercyclical (Aghion et al., 2005).

Several factors can suppress this process of productivity enhancement though. The reallocation of resources from less productive firms to more productive ones might be hampered by frictions in the credit market (Aghion et al., 2005; Barlevy, 2003), or by public support schemes that are intended to help preserve jobs during crises (Cooper et al., 2017; Giupponi and Landais, 2020). The cleansing effect of recessions can be offset if the quality of the job matches that are made during a crisis is poorer, an effect called sullying (Barlevy, 2002), or by the scarring effects on young firms that are more likely to exit in the crisis without reaching their full potential (Ouyang, 2009). These multiple and possibly opposite effects make determining the net outcome for productivity largely a matter of empirical investigation.

We study the link between firm productivity and job reallocation in Estonia and contribute to two lines of research. First, we extend the literature on the effects of economic crises on firm productivity. Our main interest is in the Covid-19 crisis, which mainly took place in the first half of 2020 and for which limited data are available so far, though our sample period also covers another major crisis, the Great Recession, which provides a useful historical comparison and perspective. Second, we contribute to the literature on how job retention schemes affect productivity. Our key research question is whether jobs were saved at the cost of productivity in the Covid-19 crisis.

There is some empirical evidence that clearly supports the hypothesis that recessions enhance productivity (Dias and Robalo Marques, 2021; Foster et al., 2016; Garcia-Louzao and Tarasonis, 2021). There are equally studies that could not find any support for it (Carreira and Teixeira, 2016; Domini and Moschella, 2022; Hallward-Driemeier and Rijkers, 2013; Mina and Santoleri, 2021). There is also evidence that the cleansing effect was more muted in the Great Recession than in previous recessions (Bartelsman et al., 2019; Foster et al., 2016), mainly because of the large disruptions that were caused to international trade and the widespread credit constraints (Bartelsman et al., 2019; Domini and Moschella, 2022). The cleansing effect is weaker when exporters, which generally have higher productivity, are more adversely affected in the crisis than non-exporters are, and when credit constraints prevent the most productive firms from growing.

The outbreak of the Covid-19 pandemic started another chapter in the literature on the productivity-enhancing reallocation of jobs. The adverse effects of the Covid-19 crisis have been concentrated in low-productivity services like hotels and restaurants, and arts and entertainment, and the reallocation of resources from these sectors has contributed positively to aggregate productivity, at least in the short run (Bloom et al., 2020; Garnadt et al., 2021; Lopez-Garcia and Szörfi, 2021). The evidence for productivity and within-sector reallocation of jobs in the Covid-19 pandemic has, however, been mixed so far, the link strengthened in Australia and the UK but weakened in New Zealand (Andrews et al., 2021a, 2021b). There is also evidence that the link between firm productivity and firm exit weakened in Portugal during the Covid-19 crisis (Kozeniauskas et al., 2022). We extend the literature by providing estimates of how the Covid-19 pandemic affected productivity, using extensive and high-quality administrative firm-level data from the Estonian Business Register for the period from 2004 to 2020. The empirical papers written so far have had limited data at their disposal, as they have not been able to observe productivity over time and have not tested the cleansing hypothesis in a panel setting.

There is ample research on how crisis policy measures such as job retention schemes impact employment and labour hoarding, while much less is known about how they impact productivity. A probable reason why productivity-enhancing reallocation occurred in a mixed way in the Covid-19 pandemic is the generosity of job retention schemes (Andrews et al., 2021a; Garnadt et al., 2021; Kozeniauskas et al., 2022). The number of people supported through such schemes has generally been larger than the number of jobs that have been saved (Boeri and Bruecker, 2011), though it has been shown that targeting the firms that are most severely hit can be an effective way of saving jobs (Cahuc et al., 2021; Kopp and Siegenthaler, 2021). Saving jobs may have come at the cost of lower allocative efficiency and reduced aggregate productivity in the medium term (Cooper et al., 2017; Giupponi and Landais, 2020), decreasing welfare gains and potentially even causing overall welfare losses. What further matters for efficiency is how long the support lasts, as the longer it lasts, the more distortive it becomes for productivity (Andrews et al., 2021b). Reducing productivity-enhancing reallocation can make a substantial negative contribution to aggregate productivity (Andrews and Hansell, 2021; Decker et al., 2020).

Job retention schemes have been much more widely used during the Covid-19 pandemic than they were in the Great Recession (Scarpetta et al., 2020), which makes it even more topical to ask whether job preservation was achieved at the cost of productivity. The findings so far are inconclusive. It has been found that less productive firms were more likely to receive support, suggesting that the support muted productivity-enhancing reallocation (Harasztosi et al., 2022; Kozeniauskas et al., 2022), but it has equally been found that the relationship may be either negative or positive depending on the country (Bighelli et al., 2022), and that the relationship turns from positive to negative over the duration of the retention scheme (Andrews and Hansell, 2021). None of the studies so far have used firm-level data on total factor productivity (TFP) from the Covid-19 period, have directly linked subsidies at the firm level to firm exits, or have controlled for the role of other subsidies. Again, our contribution benefits from access to an up–to-date and extensive dataset, allowing a better design for the research.

We apply conventional decomposition methods at the industry and firm levels, and the approach of Foster et al. (2016) to provide conditional estimates of the cleansing effect within industries. We further estimate how the job retention scheme that was introduced in Estonia during the Covid-19 pandemic affected productivity. This is done by contrasting the industries that were more exposed to the retention scheme with those that were less exposed. We derive a counterfactual distribution of firm productivity without any retention scheme by using the approach of Decker (2020), which was also applied by Andrews et al. (2021b) in a similar exercise to ours. To assess how the job retention scheme affects employment, we conduct a matching analysis with the subgroup of potentially eligible firms only, where those that participated in the scheme form the treatment group, and those that did not participate but experienced a similar adverse shock are used as the control group. We follow a similar approach to Kopp and Siegenthaler (2021), who performed policy evaluation on the subgroup of firms that had applied for support.

We focus on job retention schemes here, but we also control for the use of other types of support that were provided by the public sector to ensure the liquidity and survival of firms during the Covid-19 pandemic. Job retention schemes were the main form of policy support that was given to businesses during Covid-19 in Europe (Harasztosi et al., 2022), and this was also the case in Estonia. Liquidity support and direct non-refundable subsidies were also widely provided though, and so not controlling for these could cause an omitted variable bias. A further contribution is that we estimate the welfare effects of the recession, which account not only for changes in aggregate productivity but also for changes in unemployment.

We find persistent evidence of jobs being reallocated towards more productive sectors and firms. The within-industry reallocation towards more productive firms accelerated during the Great Recession at both the intensive and extensive margins, but this did not happen during the Covid-19 crisis. The Covid-19 pandemic led to the introduction of a generous job retention scheme in our sample country, and we show that this muted the link between firm productivity and job reallocation. Our counterfactual estimates of the distribution of employment without the policy in 2020, or without the muted impact on productivity-enhancing reallocation in the industries exposed, suggest that the scheme reduced aggregate productivity by up to 11%. We also estimate that the job retention scheme saved about 14,000–26,000 jobs in 2020, which roughly corresponds to one job in five saved among all the workers taking up the scheme in our sample, and the unemployment rate would have been 2–4 percentage points higher without the scheme. These positive employment effects are, however, not sufficiently large to offset the negative productivity effects in our preferred model specifications.

The paper is organised as follows. The next section briefly describes the macroeconomic context of the Covid-19 crisis, contrasting our sample country with the EU, and explains the key characteristics of the job retention scheme in Estonia. Section 3 describes the data and defines key concepts. The fourth section provides a descriptive analysis of aggregate job flows and productivity growth, and Section 5 continues with econometric firm-level estimates of productivity and employment reallocation. Section 6 complements that by evaluating the effects of the job retention scheme on productivity growth and gauges the overall welfare effects. The final section summarises the findings.

2. The Covid-19 pandemic and policy support measures

The Covid-19 pandemic caused an economic shock in Europe that was similar in magnitude to that in the previous major crisis in the late 2000s, as shown in Figure 1. The figure also compares our sample country with the EU-27, as defined from 2020, and shows that GDP growth in Estonia had very similar dynamics in the Covid-19 crisis to those in the whole EU-27, while the unemployment rate increased more in Estonia and closed the gap to the EU-27 level.

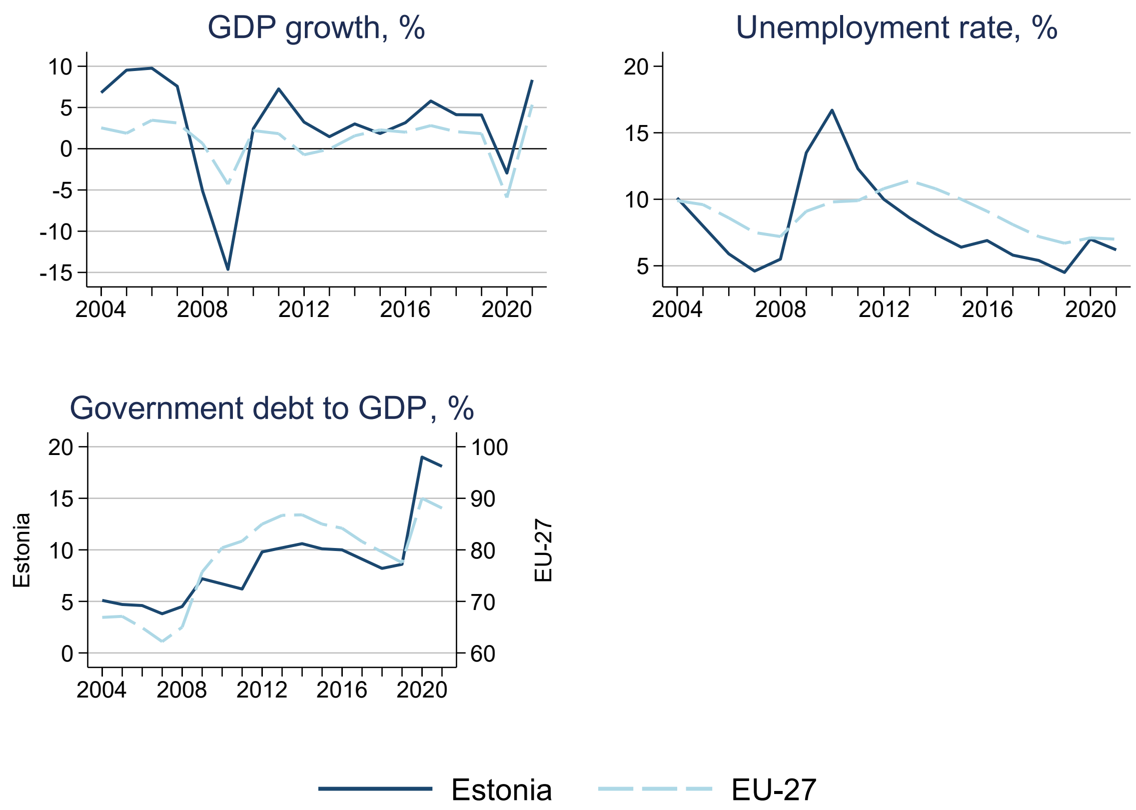

Figure 1. Key macro-economic indicators for Estonia and the EU-27, 2004–2021
Source: Eurostat series (nama_10_gdp, une_rt_a, une_rt_a_h, gov_10dd_edpt).

The Covid-19 shock has been very different from previous crises in all aspects apart from the size of economic contraction. It started as a health crisis that was not driven by economic fundamentals, and there has since been a smaller decline in corporate profits and investment, and a smaller increase in lending spreads and firm exits than those that were seen in the Great Recession (Harasztosi et al., 2022). One reason for that the Covid-19 pandemic has had milder negative effects on firms is its relatively random nature. The economic shock caused by Covid-19 has been labelled an exogenous shock or a random shock to reflect the weaker link between the idiosyncratic performance of firms and how much they were affected by the shock (Andrews et al., 2021a, 2021b). The measures taken to limit contacts between people meant that Covid-19 affected the services sector much more than other industries and the negative impact was more concentrated in certain industries than it was during other recessions.

Governments have also played a greater role in supporting economic recovery this time, and the ratio of general government gross debt to GDP increased by roughly 10 percentage points both in 2009 and 2020, see the last subplot of Figure 1 (note there are dual vertical axes). Again, our sample country provides a representative case for the reaction of the public sector to Covid-19. During the Great Recession there was no direct public sector support in Estonia to protect jobs and adjustments took place through high unemployment and internal devaluation, but the reaction to the Covid-19 pandemic has been different, as a temporary support scheme of the German Kurzarbeit style was introduced for the first time to protect jobs in 2020. Many countries had similar experiences, and Scarpetta et al. (2020) estimate that ten times as many jobs were supported in the OECD countries in the Covid-19 crisis as in the Great Recession. Another reason why Covid-19 had a milder negative impact on firms is consequently the stronger public sector focus on saving jobs than in earlier recessions.

On average a quarter of European workers participated in short-term job protection schemes in 2020 (Müller and Schulten, 2020), meaning they received or applied for support. The uptake varied significantly across countries, from 48% in Switzerland to 3% in Poland (Müller and Schulten, 2020). One worker in five, or 21%, received support in Estonia, making 137,000 workers out of 654,000, which corresponds to the average level in Europe and places Estonia in the same group as countries like Germany, Spain, the UK and the Netherlands.

The inclusiveness and generosity of the job retention benefit in Estonia also corresponded to the average level in other countries (Müller and Schulten, 2020). The benefits were initially paid from March to June 2020, and to be eligible for them, applicants had to meet two of three criteria, which were a decline of at least 30% in turnover, cuts in working hours for at least 30% of workers, or a decline of at least 30% in wages for at least 30% of workers. The reference time was the same month a year earlier. The size of the benefit was 70% of the average monthly wage of the employee, capped at 1,000 euros, and the employer was required to continue paying a salary of at least 150 euros. The first eligibility criterion was tightened from a 30% decline to a 50% decline in turnover and the maximum amount of the benefit was reduced from 1,000 euros to 800 euros in June 2020. If the worker was laid off in the month when the benefit was received, the employer had to pay it back.[1]

The job retention benefit in Estonia accounted for 3.6% of the total wage cost of the private non-financial sector in 2020 and 28% of the workers in that sector received the support.[2] The support was concentrated in the industries that were affected most, and the wage support provided as much as 12% of the total amount spent on wages in the hotels and restaurants sector in 2020. Assessments using fiscal microsimulation suggest that the Covid-19 crisis affected the incomes of low-wage workers more than those of high-wage workers, and that the benefit played an important role in keeping the poverty rate stable (Almeida et al., 2021). That analysis also estimated that there would have been almost twice as many job losses in Estonia without the benefit.

The job retention scheme adds up to roughly half of all the Covid-19 related financial support given to enterprises in Estonia in 2020, totalling 238 million euros for private non-financial corporations, while other types of support to enterprises only amounted to 48 million euros. In addition, liquidity support of 306 million euros was allocated through the state-owned financial intermediation institution (Kredex), mostly in the form of loan guarantees or working capital loans.[3] The Covid-19 related discretionary fiscal support reached 5% of GDP in Estonia in 2020, which was slightly above the EU average (Almeida et al., 2021).

3. Data

3.1 Data sources

We use administrative firm-level data for Estonia that cover the period from 2004 to 2020. Data from five administrative registers were merged using firm ID numbers: annual business reports from the Business Register in 2004–2020; quarterly declared labour taxes from the Tax and Customs Board in 2004–2021; temporary wage subsidies from the Unemployment Insurance Fund in 2020; direct financial support to firms from Enterprise Estonia in 2020; and liquidity support from the state-owned financial intermediation institution Kredex in 2020. The sample consists of the whole private sector apart from financial intermediation. The unit of analysis is the firm and the data are organised with annual frequency.

The Business Register includes key financial variables to calculate value added and estimate total factor productivity (TFP), and provides further background information on the firms. Our

[1] The official name of the benefit scheme is the temporary subsidy programme (töötasu hüvitis) and it is administrated by the Unemployment Insurance Fund, see more at https://www.tootukassa.ee/et/uudised/tootasu-huvitise-taotluste-vastuvotmine-algab-6-aprillil. Another scheme, the salary grant (töötasu toetus), that was introduced in 2021 is not covered in our analysis as our main data source provides information up to 2020 at this point.
[2] Authors' calculations using administrative data.
[3] To derive these numbers, we excluded public sector institutions like museums and theatres from the recipients.

definition of value added follows the one used in Bureau van Dijk's Orbis database, where it is constructed as the sum of the components: profits or losses (net income), corporate income tax, cost of employees, depreciation, and interest paid.[4] Our baseline measure of productivity is TFP, which is derived by applying the production function method and using material costs to address the endogeneity of inputs. The two-step procedure of Levinsohn and Petrin (2003) is applied, where value added is the dependent variable.[5] The capital stock is measured by the net book value of tangible fixed assets. Firm-level employment is given as the yearly average in full time equivalent units.

Declared labour taxes from the Tax and Customs Board provide a valuable supplement to the Business Register. We use the labour taxes to impute missing observations in wage costs and employment data. Not all the firms report the cost of employees in the Business Register, because firms can choose between two forms of income statement, and the cost of employees is only given in one of them. Employment data are also often unavailable in the Business Register, because they are reported as information supplementary to the main forms.

Data from the Tax and Customs Board cover all the firm-level wage transactions and can be used to substitute the missing observations for wage costs and employment in our primary data source. The main shortcoming of the data source is that it is not possible to derive employment in full-time equivalent units from it. However, as part-time work is not very common in Estonia, with only 12% of workers having done part-time work in 2020 (Eurostat series lfsa_eppga), the probable bias from this inconsistency is minor. After the imputations, our database covers more than 95% of the aggregate value added and more than 80% of the aggregate employment of non-financial corporations.

Further to this, we use tax information from the Tax and Customs Board to complement the Business Register, where a number of firms have not yet submitted their annual report for 2019 and 2020, the last years of the sample. The official deadline for submitting annual financial reports is six months after the end of the financial year, but at the end of 2021 there were still a lot of firms that had not submitted their reports for 2019 and 2020. Some firms submit their reports late and so their records show up in the register long after the deadline. However, it is critical for our research purposes that we disentangle delayed reports from firm exits at the end of the sample period and so we use the Tax and Customs Board data to establish whether or not the firm was still operating in 2020.

If the firm had not submitted its annual financial report for 2019 and 2020 to the Business Register by the end of 2021 but was shown by the Tax and Customs Board register to have had positive employment or turnover in 2021, we consider the firm not to have exited in 2019 or 2020, and so we impute the missing information. This increases the number of observations by

[4] See their user guide on page 20:
https://www2.bib.uni-mannheim.de/fileadmin/ub/pdf/Fachref/BWL/OrbisInternetUserGuide.pdf
[5] We apply *levpet* command for Stata by Petrin et al. (2004).

7% in 2020 and by 1% in 2019 in our dataset. That means that 1% of firms had no reports filed in either 2019 or 2020, but were operating in 2021 according to the Tax and Customs Board register. The missing values for firm employment and turnover are extrapolated using the growth rates of those variables from the Tax and Customs Board register. To extrapolate the missing values for value added, we use the growth rate of value added taxes; for wage costs we use the growth rate of employment and other direct taxes; and for material costs we use the growth rate of turnover from the same register.

Finally, data on temporary wage subsidies from the Unemployment Insurance Fund are used to control for whether the firm received support from the Covid-19 job retention scheme in 2020. These data show how many workers in each firm received the support and how large the total wage cost was that was subsidised. The direct support from Enterprise Estonia and Kredex are also available at the firm level.

The value added series is deflated using value added deflators from the national accounts and at the NACE 2-digit level for the majority of industries (Eurostat series NAMA_10_A64). Capital, material costs and turnover are deflated using the same source and the same level of disaggregation. The capital stock is deflated using the deflator of consumption of fixed capital, material costs are deflated using the intermediate consumption deflator, and turnover is deflated using the output deflator.

3.2 Measurement of job creation and destruction

To measure job creation and destruction, we use the methodology of Davis et al. (1996), which has become the standard in the literature. This approach derives firm-level employment growth relative to average employment over two periods, rather than the conventional growth rate measure that derives it in terms of the employment level in the previous period. The benefits of this method are that the employment growth rate varies within a definite range of between –2 and 2, and firm entries and firm exits can easily be incorporated into the job flow analysis. The job creation and destruction rates in sector j in period t are derived as:

$$G_{jt}^{+} = \sum_{i \in g^{+}} \left(\frac{Z_{ijt}}{Z_{jt}}\right) g_{ijt} \tag{1}$$

$$G_{jt}^{-} = \sum_{i \in g^{-}} \left(\frac{Z_{ijt}}{Z_{jt}}\right) \left|g_{ijt}\right| \tag{2}$$

The variable $g_{ijt} = (L_{ijt} - L_{ij,t-1})/[(L_{ijt} + L_{ij,t-1})/2]$ denotes the employment growth rate of firm i in period t, $Z_{ijt} = (L_{ijt} + L_{ij,t-1})/2$ and $Z_{jt} = (L_{jt} + L_{j,t-1})/2$ are the average employment in the firm and in the sector in period t, and L denotes employment in levels. The job creation rate in sector j in period t, G_{jt}^{+}, is the sum of employment-weighted employment growth rates that were positive in period t, g^{+}, while the job destruction rate, G_{jt}^{-}, is the sum of employment-weighted employment growth rates that were negative in the period t, g^{-}. The net employment growth, N_{jt}, can be decomposed as the difference between job creation and destruction rates, and, following the notation of Garcia-Louzao and Tarasonis (2021), further rearranged to highlight the role of firm entry and exit:

$$N_{jt} = G_{jt}^{+} - G_{jt}^{-}$$
$$= \sum_{i \in S^{+}} \left(\frac{z_{ijt}}{Z_{jt}}\right) g_{ijt} + \sum_{i \in E^{+}} \left(\frac{z_{ijt}}{Z_{jt}}\right) g_{ijt} + \sum_{i \in S^{-}} \left(\frac{z_{ijt}}{Z_{jt}}\right) g_{ijt} + \sum_{i \in E^{-}} \left(\frac{z_{ijt}}{Z_{jt}}\right) g_{ijt} \qquad (3)$$

where the variables S^{+} and S^{-} denote surviving firms with positive and negative employment growth rate, and E^{+} and E^{-} denote firm entries and firm exits.

Table 1 reports descriptive statistics of the variables that are used in our empirical analysis. All the estimations in Table 1 and in the conditional analysis are weighted so that larger firms get a higher weight, and the results are representative to the aggregate dynamics. The weight for each firm is calculated as the firm's average employment over the whole sample period. The average annual employment growth in the whole sample is 0.5%; the average relative productivity is close to zero by construction, explained in detail in Section 5.1; and the average regional unemployment growth at the county level is 2.4%. Note that as the estimates are weighted for employment, the employment at the firm does not show the average firm size, but the average size of the employer. The job retention scheme supported the wages of 28% of all non-financial private sector workers in 2020, which is higher than the country average of 21% as the public sector employment was not eligible for support. In total this support made up 3.6% of all the spending by the private non-financial sector on wages in 2020.

Table 1. Descriptive statistics of the variables used in the regression analysis, 2005–2020, n=478,345

	Mean	Std. dev.
Employment growth	0.005	0.292
Firm exit	0.018	0.134
Relative productivity	0.011	0.716
Unemployment growth	0.024	0.407
Employment	267.0	606.1
Young firm, age<5 years	0.149	0.356
Mature firm, age>=5 years	0.854	0.353
Share of private workers in the job retention scheme in 2020	0.279	0.411
Share of wage costs from the job retention scheme in 2020	0.036	0.062

Notes: Weighted by the firm's average employment over the whole sample period. The number of observations for the last two variables is 34,636 firms.
Source: Authors' calculations using administrative data.

4. Aggregate estimates

4.1 Job creation and destruction

We first consider the dynamics of aggregate employment over business cycles and derive job creation and destruction rates for the whole economy, as explained in the previous subsection. As Figure 2 shows, the job creation rate has been 2–3 percentage points higher than the job destruction rate in the past decade, meaning that the total net change in employment has been positive. The job destruction rate was much higher around the time of the Great Recession, while the job creation rate dropped dramatically at the peak of the recession in 2009. The latest sample year, 2020, reveals clear signs of another crisis, as the job creation rate decreases and the job destruction rate goes up, resulting in a decline of 3% in employment. However, the repercussions of Covid-19 for unemployment are clearly less severe than those felt during the crisis of 2009, when the employment declined by 16%. Figure A1 in Appendix A additionally compares job creation and destruction rates by NACE 1-digit industries for 2009 and 2020, and shows that the job destruction rate was highest in the real estate sector in 2009 and in the hotels and restaurants sector in 2020.

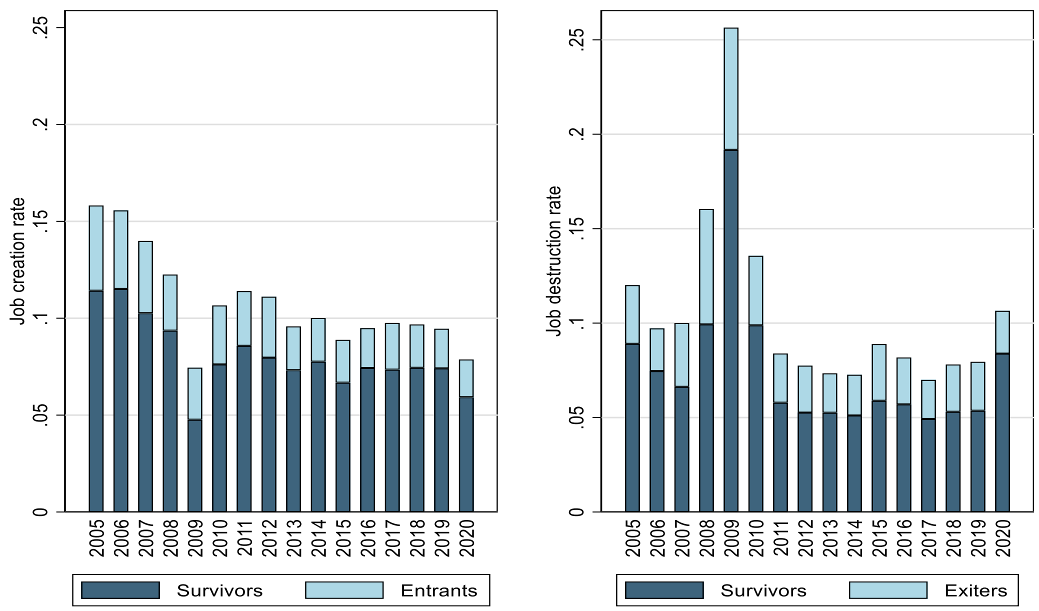

Figure 2. Aggregate job creation and destruction rates, 2005–2020

Notes: Weighted by the firm's average employment over the whole sample period.
Source: Authors' calculations using administrative data.

It is apparent that job destruction is more responsive to business cycles than job creation is, as predicted by the cleansing model of Caballero and Hammour (1994). Reallocation increased substantially during the Great Recession, but only did so slightly during the Covid-19 crisis. There seems to be less job reallocation in the Estonian labour market than inter-nationally, where job creation flows are above 15% for the same 2–3% of growth in aggregate employment (Foster et al., 2016; Haltiwanger et al., 2014). Estonia's job flows are very similar to those found for Lithuania (Garcia-Louzao and Tarasonis, 2021), which might reflect the characteristics of small labour markets. Firm dynamics also have a more muted role in the reallocation in our sample country. We find that 25–30% of job flows can be attributed to firm entries and exits, while it is usually around 30–40% internationally (Haltiwanger et al., 2014).

4.2 Productivity and reallocation within and between industries

A first look at the relationship between employment growth and productivity, found by plotting the average employment growth in year t across decile groups of total factor productivity in year $t-1$, reveals a positive correlation between them in every year throughout the sample period; see Figure A2 in Appendix A. The correlation appears to have become weaker over time with the Pearson *r* correlation coefficient ranging between 0.1 and 0.23 in 2005–2010 and between 0.03 and 0.11 in 2011–2020. There is also an indication that the correlation strengthened during the Covid-19 pandemic, though not as much as it did in the Great Recession, providing preliminary support to the hypothesis that recessions accelerate the reallocation of labour resources towards more productive firms.

However, this unconditional correlation can originate from either within-sector reallocation towards more productive firms, or reallocation between sectors because of structural changes. Given that the effects of Covid-19 have been concentrated in low-productivity services like hotels and restaurants[6], it is important to understand the source of this unconditional correlation. The stronger correlation between productivity and growth in 2020 may just reflect the forced closure of sectors that happened to have low productivity, and not a reallocation towards firms with better prospects.

To investigate this, we run two decomposition exercises. For the first, we decompose industry-level labour productivity growth into the parts that originate from within and between sectors, and for the second, we assess firm-level allocative efficiency within industries using the Olley-Pakes decomposition. The first decomposition is implemented as: $\Delta P_t = \sum_{j=1}^{J} s_{j,t-1} \times \Delta p_{j,t} + \sum_{j=1}^{J} \Delta s_{j,t} \times p_{j,t-1} + \sum_{j=1}^{J} \Delta s_{j,t} \times \Delta p_{j,t}$, where j refers to private non-financial sectors from $j = 1, \dots, 57$ at the NACE 2-digit level, ΔP_t denotes annual total change in real labour productivity in thousands of euros, s_{t-1} is the industry's employment share in total employment in the year $t-1$, and Δp_{jt} is the annual change in productivity in industry j. The first term captures the within-industry effect, showing how much of the productivity growth would have come from within-industry developments if the employment structure had been constant. The second term captures the between industry effect, or how much of the productivity growth

[6] The hotels and restaurants sector is usually taken as the reference group for firms with no surplus or zero pay premium that have the lowest productivity and pay the lowest wages; see e.g. Card et al. (2016).

would have come from structural changes if the productivity in each sector had been constant. The last term is the covariation effect, or a residual term, capturing how much of the productivity growth comes from fast-growing industries increasing their productivity faster.

The results using data from the national accounts are shown in Figure 3. Most productivity growth usually originates from developments within sectors, and this is also the case in our results, where it accounts for 70% of productivity growth over the sample period. The within-sector component is usually strongly cyclical, increasing in periods of fast growth and declining in recessions. The between-sector component for structural changes has mostly been positive and it has increased during crises. The Covid-19 crisis was no exception, as the within-sector effect was negative and dominant in 2020, while structural changes in sectors other than the low-productivity services that were hit the hardest made a positive contribution to productivity. Bloom et al. (2020), Lopez-Garcia and Szörfi (2021) and Garnadt et al. (2021) similarly find that the between-industry component made a positive contribution to the dynamics of productivity in the Covid-19 crisis.

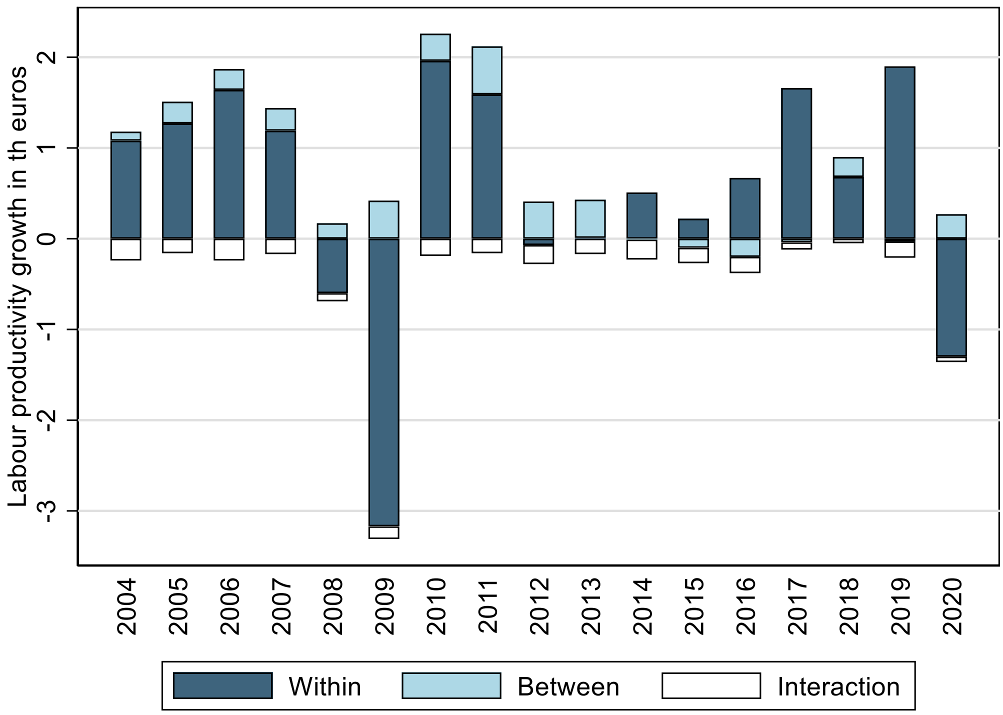

Figure 3. Labour productivity decomposition at the industry level, 2004–2020

Notes: Labour productivity is derived as value added per employee in real terms. We consider private sector industries without financial intermediation (n=57); the air transportation industry has been excluded because of its high volatility in real value added.
Source: Eurostat series (nama_10_a64).

We also apply the method of Olley and Pakes (1996) to decompose industry productivity P_t as: $P_t = \sum_{i=1}^{I} s_{i,t} \times p_{i,t} = \bar{p}_t + \sum_{i=1}^{I}(s_{i,t} - \bar{s}_t) \times (p_{i,t} - \bar{p}_t)$, where $s_{i,t}$ refers to the employment share of firm i at time t in the industry, and $p_{i,t}$ to the productivity of firm i at time t; while $\bar{p}_t$ refers to the unweighted average productivity in the industry at time t, and $\bar{s}_t$ to the unweighted average share of the employment of firm i in the total employment of the industry. The second term in the decomposition is called the Olley-Pakes gap and it shows how much industry productivity, meaning a weighted average of firm-level productivity, differs from the unweighted average productivity of firms in the industry. The Olley-Pakes gap reflects the covariation between firm size and productivity and the larger it is the more effectively resources are allocated within the industry. If productivity is logarithmic, as it is in our case, then the Olley-Pakes gap shows in log points how much higher or lower the industry's productivity is than the average as a result of the allocation of resources between firms.

We use firm-level information to perform this decomposition for each of 17 industries at the 1-digit NACE level, and the results show that there are notable differences in the role of within-sector allocative efficiency across industries; see Figure 4 and Figure A3 in Appendix A. There are industries where allocative efficiency makes a large contribution to the aggregate productivity of the industry, like electricity, transport, real estate, and arts and entertainment, while in other industries like mining, retail, information and communication, and health services, the contribution of allocative efficiency is small. The contribution of allocative efficiency to productivity in Estonia seems to be slightly below the OECD average; allocative efficiency explains around 17%, or 16 log points, of the total factor productivity in the Estonian manufacturing sector in 2004–2020 for example, while the OECD average for manufacturing was around 22–35%, or 20–30 log points, in 2005 (Andrews and Cingano, 2014).

There is also evidence that the allocative efficiency in manufacturing improved during and after the Great Recession and in the Covid-19 crisis, which indicates that the crises had a cleansing effect in this industry; allocative efficiency similarly increased during and after the Great Recession in such industries as transport, professional services and administration. Different dynamics can be observed for other industries though, and the contribution of allocative efficiency to productivity in hotels and restaurants vanishes in recessions and recovers after crises, as the Great Recession showed. These dynamics are also similar for the segments of hotels and restaurants separately, except that the covariation between firm productivity and firm size is stronger in the hotel segment than in the restaurant segment.

Larger hotels and restaurants, which are usually more productive, may be able to reduce their employment proportionally more in a crisis, and as a result the larger and more productive firms shrink more in crises and so the covariation between firm size and productivity weakens. It may also be that it is easier for smaller hotels and restaurants to adjust to changes in the market situation than it is for larger businesses. The effect of such changes in allocative efficiency is substantial, as allocative efficiency accounts for 21%, or 19 log points, of the productivity of hotels and restaurants in 2019 and drops to 4%, or 4 log points, in 2020. More than half of the decline in productivity in this industry is related to a drop in allocative efficiency in 2020. As hotels and restaurants was the industry hit most seriously in the crisis of Covid-19 and accounted for a third of the total job losses in 2020, this within-industry adjustment anomaly plays an important role in the cleansing effect of the Covid-19 crisis being weaker.

Figure 4. Olley-Pakes decomposition, 2004–2020

Notes: The stacked columns show the industry's weighted average TFP in logarithms. The subheadings refer to the NACE 1-digit industry.
Source: Authors' calculations using administrative data.

5. Firm-level estimates

5.1 Empirical specification

We follow Foster et al. (2016) to study the effect of productivity on job reallocation. The idea of their specification is simple and is that employment growth at a firm in period t is explained by the productivity of the firm relative to its industry average in the period $t-1$. This allows the effect of the *within-industry* dispersion of productivity on the reallocation of labour to be analysed. Resource reallocation is productivity-enhancing if firms that are relatively more productive in the industry grow faster. We estimate the following specification:

$$g_{it} = \beta_0 + \beta_1 rel\_TFP_{i,t-1} + \beta_2 Cycle_{ct} + \beta_3 rel\_TFP_{i,t-1} \times Cycle_{ct} + X'_{i,t-1}\theta + \tau_t + \varepsilon_{it} \tag{4}$$

The variable g_{it} denotes the employment growth in firm i measured in period t and is defined as in equations (1) and (2), with the two-period average used in the denominator instead of the lagged value. Relative TFP one year ago is denoted by rel_TFP_{it-1} and is measured as the deviation of the firm's TFP from its NACE 2-digit industry average, using the log difference between the firm's TFP and the industry average TFP. We expect β_1 to be positive so that more productive firms grow faster than less productive firms do. We also control for economic cycles and their interaction with the lagged relative TFP. Economic cycles are measured at the level of the 15 Estonian counties, indexed by c, and proxied by the growth in the registered unemployment rate[7], where the growth rate is again measured as in equations (1) and (2). We expect β_2 to be negative because higher unemployment growth in a region is related to lower employment growth for firms. The interaction term between the lagged productivity and the economic cycle captures the cleansing effect. If β_3 is positive, then recessions are productivity-enhancing, so that the reallocation towards more productive firms speeds up during economic downturns.

In addition to this, we control for other firm-level characteristics in period $t-1$, which are denoted by X'_{it-1}; these are the lagged logarithm of employment at the firm at $t-1$ and the fixed effects of the firm's NACE 2-digit level industry at $t-1$. Time fixed effects are captured by τ_t, and ε_{it} is an error term with conventional properties. The specification in (4) is estimated first for the employment growth of surviving firms, g_{it}, using a pooled OLS and fixed effects model, and then for the probability of firm entry and exit using a linear probability model.

The previous empirical literature has used various specifications to examine the cleansing effect of recessions; see Table A1 in Appendix A. The key differences between the specifications are whether firm fixed effects have been controlled for and whether the business cycle proxy is

[7] We prefer the registered unemployment rate from the Unemployment Insurance Fund instead of the unemployment rate calculated from the Labour Force Survey by Statistics Estonia, because the latter contains missing observations for smaller counties.

captured with a continuous variable or using year dummies for recessions. We base our baseline specification on Foster et al. (2016), where business cycles are proxied by regional unemployment growth and firm fixed effects are not controlled for, but we test the robustness of our findings with alternative specifications.

5.2 Surviving firms

Table 2 presents the results of the estimation in the style of Foster et al. (2016) as specified in equation (4). Our baseline estimates that do not control for firm fixed effects are presented in column (1), and the estimates with firm fixed effects in column (2). Both specifications demonstrate that productivity has a positive effect on reallocation and that this link strengthens in recessions, as the coefficients on Rel_TFP and its interaction with the business cycles are positive and statistically significant. An increase of one standard deviation (0.716) in relative TFP in year t, when unemployment growth is set at zero, corresponds to employment growth in year $t+1$ that is 3.7 percentage points higher (0.051×0.716). The effect is sizeable and similar in magnitude to the estimates in previous studies (Andrews et al., 2021a). The effect of productivity on reallocation becomes larger in recessions, as an increase of one standard deviation in relative TFP when unemployment growth has also increased by one standard deviation (0.407) corresponds to employment growth being 4.5 percentage points higher (0.051×0.716+0.028×0.716×0.407).

Table 2. TFP and the reallocation of employment among survivors, 2005–2020

Dependent variable: employment growth	(1) OLS estimates	(2) FE estimates
Rel_TFP_{t-1}	0.051^{***}	0.044^{***}
	(0.002)	(0.005)
$Cycle_t$	$–0.058^{*}$	–0.045
	(0.035)	(0.038)
$Rel\_TFP_{t-1} \times Cycle_t$	0.028^{***}	0.032^{***}
	(0.006)	(0.006)
$Log(employment)_{t-1}$	$–0.019^{***}$	$–0.198^{***}$
	(0.001)	(0.010)
Industry FE	yes	yes
Year FE	yes	yes
Firm FE	no	yes
No of obs.	478,345	478,345
R^2	0.063	0.183

Notes: Weighted by the firm's average employment over the whole sample period. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations using administrative data.

We test the robustness of the findings by using year fixed effects as an alternative measure for business cycles, omitting the local unemployment variable from equation (4) and interacting firm productivity with the year fixed effects instead. The advantage of this approach is that it allows the link between productivity and reallocation to change over years, so that we can test whether the reaction to Covid-19 has been different from that to the Great Recession. The results are presented in Figure 5. The link between productivity and reallocation increased in the Great Recession, but it did not react to Covid-19 in either the OLS or the fixed effects estimation. This demonstrates that the within-industry reallocation of jobs was different in the Covid-19 crisis, as the reallocation of resources from less productive firms to more productive ones within industries did not speed up. The hotels and restaurants sector alone cannot explain this effect, as the results are robust to the exclusion of this sector from the estimation sample. An important factor that affected the within-industry adjustments during the Covid-19 pandemic may have been the new and wide-scale job retention schemes, which we will analyse further below.

Lastly, we examine the robustness of our findings by estimating the specification (4) for each sample year separately. This approach is similar to that taken in the first studies on the cleansing effect during the Covid-19 crisis, where employment growth in the pandemic was explained by time-invariant productivity in 2018 or 2019 (Andrews et al., 2021a, 2021b). Table A2 in Appendix A presents the results and confirms the findings of Figure 5 that the productivity-reallocation sensitivity went up in the Great Recession, but not in the Covid-19 pandemic.

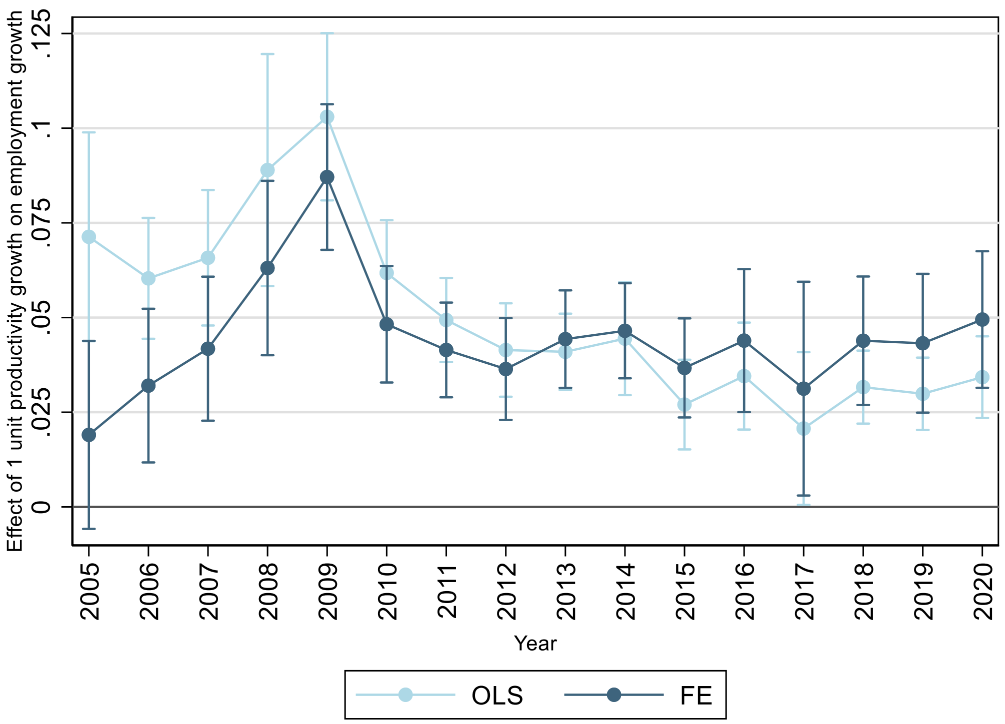

Figure 5. The effect of relative productivity on employment growth, survivors, 2005–2020

Notes: 90% confidence bounds shown.

Source: Authors’ calculations using administrative data.

We further seek to understand the mechanism behind the cleansing effect by focusing on credit frictions, foreign trade and firm age, and so by extending equation (4) with additional interaction variables. We split industries into two groups using the threshold of average external finance dependency and average export intensity, and the list of industries in these groups is reported in Table A5 in Appendix A. The industry-level definition of dependency on external finance and export intensity is preferred over the firm-level definition to avoid potential endogeneity issues.

Credit market frictions are considered to be a major factor in muting the reallocation of resources from firms with fewer prospects to ones with a better outlook in recessions (Aghion et al., 2005; Barlevy, 2003), and it has been shown that the tightness of credit markets was one reason why reallocation was weak in the Great Recession (Bartelsman et al., 2019; Domini and Moschella, 2022). Our results, which are reported in full detail in Table A3 in Appendix A, show that the productivity-enhancing effect is smaller for firms operating in industries where external finance is widely used than it is for firms operating in industries where external finance is less common. We also check for differences in the reallocation process between tradable and non-tradable industries, but find no evidence of any, though the earlier literature has shown that tradable industries contributed to cleansing effects being weaker in the Great Recession (Bartelsman et al., 2019).

Lastly, we find that young firms are hit more severely by recessions, which can cause recessions to have a scarring effect if young firms exit before reaching their full potential, as suggested by Ouyang (2009). We do not observe, however, that the productivity-reallocation sensitivity is higher among young firms than it is at mature firms, unlike in Foster et al. (2016).

5.3 Entering and exiting firms

Firm entries and exits account for a smaller, but still significant, part of job creation and destruction, at 25–30% in our sample. We also estimate the effect of relative productivity on firm entry and exits and test whether this link is stronger in recessions, as the cleansing hypothesis suggests it should be, using the same specification as in equation (4), except that the dependent variable now indicates firm entries or exits. As we cannot observe the productivity in the previous year at the time of firm entry, the productivity in the entry year is used in this specification. We estimate these specifications with a linear probability model that is the standard in the related literature (Dias and Robalo Marques, 2021; Foster et al., 2016; Garcia-Louzao and Tarasonis, 2021).[8]

Table 3 reports the results that provide partial support for productivity-enhancing reallocation at the extensive margin. The probability of firm exit is inversely related to relative productivity, implying that exiting firms tend on average to have lower relative productivity than surviving

[8] Alternatively, factors affecting firm entry need to be left aside and firm exits modelled with the Cox proportional hazard model (Carreira and Teixeira, 2016; Domini and Moschella, 2022; Hallward-Driemeier and Rijkers, 2013).

firms, and this relationship becomes more pronounced during economic crises. However, the results also indicate that the probability of firm entry is inversely related to relative productivity and that entering firms have lower relative productivity than surviving firms on average as well, though less so during economic downturns. This is puzzling, but one explanation could be that the productivity level in the year of entry may be distorted by business expenses initially growing faster than revenues, and so this might not be a good proxy for the potential productivity of entering firms in the short term.

Table 3. TFP and firm entry and exit, 2005–2020

Dependent variable:	Firm entry	Firm exit
Rel_TFP_t	-0.0099^{***}	
	(0.0008)	
Rel_TFP_{t-1}		-0.0071^{***}
		(0.0007)
$Cycle_t$	0.0053	–0.0132
	(0.067)	(0.0170)
$Rel\_TFP_t \times Cycle_t$	0.0044^{**}	
	(0.0019)	
$Rel\_TFP_{t-1} \times Cycle_t$		-0.0052^{***}
		(0.0017)
$Log(employment)_t$	-0.0072^{***}	
	(0.0003)	
$Log(employment)_{t-1}$		-0.0021^{***}
		(0.0004)
Industry FE	yes	yes
Year FE	yes	yes
Firm FE	no	no
No of obs.	527,466	513,824
R^2	0.022	0.018

Notes: The table reports coefficients from linear probability models. Weighted by the firm's average employment over the whole sample period. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations using administrative data.

The robustness of these findings is confirmed by using year fixed effects instead to control for business cycles; see Figure 6. This specification allows us to study as well whether the productivity-reallocation sensitivity reacted differently in the Great Recession and in the Covid-19 pandemic. The results suggest that the cleansing mechanism on the extensive margin was only present in the Great Recession. Firm entries became less sensitive to firm productivity during the Great Recession and firm exits more sensitive , indicating that the firms that entered were relatively more productive and the firms that exited relatively less productive during the Great Recession in comparison to the entry and exit patterns from before the recession. The sensitivity of firm entry and exit to productivity is noisier after the Great Recession and there is no evidence that the sensitivity to productivity changed during the Covid-19 crisis from the pattern before the pandemic.

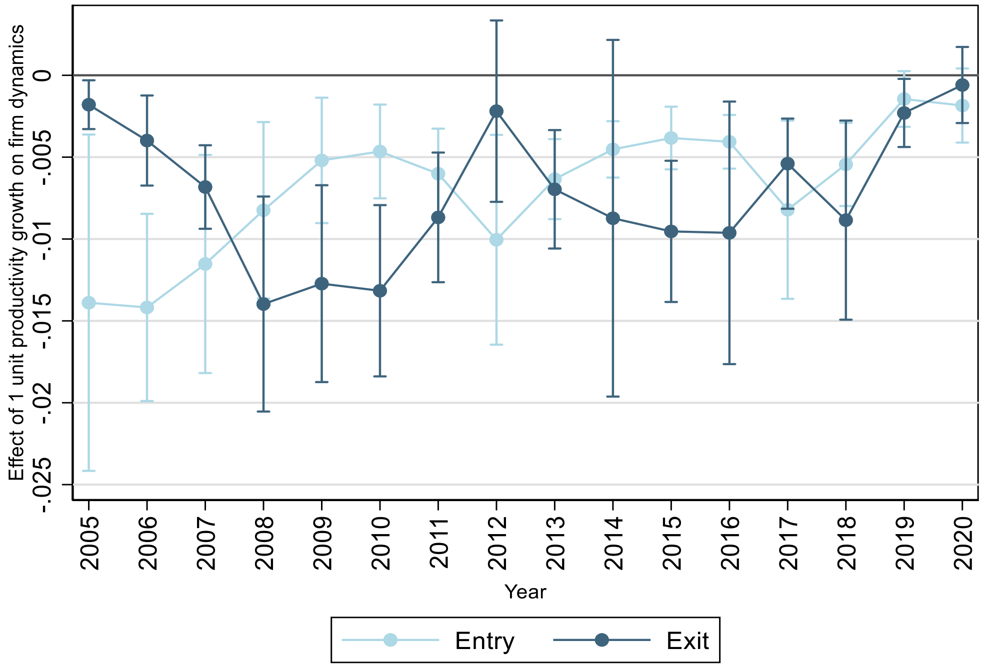

Figure 6. The effect of relative productivity on firm entry and exit, 2005–2020

Notes: 90% confidence bounds shown.
Source: Authors' calculations using administrative data.

We also estimate the probabilities of firm exit conditional on firm characteristics such as dependence on external finance, foreign trade and firm age; see Table A4 in Appendix A. While firm entries and exits are less likely in industries that are dependent on external finance and more likely in tradable industries, there is no evidence that the link between productivity and growth is different in these industries in recessions. Similarly, while firm age is an important determinant of firm exit, with younger firms being more likely to exit than mature firms, there is no evidence that the link between productivity and growth is different for young firms to what it is for mature firms during recessions.

6. The job retention scheme and productivity

6.1 Productivity-reallocation sensitivity to the take-up of job retention

Finally we investigate whether the generous job retention scheme introduced in 2020 could have held back productivity-enhancing reallocation in the Covid-19 crisis. As participation in the scheme is endogenous at the firm level, we use two different approaches to define the exposure at the level of NACE 2-digit industry. For the first, we divide industries by the average take-up level of the scheme, with industries where the take-up rate was above the average considered to be the exposed industries, and the industries where the take-up rate was below the average as the reference group. The resulting list of industries in the two subgroups is provided in Table A5 in Appendix A. The take-up of the scheme was concentrated in a small number of industries that provided 30% of total employment in 2020 but received 52% of all the benefits from the scheme. As a second proxy for firm participation, we use the average take-up level by industry. Both proxy variables vary only at the level of industry in 2020, when the job retention scheme was introduced, and are equal to zero before that.

Table 4 presents the results for an extended version of equation (4) that additionally includes i) a dummy variable *retention* indicating whether the firm operates in an exposed industry, and its interactions; and ii) a continuous variable *take-up rate* indicating the average rate in the industry, and its interactions. Including additional terms has almost no effect on the coefficients for the initial set of variables, except for the coefficient for the business cycle variable, but that was already imprecisely estimated earlier (cf. Table 2), and we continue to see that productivity has a positive effect on employment growth and that crises reinforce this effect. The additional terms clearly indicate, however, that the job retention scheme dampens this cleansing effect in both approaches.

To quantify this, we may once again consider marginal effects. Productivity at the firms in the exposed industries has a much smaller and statistically insignificant effect on employment growth in 2020 than it does at the firms in the reference group. The coefficients from column (1) without the firm fixed effects imply that an increase of one standard deviation (0.716) in relative productivity at the average unemployment growth of 0.400 in 2020 is related to employment growth being 4.5 percentage points higher in the unexposed industries[9], and 1.8 percentage points higher in the exposed industries[10]. The coefficients from column (2) with firm fixed effects would imply relative growth rates of 4.3 percentage points and 2.4 percentage points.

The other specification, with a continuous take-up rate at the industry level shown in column (4), only shows a statistically significant effect for the specification with firm fixed effects. In this specification, an increase of one standard deviation in relative productivity would imply that employment growth is 4.3 percentage points higher in the industries that did not participate in the retention scheme[11], and 2.9 percentage points higher in the industries where the take-up rate was one standard deviation above the average (0.057)[12].

[9] Derived using the coefficients from column (1) of Table 4 and setting the retention variable equal to zero: $0.052\times0.716+0.028\times0.716\times0.4$.

[10] Derived using the coefficients from column (1) of Table 4 and setting the retention variable equal to one: $0.052\times0.716+0.028\times0.716\times0.4+0.047\times0.716-0.212\times0.716\times0.4$.

[11] Derived using the coefficients from column (4) of Table 4 and setting the take-up rate equal to zero: $0.047\times0.716+0.032\times0.716\times0.4$.

[12] Derived using the coefficients from column (4) of Table 4 and setting the take-up rate equal to 0.057: $0.047\times0.716+0.032\times0.716\times0.4+0.874\times0.716\times0.057-3.025\times0.716\times0.4\times0.057$.

Table 4. Job retention schemes and the productivity-reallocation link among survivors, 2005–2020

	Retention industry		Take-up rate	
	(1)	(2)	(3)	(4)
Dependent variable: Employment growth	OLS	FE	OLS	FE
Rel_TFP_{t-1}	0.052^{***}	0.047^{***}	0.052^{***}	0.047^{***}
	(0.002)	(0.004)	(0.002)	(0.004)
$Cycle_t$	–0.006	0.000	–0.004	0.003
	(0.020)	(0.019)	(0.021)	(0.019)
$Rel\_TFP_{t-1} \times Cycle_t$	0.028^{***}	0.032^{***}	0.029^{***}	0.032^{***}
	(0.006)	(0.006)	(0.007)	(0.006)
$Retention_t$	0.020	0.034		
	(0.032)	(0.033)		
$Retention_t \times Rel\_TFP_{t-1}$	0.047	0.086^{**}		
	(0.037)	(0.042)		
$Retention_t \times Cycle_t$	-0.180^{*}	-0.241^{**}		
	(0.096)	(0.100)		
$Retention_t \times Rel\_TFP_{t-1} \times Cycle_t$	-0.212^{**}	-0.280^{**}		
	(0.108)	(0.127)		
$Take\text{-}up\ rate_t$			–0.510	–0.444
			(0.376)	(0.363)
$Take\text{-}up\ rate_t \times Rel\_TFP_{t-1}$			0.261	0.874
			(0.489)	(0.554)
$Take\text{-}up\ rate_t \times Cycle_t$			-2.575^{***}	-3.440^{***}
			(0.995)	(1.012)
$Take\text{-}up\ rate_t \times Rel\_TFP_{t-1} \times Cycle_t$			–2.281	-3.025^{**}
			(1.416)	(1.523)
$Log(employment)_{t-1}$	-0.019^{***}	-0.200^{***}	-0.019^{***}	-0.200^{***}
	(0.001)	(0.008)	(0.001)	(0.008)
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Firm FE	no	yes	no	yes
No of obs.	478,175	478,175	478,175	478,175
R^2	0.066	0.191	0.067	0.192

Notes: The split into retention and non-retention industries is made by deriving the average share of wages from the scheme to total wages in each industry; industries whose share of wages from the scheme is above the average level are considered retention industries. The take-up rate refers to the average share of wages from the scheme to total wages in industries. See Table A5 in Appendix A for the list of retention industries and the average take-up rate. Weighted by the firm's average employment over the whole sample period. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations using administrative data.

There is no evidence that the link between productivity and reallocation in the exposed industries was different to that in the unexposed industries for exiting firms, as the interaction terms between the variables for relative TFP, the business cycle and retention are statistically insignificant, as shown in columns (2) and (4) of Table A6 in Appendix A. There is evidence that firms entering the exposed industries in 2020 had lower relative productivity than firms entering the unexposed industries, suggesting that a weaker link between productivity and reallocation allowed less productive firms to enter. Overall, the results indicate that the job retention scheme held back productivity-enhancing reallocation mostly through the intensive margin and less so through the extensive margin.

6.2 Aggregate implications

We also estimate the effect on aggregate productivity from the link between productivity and reallocation being suppressed in the exposed industries by creating a counterfactual productivity distribution in 2020 using the approach suggested by Decker (2020), a method that is also applied by Andrews et al. (2021b) in a similar exercise to ours. The effect of the job retention scheme on aggregate productivity, ΔP_{2020}^{policy}, is obtained as the difference for surviving firms between the aggregate TFPs with and without the policy:

$$\Delta P_{2020}^{policy} = \sum_{i=1}^{I} s_{i,2020} \times p_{i,2019} - \sum_{i=1}^{I} \tilde{s}_{i,2020} \times p_{i,2019} \tag{5}$$

where the variable $p_{i,2019}$ refers to the firm's TFP in 2019, $s_{i,2020}$ to the firm's employment share in total employment in 2020, and $\tilde{s}_{i,2020}$ to the firm's *counterfactual* employment share in total employment had the link between productivity and growth not been suppressed in 2020.

The estimates from Table 4 are used to predict the values for the employment growth of the firms under the two scenarios.[13] The predicted values obtained with the full set of estimated coefficients are taken as the actual outcome, and the predicted values obtained when the two interaction terms in either specification that contain the variables for retention and relative productivity are excluded are taken as the counterfactual outcome. The underlying intuition is to define the counterfactual outcome as what it would have been if the link between productivity and reallocation had remained unaltered in the exposed industries in 2020, meaning if the link between productivity and growth had not been suppressed.

The upper panel of Table 5 presents the results. We estimate that the policy reduced aggregate productivity by up to 11.0%. The effects are stronger when the take-up rate is used at the industry level, as shown in columns (3) and (4), rather than when the industries are separated into two groups, as seen in columns (1) and (2), and they also vary with the estimator. Nevertheless, all the estimates suggest that the retention scheme caused a substantial reduction in aggregate productivity and muted the link between productivity and reallocation.

[13] The estimates in column (3) of Table 4, where the key interaction term is not statistically significant, are also used for the sake of completeness but the corresponding results in Table 5 are excluded from the subsequent discussion.

Table 5. The effect of the job retention scheme on aggregate productivity and welfare, 2020

	Retention industry		Take-up rate	
	(1)	(2)	(3)[(a)]	(4)
	OLS	FE	OLS	FE
The effect of policy on the aggregate productivity				
With policy: where employment growth is predicted with the full model, $\sum_{i=1}^{I} s_{i,2020} \times p_{i,2019}$	30.0	32.0	29.1	30.3
Without policy: where employment growth is predicted without the interactions of productivity and retention/take-up rate, $\sum_{i=1}^{I} \tilde{s}_{i,2020} \times p_{i,2019}$	31.0	32.5	30.6	34.1
Policy effect in %, $\Delta P_{2020}^{policy} / \sum_{i=1}^{I} \tilde{s}_{i,2020} \times p_{i,2019}$	–3.2%	–1.6%	–5.0%	–11.0%
The welfare effect of policy				
With policy: employment growth is predicted with the full model, and welfare effects account for observed unemployment	26.4	28.1	25.5	26.7
Without policy: employment growth is predicted without the interactions of productivity and retention/take-up rate, and welfare effects account for 2.9pp higher unemployment rate	26.0	27.2	25.6	28.5
Policy effect in %	1.6%	3.3%	–0.3%	–6.6%

Notes: [(a)] refers to the regression where the retention scheme did not have a statistically significant effect on reallocation and the results are not used in the interpretation.
Source: Authors' calculations using administrative data.

However, the lower aggregate productivity alone does not capture the welfare effects of the retention scheme, as there would probably also have been higher unemployment without the scheme.[14] We propose a simple estimation of the welfare consequences of the scheme. We add unemployed individuals to the aggregate productivity calculation by creating an artificial firm that represents all the unemployed individuals and yields zero productivity, and we re-estimate equation (5). The total utilitarian welfare in the economy is defined as the weighted average of the productivity of all economically active individuals, covering also the unemployed, who have zero productivity.[15] To do this we need to estimate the impact of the retention scheme on employment, which is done using the matching techniques described in Appendix B. We find that the scheme had a positive effect on employment and saved about 14,000–26,000 jobs, as

[14] Note that predicting employment growth at the firm level among surviving firms can also affect their aggregate employment, but as aggregate changes were minuscule, we can consider the results in the upper part of Table 5 to reflect employment changes essentially at the intensive margin.
[15] We exclude the public sector from this exercise as the estimation of TFP for this sector is not plausible. As public sector employment was quite stable over the business cycle and not affected much by the retention scheme, it should not affect the main conclusion from our welfare estimates.

roughly one worker in five participating in the scheme would have lost their job without the support and the unemployment rate would have increased by 2–4 percentage points in 2020.

After adding the unemployed to aggregate productivity and estimating utilitarian welfare with and without the policy, we find that the welfare effects of the policy are ambiguous and depend on the identification strategy (see Table 5, lower panel). When the industries are simply divided into two groups, the positive effect from higher retained employment is sufficient to offset the negative effect on aggregate productivity, implying that the policy improved total welfare by 1.6% (OLS) or 3.3% (FE). However, when the take-up rate at the industry level is used, the estimated negative effect on aggregate productivity becomes larger and implies that total welfare declined by 6.6%.

Our preferred approach is the one with a continuous take-up rate at the industry level, shown in the last two columns in Tables 4 and 5, because here the counterfactual implies no participation in the scheme. With the binary split of industries distinguishing between those with a take-up rate above the average from those with a rate below the average, as in the first two columns in Tables 4 and 5, the counterfactual would include industries that used the scheme, but to a lesser extent. In our preferred estimation, the scheme had a large negative effect on aggregate productivity that the positive effect on jobs was not sufficient to offset, resulting in a negative welfare effect.

7. Conclusions

The paper adds to the existing evidence on whether recessions enhance productivity and accelerate the reallocation of resources from less productive firms to more productive ones. We contribute to the literature by focusing on the Covid-19 crisis, and we also evaluate the role of job retention schemes in this relationship, asking whether jobs were saved at the cost of productivity in the Covid-19 crisis. We use Estonian firm-level administrative data from 2004–2020.

We conclude that structural changes towards more productive industries have consistently made a positive contribution to aggregate productivity. Our analysis also shows that while the Great Recession further strengthened the link between productivity and reallocation and had a strong within-industry cleansing effect at both the intensive and extensive margins of firm growth, this effect was not found in the Covid-19 pandemic.

We find that the introduction of a generous job retention scheme in response to Covid-19 protected jobs, but came at the cost of a weaker link between firm productivity and job reallocation. We demonstrate this by comparing the industries that were more exposed to the scheme with those that were less exposed, and we show that the link between productivity and growth was much weaker in the exposed industries. In our preferred specifications, the weakening of the link between productivity and reallocation because of the retention scheme reduced aggregate productivity in 2020 by up to 11%.

Further estimates with a matching technique show the retention scheme had a positive effect on firm employment. We find that about one participant in five in the scheme would have lost their job without the support, and the unemployment rate would ceteris paribus have been 2–4 percentage points higher. Weighing excess outflows to unemployment against productivity effects illustrates the delicate balance between saving jobs and fostering productivity. Our preferred models suggest that there were welfare losses from the scheme as the positive effect on jobs is outweighed by the negative effect on aggregate productivity.

Future studies could explore the long-term implications of the Covid-19 pandemic on productivity. One useful direction could be to use linked employer-employee data to study the transitions of individual workers between firms with different characteristics. Another avenue for future studies could be to investigate the cost-effectiveness of job retention schemes, and compare the effectiveness of such support measures with that of more conventional unemployment insurance schemes.

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Appendix A. Supplementary figures and tables

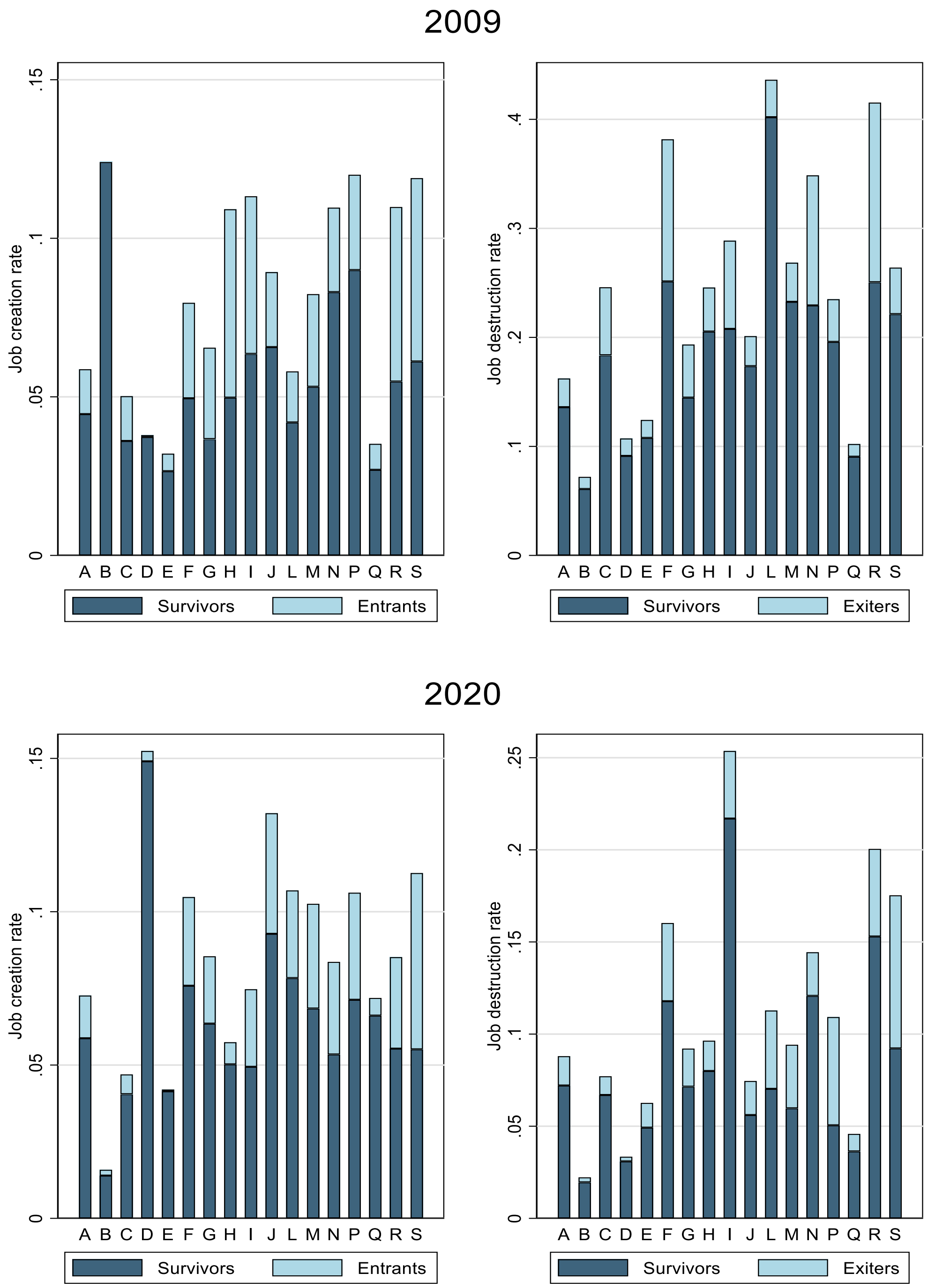

Figure A1. Job reallocation at the intensive and extensive margins by sector

Notes: NACE 1-digit industries at the horizontal axis: A agriculture, B mining, C manufacturing, D electricity, E water, F construction, G retail, H transport, I hotels and restaurants, J information and communication, L real estate, M professional services, N support services, P education, Q health, R arts and entertainment, S other services.

Source: Authors' calculations using administrative data.

Figure A2. Correlation between lagged productivity and employment growth by productivity decile group and year, 2004–2020

Notes: The figures report average employment growth by TFP decile groups one year ago; and Pearson *r* indicates the correlation between employment growth and continuous TFP a year ago. TFP decile groups are derived separately for each sample year. The firm-level TFP is not derived relative to the industry's average here. Weighted by firm employment for each sample year.
Source: Authors' calculations using administrative data.

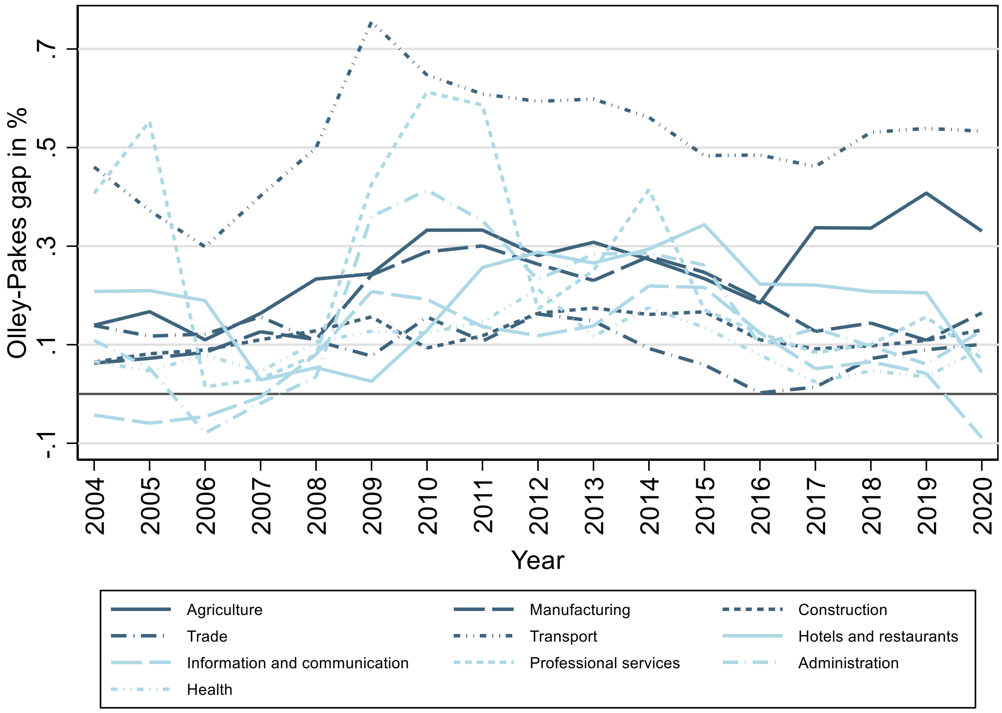

Figure A3. Olley-Pakes gap in larger industries, 2004–2020

Notes: The Olley-Pakes gap shows static allocative efficiency in a given year, or how much of industry productivity originates from larger firms having higher productivity. Results for industries employing fewer than 10,000 workers in 2020 are reported in Figure A3 in Appendix A.
Source: Authors' calculations using administrative data.

Table A1. Papers studying the responsiveness of job reallocation to productivity

Authors	Specification	Country	Period	Sector	Result
Responsiveness to pre Covid-19 recessions					
Hallward-Driemeier and Rijkers (2013)	Cleansing: lagged productivity interacted with a crisis dummy; Control for firm FE	Indonesia	1991–2001	Manufacturing	No cleansing
Carreira and Teixeira (2016)	Cleansing: lagged productivity interacted with a crisis dummy; Control for firm FE	Portugal	2004–2012	Manufacturing	No cleansing
Foster et al. (2016)	Cleansing: lagged productivity interacted with regional unemployment growth; **Do not control for firm FE**	US	1981–2010	Manufacturing	Cleansing
Dias et al. (2021)	Cleansing: lagged productivity interacted with a crisis dummy; **Do not control for firm FE**	Portugal	2006–2015	All nonfinancial firms	Cleansing
Mina and Santoleri (2021)	Cleansing: lagged productivity in the period before and during the crisis; **Do not control for firm FE**	10 Eurozone countries	2001–2013	Manufacturing and services	No cleansing
Garcia-Louzao and Tarasonis (2021)	Cleansing: lagged productivity interacted with a crisis dummy; Control for firm FE	Lithuania	2000–2015	All nonfinancial firms	Cleansing
Domini and Moschella (2022)	Cleansing: lagged productivity interacted with a crisis dummy; Control for firm FE interacted with crisis dummy as a robustness	France	2002–2013	Manufacturing	No cleansing
Responsiveness to Covid-19 recession					
Andrews et al. (2021a)	Cleansing: lagged productivity interacted with a crisis dummy; Productivity not time-varying	Australia, New Zealand, UK	2019–2021	All private sector	Cleansing: AU, UK, No cleansing: NZ
Andrews et al. (2021b)	Cleansing: lagged productivity interacted with a crisis dummy; Productivity not time-varying	Australia	2018–2020	All private sector	Cleansing
Trend in responsiveness					
Decker et al. (2020)	Job growth responsiveness to productivity; **Do not control for firm FE**	US	1996–2013	Economy wide	Responsiveness has been declining
Andrews and Hansell (2021)	Job growth responsiveness to productivity; **Do not control for firm FE**	Australia	2002–2016	Manufacturing and services	Responsiveness has been declining

Source: Compiled by authors.

Table A2. TFP and reallocation of employment among survivors, year-by-year estimates, 2005–2020

Dependent: employment growth	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
Rel_TFP_{t-1}	0.082***	0.064***	0.061***	0.154***	0.115***	0.062***	0.058***	0.034***	0.038***	0.038***
	(0.011)	(0.008)	(0.007)	(0.035)	(0.011)	(0.007)	(0.009)	(0.006)	(0.004)	(0.005)
$Log(employment)_{t-1}$	–0.023***	–0.014***	–0.008***	–0.033***	–0.020*	–0.006*	–0.012***	–0.001	–0.004**	–0.013***
	(0.006)	(0.004)	(0.003)	(0.012)	(0.012)	(0.003)	(0.003)	(0.003)	(0.002)	(0.005)
Industry FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
County FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	no	no	no	no	no	no	no	no	no	no
No of obs.	20938	22441	24888	27458	28184	26618	28407	30165	31984	33463
R^2	0.294	0.067	0.078	0.274	0.159	0.090	0.071	0.033	0.036	0.099

Table A2. continued

Dependent: employment growth	2015	2016	2017	2018	2019	2020
Rel_TFP_{t-1}	0.031***	0.035***	0.030***	0.031***	0.032***	0.040***
	(0.007)	(0.006)	(0.007)	(0.005)	(0.004)	(0.005)
$Log(employment)_{t-1}$	–0.004	0.001	–0.001	–0.003	–0.001	0.002
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Industry FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
County FE	yes	yes	yes	yes	yes	yes
Firm FE	no	no	no	no	no	no
No of obs.	34696	35569	32857	32454	33580	34643
R^2	0.053	0.038	0.029	0.028	0.030	0.081

Notes: Weighted by the firm's average employment in period $t-1$. Cycle proxy and its interaction with productivity is omitted from these year-by-year specifications and county fixed effects are added instead. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations using administrative data.

Table A3. TFP and reallocation of employment among survivors by firm characteristics, 2005–2020

	Dependent variable: Employment growth in year t among surviving firms					
	External finance dependent industry vs other		Tradable vs non-tradable industry		Young vs mature firms	
	OLS	FE	OLS	FE	OLS	FE
Rel_TFP_{t-1}	0.052^{***}	0.047^{***}	0.051^{***}	0.052^{***}	0.052^{***}	0.045^{***}
	(0.003)	(0.007)	(0.002)	(0.004)	(0.002)	(0.005)
$Cycle_t$	–0.009	–0.001	–0.012	–0.004	–0.051	–0.038
	(0.020)	(0.020)	(0.021)	(0.020)	(0.035)	(0.037)
$Rel\_TFP_{t-1} \times Cycle_t$	0.038^{***}	0.045^{***}	0.025^{***}	0.030^{***}	0.023^{***}	0.028^{***}
	(0.008)	(0.009)	(0.007)	(0.006)	(0.007)	(0.007)
Ext_dep_{t-1}	–0.006	0.011				
	(0.014)	(0.017)				
$Ext\_dep_t \times Rel\_TFP_{t-1}$	–0.001	–0.002				
	(0.004)	(0.008)				
$Ext\_dep_t \times Cycle_t$	–0.010	$–0.020^{*}$				
	(0.011)	(0.011)				
$Ext\_dep_t \times Rel\_TFP_{t-1} \times Cycle_t$	$–0.027^{**}$	$–0.033^{***}$				
	0.052^{***}	0.047^{***}				
$Tradable_t$			0.050^{***}	0.061^{***}		
			(0.013)	(0.017)		
$Tradable_t \times Rel\_TFP_{t-1}$			–0.001	$–0.025^{*}$		
			(0.006)	(0.014)		
$Tradable_t \times Cycle_t$			0.000	-0.008		
			(0.011)	(0.011)		
$Tradable_t \times Rel\_TFP_{t-1} \times Cycle_t$			0.003	0.003		
			(0.016)	(0.017)		
$Young_t$					0.108^{***}	0.045^{***}
					(0.004)	(0.006)
$Young_t \times Rel\_TFP_{t-1}$					0.007	0.001
					(0.007)	(0.007)
$Young_t \times Cycle_t$					$–0.047^{***}$	$–0.049^{***}$
					(0.011)	(0.013)
$Young_t \times Rel\_TFP_{t-1} \times Cycle_t$					0.020	0.020
					(0.015)	(0.013)
$Log(employment)_{t-1}$	$–0.019^{***}$	$–0.199^{***}$	$–0.019^{***}$	$–0.200^{***}$	$–0.015^{***}$	$–0.194^{***}$
	(0.001)	(0.009)	(0.001)	(0.008)	(0.001)	(0.010)
Industry FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Firm FE	no	yes	no	yes	no	yes
No of obs.	478175	478175	478175	478175	478345	478345
R^2	0.066	0.190	0.066	0.191	0.076	0.185

Notes: Weighted by the firm's average employment over the whole sample period. Industries are divided into those dependent on external finance and those that are not according to whether their ratio of short-term and long-term debt to total assets is above or below the average across all industries. The split into tradable and non-tradable industries is done similarly, using the ratio of exports in goods and services to total turnover. See Table A5 in Appendix A. The number of observations is slightly lower than in Table 2 as small sectors with fewer than 10 observations per year are excluded here. Young firms are defined as those aged under 5 years. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations using administrative data.

Table A4. TFP and firm entries and exits by firm characteristics, 2005–2020

	External finance dependent industry vs other		Tradable vs non-tradable industry		Young vs mature firms
	Dependent variable: Entry	Dependent variable: Exit	Dependent variable: Entry	Dependent variable: Exit	Dependent variable: Exit
Rel_TFP_{t-1}	-0.011^{***}	-0.007^{***}	-0.009^{***}	-0.006^{***}	-0.006^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$Cycle_t$	0.005	-0.015	0.005	–0.016	–0.016
	(0.007)	(0.019)	(0.007)	(0.019)	(0.016)
$Rel\_TFP_{t-1} \times Cycle_t$	0.007^{***}	-0.007^{***}	0.006^{***}	-0.006^{***}	-0.005^{**}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Ext_dep_t	-0.023^{***}	-0.013^{***}			
	(0.002)	(0.003)			
$Ext\_dep_t \times Rel\_TFP_{t-1}$	0.003^{*}	0.000			
	(0.002)	(0.001)			
$Ext\_dep_t \times Cycle_t$	0.004	0.003			
	(0.004)	(0.007)			
$Ext\_dep_t \times Rel\_TFP_{t-1} \times Cycle_t$	–0.006	0.003			
	(0.004)	(0.003)			
$Tradable_t$			0.049^{***}	0.020^{***}	
			(0.009)	(0.006)	
$Tradable_t \times Rel\_TFP_{t-1}$			–0.003	–0.003	
			(0.003)	(0.002)	
$Tradable_t \times Cycle_t$			0.003	0.004	
			(0.005)	(0.009)	
$Tradable_t \times Rel\_TFP_{t-1} \times Cycle_t$			–0.007	0.002	
			(0.006)	(0.005)	
$Young_t$					0.016^{***}
					(0.001)
$Young_t \times Rel\_TFP_{t-1}$					-0.003^{***}
					(0.001)
$Young_t \times Cycle_t$					0.014^{***}
					(0.005)
$Young_t \times Rel\_TFP_{t-1} \times Cycle_t$					–0.000
					(0.003)
$Log(employment)_{t-1}$	-0.007^{***}	-0.002^{***}	-0.007^{***}	-0.002^{***}	-0.001^{***}
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Industry FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Firm FE	no	no	no	no	no
No of obs.	527424	513781	527424	513781	513824
R^2	0.022	0.018	0.022	0.018	0.020

Notes: Weighted by the firm's average employment over the whole sample period. See also notes on Table A3. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations using administrative data.

Table A5. Mean external finance dependency, export intensity and take-up of job retention

Sector code	Sector name	Short and long-term debt to total assets, 2004–2020	Exports of goods and services to turnover, 2004–2020	Share of support from job retention to total wage costs in 2020
A01	Crop and animal production, hunting and related service activities	0.301	0.015	0.003
A02	Forestry and logging	0.239	0.019	0.022
A03	Fishing and aquaculture	0.221	0.093	0.013
B08	Other mining and quarrying	0.146	0.181	0.006
B09	Mining support service activities	0.218	0.019	0.027
C10	Manufacture of food products	0.243	0.144	0.031
C11	Manufacture of beverages	0.224	0.216	0.014
C13	Manufacture of textiles	0.187	0.308	0.073
C14	Manufacture of wearing apparel	0.151	0.250	0.061
C15	Manufacture of leather and related products	0.111	0.374	0.085
C16	Manufacture of wood and of products of wood and cork, except furniture	0.230	0.308	0.026
C17	Manufacture of paper and paper products	0.265	0.366	0.006
C18	Printing and reproduction of recorded media	0.259	0.198	0.076
C19	Manufacture of coke and refined petroleum products	0.229	0.134	0.002
C20	Manufacture of chemicals and chemical products	0.191	0.371	0.018
C21	Manufacture of basic pharmaceutical products and pharmaceutical preparations	0.177	0.501	0.010
C22	Manufacture of rubber and plastic products	0.212	0.301	0.032
C23	Manufacture of other non-metallic mineral products	0.188	0.193	0.03
C24	Manufacture of basic metals	0.178	0.396	0.025
C25	Manufacture of fabricated metal products, except machinery and equipment	0.199	0.240	0.031
C26	Manufacture of computer, electronic and optical products	0.159	0.427	0.006
C27	Manufacture of electrical equipment	0.163	0.422	0.007
C28	Manufacture of machinery and equipment n.e.c.	0.154	0.364	0.030
C29	Manufacture of motor vehicles, trailers and semi-trailers	0.111	0.466	0.086
C30	Manufacture of other transport equipment	0.154	0.307	0.026
C31	Manufacture of furniture	0.227	0.281	0.064
C32	Other manufacturing	0.121	0.312	0.047
C33	Repair and installation of machinery and equipment	0.111	0.153	0.030
D35	Electricity, gas, steam and air conditioning supply	0.230	0.025	0.008
E36	Water collection, treatment and supply	0.138	0.062	0.004
E37	Sewerage	0.096	0.004	0.003
E38	Waste collection, treatment and disposal activities; materials recovery	0.211	0.110	0.011
E39	Remediation activities and other waste management services	0.180	0.058	0.024
F41	Construction of buildings	0.117	0.084	0.025
F42	Civil engineering	0.126	0.031	0.014
F43	Specialised construction activities	0.104	0.051	0.031

G45	Wholesale and retail trade and repair of motor vehicles and motorcycles	0.206	0.032	0.042
G46	Wholesale trade, except of motor vehicles and motorcycles	0.154	0.079	0.028
G47	Retail trade, except of motor vehicles and motorcycles	0.138	0.014	0.038
H49	Land transport and transport via pipelines	0.293	0.102	0.027
H50	Water transport	0.417	0.460	0.081
H51	Air transport	0.242	0.505	0.075
H52	Warehousing and support activities for transportation	0.172	0.306	0.024
H53	Postal and courier activities	0.079	0.174	0.005
I55	Accommodation	0.298	0.103	0.118
I56	Food and beverage service activities	0.237	0.006	0.108
J58	Publishing activities	0.116	0.026	0.043
J59	Motion picture, video and television programme production, sound recording	0.117	0.089	0.057
J60	Programming and broadcasting activities	0.235	0.021	0.032
J61	Telecommunications	0.153	0.116	0.002
J62	Computer programming, consultancy and related activities	0.089	0.325	0.007
J63	Information service activities	0.15	0.242	0.013
L68	Real estate activities	0.235	0.016	0.018
M69	Legal and accounting activities	0.093	0.071	0.017
M70	Activities of head offices; management consultancy activities	0.092	0.078	0.014
M71	Architectural and engineering activities; technical testing and analysis	0.091	0.067	0.018
M72	Scientific research and development	0.139	0.284	0.013
M73	Advertising and market research	0.109	0.125	0.054
M74	Other professional, scientific and technical activities	0.118	0.099	0.03
M75	Veterinary activities	0.227	0.002	0.002
N77	Rental and leasing activities	0.225	0.080	0.026
N78	Employment activities	0.055	0.139	0.051
N79	Travel agency, tour operator and other reservation service and related activities	0.082	0.183	0.131
N80	Security and investigation activities	0.094	0.016	0.004
N81	Services to buildings and landscape activities	0.125	0.012	0.015
N82	Office administrative, office support and other business support activities	0.127	0.239	0.020
P85	Education	0.142	0.010	0.039
Q86	Human health activities	0.173	0.011	0.021
Q87	Residential care activities	0.141	0.000	0.001
Q88	Social work activities without accommodation	0.149	0.001	0.031
R90	Creative, arts and entertainment activities	0.097	0.026	0.065
R91	Libraries, archives, museums and other cultural activities	0.199	0.009	0.066
R92	Gambling and betting activities	0.125	0.028	0.051
R93	Sports activities and amusement and recreation activities	0.262	0.019	0.078
S94	Activities of membership organisations	0.303	0.008	0.000
S95	Repair of computers and personal and household goods	0.131	0.033	0.042
S96	Other personal service activities	0.158	0.035	0.064
	Cross-sector average	0.173	0.156	0.033

Notes: Cells filled in with grey show industries with values above the cross-sector average.
Source: Authors' calculations using administrative data.

Table A6. Job retention schemes and the productivity-reallocation link, entry and exit, 2005–2020

	Retention		Take-up rate	
	(1)	(2)	(3)	(4)
Dependent variable: firm entry/exit	Entry	Exit	Entry	Exit
Rel_TFP	–0.010***	–0.007***	–0.010***	–0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
$Cycle_t$	0.005	-0.017	0.005	–0.018
	(0.007)	(0.018)	(0.007)	(0.019)
Rel_TFP × $Cycle_t$	0.004**	–0.005***	0.004**	–0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
$Retention_t$	–0.006	–0.015		
	(0.006)	(0.012)		
$Retention_t$ × Rel_TFP	0.024***	0.006***		
	(0.008)	(0.002)		
$Retention_t$ × $Cycle_t$	0.010	0.032		
	(0.015)	(0.027)		
$Retention_t$ × Rel_TFP × $Cycle_t$	–0.044**	0.008		
	(0.022)	(0.006)		
Take-up $rate_t$			–0.218**	–0.312
			(0.094)	(0.200)
Take-up $rate_t$ × Rel_TFP			0.424***	0.143***
			(0.103)	(0.039)
Take-up $rate_t$ × $Cycle_t$			0.250	0.509
			(0.224)	(0.440)
Take-up $rate_t$ × Rel_TFP × $Cycle_t$			–0.828***	0.120
			(0.307)	(0.089)
$Log(employment)_{t-1}$	–0.007***	–0.002***	-0.007***	–0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Industry FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Firm FE	no	no	no	no
No of obs.	527,424	513,428	527,424	513,428
R^2	0.022	0.019	0.022	0.019

Notes: Relative TFP refers to period $t-1$ for firm exit and to period t for firm entry. See Table A5 in Appendix A for the list of industries in retention and the average take-up rate. Weighted by the firm's average employment over the whole sample period. Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations using administrative data.

Appendix B. The effect of the job retention scheme on employment

To identify and quantify the effect of the job retention scheme on employment, we use quarterly sales and employment data from the Tax and Customs Board register and matching techniques to distinguish between firms depending on whether or not they were eligible for the scheme and whether or not they actually received the support.

The actual eligibility of each firm is not observed, and so we approximate it by simulating potential eligibility using the official criterion for firm turnover, which considers firms to be eligible if their turnover dropped by more than 30% in the first or second quarter in 2020 from what it was in the same quarter a year before. The available data do not contain sufficient information for us to consider the other two criteria described in Section 2.

Overall, four subgroups of firms can be distinguished, as shown in Table B1. The firms that were not eligible and did not get the support are in group 1; the firms that were not eligible but still received the support are group 2; the firms that were eligible but did not receive the support are group 3; and the firms that were eligible and received the support are group 4.

Table B1. Employment statistics by the eligibility and receipt of the support

Group	Average employment in 2019–2020 (1)	Employment share in 2019–2020 (2)	Actual employment growth in 2020 (3)
All firms	387,994	1.000	–0.069
Group 1: Eligible 0 & support 0	194,561	0.501	–0.009
Group 2: Eligible 0 & support 1	55,197	0.142	–0.033
Group 3: Eligible 1 & support 0	59,335	0.153	–0.156
Group 4: Eligible 1 & support 1	78,901	0.203	–0.180

Notes: Employment growth is defined as in equation (1).
Source: Authors' calculations using administrative data.

The largest group consists of the firms that were not eligible and did not receive the support, which accounted for 50% of total employment; 36% of firms received the support, and 14% of firms were eligible for it, but did not receive it, as shown in the second column of Table B1. These shares are employment weighted. How the four groups differ by their average annual change in firm turnover in each quarter of 2020 is shown in Figure B1, which reveals large heterogeneity. The eligible firms were clearly the most severely hit in 2020, while the ineligible firms that did not receive the support experienced growth in turnover on average throughout the year. The second group that received the support while supposedly not being eligible is an intermediate group that was doing quite well in the first quarter of 2020 and experienced a mild recession in the second quarter. It is likely that these firms had a short-lived shock that our quarterly data cannot capture.

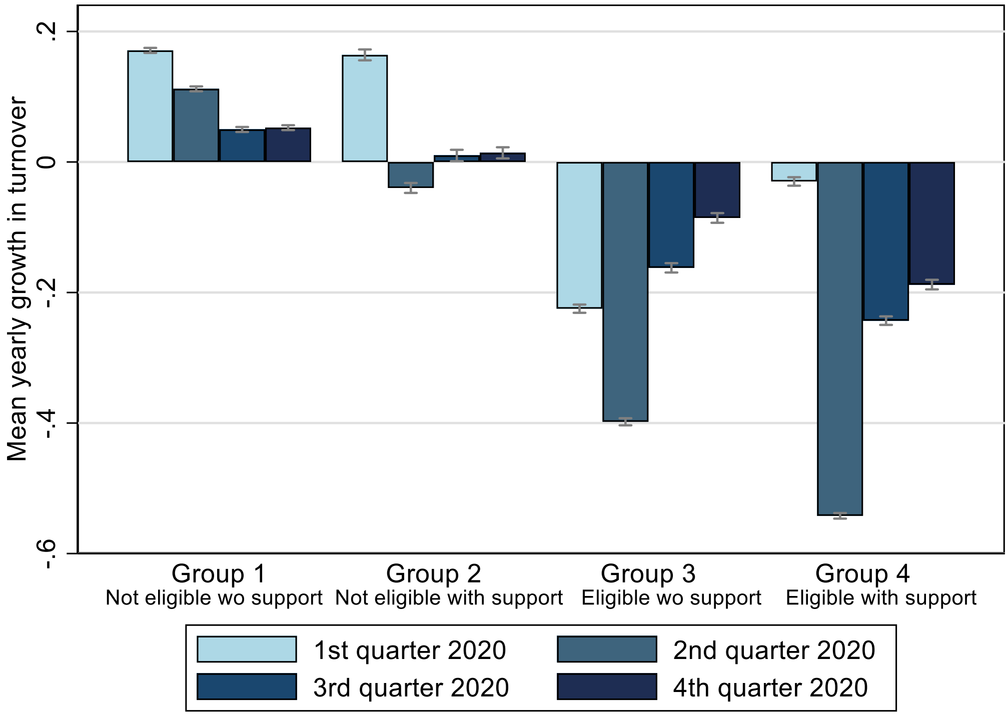

Figure B1. Mean annual growth of firm turnover over the quarters of 2020

Notes: Eligibility is defined as a fall of 30% or more in the turnover from the 1st quarter of 2019 to the 1st quarter of 2020 or from the 2nd quarter of 2019 to the 2nd quarter of 2020. 90% confidence bounds shown.
Source: Authors' calculations using administrative data.

We perform a matching exercise to derive the average treatment effect on the treated, using propensity score matching with the three nearest neighbours within the 1 percentage point caliper. The idea of matching estimation is that the differences between the treatment group and the control group in the probability of receiving the support are addressed by re-weighting the control group. We define the treatment group as the firms from group 2 and group 4, which are the firms that received the support, while the control group is defined as the firms from group 3, which did not receive the support but were eligible for it because of the large decline in their turnover. The firms that are not eligible and not observed in receipt of the support, which are in group 1, are excluded from the matching.

As we cannot assess all the eligibility criteria because information is limited, it is possible that the control group contains some firms that were not actually eligible. This would probably bias the employment growth in the control group upwards and imply that the treatment effect is more likely to be underestimated than overestimated, and so we should consider it to be a conservative estimate. By constructing the counterfactual from a similar subgroup of severely hit firms, we expect to identify the causal effect better than we can by constructing the counterfactual from the whole population. This approach is similar to that of Kopp and Siegenthaler (2021), who estimated the effect of short-time work support on the subgroup of firms that applied for the support, and formed the control group from those firms who applied for the support but did not get it.

If low-productivity firms are more likely to receive the support and the shock is persistent, then the short-time work support can have negative effects on aggregate productivity by supressing the reallocation towards high-productivity firms (Giupponi and Landais, 2020). Among all the firms in our sample, low-productivity firms had the highest probability of receiving the wage cost support and direct subsidies, unlike for the liquidity support; see Figure B2. However, when we leave group 1 aside and only consider the firms that were eligible for the support or that received it, the probability of receiving the support no longer depends on productivity; as shown in Table B2. This demonstrates that the way the control group is constructed matters and can affect the conclusions about how well the support was targeted, especially when the crisis is concentrated in a few low-productivity industries, as the Covid-19 crisis was.

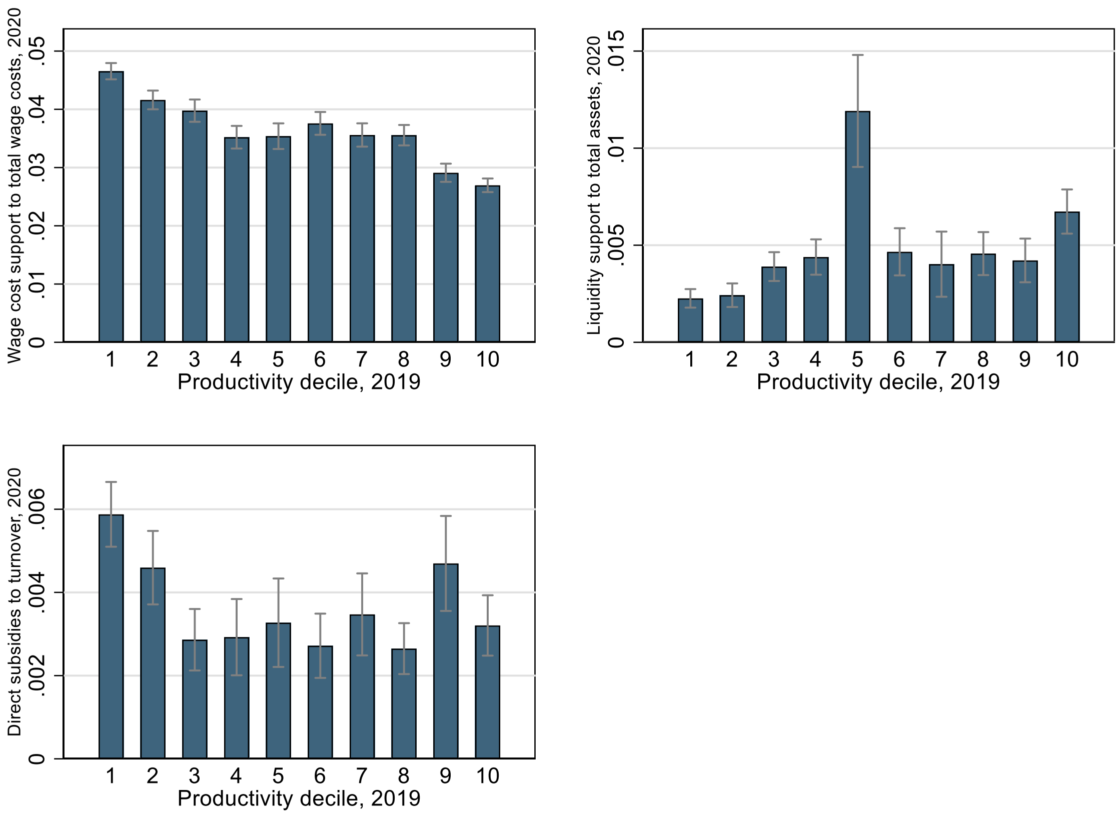

Figure B2. Relative productivity in 2019 and the intensity of support in 2020

Notes: Wage support refers to the job retention scheme delivered by the Unemployment Insurance Fund, liquidity support to loans and loan guarantees from Kredex, and direct subsidies to one-time non-refundable support from Enterprise Estonia. 90% confidence bounds shown.
Source: Authors' calculations using administrative data.

The probit model in Table B2 is used to weight the control group in matching. The probability of receiving the job retention support is positively related to the economic cycle shown as the size of the unemployment shock, to firm size, and to participation in other support measures such as liquidity support and direct subsidies. The three policy measures are tightly related; receiving the liquidity support and direct subsidies increases the probability of receiving the job retention support by 27 percentage point and 41 percentage point. After matching weights are applied to the control group, the two groups become similar in their firm characteristics, as shown in Table B3. The predicted average probability of receiving the support is 45% for the control group and 66% for the treatment group before matching, and also 66% in the control group after matching.

Table B2. Probit model for receiving the support, 2020

Dependent variable: 1=obtained support in 2020; 0=did not obtain support in 2020, but was eligible by the fall in turnover	Matching equation for employment growth
Rel_TFP_{2019}	0.015 (0.018)
$Cycle_{2020}$	0.083^{*} (0.043)
$Rel\_TFP_{2019} \times Cycle_{2020}$	–0.043 (0.044)
$Log(employment)_{2019}$	0.172^{***} (0.005)
Received liquidity $support_{2020}$	0.274^{***} (0.074)
Received direct $subsidies_{2020}$	0.409^{***} (0.018)
Industry FE	yes
N	18,803
Pseudo R^2	0.218

Notes: The table reports marginal effects at mean from the probit model.
Source: Authors' calculations using administrative data.

Table B3. Descriptive statistics for the treatment and control group, 2020

	Treated	Control before matching	Control after matching	Remaining difference after matching
Rel_TFP_{2019}	–0.107	–0.148	–0.122	0.015
$Cycle_{2020}$	0.396	0.389	0.396	0.000
$Rel\_TFP_{2019} \times Cycle_{2020}$	–0.034	–0.049	–0.039	0.005
$Log(employment)_{2019}$	1.652	1.021	1.812	-0.160^{***}
Received job retention $support_{2020}$	0.663	0.453	0.663	0.0
Received liquidity $support_{2020}$	0.019	0.002	0.012	0.007^{***}
Received direct $subsidies_{2020}$	0.168	0.035	0.139	0.029^{***}

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Authors' calculations using administrative data.

The remaining difference between the employment growth rates in the treatment group and the control group after the matching is 9.6 percentage points, which is statistically significant and can be attributed to this policy; see Table B4. In other words, job losses in the treatment group would have been 2.2 times larger without the support (–0.173/–0.077=2.2); the 90% confidence bounds for the estimate range from 1.8 to 2.6. By applying this estimate to the actual employment growth, as shown in column (3) of Table B1, we can finally derive counterfactual employment growth for firms in group 2 and group 4; shown in Table B5. This suggests that the total employment in the private sector would have declined by an additional 5.1 percentage points without the policy. In order words, roughly 20,000 jobs were saved by the job retention scheme, or 14,000–26,000 jobs with the 90% confidence level, and about one job in five that was supported by the scheme in our private sector sample was saved.[16] The unemployment rate would have increased from 6.9% to 9.8% in 2020 without the scheme and by 2-4 percentage points with the 90% confidence level.[17]

Table B4. Employment growth rates in the treatment and control groups, 2020

	Treated	Control	Difference	Standard error
Before matching	–0.077	–0.093	0.016***	0.005
After matching	–0.077	–0.173	0.096***	0.010

Source: Authors' calculations using administrative data.

Table B5. The effects of the job retention scheme on employment

Group	Counterfactual employment growth in 2020 (1)	Difference from actual employment growth in 2020 (2)
All firms	–0.121	–0.051
Group 1: Eligible 0 & support 0	–0.009	n/a
Group 2: Eligible 0 & support 1	–0.074	–0.041
Group 3: Eligible 1 & support 0	–0.156	n/a
Group 4: Eligible 1 & support 1	–0.404	–0.224

Notes: The counterfactual growth rates for groups 2 and 4 are derived by multiplying the policy effect from Table B4 (9.6pp) with the observed growth in 2019–2020 from Table B1. It is assumed that groups 1 and 3, which did not participate in the scheme, are unaffected by the policy. Column (2) shows the difference between column (1) of this table and column (3) of Table B1.
Source: Authors' calculations using administrative data.

This estimate is at the lower end of the share of jobs saved by short-time work support schemes in the Great Recession (Boeri and Bruecker, 2011). The estimates for Estonia were also higher in a related study by Almeida et al. (2021), which found that every other job covered by the scheme was saved. However, that assumed a much more severe crisis for 2020 than what actually transpired, assuming that GDP in Estonia would drop by 7%, while it actually declined by only 2.6%.[18] Evaluations that use microdata and meticulous identification strategies have provided more conservative estimates that vary from no long-term effect on employment (Giupponi and Landais, 2020) to about one tenth of jobs saved among all the participants (Cahuc et al., 2021) and to between one job in five and one in three saved among all the participants (Kopp and Siegenthaler, 2021). Our estimates are close to the effects of the Covid-

[16] In total, 101,000 workers in the private sector received support from the job retention scheme. Total private sector employment is shown in column (1) of Table B1.
[17] From adding the point estimate, an additional 20,000 laid-off workers to those 48,400 who were unemployed in 2020, according to Statistics Estonia's online database (Table TT0151).
[18] See Statistics Estonia's online database, Table RAA0012.

19 support that were estimated on Danish data, where it was found that approximately one job in three was saved among the firms that received some form of support and half of these can be attributed to the job retention support (Bennedsen et al., 2020).

Giupponi and Landais (2020) show that the pre-crisis productivity of the firms taking up the job retention scheme is a key determinant for how the support will affect firm productivity. For low-productivity firms, the scheme could just postpone the destruction of those jobs and have no effect on resource allocation and productivity in the longer term, while for high-productivity firms the scheme may have a positive effect on employment. We do not find a link between the pre-pandemic relative productivity of the firm and the size of the effect. Our results therefore suggest that the policy measure had a strong effect on preserving jobs and there was no adverse selection into receiving the support by firm productivity.

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