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Job Retention Scheme in Slovakia

Impact on Dismissals and Firm Survival in the COVID-19 Pandemic

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Abstract

This paper evaluates the impact of the Slovak job retention scheme "First Aid" on dismissals and survival of small firms during the COVID-19 pandemic. We exploit exogenous variation in epidemic intensity, measured as new COVID-19 cases within a district (LAU 1) per month. We show that the sensitivity of dismissals to intensifying epidemic among unsupported firms was low, i.e. these firms were relatively unaffected by the shock. In contrast, participation in the job retention scheme reduced the sensitivity of firms' dismissals to the intensification of the coronavirus epidemic, preserving endangered jobs. Our results indicate that there was very little treatment effect heterogeneity, i.e. supported firms benefitted by roughly the same amount, regardless of their management quality or financial vulnerability. Supported firms were also significantly less likely to fail than unsupported firms.

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1 INTRODUCTION

Job retention schemes have been one of the primary policy instruments used to preserve the employment and protect incomes of workers during the COVID-19 pandemic. Typically, they take the form of short-time work schemes or wage subsidies (Scarpetta et al. 2020). Traditional short-time work schemes alleviate the labor costs by directly subsidizing hours not worked. These schemes were extensively used during the Great Recession and inspired a number of causal studies evaluating their effectiveness (e.g. Cahuc et al. 2021; Giupponi and Landais 2018; Kopp and Siegenthaler 2021; see also Aiyar and Dao 2021)). In contrast, temporary wage subsidy schemes are not contingent on hours worked, which makes them more attractive to employers but also raises questions about their effectiveness (OECD 2021).

In this paper, we investigate the causal impact of the job retention scheme on dismissals and survival of small firms during the COVID-19 pandemic in Slovakia. The job retention scheme “First Aid” was introduced in March 2020 as a combination of wage subsidies and a short-time work arrangement. It constituted the bulk of the direct financial support for businesses: as of September 2021, the financial aid of more than 2 billion euro was used to support approximately one third of all jobs in Slovakia (Baliak et al. 2021). However, it should be noted that the size of the overall fiscal stimulus package in Slovakia has been significantly smaller compared to the most advanced economies (IMF 2021).

The key challenge of evaluating the effectiveness of job retention schemes, such as the “First Aid”, is the selection problem. In general, firms apply for the government support because they are more affected by the crisis compared to firms which do not apply for the support (Bellmann et al. 2015). As a result, a simple comparison of participating and non-participating firms will bias the impact estimate downwards. Furthermore, traditional matching and instrumental variable techniques do not fully correct (Kopp and Siegenthaler 2021). The most recent strand of literature therefore attempts to identify the causal effect by exploiting some exogenous variation, e.g. in eligibility criteria (Giupponi and Landais 2018) or regional variation in short-time work approval (Cahuc et al. 2021; Kopp and Siegenthaler 2021).

There is persuasive evidence of selection of Slovak firms into the scheme: more productive, privately owned, foreign-demand oriented firms were more likely to obtain government funding (Lalinsky and Pal 2020; Lalinsky 2021). To deal with the selection problem, we exploit exogenous variation in the epidemic intensity, measured as new COVID-19 cases within a district (equal to LAU 1 level) per month. We assume that absent the intervention, firms in districts with more COVID-19 cases would adjust employment more strongly compared to firms in districts with fewer COVID-19 cases. As Baldwin (2020) puts it, "the disease hits output by putting workers into their sickbeds". Temporary labor shortages are further exacerbated by containment measures to control the spread of SARS-CoV-2, such as quarantine requirements for infected or symptomatic individuals, as well as their contacts. There is also evidence that increasing number of local COVID-19 cases leads to a voluntary decrease in mobility, which is far greater than the decrease induced by formal measures and disproportionately larger in establishments that were busier before COVID (Goolsbee and Syverson 2021; Maloney and Taskin 2020; Kahanec et al. 2020). Furthermore, fear of infection not only keeps consumers at home; it also shifts their preferences towards necessities (Di Crosta et al. 2021). These changes in consumer behavior can severely affect revenues of local firms, particularly those providing face-to-face services.

Therefore, we estimate firms’ reaction profiles to the intensity of the coronavirus epidemic. Among unsupported firms, the relatively flat reaction curve implies that these firms were mostly unaffected by the shock. As a consequence, it may be argued that the aid was judiciously targeted because had we observed increasing rate of dismissals in response to worsening epidemic situation among the unsupported firms, it would be grounds for claiming that more firms should have been included in the scheme in order to prevent these job losses. In contrast, the dismissals among the supported firms fall dramatically with the increasing epidemic intensity, i.e. the job retention scheme enabled firms to retain more workers despite the worsening situation. Supported firms were also much less likely to fail than unsupported firms.

The fact that more productive firms were more likely to receive the government support in Slovakia (Lalinsky and Pal 2020; Lalinsky 2021) raises the question of a potential positive selection bias. For example, if *ex ante* better managed firms adapted more adroitly to the intensifying crisis, our identification strategy would fail to avoid endogeneity problems. However, there is little evidence to support this view in the period immediately following the outbreak of the pandemic. Cette et al. (2020) examine the possible impact of management practices on macroeconomic adjustment during the Great Recession and find that higher quality management quality preserves employment at the expense of real wages and with almost no significant impact on labor productivity. Although the resilience increases with the size of industry shocks, it must be noted that the impact is heterogeneous in time: during the first recession year of 2009, the impact of one standard error increase of the management quality had no effect on the value added and a very modest effect on employment. This indicates that reorganization of tasks and logistics during the crisis requires time and firms benefit from higher quality management only in the medium- to long-run. This country-level evidence from the Great Recession is supported also by the firm-level evidence during the Covid-19 pandemic. Businesses whose revenues declined during the pandemic tried to reduce expenses by cancelling night shifts or laying off workers but made no adjustment to the business activity itself (Harel 2021). Finally, there is evidence that the Covid-19 pandemic led to a significant decline in the overall productivity in Slovakia. This drop was driven by a huge temporary deterioration in the within-firm productivity (Bighelli et al. 2021; Lalinsky 2021). Although the between-firm productivity growth remained positive in 2020, it was smaller compared to the pre-pandemic level. Furthermore, the overall impact of Covid-19 subsidies on productivity was positive but relatively mild with respect to the pandemic shock (ibid). Based on the above, we argue that it is not reasonable to assume that small businesses in Slovakia were able to rapidly adjust their activities to lessen the negative effects of lockdowns, social distancing rules and especially well-documented voluntary decreases in mobility during the examined first seven months of the pandemic.

To consider this question empirically, we examine whether more vulnerable firms within the treated group benefited more from the support compared to less vulnerable firms. We use two proxies for the overall condition of the business, namely days worked in a given month between 2018 and 2019, as well as Altman Z-score. Our results indicate that there was very little treatment effect heterogeneity, i.e. all treated firms benefitted by roughly the same amount, regardless of the management quality or financial vulnerability.

Finally, as this type of regression does not enable us to identify the level effect of treatment, we estimate difference-in-differences model (see Wooldridge 2021), in which we allow for firm-specific responses to changes in workplace mobility in the particular district. Our results indicate that the job retention scheme saved about 0.1 jobs a month per firm after the termination of the support.

The rest of the paper is organized as follows. In section 2, we review the existing literature, with the emphasis on the selection problem. Section 3 provides details about the design and implementation of the “First Aid” Scheme in Slovakia. Section 4 includes the description of the methodology and data. Section 5 discusses the results. Section 6 concludes.

2 IMPACT OF JOB RETENTION SCHEMES

Various short-time work arrangements were used to subsidize hours not worked during the Great Recession in 25 out of 33 OECD countries. Their popularity motivated a growing number of academic studies. Most of the theoretical research on short-time work starts with a comparison with unemployment insurance in a frictionless labor market. Unemployment insurance reduces employment because employers are motivated to temporarily lay off employees during economic downturns, while hours per worker remain constant. However, in a system with short-time work, the firm is motivated to reduce the working hours instead of laying off (Burdett and Wright 1989). It is important to note that both policies create distortions: whereas unemployment insurance causes inefficient layoffs, short-time work induces inefficient hours per worker (ibid). If firms have access to private insurance, short-time work has a potential to improve welfare by mitigating distortions caused by public unemployment insurance (Braun and Brügemann 2017). However, if both unemployment insurance and short-time work are available, the firm's ability to adjust working hours is limited, because workers may choose to quit and draw unemployment benefits. To induce workers into accepting reduced working hours (or *de facto* part-time unemployment), short-time work programs have to be more generous than the traditional unemployment insurance systems (Van Audenrode 1994).

The second strand of theoretical research analyzes short-time work using search and matching models, which relax several assumptions of the labor contracting models discussed above. Tilly and Niedermayer (2016) demonstrate that short-time work take-up increases with increasing experience and tenure because more experienced workers with a higher tenure are more likely to experience severe earning losses when they lose their jobs. This implies that the welfare gains are modest because workers who would have been laid off without short-time work are workers for whom the earnings loss associated with unemployment is low. Hoarding of the most productive workers has negative consequences: as fewer workers are released into the unemployment pool, productive firms find it costlier to hire labor (Cooper et al. 2017). Thus, short-time work saves jobs but at the same time reduces output. Furthermore, short-time work may have windfall effects as firms facing a limited decrease in revenues also use short-time work to reduce hours of work for jobs at no risk of being destroyed (Cahuc et al. 2021).

Empirical evaluations were initially limited to macro-level evaluations, relying on evidence across OECD countries or across U.S. states. Most of these studies find that short time work prevents a surge in unemployment during recession (Abraham & Houseman, 1994; Boeri & Bruecker, 2011; Cahuc & Carcillo, 2011; Hijzen & Martin, 2013). However, these findings may be biased by the selection problem, meaning that the availability of short-time work may be correlated with the severity of the crisis (Cahuc and Carcillo 2011) or with other institutions and policies that promote labor hoarding (Aricò & Stein; Tracey & Polachek 2020).

At the firm-level, empirical evaluations are challenging because of the firms' self-selection into short-time work. In general, firms choose to participate in short-time work schemes because they are more affected by a negative shock compared to firms which choose not to participate. As a result, participating firms adjust employment more strongly in the crisis period compared to non-participating firms (Bellmann et al. 2015). Any comparison of the employment growth between economically distressed firms with STW and healthy firms without STW will therefore bias the impact estimate downwards. Indeed, early evaluations suggest small or even negative effects of short-time work on employment (Calavrezo et al. 2009, 2010; Kruppe and Scholz 2014; Tracey and Polachek 2018; Boeri and Bruecker 2011 being an exception). These counterintuitive results may be a direct consequence of the selection bias, which is not fully addressed by the matching and instrumental variable approaches used in these studies (Kopp and Siegenthaler 2021).

The new generation of empirical evaluations attempts to overcome the selection problem by exploiting exogenous eligibility rules (Giupponi and Landais 2018) or exogenous regional variation in short-time work approval (Cahuc et al. 2021; Kopp and Siegenthaler 2021). Giupponi and Landais (2018) study short-time work in Italy during the 2008-2009 Great Recession and show that it had a positive impact on employment. However, the employment effects disappear quickly after the subsidy ends. This most likely reflects the protracted nature of the Italian recession, which created disincentive to hoard labor. Furthermore, the short-time work kept workers in unproductive firms, preventing efficient reallocation of labor. Kopp and Siegenthaler (2021) study the short-time work in Switzerland in the 2009-2015 period and find that it increased firm survival and prevented rather than postponed dismissals. They also estimate that spending on short-time work was lower than the would-be unemployment benefits payments. However, the authors point out that unlike in Italy, the Swiss recession was V-shaped, which may have favored the effectiveness of the Swiss short-time work scheme. Finally, Cahuc et al. (2021) study the impact of short-time work in France during the 2008-2009 Great Recession and find that short-time work saved jobs in firms hit by strong negative revenue shocks but not in less severely-hit firms, which reduced hours for jobs at no risk of being destroyed. Despite large windfall effects, short-time work policies were more cost-efficient at saving jobs than wage subsidies. To reduce the windfall effects, Cahuc et al. (2021) recommend targeting the scheme at firms facing large drops in their revenues and implementing the scheme cautiously outside recessions.

Existing research on the effectiveness of ad-hoc wage subsidies during the pandemic is sparse.[1] For example, Gourinchas et al. (2020) estimate the impact of COVID-19 on business failures for small and medium sized enterprises (SMEs) using firm-level data in seventeen countries and find that the government support reduced failure rate of SMEs.

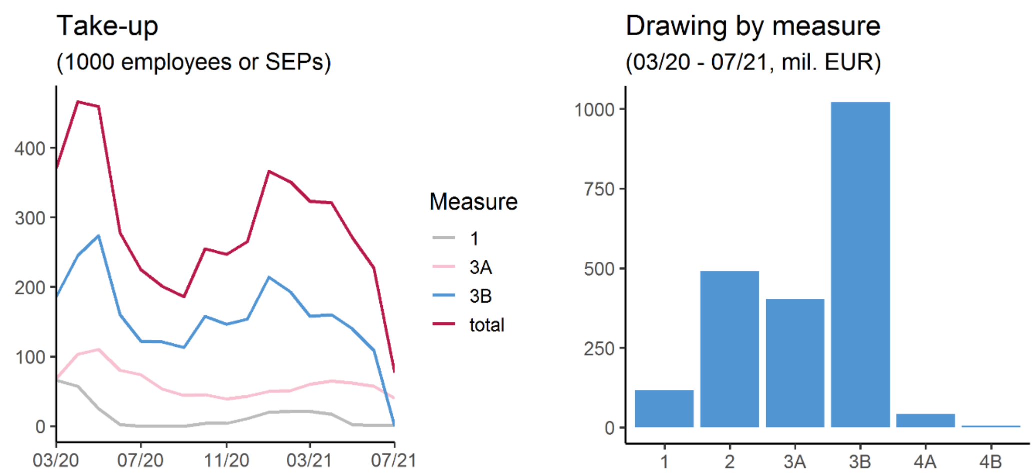
Scarpetta et al. (2020) conducted a micro-simulation analysis based on the firm-level data, covering approximately 1 million firms across 14 European countries. They find that short-time work subsidies target firms with greater financial difficulties better than wage subsidies and, as a result, are more effective in addressing liquidity problems in firms. In contrast, wage subsidies are more generous compared to short-time work instruments because they reduce the cost of hours worked as well. This increases the risk that support goes to jobs that do not need it (deadweight) but at the same time motivates firms to keep hours worked up and increase them quickly once the conditions improve.

In the Slovak context, Lalinsky and Pal (2020) find that there was a relatively strong selection into the wage subsidy scheme: more productive firms with a higher share of labor cost and ex-ante experience in dealing with the state received support with higher probability, whereas financially less disciplined, distressed and "zombie" firms had a lower chance of being supported. They also argue that the support was directed to firms from sectors in need, subdued their illiquidity or insolvency and saved jobs. However, their findings regarding the efficiency are based on a strong assumption that pandemic firm-level sales in Slovakia equal to pre-pandemic firm-level sales weighted by the pandemic change in turnover at the EU-level (17 EU countries).

3 JOB RETENTION SCHEME IN SLOVAKIA

"First Aid" is a Job Retention scheme introduced in March 2020 in Slovakia to support businesses, preserve jobs and secure incomes of workers during the COVID-19 pandemic. The take-up of the scheme was swift because of strict anti-pandemic measures introduced in spring 2020, as well as worsening economic conditions and high uncertainty (see Figure 1). Overall, the financial aid of more than 2 billion euro was used to support approximately one third of all jobs in Slovakia between March and July 2021 (Baliak et al. 2021). The scheme was revamped twice, extending coverage and raising generosity (see Appendix A). A gradual phase-out started in July 2021 in response to improving pandemic situation.

Figure 1: Job retention scheme in Slovakia



Source: Ministry of Labor, Social Affairs and Family of the Slovak Republic. Note: Data for July 2021 are preliminary and will be adjusted based on incoming requests for support. "SEPs" refers to self-employed persons.

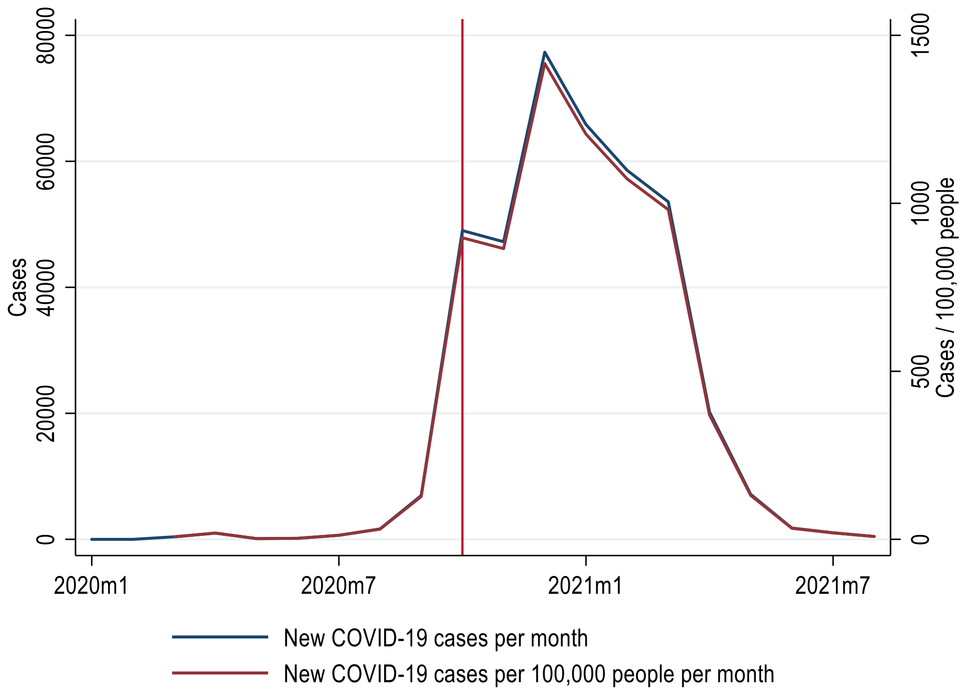
[1] Note that this type of wage subsidy is different from hiring wage subsidy, which aims to motivate employers to hire disadvantaged or long-term unemployed workers. In the Slovak context, the hiring wage subsidy was analyzed, e.g. by Hidas (2016) or Petráš (2019.

The scheme combines two main features: short-time work and wage subsidies. Almost 73% of the financial support was drawn through wage subsidies, which were provided to employers (measure 3B) and self-employed persons (measure 2), who have seen a decline in sales. Measure 3B was discontinued in July 2021. Approximately 25% of the financial support was drawn through short-time work subsidies (measures 1 and 3A), which covered all or part of the cost of hours not worked. The remaining 2.3% of the financial support was drawn by self-employed persons, who were not eligible to draw support through measure 2, or single-member private limited liability companies (measures 4A and 4B).

In line with Lalinsky and Pal (2020), we exclude self-employed persons and single-member private limited liability companies from our analysis due to lack of information. In other words, we evaluate only subjects supported through measures 1 and 3A (short-time work) and 3B (wage subsidy). Furthermore, the distribution of the financial support by measure indicates that in addition to selection into the "First Aid" scheme, firms also selected into particular measures within the scheme.[2] This could further exacerbate the selection problem; therefore, we do not report the results for the wage subsidy and short-time work separately.

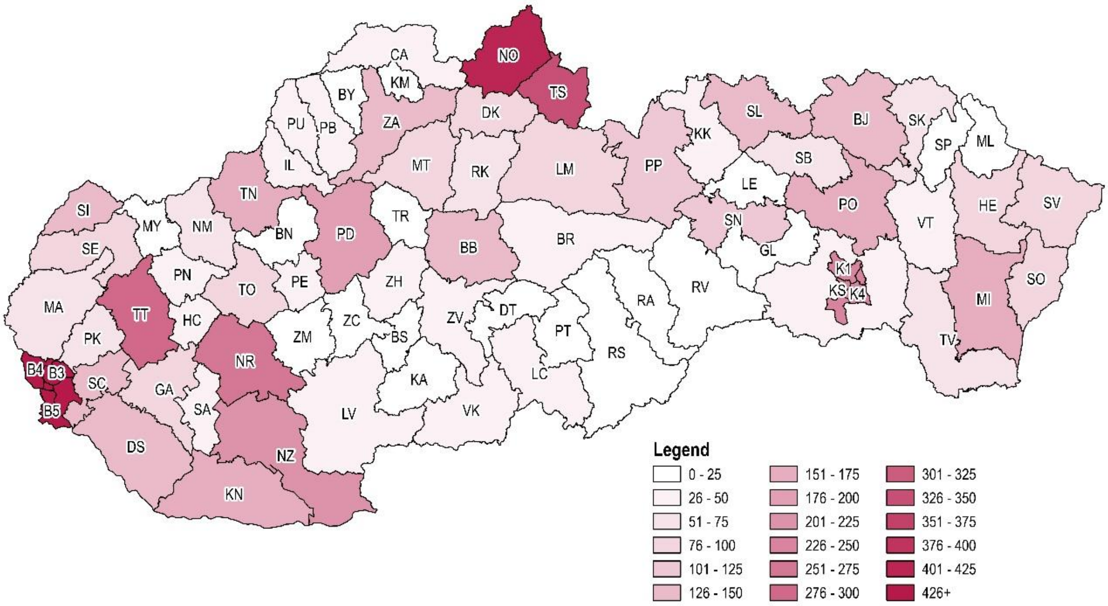
We focus on the initial phase of the "First Aid" scheme, which was implemented from March to September 2020. It is important to note that the generosity of the scheme during this phase of the pandemic was significantly lower compared to the later phases. For example, the maximum wage subsidy available through the most extensively used Measure 3B increased by 50% from 540 to 810 euro in October 2020. By focusing on the initial, less generous scheme with a slightly smaller coverage, we implicitly attempt to answer the question whether even relatively low levels of the government support helped to mitigate the impact of the crisis.

Figure 2: Pandemic intensity in Slovakia



[2] Until June 2020, it was not possible to switch between short-time work (measure 3A) and wage subsidy (measure 3B). As a result, consulting companies and newspapers produced numerous guidelines on which measure to choose in order to maximize the financial support (e.g. see Kollárová 2020 or Mihál 2020). However, firms have had an incentive to select into various measures even once these restrictions were lifted because wage subsidies enable them to manage their hours more flexibly (Scarpetta et al. 2020: 6).

Figure 3: Regional pandemic intensity (new COVID-19 cases) in September 2020



The initial phase of the "First Aid" coincides with the first and the beginning of the second wave of the pandemic (Figure 2). The number of new COVID-19 cases remained stable throughout the period until August 2020 when it started increasing dramatically. However, there was a significant regional variation in the number of new cases, i.e. some enterprises were more likely to be hit by anti-pandemic restrictions – such as mandatory quarantine for infected individuals and their contacts – or fear-induced changes in consumer behavior (Figure 3). We exploit this regional variation in the pandemic intensity to estimate the causal impact of the "First Aid" (see section 4: Methodology and Data).

The available administrative data enables us to link firms to a particular district solely based on the company's headquarters. Therefore, we limit our analysis to small enterprises (up to 50 employees), which are less likely to operate in multiple districts compared to large and medium-sized enterprises. These small enterprises account for 98.7% of all employer firms, 26% of private sector employment and 27.1% of gross production (Slovak Business Agency 2021). Between March and September 2020, they drew 177 out of 658 million euro distributed through the "First Aid" scheme, i.e. 27% of the total support.

4 METHODOLOGY AND DATA

Our main specification takes the form:

$$Y_{ijt} = \alpha_{ij} + \alpha_t + \sum_{k=1}^{K} \left[\gamma_k C_{jt}^k + \beta_k \left(C_{jt}^k \times T_{ij}\right)\right] + U_{ijt} + \varepsilon_{ijt}, \quad (1)$$

where Y_{ijt} is the outcome variable (dismissals, to be defined below) in firm i located in district j during month t. Dummies α_{ij} and α_t are thus firm- and time- fixed effects. Polynomial terms C_{jt}^k contain the number of new coronavirus infections in district j raised to the power k up to the cubic term ($K = 3$). Coefficients γ_k thus measure the reaction profile of the outcome variable with respect to the epidemic intensity. The polynomial terms appear also in the interaction with time-invariant treatment indicator T_{ij} such that coefficients β_k capture the reaction profile for the treated firms. The unobserved components are separated into two terms, U_{ijt} and ε_{ijt} in order to allow for the presence of unobserved firm characteristics that might correlate with regressors (U_{ijt}) and the genuinely unpredictable error terms (ε_{ijt}). To account for the potentially erratic behavior of the polynomial terms, we also re-estimate (1) with step function of the epidemic intensity. Furthermore, we also consider number of new cases relative to the district population in order to account for differing population concentrations in different regions. Finally, to account for the inherent non-negativity of the dependent variable (number of jobs lost), we also re-estimate (1) as a Poisson model with fixed effects. The key results are not affected by these changes of specification (see Appendix D for further robustness checks).

The regression (1) carries the drawback of not identifying the level effect of treatment. In other words, it identifies the effect of treatment on the job losses that were caused by the intensifying epidemic but it does not resolve the effect on job losses overall, as the level of dismissals for treated and non-treated firms is normalized to be identical in the absence of new COVID infections. One might consider a more ambitious specification:

$$Y_{ijt} = \alpha_{ij} + \alpha_t + \beta_0 T_{ijt} + \sum_{k=1}^{K} \left[\gamma_k C_{jt}^k + \beta_k \left(C_{jt}^k \times T_{ijt}\right)\right] + U_{ijt} + \varepsilon_{ijt}, \tag{1*}$$

which attempts to exploit the variation in the timing of treatment to identify the level shift β_0. However, due to the presence of the selection terms U_{ijt}, the consistency of $\hat{\beta}_0$ is more dubious than the consistency of the interaction terms. Thus, even if (1*) was estimated, it would still be advisable to interpret the interaction terms only. On the relatively benign assumption of the exogeneity of the epidemic intensity (C_{jt}), we can make the following heuristic argument that the bias in β_0 will be more severe than the bias for the interaction coefficients $\beta_k\ \forall k = 1 \dots K$. Note first that bias in $\hat{\beta}_k$ depends on $\operatorname{cov}\left(C_{jt}^k \times T_{ijt}, U_{ijt} | C_{jt}^k, T_{ijt}, \Theta_{ijt}\right)$ while bias in $\hat{\beta}_0$ depends on $\operatorname{cov}\left(T_{ijt}, U_{ijt} | C_{jt}^k, \Theta_{ijt}\right)$, where Θ_{ijt} is the conditioning set consisting of the fixed effects. Estimates $\hat{\beta}_k$ thus possess the advantage of controlling for the level shift (T_{ijt}), unlike the estimate $\hat{\beta}_0$. Suppressing the indices and Θ_{ijt} to avoid clutter, we can express these covariances using the notation of Bohrnstedt and Goldberger (1969), where $\Delta X \equiv X - \mathbb{E}[X]$, as follows:

$$\begin{aligned} &\operatorname*{plim}_{N\to\infty}\left(\operatorname{cov}(CT, U|C,T)\right) \\ &\qquad = \underbrace{\mathbb{E}[C|C,T]}_{=0} \operatorname{cov}(T,U|C,T) + \underbrace{\mathbb{E}[T|C,T]}_{=0} \operatorname{cov}(C,U|C,T) \\ &\qquad + \mathbb{E}[(\Delta T)(\Delta C)(\Delta U)|C,T] = \mathbb{E}[(\Delta T)(\Delta U)(\Delta C)|C,T], \end{aligned} \tag{2}$$

while

$$\operatorname*{plim}_{N\to\infty}\left(\operatorname{cov}(T,U|C)\right) = \mathbb{E}[(\Delta T)(\Delta U)|C]. \tag{2*}$$

The endogeneity term for $\hat{\beta}_k$, $\mathbb{E}[(\Delta T)(\Delta U)(\Delta C)|C,T]$, can be expected to be closer to zero than $\mathbb{E}[(\Delta T)(\Delta U)|C]$ for two reasons: (a) as stated above, it contains a richer conditioning set, which might better sever the relationship between ΔT and ΔU, and (b) the presence of the exogenous regressor ΔC further attenuates this relationship.

As a further justification for using the time-invariant treatment indicator, it bears noting that the selection problem into treatment exists along two dimensions, namely selection into a particular form of treatment (if any), and selecting into the timing of that treatment. Thus, depending on the definition of the treatment variable, the econometric specification needs to account for four possible selection mechanisms:

i. whether a firm qualifies for any type of treatment in any period,
ii. whether a firm qualifies for any type of treatment in a specific period,
iii. whether a firm qualifies for a specific type of treatment in any period.
iv. whether a firm qualifies for a specific type of treatment in a specific period.

Therefore, if the variation in the timing of treatment was to be used, the model would have to account for the more complex selection mechanism (ii) as opposed to the simpler problem (i), which can plausibly be accounted for by firm-specific fixed effects. The even more complex selection problems (iii) and (iv) call for the multinomial selection into the various specific types of treatment. However, modelling (iii) and (iv) would only be necessary for comparisons of the effectivity of different treatment schemes. Since the plan for this paper is to evaluate the effect of the overall aid package, we will disregard the multinomial selection problem. Furthermore, comparing the magnitudes of the treatment effect would require the bias terms (2) or (2*) to be equal for each of the different treatment schemes, which is scarcely believable. To illustrate the selection problem, we estimate models with time-varying treatment indicators in Appendix C, where we show that the estimated treatment effect is notably larger in periods *after* the treatment has ended, indicating that indeed the duration of the treatment coincides with the greatest shock.

For these reasons, our preferred specification is the regression (1), where we leverage the exogeneity of the coronavirus pandemic to identify the effect of a time-invariant treatment indicator while maintaining firm-specific fixed effects to control for the selection. To the extent that the results are biased according to (2), we submit that $\mathbb{E}[(\Delta T)(\Delta U)(\Delta C)|C,T]$ has the opposite sign as the treatment effect since treated firms are expected to be more sensitive to the pandemic than the control ones, almost by construction since claiming aid was predicated in most cases in our sample (about 85%) by a firm proving loss of revenues or being closed down due to epidemiological measures. Difference-in-differences models discussed below and Appendix C further report suggestive evidence that indeed treated firms experienced negative unobserved shocks since the treatment effect appears (counterintuitively) to be larger *after* firms stopped claiming aid than during the aid period. This suggests that the period of claiming aid coincides with a negative shock which masks the effect of the intervention. Therefore, the treatment effects obtained from (1) may be attenuated by (2) but they are not likely to be exaggerated.

In order to estimate the models above, we use data on the "First Aid" granted from the Central Office of Labor, Social Affairs and Family (ÚPSVaR), which contain the amounts paid to individual companies in each month, disaggregated according to the statutory provision by which the aid was granted. The numbers of employees working in Slovak companies were drawn from the data of the Social Insurance Agency (Sociálna poisťovňa). Cases when an employee worked in a given company for only part of a month were scaled on the basis of days worked (for example, an employee hired in the middle of a given month is considered to be 0.5 employees in that month). Formally, we compute employment in firm i and month t as:

$$emp_{it} = \sum_{w=1}^{W_{it}} \min\left\{\frac{\sum_{e=1}^{E_{iw}} \text{days worked}_{eiwt}}{\text{work days in month } t}, 1\right\}, \tag{3}$$

where the subscript w indexes individual workers and W_{it} is the total number of workers of firm i and month t recorded by the *Social Insurance Agency*. One complication is that some jobs might be formally covered by multiple contracts, e.g. a cashier may also be in charge of managing the shop's inventory and hold two separate contracts corresponding to the two sets of duties, both of which are recorded in the data. To address this in equation (3), each worker w may hold E_{iw} contracts with the same firm i. Therefore, we sum days worked by person w for firm i across all of their employer-employee relationships indexed by e. This sum is truncated at unity to prevent multiple counting of jobs with multiple contracts with the same firm (same worker holding contracts with multiple firms is counted as employed in multiple jobs). Consequently, the number of dismissals is:

$$dism_{it} = |\min\{emp_{it} - emp_{it-1}, 0\}|, \tag{3*}$$

i.e. if the monthly change in employment is negative, its absolute value is considered the number of jobs lost, else the variable is recorded as zero.

The dependent variable Dismissals as defined in (3*) is open to the objection that it ignores potential replacements of the dismissed workforce. This objection, however, only matters in firms where the replacement takes more than a month to complete, since (3*) constructs dismissals from the monthly changes of days worked. If, for instance, a worker finds a better job, then the employer can use the notice period to find a replacement and thus maintain the number of workers from one month to the next even though the job is held by a different person. However, to address this limitation, we construct an alternative dependent variable from inter-year comparisons (Jurajda and Doleželová, 2021) of days worked as follows:

$$\text{days worked}_{it}^{y} = \sum_{w=1}^{W_{it}} \min\left\{\sum_{e=1}^{E_{iw}} \text{days worked}_{eiwt}, \text{work days in month } t\right\}. \tag{4}$$

which enables us to construct:

$$\text{days lost}_{it} = \left|\min\left\{\text{days worked}_{it}^{2020} - \text{days worked}_{it}^{2019}, 0\right\}\right| / \text{days worked}_{it}^{2019}, \tag{4*}$$

The losses of work days in month t are calculated as the difference between person-days worked in month t in 2020 and the person-days worked in month t in 2019. Thus, (4*) measures employment losses against the non-crisis year 2019. Condition (4) is analogous to (3) as it prevents counting simultaneous contracts with the same employer. Both measures (3*) and (4*), of course, potentially include persons that found positions in other firms, so estimates are interpreted as effects on firms, rather than the whole economy.

In an attempt to identify level effects that are eliminated in the baseline model, we proceed in a way suggested by Wooldridge (2021) and estimate difference-in-differences model of the following general form

$$Y_{ijt} = \alpha_{ij} + \alpha_t + \beta_d \mathbb{I}[\text{During treatment}_{ijt}] + \beta_a \mathbb{I}[\text{After treatment}_{ijt}] + \theta_{ij} M_{jt} + \varepsilon_{ijt}, \tag{5}$$

where dummies $\mathbb{I}[\text{During treatment}_{ijt}]$ and $\mathbb{I}[\text{After treatment}_{ijt}]$ take value of one if firm i located in district j is receiving treatment in period t or has completed treatment prior to period t. Since the timing of treatment is likely to be dictated by firm-specific shocks that are unobserved in the data, we attempt to control for this dynamic firm-specific heterogeneity by allowing for firm specific responses to changes in mobility in the district ($\theta_{ij} M_{jt}$). Mobility data M_{jt} are taken from *Google COVID-19 Mobility Reports*[3], from which we use the Workplace mobility, which is the best reported in Slovakia. M_{jt} is thus the percentage change in people's

[3] https://www.google.com/covid19/mobility/

movements to their places of work compared to the median mobility in the pre-pandemic period of January 3 – February 6, 2020 and thus gives us a proxy for the impact of the restrictive measures. To avoid the need to estimate nearly 400,000 parameters θ_{ij}, we use the "pre-filtering" method suggested by Wooldridge (2021), whereby we project the dependent variable and the treatment dummies on M_{jt} firm-by-firm and use the filtered values in regression and use cluster-robust standard errors. Given the short observation window of 7 months, we cannot invoke large-T asymptotics along the lines of Kneip, Sickles, and Song (2012) that would allow us to estimate more flexible firm-specific trends. It may be noted that the IV strategy utilizing regional variation in leniency in granting aid (Kopp & Siegenthaler, 2021) is unavailable to us as regional offices distributed workload among themselves and so the plausibly exogenous association between firm and the leniency of its Office of Labor is disrupted in our data.

In addition to the model of job losses, we provide models of the survival differential between treated and control firms. This is accomplished by approximating the hazard function in month t, $\lambda(t)$, by the following proportional hazard model:

$$\begin{aligned}\lambda(t) &\equiv \mathbb{P}\left[\text{failure time}_{ij} = t \,|\, \text{failure time}_{ij} \geq t\right] \\ &\approx \left[1 + \exp\left(-\left(g(t) + \beta T_{ij} + \mathbf{X}_{ijt}\boldsymbol{\delta}\right)\right)\right]^{-1},\end{aligned} \tag{6}$$

where failure time_{ij} measures the time of the failure of firm i located in district j, which might be subject to censoring.[4] The hazard is approximated by a logistic function, in which we control for duration dependence by a flexible function ($g(t)$), which can be a spline or monthly dummies. Confounding firm characteristics are collected in the matrix $\mathbf{X}_{ijt}$ that includes dummies for discretized bins of revenue, labor productivity, and indebtedness; regional dummies according to ISO 3166-2:SK, NACE indicators, the intensity of the coronavirus epidemic; and days lost between 2018 and 2019. The parameter vector $\boldsymbol{\delta}$ thus captures the impact of these control variables. The results are similar if we vary the controls and use smooth duration dependence as opposed to the fixed effects (Royston & Parmar, 2002). The parameter β thus measures the impact of the treatment (T_{ij}) holding the observed confounders constant. We opt for time-invariant treatment indicator for the reasons stated above and in order to achieve a clearer separation of the treatment and control group. Having a time-varying treatment indicator would create compositional shifts in these groups as firms switch from treatment to control and vice versa. Furthermore, selectivity is expected to be much more pronounced once timing of treatment is introduced (see below).

Variables related to company characteristics (location, sales, liabilities, Altman's Z-score) were drawn from the FinStat database. Data on the number of new cases of COVID-19 infection come from a database published by the Institute for Healthcare Analyses of the Ministry of Health of the Slovak Republic.[5] We use the *PositiveTests_District* panel, which contains daily data on the number of new cases detected in a particular district (LAU 1 level), with the exception of the metropolitan areas of Bratislava and Košice, which are considered "districts" regardless of their actual division into districts by boroughs. We aggregate these daily case counts into a monthly total, which we use as a proxy variable for the intensity of the epidemic in the district. The conclusions do not change if we use the share of new cases per population of the district instead of the absolute number of cases, cf. Appendix D (we extracted the population data from the Statistical Office of the Slovak Republic, panel [om7102rr]).

[4] We constrain the sample to firms with fewer than 50 employees. Larger firms operating in multiple regions, may be difficult to associate with the regional epidemic intensity and NUTS dummies.
[5] https://github.com/Institut-Zdravotnych-Analyz/covid19-data

The resulting panel dataset contains 2,290,230 firm-month observations covering the time span between March and September 2020 and includes 394,857 firms employing fewer than 50 employees. The control group consists of 379,984 firms and the treated group covers the remaining 14,873 entities. The FinStat database records the dates in which a firm formally ended its operations, however, these account for only 4,441 cases in the observed period. More commonly, a firm is recorded as functioning but with zero employees. Since the object of this study is to model events leading to job losses, we expand the definition of firm "failure" to include cases when a firm fired at least 90% of its workforce within a month as well as formal cessation of operations. Under this definition, we observe 14,873 failure events in the sample.

5 RESULTS

Proceeding now to the analysis of the firms' reaction profiles to the intensity of the coronavirus epidemic, it is apparent that the treatment resulted in a reduction of dismissals during the periods of higher epidemic intensity compared to the baseline of zero COVID cases per month. Figure 4 shows graphically the key empirical results. At zero coronavirus cases per month, treated and control groups coincide by construction since the treatment dummy is time-invariant and thus it is absorbed by the firm fixed effects. Encouragingly, there is little difference in the dismissal trajectory between the treated and control groups at low levels of infection rates (under 250 cases per district and month). This suggests that when the impact of the epidemic was mild, both groups tended to behave in a similar way. Once the epidemic starts to intensify, however, control firms tend to increase their dismissals, if slightly. The low sensitivity of the control firms is hardly surprising as these firms probably were the least affected by the shock, else they would have been treated. On the other hand, we see a sharp decrease in the dismissals within the treated group, opening a statistically significant wedge between the two groups.

At this point, it might be objected that comparing dismissal rates by treated and control firms is a tautology as treated firms were obligated to retain the supported workers. However, since the estimand in this model is not the average difference in dismissals by the treated and control firms but rather the difference in *responses* to the intensifying epidemic, this objection is largely disarmed. One might very well expect that treated firms would exhibit fewer dismissals on average, but they might be dismissing non-supported workers in response to the worsening epidemic conditions. According to the results above, this scenario has not occurred, which indicates that the treatment was effective at preserving endangered jobs.

We also consider the question whether more vulnerable firms within the treated group benefited more from the intervention than others. To that end, we utilized growth rates in days worked in a given month between 2018 and 2019 in order to create a proxy for the overall condition of the business (see Appendix D for alternative measurement using Altman Z-score).

The results indicate that there is limited evidence of treatment effect heterogeneity, i.e. all treated firms benefitted by roughly the same amount. Encouragingly, the reaction curves for the control group are also relatively homogeneous, which indicates that the non-eligible firms were truly those least affected by the epidemic and the attendant restrictions. Had we observed a pronounced rise in job losses among vulnerable control firms, it might have been an indication of improper exclusion of these firms from the treatment. Similarly, if the most prosperous treated firms exhibited significantly flatter job loss profile than the other treated firms, there would have been evidence that the aid targeted firms, which did not need support. Our data, however, indicate that, on average, the aid was targeted successfully at firms most affected by the crisis.

Figure 4: Dismissals (at the mean of the firm fixed effects) as a function of treatment and epidemic intensity (95% Cis using standard errors clustered by 5-digit NACE codes)

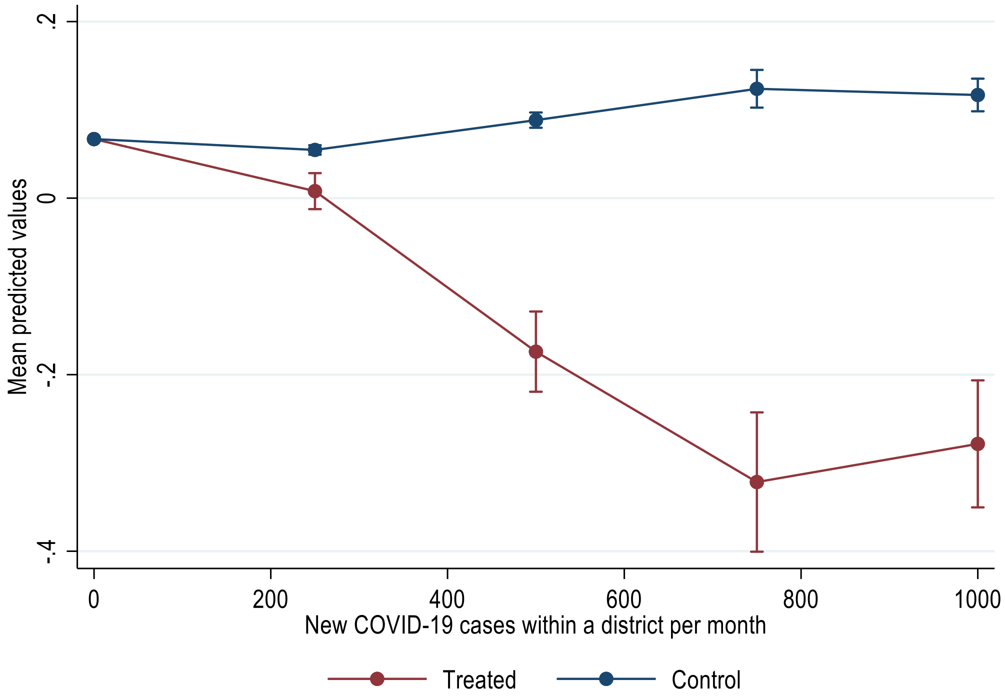
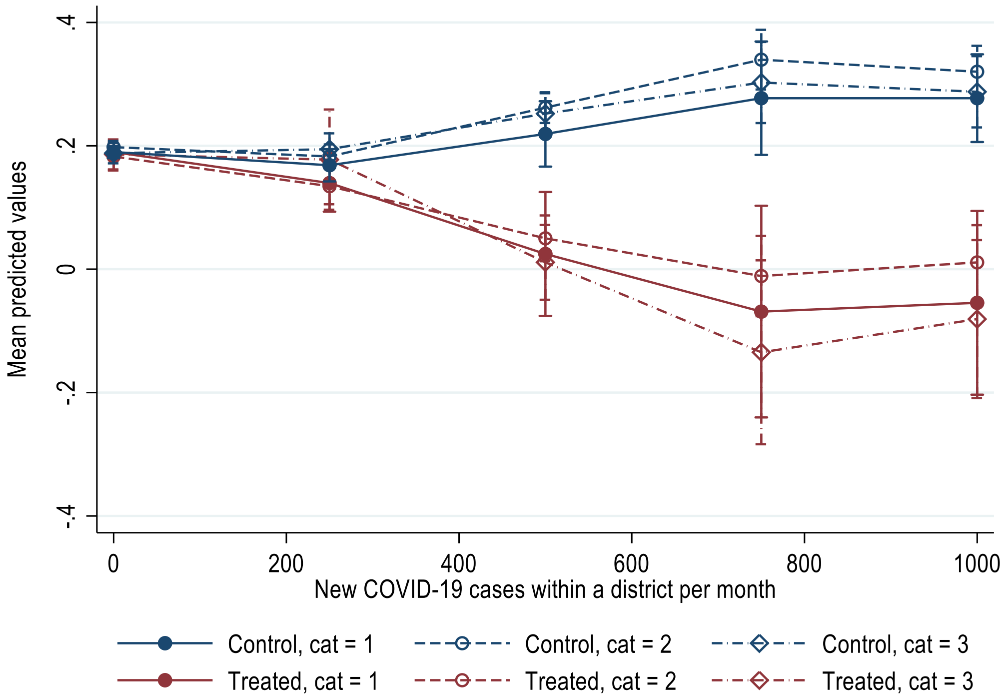
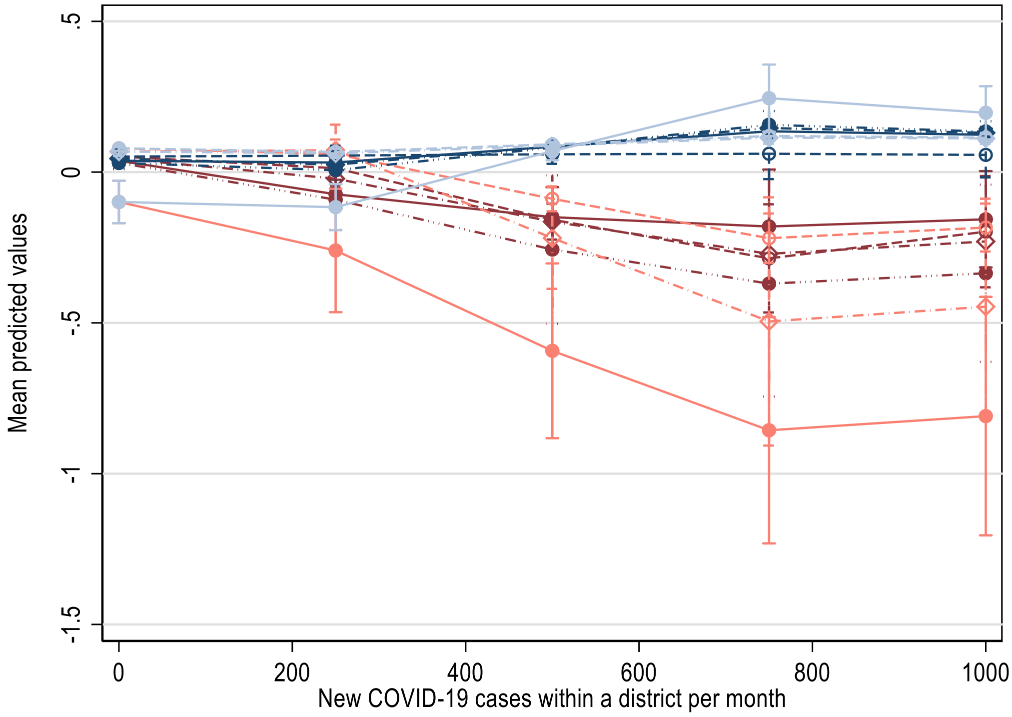


Figure 5: Dismissals (at the mean of the firm fixed effects) as a function of treatment and epidemic intensity for different categories of the 2018-2019 employment growth (cat 1=firms increasing days worked by 10% or more; cat 2 = firms changing their days worked between -10% and +10%; cat 3=firms losing 10% or more of work days compared to the year before)



A related concern might be that different sectors experienced different treatment effects. To that end, we re-estimated the baseline model augmented by an interaction with the NACE sector indicators. Figure 6 shows a rather noisy but similar picture to the previous three. It is of note that one of the most heavily affected sectors, Food and Accommodation, is shown to have benefitted the most from the intervention. However, the confidence intervals are wide, probably due to over-parametrization of the model, so identifying sector-specific treatment effects would be difficult.

Figure 6: Dismissals (at the mean of the firm fixed effects) as a function of treatment and epidemic intensity for different NACE sectors (sectors with fewer than 2,000 treated firms are omitted). Treated firms in red, Control firms in blue with analogous sector indication (95% Cis using standard errors clustered by 5-digit NACE codes).



Treated group:

C Manufacturing
F Construction
G Wholesale and retail; auto repair
H Transportation & storage
I Food & Accommodation
M Professional & technical activities
N Administrative & support activities

Control group:

C Manufacturing
F Construction
G Wholesale and retail; auto repair
H Transportation & storage
I Food & Accommodation
M Professional & technical activities
N Administrative & support activities

In addition to the baseline model, Table 1 shows result for estimates of the alternative specification, in which we use inter-year losses of days worked (4*) and a discretization of the epidemic intensity as opposed to polynomial version. Here we also see clearly that treated firms responded to intensifying epidemic situation with smaller losses of days worked compared to the control group. Compared to the baseline regime of 0 – 100 COVID cases within a district, treated firms lost 0.025 fewer workers in the 600 – 1200 cases regime (column 1). Poisson model (column 2) indicates that treated firms lost about 14% (i.e. 1-exp(-0.156)) fewer days worked in a month. Similar results are yielded by models with cases of COVID infections per district population, which provides strong indications of the robustness of our results to specification changes (see Appendix C for further robustness checks). It is also of note that all models indicate that control firms experienced higher job losses with intensifying epidemic, and so equality between treated and control groups' reaction profiles is rejected in all models considered.

Table 1: Regression coefficients with discretisation of the COVID-19 monthly cases per district. Dependent variable = Losses of work days in (4*). The bin [0,100) COVID-19 cases is the omitted reference category. OLS = linear model estimated by least squares; Poisson = Poisson quasi-maximum likelihood with the logarithmic link; COVID intensity = specification of the proxy intensity of the COVID-19 epidemic. Test (p) = p-value for the joint test of equality between estimated coefficients for the Treated group and the Control group; N Treated = number of treated firms; N Control = number of control firms; N Clusters = number of clusters used for calculating cluster-robust variance-covariance matrix; N Firms = total number of firms; N Obs = total number of observations (firm×months).

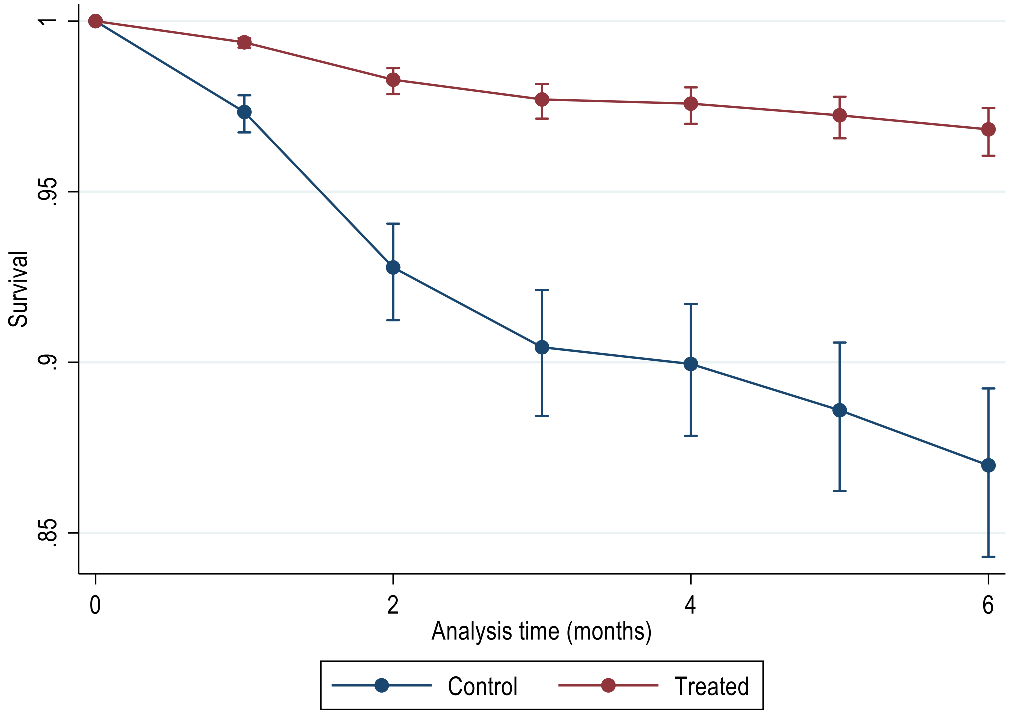
	(1)	(2)	(3)	(4)
Model:	OLS	Poisson	OLS	Poi
COVID intensity:	Cases	Cases	Cases/Population	Cases/Population
Treated×[100-300)	-0.0249***	-0.0920***	-0.0180***	-0.114***
	(0.002)	(0.013)	(0.002)	(0.015)
Treated×[300-600)	-0.0252***	-0.144***	-0.0292	-0.447
	(0.002)	(0.019)	(0.026)	(0.332)
Treated×[600-1200)	-0.0250***	-0.156***	-0.0319***	-0.228**
	(0.002)	(0.022)	(0.010)	(0.092)
Control×[100-300)	0.00448***	0.0545***	0.00397***	0.0840***
	(0.001)	(0.008)	(0.001)	(0.010)
Control×[300-600)	0.00677***	0.136***	0.0110***	0.334***
	(0.001)	(0.014)	(0.004)	(0.128)
Control×[600-1200)	0.00946***	0.198***	0.00300	0.0813*
	(0.001)	(0.018)	(0.002)	(0.044)
Firm FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
Test (p)	<0.001	<0.001	<0.001	<0.001
N Treated	31,158	20,371	31,158	20,371
N Control	364,549	43,106	364,549	43,106
N Clusters	612	574	612	574
N Obs	2,725,680	444,269	2,725,680	444,269

standard errors in parentheses clustered by 5-digit NACE codes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Having surveyed the impact on job retention in response to the intensification of the COVID-19 spread, we proceed with survival analysis of firms during the first wave of the epidemic. Examining the covariate-adjusted survival functions[6] (survival in month $t = \prod_{s=1}^{t}(1-\lambda(s))$) shows that even after adjusting for firm-specific confounders, treated firms were much less likely to fail than controls by a statistically significant margin.

Figure 6: Covariate-adjusted survival curves with 95% CIs

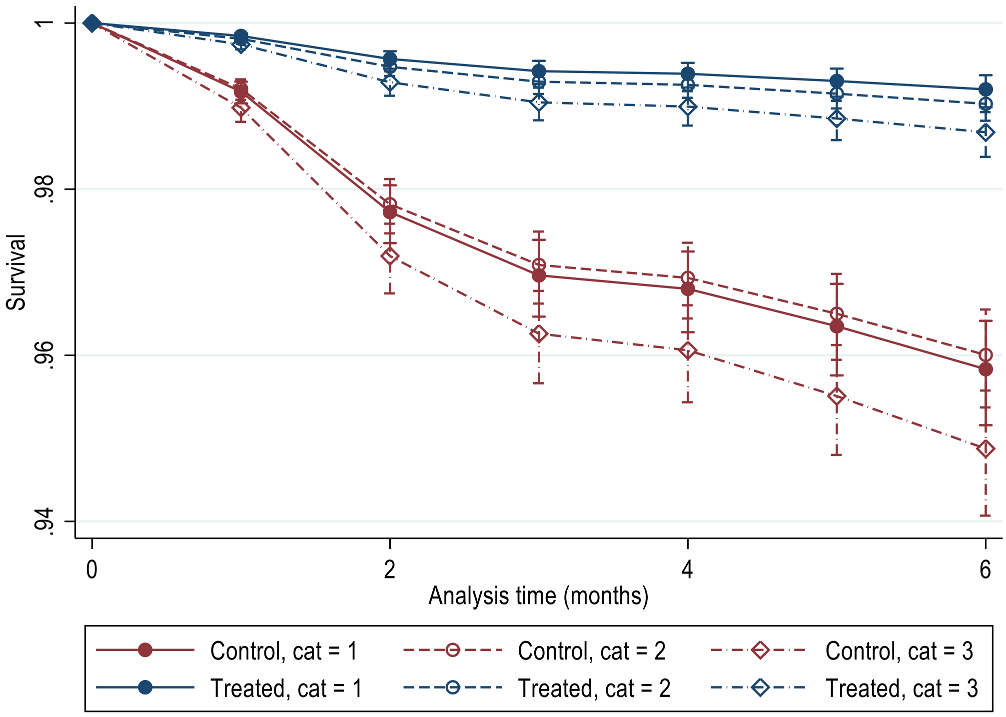


Assuming a scenario in which the epidemic and treatment schemes continue as in the first wave, it is possible to extrapolate the survival curves and compute the unrestricted (i.e. non-censored) mean survival times. While the unrestricted extrapolations based on just 6 months of data are somewhat specification-sensitive, we found that the treated group was expected to survive roughly 5 times as long as the control group with statistical significance under 1%. The statistical significance of the treated group's survival advantage is similar whether we use the discrete Cox specification or the smoothed Royston-Parmar model. Similarly, clustering by NACE sectors did not change the standard errors in an appreciable way compared to the classical MLE standard errors.

These results indicate that the magnitude of the treatment was sufficient to produce a meaningful impact on the survival of Slovak firms. While the treated group still experienced a non-negligible failure rate throughout the observation period, the failure rates of the control group were much more pronounced. These estimates admit causal interpretation because the treated group is expected to be more vulnerable to the COVID-19 crisis than the control group and yet the treated firms survived longer than the controls. Therefore, the estimated survival advantage of the treated firms can be attributed to the intervention with confidence because the unobserved heterogeneity would bias them towards shorter duration. In other words, the estimated effect of the First Aid package is attenuated, if anything.

[6] Control variables from Finstat database contain many missing variables, thereby severely limiting the sample. However, the conclusions remain unchanged if we use the full sample sans controls (see Appendix B).

Figure 7: Covariate-adjusted survival curves with 95% Cis for for different categories of the 2018-2019 employment growth (cat 1=firms increasing days worked by 10% or more; cat 2 = firms changing their days worked between -10% and +10%; cat 3 = firms losing 10% or more of work days compared to the year before)



Finally, we note that the survival advantage, like the job retention profiles, is little affected by the degree of financial distress as measured by the Altman Z-score (using liabilities-to-revenue ratio yields similar results), Again, this can be interpreted as an indication of successful targeting of the aid as firms' survival depended more heavily on their exposure to the COVID-19 shock rather than their financial health. Perhaps the most financially distressed control firms faced somewhat higher risk of failure (their baseline hazard is higher than that of the most "prosperous" firms by a statistically significant margin) but their higher failure rate is expected a priori so it is difficult to interpret their survival disadvantage as evidence that aid should have been directed at these firms.

6 CONCLUSIONS

This paper adds to the scarce micro-level evidence on the causal effect of job retention schemes on firm-level dismissals, employment, and establishment survival (Cahuc et al. 2021; Giupponi and Landais 2018; Kopp and Siegenthaler 2021). More precisely, we evaluate the causal impact of the Slovak job retention scheme "First Aid" on dismissals and survival of small firms during the COVID-19 pandemic.

We ask three inter-related questions. First, we ask whether the scheme helped to preserve jobs. To answer this question, we exploit exogenous variation in the pandemic intensity at the district (LAU 1) level to estimate firms' reaction profile to the intensity of the coronavirus epidemic. Our findings indicate that there was a significant difference in sensitivity to the intensifying epidemic between supported and unsupported firms. When the COVID-19 cases in the region started increasing, unsupported firms increased dismissals only slightly. This implies that unsupported firms were relatively unaffected by the shock and did not require the support. In contrast, the financial support significantly reduced the number of dismissals among the supported firms, especially once the number of new coronavirus cases exceeded 500 per district. Based on the above, it is reasonable to conclude that the job retention scheme helped save endangered jobs. As our preferred specification does not enable us to identify the level effect of treatment, we attempt to address this question by estimating a difference-in-differences model (see Wooldridge 2021) with firm specific responses to changes in workplace mobility in the particular district. Our results indicate that over the examined 7-month period, the Slovak job retention scheme saved about 0.1 jobs a month per firm after the termination of the support. Thus, our findings lend credibility to early micro-simulation studies (see Scarpetta et al. 2020 or Lalinsky and Pal 2020) and impact evaluations (Aiyar and Dao 2021).

Second, there is an on-going debate about the effectiveness of temporary wage subsidies, which are non-targeted and therefore could end up subsidizing also firms which were not hit by negative shocks (Blanchard et al. 2020; Giupponi et al. 2021; OECD 2021; for a discussion about windfall effects of the short-time work arrangement, see Cahuc et al. 2021). The Slovak case provides a unique opportunity to examine the effectiveness of wage subsidies in more detail, as these constituted the bulk of the financial support distributed through the "First Aid" scheme. To this end, we examine the treatment effect heterogeneity and assume the following. First, if the better managed or financially sound firms in the group of supported firms showed lower sensitivity to the worsening pandemic situation than the worse managed or financially distressed firms, one could argue that the support was provided to firms, which did not need it. Second, if the worse managed or financially distressed firms in the unsupported group showed greater sensitivity to the worsening pandemic situation, one could argue that the support failed to target firms, which needed it. However, we do not observe this in our data. The treatment effect was homogeneous, i.e. firms benefited from participation in the scheme equally, regardless of their overall condition. The sensitivity curves of the control group are also homogeneous. Finally, we do observe some heterogeneity when looking at different sectors: the heavily affected Food and Accommodation sector benefitted the most from the support. Therefore, it is possible to conclude that the job retention scheme managed to target the firms in need despite the speedy distribution of the aid.

Finally, the size of the overall fiscal stimulus package in Slovakia relative to its GDP is significantly smaller compared to the most advanced economies (IMF 2021). This raises the question whether the job retention scheme was big enough. To explore this question, we conduct a survival analysis and show that supported firms were significantly less likely to fail than unsupported firms. This indicates that the magnitude of the financial support was robust enough to reduce the risk of firm failure. These results are encouraging especially from the perspective of the newly institutionalized 'Kurzarbeit' scheme in Slovakia, which was largely modelled on the experience with the short-time work measures 1 and 3 of the "First Aid" scheme and will take effect in March 2022. It is reasonable to assume that in case of a crisis comparable to the first wave of the COVID-19 pandemic, the institutionalized 'Kurzarbeit' will effectively save jobs and protect worker incomes.

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Appendix A: Job retention scheme in Slovakia

Measure	Eligibility	First Aid	First Aid+	First Aid++
		(March 12, 2020 - September 2020; September - December 2021)	(October 2020 - January 2021)	(February - July 2021), measure 3B unavailable in July 2021
1	Employers who were forced to shut down their operations based on the measures of the Slovak Public Health Authority; employers with furloughed workers.	80% of the employee's average salary (max. 1 100 €)	80% of the employee's average salary (max. 1 100 €)	100% of the employee's average salary (max. 1 100 €)
2	Self-employed persons with a drop in revenues	90 € to 270 € per worker in March 2020, 180 € to 540 € thereafter, depending on the extent of revenue drop, up to 80% of the average employee's wage	270 € to 810 € per worker, depending on the extent of revenue drop	330 € to 870 € per worker, depending on the extent of revenue drop
3A	Activity of employers affected by economic slowdown; employers with furloughed workers	Up to 80% of the employee's average salary (max. 880 €)	Up to 80% of the employee's average salary (max. 1100 €)	100% of the employee's average salary (max. 1 100 €)
3B	Employers with a drop in revenues	90 € to 270 € per worker in March 2020, 180 € to 540 € thereafter, depending on the extent of revenue drop, up to 80% of the average employee's wage	270 € to 810 € per worker, depending on the extent of revenue drop	330 € to 870 € per worker, depending on the extent of revenue drop
4A	Self-employed persons	Flat contribution of 210 € (105 € in March 2020)	Flat contribution of 315 €	Flat contribution of 360 €
4B	Single-member private limited liability company	Flat contribution of 210 € (105 € in March 2020)	Flat contribution of 315 €	Flat contribution of 360 €

Appendix B: Parameter estimates from survival models

	(1) Cox	(2) R-P	(3) Cox	(4) R-P	(5) Cox	(6) R-P	(7) Cox	(8) R-P
Treat=1	0.780**	0.774***	0.651***	0.647***	0.236***	0.231***	0.192***	0.188***
	(-2.37)	(-7.55)	(-7.31)	(-12.73)	(-22.96)	(-31.26)	(-15.11)	(-15.96)
Gr cat=2					0.986	0.979	0.966	0.958
					(-0.44)	(-0.64)	(-1.07)	(-1.21)
Gr cat=3					1.278***	1.269***	1.244***	1.236***
					(6.77)	(6.40)	(5.46)	(5.44)
treat=1 × Gr cat = 2							1.272*	1.273*
							(1.92)	(1.94)
treat=1 × Gr cat = 3							1.345**	1.336**
							(2.26)	(2.20)
Duration dep.	FE	Spline	FE	Spline	FE	Spline	FE	Spline
Reg FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes
NACE FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	No	No	No	No	Yes	Yes	Yes	Yes
Clusters	612	—	612	—	567	—	567	—
Subjects	394,857	394,857	394,857	394,857	83,837	83,837	83,837	83,837
Failures	14,873	14,873	14,873	14,873	6,373	6,373	6,373	6,373
N	2,290,230	2,290,236	2,290,230	2,290,230	466,910	466,910	466,910	466,910

Exponentiated coefficients; *t* statistics in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Exponentiated coefficients indicate the multiplicative impact of a covariate on the failure hazard. Cox = Cox proportional hazard model; R-P = Royston-Parmar proportional hazard model; Treat = time-invariant treatment dummy; Gr cat = category of growth rate in employment between 2018-2019 (cat 1 = more than 10% increase in days worked; cat 3 = more than 10% loss in days worked), where 1 indicates the most financially sound firms (base category), 2 average firms, and 3 indicates firms in financial distress; Duration dep. = specification for duration dependence, where "FE" indicates that monthly dummies are partialled out and "Spline" indicates a cubic spline; Reg FEs: dummies for NUTS 3 regional divisions; NACE FEs = dummies for level 1 sectoral divisions by NACE classification; Other controls = terciles of labor productivity and revenues for previous year. Cox model was estimated with cluster-robust standard errors with clusters by the 5-digit NACE classification, R-P models use classical standard errors for comparison.

Appendix C: Level effects and selection

Here we present results from regressions of the dismissals on time-varying treatment dummy plus a dummy for the period after the treatment stopped for firms in which we observe cessation of "First Aid" during the sample period as in (5) but we disaggregate the results by the Measure through which the aid was approved.

The results clearly indicate the selection problem we discussed in the main text, namely, in all three Measures (Opatrenia, see Appendix A) of "First Aid" we find that the impact was more pronounced *after* the firm had stopped receiving aid. This counter-intuitive result is readily explained by the fact that during the treated period firms experienced the greatest (unobserved) shock and thus the aid package had seemingly weaker effect. By contrast, once the firm was no longer eligible for treatment (i.e. the immediate shock has passed), we observe a distinct advantage among treated firms compared to the control group. It might also be noted that Measure 1 was intended for firms forced into temporary shutdown and thus it presents the most difficult challenge in finding a counterfactual from the control group, which may be why job losses relative to 2019 appear higher in the treated group. However, the opposite holds for month-to-month job losses.

Monthly job losses (absolute)

Measure 1	(1)	(2)	(3)	(4)
During treatment	-0.148***	-0.143**	-0.205***	-0.198***
	(0.057)	(0.056)	(0.057)	(0.057)
After treatment	-0.669***	-0.623***	-0.549***	-0.517***
	(0.062)	(0.061)	(0.052)	(0.051)
Firm FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Firm trends	No	No	Yes	Yes
Clusters	608	608	608	608
Treated firms	9,487	9,487	9,485	9,485
Control firms	364,549	364,549	361,988	361,988
N	2,573,995	2,573,995	2,568,546	2,568,546

Measure 3A	(1)	(2)	(3)	(4)
During treatment	-0.330***	-0.307***	-0.171***	-0.158***
	(0.049)	(0.048)	(0.025)	(0.024)
After treatment	-0.589***	-0.537***	-0.260***	-0.239***
	(0.055)	(0.054)	(0.033)	(0.033)
Firm FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Firm trends	No	No	Yes	Yes
Clusters	608	608	608	608
Treated firms	5,132	5,132	5,132	5,132
Control firms	364,549	364,549	361,988	361,988
N	2,543,509	2,543,509	2,538,074	2,538,074

Measure 3B	(1)	(2)	(3)	(4)

During treatment	-0.176***	-0.159***	-0.0673***	-0.0602***
	(0.036)	(0.035)	(0.013)	(0.013)
After treatment	-0.474***	-0.420***	-0.166***	-0.150***
	(0.044)	(0.042)	(0.021)	(0.020)
Firm FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Firm trends	No	No	Yes	Yes
Clusters	612	612	612	612
Treated firms	21,862	21,862	21,852	21,852
Control firms	364,549	364,549	361,988	361,988
N	2,660,618	2,660,618	2,655,113	2,655,113

Year-to-year job losses (relative)

Measure 1	(1)	(2)	(3)	(4)
During treatment	0.00602	-0.000388	0.0209***	0.0143*
	(0.009)	(0.009)	(0.008)	(0.008)
After treatment	-0.00714	-0.0127	0.0304***	0.0203**
	(0.009)	(0.008)	(0.008)	(0.008)
Firm FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Firm trends	No	No	Yes	Yes
Clusters	608	608	608	608
Treated firms	9,487	9,487	9,485	9,485
Control firms	364,549	364,549	361,988	361,988
N	2,573,995	2,573,995	2,568,546	2,568,546

Measure 3A	(1)	(2)	(3)	(4)
During treatment	-0.0146***	-0.0168***	0.00192	-0.00281
	(0.004)	(0.004)	(0.004)	(0.004)
After treatment	-0.0279***	-0.0269***	-0.00848**	-0.0117***
	(0.004)	(0.004)	(0.004)	(0.004)
Firm FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Firm trends	No	No	Yes	Yes
Clusters	608	608	608	608
Treated firms	5,132	5,132	5,132	5,132
Control firms	364,549	364,549	361,988	361,988
N	2,543,509	2,543,509	2,538,074	2,538,074

Measure 3B	(1)	(2)	(3)	(4)

During treatment	-0.0104***	-0.0122***	0.00116	-0.00222
	(0.002)	(0.002)	(0.002)	(0.002)
After treatment	-0.0381***	-0.0365***	-0.0180***	-0.0200***
	(0.003)	(0.003)	(0.002)	(0.002)
Firm FE	Yes	Yes	Yes	Yes
Month FE	No	Yes	No	Yes
Firm trends	No	No	Yes	Yes
Clusters	612	612	612	612
Treated firms	21,862	21,862	21,852	21,852
Control firms	364,549	364,549	361,988	361,988
N	2,660,618	2,660,618	2,655,113	2,655,113

Appendix D: Robustness to specification changes

Here we test the robustness of our results around four axes: (a) changes from the polynomial specification of the COVID-19 cases to a parsimonious step function; (b) changes from the absolute number of cases to the number of COVID-19 cases relative to the district population; (c) change of the dependent variable from the number of dismissals to the number of workers, including contractors; and (d) change from the linear specification to the Poisson model with a logarithmic link which takes into account the intrinsic non-negativity of the dependent variable.

The equation to be estimated is analogous to the one in the main text:

$$\mathbb{E}\left[Y_{ijt}|\Theta_{ijt}\right] = h^{-1}\left\{\alpha_{ij} + \alpha_t + \sum_{k=1}^{K} \gamma_k \mathbb{I}\left[C_{jt} \in \mathcal{B}_k\right] + \beta_k\left(\mathbb{I}\left[C_{jt} \in \mathcal{B}_k\right] \times T_{ij}\right)\right\}, \tag{D1}$$

where $\mathbb{E}\left[Y_{ijt}|\Theta_{ijt}\right]$ is the expected value of the outcome variable (dismissals or employment) in firm i located in district j during month t conditional on the set Θ_{ijt}. Inverse link function (h^{-1}) is identity in the OLS specification and exponential in the Poisson model. Dummies α_{ij} and α_t are firm- and time- fixed effects. Dummy variables $\mathbb{I}\left[C_{jt} \in \mathcal{B}_k\right]$ take the value of one if the number of new coronavirus infections (possibly normalised by the district population) in district j, C_{jt}, lies within a pre-specified bin $\mathcal{B}_k$ and zero otherwise. Just as in the main text, coefficients γ_k thus measure the sensitivity of the outcome variable with respect to the epidemic intensity for the control group. Similarly, coefficients β_k for the $\mathbb{I}\left[C_{jt} \in \mathcal{B}_k\right]$ dummies interacted with time-invariant treatment indicator (T_{ij}) capture the sensitivity of the control firms.

Since the number of cases in the observed period skews left (see Figure 3 in the main text), we selected the partitions of the case numbers in “Four bins” specifications ($K = 4$) partition as follows: [0,100), [100,300), [300,600), and [600,1300). “Four bins” specifications are comparable to the model in the main text as the number of parameters estimated for each reaction profile are the same since cubic polynomials uses 3 coefficients for each group of firms (6 in total) and here we use 3 dummies for each group (the fourth being reference category). “Six bins” specifications ($K = 6$) partition the cases into groups: [0,100), [100,200), [200,300), [300,400), [400,800), and [800,1300). These partitions are used both for the absolute numbers of new COVID-19 cases as well as number of COVID-19 cases per 100,000 persons living in the district. Using a step-function instead of a polynomial has a theoretical advantage as it avoids the erratic behavior of polynomials, especially at the tails of the distribution related to the “Runge phenomenon.” Step functions can also be directly tabulated as the coefficients are meaningful without the need to plot the polynomial.

Poisson specifications might be thought preferable to OLS as they disallow non-negative predicted values. The drawback of the Poisson fixed-effects regression is the need to discard firms that did not dismiss any employees, which raises questions of power reduction and sample selection. Using the number of workers as the dependent variable reduces this problem, but not entirely. However, the results show that while Poisson models have wider confidence intervals, the results are consistent with the linear models.

Finally, we consider the question of dismissals of contractors, i.e. workers who are not firms’ employees. Arguably, these workers might face the brunt of the COVID-19 shock as firms might find it easier to decline renewing these contracts as opposed to firing employees. On the other hand, contractors are often hired on a short-term basis so their numbers may be subject to fluctuations that are unrelated to the COVID-19 shock, which is why they are not included in the regressions in the main text.

Table D2: Regression coefficients with discretization of the COVID-19 monthly cases per district. Dependent variable = Dismissals. "Four bins" specifications partition the cases into groups: [0,100), [100,300), [300,600), and [600,1300). "Six bins" specifications partition the cases into groups: [0,100), [100,200), [200,300), [300,400), [400,800), and [800,1300). The bins the smallest number of COVID-19 cases are the omitted reference category. OLS = linear model estimated by least squares; Poi = Poisson quasi-maximum likelihood with the logarithmic link; COVID intensity = specification of the proxy intensity of the COVID-19 epidemic, where "Absolute" means that absolute numbers of new cases were used and "Relative" indicates the usage of new cases per 100,000 persons living in a district. Test (p) = p-value for the joint test of the significance of the interactions between the treated dummy and bins of COVID-19 cases; N Treated = number of treated firms; N Control = number of control firms; N Clusters = number of clusters used for calculating cluster-robust variance-covariance matrix; N Firms = total number of firms; N Obs = total number of observations (firm×months).

Panel A: Specifications with four bins

	(1)	(2)	(3)	(4)
Model:	OLS	Poi	OLS	Poi
COVID intensity:	Absolute	Absolute	Relative	Relative
Treated×[100-300)	-0.0722***	-0.195***	-0.123***	-0.140***
	(-6.44)	(-4.25)	(-9.61)	(-2.71)
Treated×[300-600)	-0.203***	-0.197***	-0.305**	-0.664
	(-10.66)	(-2.85)	(-2.48)	(-1.62)
Treated×[600-1200)	-0.189***	-0.163	-0.195***	-0.270
	(-7.87)	(-1.57)	(-5.34)	(-1.41)
Control×[100-300)	0.00220	0.0733*	0.0163***	0.111***
	(1.09)	(1.79)	(5.43)	(3.09)
Control×[300-600)	0.0193***	-0.0263	0.0106	0.115
	(5.49)	(-0.44)	(0.97)	(0.37)
Control×[600-1200)	0.0214***	0.0832	0.00237	0.0607
	(6.19)	(1.15)	(0.34)	(0.51)
Firm FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
Test (p)	<0.001	0.003	<0.001	0.009
N Treated	31,158	21,204	31,158	21,204
N Control	364,549	51,171	364,549	51,171
N Clusters	612	583	612	583
N Obs	2,725,680	506,304	2,725,680	506,304

Panel B: Specifications with six bins

	(5)	(6)	(7)	(8)
Model	OLS	Poi	OLS	Poi
COVID intensity:	Absolute	Absolute	Relative	Relative
Treated×[100-200)	-0.0558***	-0.200***	-0.112***	-0.154***
	(-5.09)	(-4.02)	(-9.85)	(-3.01)
Treated×[200-300)	-0.133***	-0.167**	-0.135***	-0.128
	(-8.28)	(-2.09)	(-7.40)	(-1.46)
Treated×[300-400)	-0.152***	-0.105	-0.305**	-0.664

	(-3.01)	(-0.35)	(-2.48)	(-1.62)
Treated×[400-800)	-0.197***	-0.201***	-0.217***	-0.354
	(-10.44)	(-2.88)	(-4.59)	(-1.50)
Treated×[800-1300)	-0.183***	-0.166	-0.152***	-0.105
	(-7.70)	(-1.61)	(-3.01)	(-0.35)
Control×[100-200)	0.000478	0.0760*	0.0117***	0.0960**
	(0.22)	(1.75)	(4.42)	(2.33)
Control×[200-300)	0.0110***	0.0564	0.0208***	0.136**
	(3.33)	(0.85)	(5.62)	(2.31)
Control×[300-400)	0.00170	-0.00344	0.0108	0.117
	(0.20)	(-0.02)	(0.98)	(0.38)
Control×[400-800)	0.0190***	-0.0260	0.00108	0.0830
	(5.44)	(-0.43)	(0.11)	(0.54)
Control×[800-1300)	0.0213***	0.0835	0.00452	0.0269
	(6.10)	(1.16)	(0.54)	(0.15)
Firm FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
Test (p)	<0.001	0.014	<0.001	0.013
N Treated	31,158	21,204	31,158	21,204
N Control	364,549	51,171	364,549	51,171
N Clusters	612	583	612	583
N Obs	2,725,680	506,304	2,725,680	506,304

t statistics in parentheses using clustering by the 5-digit NACE classification, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Table D1, we note that in the OLS model in column (1) control firms dismissed roughly as many employees per month when the epidemic reached 100-300 total new cases within a district per month compared to the baseline regime of 0-100 cases. By contrast, treated firms dismissed 0.0722 fewer employees under the regime of 100-300 total new cases compared to the baseline. If we take the analogous specification but with cases per 100,000 people (column 3), we find a significant difference between the baseline and 100-300 cases for control firms by about 0.0163 employees per firm and month, but treated firms dismiss -0.123 fewer employees by a statistically significant margin.

As mentioned above, Poisson models show somewhat lower significance levels, likely due to loss of sample size. However, they still show significant differences between the treated and the control group. In the interval between 100 and 300 COVID cases, Poisson model (column 2) indicates that the treated group dismissed employees at a rate $\exp(-0.195)$, i.e. about 18% fewer dismissals compared to the baseline regime of 0-100 cases. This Poisson results in the relative specification (column 4) show a reduction at a rate $\exp(-0.140)$, i.e. about 13%. Furthermore, the joint tests of the difference between treated and control groups are always significant at least at the 5% significance level in Table D1.

Table D3: Regression coefficients with discretization of the COVID-19 monthly cases per district. Dependent variable = Number of workers including contractors. "Four bins" specifications partition the cases into groups: [0,100), [100,300), [300,600), and [600,1300). "Six bins" specifications partition the cases into groups: [0,100), [100,200), [200,300), [300,400), [400,800), and [800,1300). The bins the smallest number of COVID-19 cases are the omitted reference category. OLS = linear model estimated by least squares; Poi = Poisson quasi-maximum likelihood with the logarithmic link; COVID intensity = specification of the proxy intensity of the COVID-19 epidemic, where "Absolute" means that absolute numbers of new cases were used and "Relative" indicates the usage of new cases per 100,000 persons living in a district. Test (p) = p-value for the joint test of the significance of the interactions between the treated dummy and bins of COVID-19 cases; N Treated = number of treated firms; N Control = number of control firms; N Clusters = number of clusters used for calculating cluster-robust variance-covariance matrix; N Firms = total number of firms; N Obs = total number of observations (firm×months).

Panel A: Specifications with three bins

	(1)	(2)	(3)	(4)
Model:	OLS	Poi	OLS	Poi
COVID intensity:	Absolute	Absolute	Relative	Relative
Treated×[100-300)	0.470***	0.0293***	0.292***	0.00857
	(9.16)	(4.25)	(4.65)	(1.05)
Treated×[300-600)	0.533***	0.0405**	0.639**	0.0856***
	(4.16)	(2.22)	(1.97)	(2.79)
Treated×[600-1200)	0.422**	0.0217	0.529***	0.0141
	(2.55)	(0.95)	(3.92)	(0.79)
Control×[100-300)	-0.0623***	-0.0114	-0.0586***	-0.0109**
	(-4.99)	(-1.54)	(-5.68)	(-2.19)
Control×[300-600)	-0.115***	-0.0363**	-0.0456	-0.0237**
	(-4.63)	(-2.17)	(-1.38)	(-2.22)
Control×[600-1200)	-0.145***	-0.0418**	0.0285	0.0145
	(-5.88)	(-2.22)	(1.20)	(1.43)
Firm FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
Test (p)	<0.001	0.001	<0.001	0.004
N Treated	31,158	31,081	31,158	31,081
N Control	364,549	111,827	364,549	111,827
N Clusters	612	596	612	596
N Obs	2,725,680	996,110	2,725,680	996,110

Panel B: Specifications with five bins

	(5)	(6)	(7)	(8)
Model	OLS	Poi	OLS	Poi
COVID int.	Abs	Abs	Rel	Rel
Treated×[100-200)	0.511***	0.0359***	0.347***	0.0135**
	(9.97)	(4.72)	(7.28)	(2.00)
Treated×[200-300)	0.326***	0.00639	0.214*	0.00577
	(5.22)	(0.68)	(1.81)	(0.36)
Treated×[300-400)	0.169	-0.0197	0.639**	0.0856***
	(0.91)	(-0.60)	(1.97)	(2.79)

Treated×[400-800)	0.554***	0.0446**	0.714***	0.0273
	(4.24)	(2.38)	(4.00)	(1.22)
Treated×[800-1300)	0.439***	0.0247	0.170	-0.0197
	(2.64)	(1.07)	(0.91)	(-0.60)
Control×[100-200)	-0.0637***	-0.0127	-0.0310***	-0.00138
	(-4.97)	(-1.56)	(-3.46)	(-0.40)
Control×[200-300)	-0.0578***	-0.00771	-0.0863***	-0.0246**
	(-3.93)	(-1.07)	(-5.89)	(-2.37)
Control×[300-400)	0.00787	0.0111	-0.0470	-0.0240**
	(0.18)	(0.63)	(-1.43)	(-2.25)
Control×[400-800)	-0.118***	-0.0383**	0.0322	0.0142
	(-4.66)	(-2.21)	(1.15)	(1.09)
Control×[800-1300)	-0.147***	-0.0430**	0.0201	0.0141
	(-5.95)	(-2.28)	(0.44)	(0.77)
Firm FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
Test (p)	<0.001	0.003	<0.001	0.009
N Treated	31,158	31,081	31,158	31,081
N Control	364,549	111,827	364,549	111,827
N Clusters	612	596	612	596
N Obs	2,725,680	996,110	2,725,680	996,110

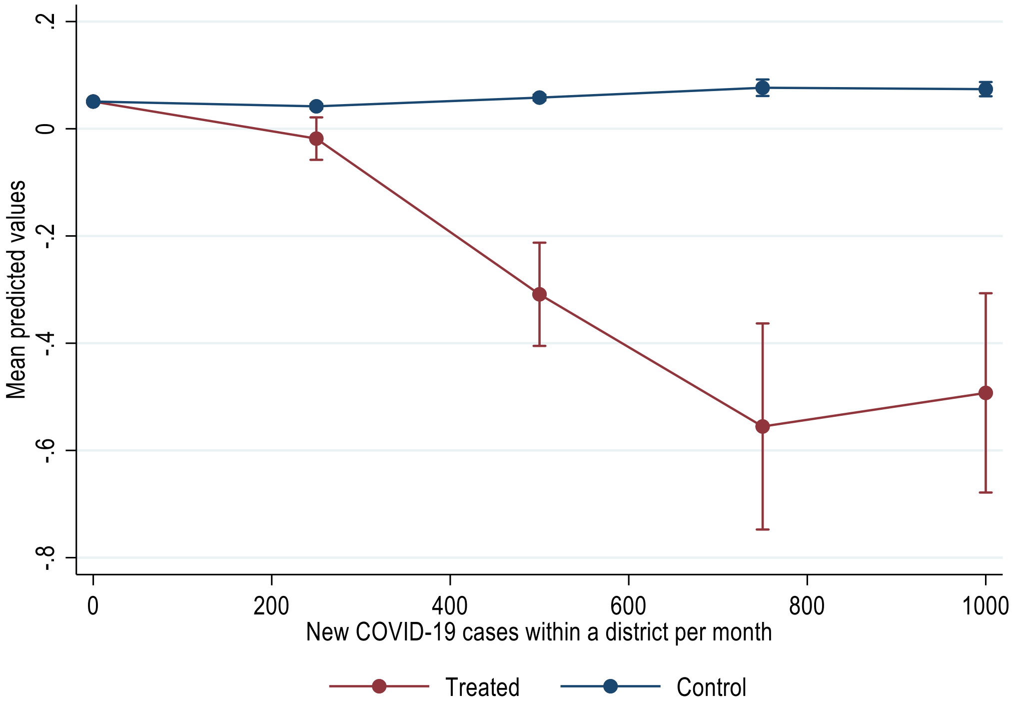
t statistics in parentheses using clustering by the 5-digit NACE classification, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Similar results are yielded by models of the number of workers, including contractors shown in Table D2. Comparing the 100-300 cases regime to the 0-100 regime in the OLS model (column 1), we find that treated firms employed about 0.511 workers more compared to the baseline, which is consistent with fewer dismissals reported in Table D1 (or, perhaps, with greater rates of hiring or a combination of the two factors). The relative specification (column 3) comparing 100-300 cases per 100,000 persons to the baseline of 0-100 shows a difference of 0.292 workers. Both of these differences are significant at 1%. Poisson model (column 2) shows that treated firms employed roughly 3% more workers ($\exp(0.0359) - 1$) in the 100-300 cases regime compared to the baseline. Moving to the relative specification (column 4), Poisson model shows treated firms employing about 1% ($\exp(0.0135) - 1$) more workers in the 100-300 cases per 100,000 persons regime compared to the baseline. In Table D2, all joint tests of equality of coefficients between the treated and control group reject the null at 1%.

These results paint a similar picture to the one presented in the main text: we find that the treated firms responded to the worsening epidemic in a manner that preserved more jobs compared to the control, regardless whether we proxy the epidemic intensity by absolute or relative case numbers.

It might bear mentioning that our decision to code treatment as time-invariant, if anything, makes our results more conservative. Figure D1 shows results when we only include treated firms that have been out of treatment for 1 month at the most during the sample period. Comparing Figures D1 and 4 shows that these firms experienced nearly double treatment effect.

Figure D1 Dismissals (at the mean of the firm fixed effects) as a function of treatment and epidemic intensity (95% CIs). Treated group consists of firms that have been at most 1 month out of treatment during sample period.



Finally, we present estimation results of the main model partitioning firms by Altman Z-score to see if similar results are yielded compared to Figure 5. Altman Z-score measures of default risk, as reported in the FinStat database, and we utilize their division of firms into three groups, category 1 being "prosperous" firms, 2 "average" firms, and 3 "distressed" firms. Comparing Figures D2 and 5 shows that indeed, there is little difference in the reaction profiles between firms in different trenches levels of "prosperity".

Figure D2: Dismissals (at the mean of the firm fixed effects) as a function of treatment and epidemic intensity for different categories of the Altman Z-score (cat 1=most financially sound firms; cat 3=firms in financial distress)