

TERMÍN: 09.01.2022

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Recenzia A
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*Prosím nezasahujte do tejto tabuľky*RECENZENT/KA (meno a priezvisko, pozícia, inštitúcia): **Daniel Dujava**NÁZOV MATERIÁLU: **Job Retention Scheme in Slovakia: Impact on Dismissals and Firm Survival in the COVID-19 Pandemic**TYP VÝSTUPU*[1]: **Analýza**

(pri spoločných výstupoch uviesť aj typy individuálnych vkladov):

ANALYTICKÝ ÚTVAR, REZORT: **Ministerstvo práce, sociálnych vecí a rodiny SR - Inštitút sociálnej politiky**AUTORI/KY: **Matěj Bělín, Marcela Veselková;**

SPOLUAUTORI/KY: - - ; - - ; - - ; - -

RECENZNÝ FORMÁT*[2]: **2****PRIPOMIENKY:**

P.č	Pripomienka sa vzťahuje k (strana, odsek):	Text pripomienky*[3]	Odôvodnenie pripomienky	Vysporiadanie sa s pripomienkou*[4]
1	Whole text	My main concern is that the proposed identification strategy does not constitute a natural experiment as it is claimed. As authors correctly point out, a key challenge of evaluating effectiveness of job retention scheme is selection problem. To solve this, they	In sake of correctly explaining used methodology to the reader.	In response to the comment, we re-phrased several points in the text removing mentions of natural experiment and Rajan-Zingales regressions. Furthermore,

use firm level data and estimate the elasticity of dismissal on level of the pandemic with the reference to Rajan – Zingales (1998), further as RZ.

The RZ identification strategy differs, however, in many different ways. RZ use theory to motivate interacting dependence on external finance of sector i with financial development of country k , i.e. thus creating a causality channel. Note that interactions are not used to solve endogeneity or selection bias (also, dependence on external finance is not an exogenous variable). Unlike RZ, the paper submitted by the ISP claims that interaction between treatment indicator and PCR incidence solves selection bias.

In the paper by RZ, it is reasonable to assume that there are no other observed and/or non-observed characteristics of industries which (i) correlate with dependence on external finance and (ii) influence relationship between financial development of country k and growth of industry i in the country k . However, it is much less likely that treatment is the only firm

we expanded the robustness checks to address the reviewer's concerns about other unobserved firm-specific confounders. To that end, Figure 5 interacts treatment with pre-pandemic growth in days worked as a proxy for the firm's health and show that the conclusions remain unchanged. Further, in Appendix D we show that essentially the same results can be obtained by using Altman Z-score as a measure of firm's financial health. For this reason, we claim justification in assuming that using COVID case numbers in conjunction with a time-trend control and firm-specific fixed effects leads to estimates that are, if anything, biased towards zero.

characteristic which influences a reaction of dismissals to district-level PCR incidence. For example, productivity, quality of management, export-orientation, private/public ownership, industry and so on are likely to determine how firm react to the increasing number of positive cases. Treated and untreated firms differ in these characteristics. Therefore, in my opinion, identification strategy used by the ISP does not address selection bias.

Furthermore, the ISP paper mentions that since variation in district-level PCR incidence is exogenous, their specification amounts to a natural experiment. Note that β_k from equation (1) measures a difference between a reaction to district-level PCR incidence of treated firms and untreated firms. What is necessary for unbiased inference is a random variation in treatment, not in district-level PCR incidence. In an ideal setting, a random variation in treatment would come from administrative borders, ad hoc eligibility criteria and so on. In this case, diff-in-diff, regression discontinuity or instrumental variables should be used.

To my knowledge, no standard technique relies on simple interaction between treatment and exogenous variable.

It is true that RZ settings solves a particular form of endogeneity. Simple cross-country regressions are plagued by reverse causality. Economic growth might be the cause, not the result of financial development. Since RZ focuses on between-industry growth differentials, they avoid this problem. However, the ISP paper does not estimate between-firm differentials because each firm is observed in just one district. Note also that because of this, in RZ, observations for particular industry have the same fixed effect in all countries. The ISP cannot mimic the RZ structure of fixed effects.

Hence, my recommendation is to remove multiple references to Rajan – Zingales (1998), to acknowledge that the proposed strategy does not deal with the selection bias, and discuss a likely direction and empirical relevance of the bias.

		<p>This approach is presented on page 2 in the paragraph discussing the effect of the quality of management on dismissals acknowledging that if better-managed firms adapted better to the pandemic the proposed identification strategy would fail. Based on the literature from the Great Recession the ISP claim that it is not likely that quality of management influenced dismissals in the short run. Authors should elaborate more on these issues since quality of management is not the only characteristics which can potentially determine how firms respond to the pandemic.</p>		
2	Whole text	<p>My second important concern is with equation (1). The equation implies that if district-level PCR incidence is zero, expected dismissals are the same for treated and untreated firms. In other words, treatment influences a reaction of dismissals to district-level PCR incidence, but not a reaction to country-level incidence. I find this assumption dubious especially as during the first pandemic wave the measures were adopted on the country-level nature. I</p>	<p>In sake of correctly explaining used methodology to the reader.</p>	<p>We thank the reviewer for the comment and clarify that our baseline model “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 normalised to be</p>

		recommend pointing this out in the text since this might not be immediately apparent to the reader.		<i>identical in the absence of new COVID infections.</i>
3	Page 3	It is not clear to me what authors mean by the following. <i>“Although the between-firm productivity remained positive in 2020, it was smaller compared to the pre-pandemic level.”</i> Do they mean between-firm productivity growth?	Possible typo.	We thank the reviewer for spotting a typo, which was corrected.
4	Page 3	I find the following summarization puzzling: <i>“Our results indicate differing sensitivity of the supported and unsupported firms to the intensifying epidemic. Among unsupported firms, the relatively low sensitivity to the epidemic intensity implies that these firms were mostly unaffected by the shock. In contrast, the dismissals among the supported firms fall dramatically with the increasing epidemic intensity, i.e. the job retention scheme was effective at preserving endangered jobs. Supported firms were much less likely to fail than unsupported firms.”</i> If unsupported firms were unaffected by the shock, why do we even use job retention schemes?	In sake of clearer interpretation of the results.	In response to this comment, we re-phrased the paragraph to emphasise the fact that flat reaction profile among non-supported firms is evidence that the scheme was judiciously targeted so that those firms that were excluded from the scheme did not actually need it. <i>“Among unsupported firms, the relatively flat reaction curve implies that these firms were mostly unaffected by the shock. As a consequence, it may</i>

		<p>And if dismissals in treated firms fall with increasing epidemic does that mean that the worse the pandemic, the better the employment? I believe that confusion is mainly due to wording. For example, equation (1) implies that by “shock” authors mean district-level PCR incidence, but, as mentioned earlier, during the first wave, firms were much more strongly influenced by country-level PCR incidence than number of positive cases in the district.</p>		<p><i>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”</i></p>
5	Page 8	<p>Authors explain that RZ approach does not allow to estimate level effect of treatment. <i>“In other words, [Rajan-Zingales-type regression] 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.”</i> I believe that the effect on job losses caused by intensifying epidemic is exactly what policy maker might be interested in. Computing predicted dismissals for treated firms assuming $T=0$ should be viable and authors should attempt to make paper more policy-relevant. However, this again suffers from the</p>	Enhancing policy-relevance of the paper.	<p>In response to this comment, we estimated differences-in-differences models, which identify the level effects. Due to the concern about violations of the requisite “parallel-trends” assumption, we included models that allow firm-specific response to the loss of mobility due to epidemiological restrictions. Consistent with our main model, DiD models indicate that the treated firms benefitted,</p>

		<p>problem that the ISP paper identifies the effect of treatment of the job losses caused by the intensifying epidemic in the district, not in the whole country.</p>		<p>especially in periods after treatment, in fact, in the post-treatment period treated firms benefited more than during treatment. This counter-intuitive result is naturally explained by the presence of a negative shock that coincides with the treatment and thus attenuates the estimated treatment effect. Thus DiD models provide a further buttress to our original argument that our primary specification provides conservative estimates of the true effect.</p>
6	Page 8	<p>I am not sure whether it is correct that bias of estimate of β_0 in (1*) depends on covariance between C^*T and U. I would expect that since (1*) includes β_0^*T, it is covariance between T and U what determines the bias. I would recommend to check this.</p>	<p>Making sure that formulas are technically correct.</p>	<p>We thank the reviewer for spotting a typo, which has been corrected. The text and Equation (2*) are now aligned.</p>
7	Page 9 and 10	<p>Dismissals as measured by (3*) include workers who found jobs elsewhere.</p>	<p>Enhancing policy-relevance of the paper.</p>	<p>In response to this comment, we note that</p>

		From the policy viewpoint, these job losses are less relevant. Also, authors focus only on period between March and September during which some treated firms were required to keep workers. It is possible that treatment led only to postponement of dismissals, not to true reduction. I would welcome if authors discussed these issues.		“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 the Conclusion hence notes that the policy relevance of this paper lies in policies directed to firms specifically, namely the newly institutionalized ‘Kurzarbeit’ scheme.
8	Page 10 and further	I would recommend using term untreated/unsupported group instead of control group since characteristics of supported and unsupported firms differ.	Using more precise terminology.	We thank the reviewer for the recommendation but prefer to use the more widely-used terminology that is used by IFP, too.
9	Page 11	Authors report level of elasticity of dismissal with respect to new coronavirus cases at a level of 500 cases per district and month. In my view, this is of little empirical relevance. Between March and September 2000 only Bratislava in September recorder more than 500 cases, i.e. one observation out of 398 (72 districts times	Enhancing policy-relevance of the paper.	Elasticity is simply $(dy/dx) \times (x/y)$ and thus its computation does not require logs. In response to the reviewer’s comment, we eliminated mention of elasticity and refer the reader to the level effects

7 months). Three observations are between 300 and 500, five between 200 and 300. 85% of observations fall into the category of less than 50 cases per district and month.

from	to	no. of obs.	freq
0	49	340	85.6 %
50	99	31	7.8%
100	199	17	4.3%
200	299	5	1.3%
300	399	1	0.3%
400	499	2	0.5%
500 and more		1	0.3%

I would welcome if authors explained how they computed the elasticities since neither dismissals, nor PCR incidence enter equation (1) in logs. To me, it seems slightly more natural to report the effect of additional PCR case on number of dismissals, i.e. slopes instead of elasticities.

estimated by DiD models instead.

		<p>Also, ISP paper claims that since elasticity for treated firms is -6.1 (at 500 cases per district and month), retention schemes reversed the trend of the pandemic. It is not clear whether this is the case. Figure 4 indicates that in districts with many positive cases treated firms laid off less workers than untreated firms (after netting out firm-specific net effects). To assess whether treatment really reversed the trend – that is whether treated firms laid off less workers than they would have laid off if there was no pandemic - it is necessary to take into account values of time dummies which control for country-level PCR incidence.</p>		
10	Page 12 and further	<p>It is not clear how confidence intervals in Figure 4 and other figures were computed. In particular, do authors assume that errors are i.i.d.? Because of the nature of pandemics, the errors may not be distributed independently across regions.</p>	Making assumptions more transparent.	<p>Variance-covariance matrices clustered by industry sectors have been used throughout this paper and this fact is now reflected in the text. We prefer to cluster by industry, as the shocks in terms of both supply and demand are likely to be correlated within firms</p>

				engaged in similar activities. We note in Appendix B, however, that industry-clustered SEs differ little from firm-clustered SEs.
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CELKOVÉ HODNOTENIE (recenzent/ka vyplní túto časť po vysporiadaní sa s pripomienkami analytickou jednotkou):

I appreciate revisions done by the ISP and I believe that revised paper is clearer and more policy relevant. Before publishing, I recommend few minor revisions:

- Treatment effect differs between sectors and depends on firm's characteristics. This is precisely the reason why paper by the IFP creates a control group using coarse exact matching. Without the control group, results of the estimation of DiD model (5) is likely to be biased. However, the bias is probably on the safe side (towards zero). I strongly recommend to clarify this in the text.
- I appreciated mentioning that measures (3*) and (4*), potentially include persons that found positions in other firms. I believe, that this should be explicitly included in the paragraph in the introduction mentioning 23 110 saved jobs over the examined 7-month period.
- Note that IFP paper uses term 'control group' only after unsupported firms are weighted. For example Figure P1 uses term 'unsupported firms' whereas Figure P2 says 'control group'. Therefore, I think ISP paper should avoid using term 'control group'
- Authors clarify that baseline model „*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 normalised to be identical in the absence of new COVID infections.*“ I recommend clarifying that effect is normalised to be identical in the absence of new COVID infections *in the district.*
- Most of the figures shows elasticities computed even for 1000 cases per district per month. I think that a reader should be informed how many observations fall into different categories, for example that 85 % of district-months cells record less than 50 cases and so on.

[1] Výber medzi: 1. analýza (komplexný analytický materiál s návrhmi konkrétnych systémových opatrení); 2. komentár (rozsahovo menší analytický materiál venujúci sa konkrétnemu čiastkovému problému); 3. manuál (metodické usmernenie vyplývajúce z potreby zjednotenia procesov a postupov v konkrétnej oblasti).

[2] Formát 1 pre komentár/manuál (2 recenzenti bez povinného odborného workshopu); Formát 2 pre analýzu (3 recenzenti a povinný odborný workshop).

[3] Do tabuľky značiť pripomienky zásadného metodologického a obsahového charakteru (nie štylistické či gramatické opravy).

[4] Vyplní analytická jednotka: pripomienka bola akceptovaná / pripomienka nebola akceptovaná a zdôvodnenie / pripomienka bola čiastočne akceptovaná a zdôvodnenie.