Ekonomická pomoc regiónom: priame a nepriame kanály

Matěj Bělín

Ondrej Buchel

Autori ďakujú Barbore Tallovej za spoluprácu pri rešerši relevantnej legislatívy a ďalej Lucii Fašungovej a Štefanovi Domonkosovi, ktorých podpora a pripomienky podstatne prispeli pri príprave tejto štúdie. Akékoľvek nedostatky sú zodpovednosťou autorov.

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Zhrnutie

Štúdia sa zameriava na vyhodnocovanie dopadov štátnej podpory investičných projektov na vývoj miery regionálnej nezamestnanosti v okresoch SR s rôznou úrovňou socioekonomickej rozvinutosti. Zároveň sa venuje odhadu medziregionálnych dopadov takejto investičnej podpory a ponúka teda komplexnejší pohľad na vyhodnocovanie dopadu štátnej podpory investičných projektov. Na údajoch pokrývajúcich obdobie 2002 až 2019 empirická analýza ukazuje, že miera nezamestnanosti po schválení dotácie na investičné projekty významne klesá predovšetkým v najmenej rozvinutých okresoch (NRO) a naopak v okresoch, ktoré nie sú NRO je dopad podpory neistý. Priemerné pozorované efekty poskytovania investičnej pomoci sú v NRO významné štatisticky aj substantívne.

Analýza venuje špeciálnu pozornosť predovšetkým skupine 12 NRO, ktoré boli takto označené ešte v rokoch 2015 a 2016, a porovnáva rozdiel v dopade podporených investičných projektov v týchto NRO a v ostatných okresoch Slovenska. Okruh 12 pôvodných NRO bol zvolený najmä preto, lebo nezamestnanosť a zásadné štrukturálne problémy ekonomiky sa ukazujú byť dlhodobo najzávažnejšími práve v týchto 12 okresoch. Pozitívny efekt investičnej pomoci v týchto okresoch sa preukázal v časovom horizonte do troch rokov od schválenia podpory. Naopak v ekonomicky rozvinutejších okresoch SR bol efekt investičnej pomoci na regionálnu nezamestnanosť malý až zanedbateľný. Rovnako analýza nenašla v dátach dôkaz o výrazných medziregionálnych spillover efektoch v prospech naviazaných okresov. Inými slovami, nie je možné tvrdiť, že by prilákanie investícií do jedného okresu znižovalo nezamestnanosť v geograficky blízkych alebo inak naviazaných okresoch. Je však možné, že vytvorenie pracovných miest môže mať iný vplyv na cieľové a na naviazané regióny, napríklad zvyšovanie miezd, prípadne zamedzenie odchodu pracovnej sily mimo hraníc SR. Takéto efekty by bolo vhodné skúmať v budúcich vedeckých príspevkoch k téme.

Uvedené závery z analýzy dát boli potvrdené aj po zohľadnení rôznych špecifikácií použitých štatistických modelov a pri rôznych predpokladaných vzťahoch medzi okresmi. Takisto rozšírenie definície NRO na okresy, ktoré boli takto klasifikované po roku 2016 vedie k podobným výsledkom. Ide teda o robustné závery, ktoré nie sú závislé na použitých metódach, či predpokladoch.

Štúdia v závere zdôrazňuje, že nastavenie investičnej pomoci, ktoré výrazne uprednostňuje ekonomicky málo rozvinuté okresy je v princípe vhodne zvolené a efektívne, nakoľko najvýznamnejšie pozitívne dopady investičných projektov na nezamestnanosť možno očakávať práve v najmenej rozvinutých okresoch SR. Inými slovami, v týchto okresoch je priestor k najväčšiemu zlepšeniu a preto v nich vidíme najvýraznejšie pozitívne efekty investícií. Konkrétne, v NRO pozorujeme redukciu tempa rastu nezamestnanosti o 0,1 percentuálneho bodu za mesiac v nadväznosti na intervenciu v sledovanom období 3 rokov po intervencii, pričom úroveň nezamestnanosti ostáva v podporených okresoch nižšia ako pred intervenciou aj v období 4 a viac rokov po intervencii. Ak by sme (konzervatívne) uvažovali o NRO s najnižším počtom obyvateľov v produktívnom veku, 0,1pb pokles tempa rastu nezamestnanosti sa prejaví ako úspora približne 1000 mesiacov nezamestnanosti v prvých troch rokoch po intervencii. Pri priemernom počte obyvateľov v produktívnom veku, tento pokles zodpovedá úspore 2880 mesiacov nezamestnanosti počas prvých troch rokoch od intervencie. Odhady ukazujú, že náklady na menšie projekty sa môžu vrátiť už v priebehu prvého roka od intervencie a náklady na investície do stredne veľkých projektov v stredne veľkých NRO sa iba na odvodoch a DPH vrátia v priebehu prvých štyroch rokov.

Analýza taktiež ukazuje smer pre nový výskum ohľadne efektov investičných stimulov na iné indikátory socioekonomického regionálneho rozvoja ako nezamestnanosť, napríklad výška mzdy alebo pridaná hodnota. Aj keď dopad na nezamestnanosť v rozvinutejších okresoch nebol potvrdený, je možné, že v týchto okresoch sa objavia iné efekty investičnej pomoci. Taktiež porovnanie, nových projektov ("green-field") s projektmi,

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ktoré rozširujú stávajúce prevádzku (“brown-field”) by mohlo vniesť nové svetlo na ekonomické mechanizmy spúšťané investičnou podporou.

Direct and indirect effects of regional economic stimuli

This paper estimates the impact of investment incentives in Slovakia on the unemployment rates in districts to which these incentives were targeted as well as potential spatial spillover effects into other districts. The results show that incentives directed to the least developed districts (LDDs) of the country are associated with a statistically significant reduction in unemployment rate growth in the targeted districts by about 0.1 percentage point per month over the course of three years following the intervention. By contrast, we estimated a precise zero effect of investment incentives targeted to non-LDDs. Estimates of the spillover effects are also centred around zero regardless of whether the incentives were targeted to LDDs or non-LDDs. Larger investment projects are found to be associated with larger reductions in unemployment in LDDs. Therefore, our results are consistent with the hypothesis of diminishing returns, which suggests that investing in LDDs brings the largest marginal benefits while investments in districts that are already developed yields much smaller returns, if any.

1 Introduction

Studies of regional disparities in unemployment trends within countries tend to show patterns of considerable variation between and stability within regions (Halleck Vega & Elhorst, 2016; Kunz, 2009; Martin, 1997; Rios, 2017). This is an obvious point of interest for policy makers since unemployment is a commonly used indicator of economic performance of a country, and by proxy of the government on the one hand and because high differences in regional unemployment are associated with lower national output and higher inflationary pressures on the other (Elhorst, 2003; Taylor, 2003). Furthermore, given that returns to scale may differ between regions in relation to their levels of unemployment, a trend of growing economic inequalities may depress the likelihood of the regions marked by higher unemployment ever catching up with the rest of the country. This is because potential investors may be discouraged from engaging in economic activities in regions that have not proven attractive for others for a number of reasons, such as higher quality of existing infrastructure and concentration of investments, higher local demand, and higher supply of various forms of capital in regions that are already better developed and may thus provide higher returns (Fujita, Krugman, & Venables, 1999; Usai & Paci, 2003). For instance, higher local unemployment rates may motivate young, educated, or skilled workers to move to economically successful regions and thus reduce the local supply of skilled labour while lowering the local demand and the overall economic attractiveness of the region. The question for policymakers is then how to reduce such disparities.

In this paper, we look at this question in the context of pre and post-2008 Slovakia, a country with some of largest regional differences in unemployment among OECD countries (OECD, 2018), where one of the government assistance programs provides investment incentives tied to goals such as job creation and a with view that efficient targeting of such incentives could help countervail the aforementioned self-reinforcing externalities of high local unemployment. While the incentives themselves target only a single firm, the extra spending on wages or contractors will be absorbed into the district economy and subject to multiplier effects, which could then be visible in district-wide economic performance. Moreover, there is a reason to expect that investment incentives would have greater effects on economic outcomes in the less developed regions as is also suggested by data from Italy and Poland (Ambroziak & Hartwell, 2018; Cerqua & Pellegrini, 2014). However, the considerations mentioned above also invite a hypothesis that if there are fewer possibilities to affect growth and employment in economically lagging regions directly, the next best option might be to invest in districts with which these have the strongest ties and stimulate the local economies and employment rates by leveraging spatial spillover effects and interdependencies such as increase in commuting that could alter the local dynamics (Cochrane, Grimes, McCann, & Poot, 2017; Gude, Álvarez, & Orea, 2018; Overman & Puga, 2014; Patacchini & Zenou, 2007; Wen, 2014). For instance, Schubert and Kroll (2016) show substantial regional spillover effects of investments into higher education institutions into neighbouring regions.

In the analysis presented in this paper, we juxtapose these two possible routes of incentivizing regional development and empirically test whether, if the goal is to reduce unemployment as a proxy for increase of economic development of a district, the government assistance in a form of investment incentives should be directed to the least developed districts and focus on direct effects of investments, or whether to rather rely on spillover effects of investments made in districts that are not lagging behind. In particular, utilizing differences-in-differences-in-differences (3D) model on district-level monthly unemployment data covering the period from 2002 to 2019, we show that government-funded investment incentives decrease unemployment in districts at the right tail of the unemployment distribution while not impacting local unemployment of the rest of the distribution. Using four distinct spatial weight matrices, we further show that spatial spillover effects on unemployment levels of related regions are limited and of indeterminate direction. In next part of the paper we briefly introduce the context of the study and the data used. We then introduce and discuss the 3D model, its spatial extension and the logic of the utilized spatial weight matrices. The latter parts of the paper present the results and identify their relevance for policy formation.

2 Regional unemployment in Slovakia and targeting of investment incentives

From 2013 to 2019, Slovakia has found itself in economic conjuncture, a period of sustained growth and decrease in unemployment following the global economic crisis of 2008. However, there were clear differences in the magnitude to which the individual regions have enjoyed the economic boom. In particular, the western part of the country has prospered while many of the eastern regions have only experienced a slow growth, which is reflected by Slovakia's status as a country with some of the largest regional disparities in both incomes and unemployment among OECD members (Gbohoui, Lam, & Lledo, 2019; OECD, 2018). Indeed, the lagging regions report a combination of high unemployment, low salaries and fewer businesses[1] and risk not only not being able to catch up with the rest of the economy but also that existing disparities may turn from persistent to permanent (Bigman & Fofack, 2000). Recognizing the issue and aiming to eliminate the established and emerging pockets of poverty, the Government of Slovakia has introduced a status of "the least developed district" (LDD)[2] with an aim to provide financial support for the lagging parts of the country.[3]

An additional policy instrument for addressing the issue of growing unemployment differences between regions could be selective targeting and support of investment incentives funded by the Ministry of Economy of the Slovak Republic. Compared to the rather modest support in the form of "regional contributions" that is only available for LDDs, investment incentives provide a considerably larger form of financial assistance but

[1] Statistical Office of the Slovak Republic

[2] The legal definition has changed multiple times over the years but, as of October 2020, a district qualifies for the category of the least developed district if the rate of unemployment in the district in at least 9 quarters out of the last consecutive 12 quarters has been higher than 1.5 times the average rate of unemployment in the country for the given quarter and above 8 %. The original definition was slightly stricter with districts being labelled LDD if their local rate of unemployment has been higher than 1.6 times the national rate of unemployment for the past 9 consecutive quarters. (cf. Journal of Laws No. 336/2015)

[3] As of June 2020, there were a total of 770 contracts for contributions listed by the Office of Deputy Prime Minister for Investment and Informatization, in a total sum of 56.5 million euros and an average contribution of 73.4 thousand euros per contract (https://www.nro.vicepremier.gov.sk/site/assets/files/1238/zoznam_zmluv_9_6.pdf).

are not limited to investments made in LDDs. Furthermore, while the stated goal of the Regional Investment Aid mechanism is to *support competitiveness and reduce regional disparities* and to promote job creation in the least developed districts, districts with greater unemployment have not been more likely to attract state-sponsored investment incentives for large scale projects compared to districts with lower unemployment (see Figure 1). From the policy-making perspective, not only does this highlight the problematic issue of persistence of inter-regional inequalities in unemployment and presumably the overall quality of life and availability of opportunities, but also draws attention to insufficient funding of already existing policies which are supposed to address or countervail the underlying factors responsible for such developments. On the flip side, this provides an opportunity to consider investment incentives an external targeted shock and evaluate their effects between and within regions with high and low unemployment. In the rest of the paper, we evaluate the effects of providing investment to firms across districts classified as LDDs and non-LDDs. In addition, we also consider that leveraging spatial spillover effects of targeting investment incentives in LDDs or non-LDDs may prove an effective strategy for reduction of unemployment.

BOX 1: Slovak legislation governing investment incentives

Applications for investment aid may only be submitted by entities registered in Slovakia that fulfil conditions laid out by the state and the EU. The projects must fall into one of the following domains: industrial production, a technological centre, a combination of the former, or a business services centre. The proposed projects may be either expansions of pre-existing operations ("brown-field") or completely new ventures ("green-field"). That is, the projects may propose establishing a new business, e.g., a factory, or plan for an extension, a diversification, or a fundamental change of the existing establishment, e.g., a new assembly line. Costs covered by the investment aid may pertain to the investment costs (e.g., acquisition or lease of land or industrial license), the wage costs, or a combination of the two. The costs that can be covered by investment aid are limited to expenses dated after the application for the investment aid has been submitted. Once the project is approved, the beneficiaries can receive investment aid in different forms and from different entities. Contributions used for obtaining fixed assets are provided by the Ministry of Economy, contributions in a form of wage costs subsidies in case of newly-created jobs are provided by the Ministry of Labor, Social Affairs and Family, and income tax relief can be claimed from the Ministry of Finance. The aid may also take the form of covering the difference between the value established by expert opinion and the transfer value in cases when there is a transfer or a lease of an immovable property below the value established by the expert opinion. In such cases, the owner of the property (a public administration entity as opposed to the central government) is the provider of the investment aid.

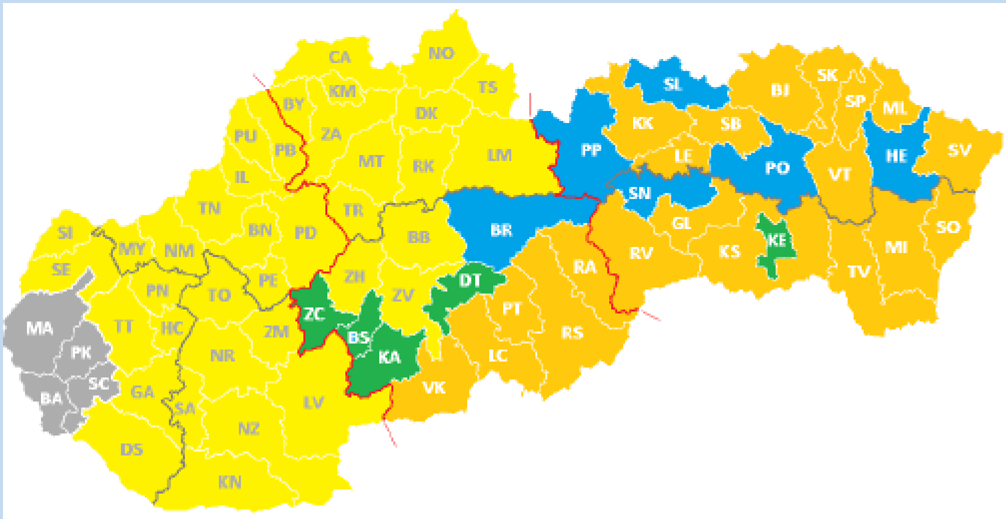

Figure 1 Classification of districts into investment zones as of 1. 1. 2020, source (Ministry of Economy, 2020)

Investment aid differs in intensity and maximum provided amount depending on the location of the project and on the sector classification of the project. Depending on the overall socio-economic situation of the region in which the project is mainly located, the total amount of the minimum planned investment and the relative required share of the total planned investment into new technologies increases. With a few exceptions, the maximum intensity and amounts of the investment aid differs between the investment projects in Western Slovakia and the Central and Eastern Slovakia, with investment projects in the latter two being eligible for contributions for newly-created jobs in addition to income tax reliefs, grants for acquired assets, and contributions to transfers or leases of immoveable property. The maximum intensity of contributions is generally 35% of the projected costs in Central and Eastern Slovakia and 25% in Western Slovakia, with the exception of the Bratislava region, where projects ineligible for investment aid. Projects located in one of the least-developed districts have significantly lower thresholds for qualifying for investment aid. For instance, while the minimum size of investment required to qualify for an income tax relief in some of the economically less developed regions of Eastern Slovakia is 1.5 million euros, projects located in one of the LDDs (orange districts) only need to meet a 200-thousand euro threshold in order to qualify for the same type of investment aid.

3 Data

For the purposes of the present study, we focus on the districts which have been recognised as LDDs in 2015, i.e. they were the first districts designated as LDDs after the adoption of the legal provision creating the legal category of LDD. The first districts to receive this designation are: Kežmarok, Lučenec, Poltár, Revúca, Rimavská Sobota, Rožňava, Sabinov, Sobrance, Svidník, Trebišov, Veľký Krtíš and Vranov nad Topľou. These twelve districts will be considered as LDDs in this analysis for the entirety of the observed time series (January 2002 - December 2019), which underlines the persistent nature of their economic underdevelopment relative to the rest of the country. Districts which have been added to the list of LDDs later on will not be considered due to concerns on selection on trends.[4] As opposed to the selection on trends, selection on levels is naturally addressed by differencing or adding fixed-effects into the model (our preferred specification does both) and therefore the case for adequate control of unobservables is much stronger than if we had included a time-varying list of LDDs.

To model unemployment levels in districts, we use publicly available data from the Central Office of Labour, Social Affairs, and Family of the Slovak Republic. In particular we use monthly data on unemployment rates at the district level and data on internal migration between regions of the Slovak Republic at the district level. In addition, we use data on state investment projects from 2002 to 2019 (we limit our observations to those before the Covid-19 crisis) provided by the Ministry of Economy of the Slovak Republic. This data contains information on the size of the planned investment, planned number of new jobs, and granted incentives. Unfortunately, this dataset suffers from the limitation that it does not resolve investment incentives directed to the different districts within the Bratislava and Košice metropolitan areas consistently. An investment incentive directed to the district of “Bratislava I” may not be distinguished from an incentive directed to “Bratislava V”. Due to this measurement error, we will discard both metropolitan areas in our baseline specification. As a robustness check, we re-run the model on the full dataset and show that the estimates remain essentially unchanged. In total, over the studied period, 9 LDDs received 33 stimuli worth 105 million compared to 176 stimuli worth 1.7 billion received by 42 non-LDDs. On average, LDDs received somewhere between one and two stimuli supporting about 300 million euro of total planned investment compared to, on average, two stimuli supporting more than 9 billion euro of total investment received by non-LDDs. The magnitude of incentives depends on the size of the proposed project, and thus more developed districts, which may be more attractive for more (and potentially larger) projects, can garner more investment incentives. Despite this stark difference

[4] Including districts that have been designated as LDDs later (Gelnica, Bardejov, Medzilaborce, Košice – okolie, Levoča, Snina, Stropkov, and Michalovce) leads to somewhat noisier estimates but it does not change the conclusions.

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in treatment intensity, we find that LDDs actually receive greater benefits than non-LDDs from the incentive scheme in terms of unemployment reductions and the conclusion is robust to controlling for the treatment intensity.

4 Model

4.1 Triple-differenced specification (3D model)

Following the recent work of Monras (2019), we specify a parsimonious version of a differences-in-differences model of regional unemployment in the form:

$$
\begin{aligned}
\Delta y_{it} = {} & \underbrace{\alpha_i}_{\substack{\text{district-specific} \\ \text{fixed effects}}} + \sum_{d=0}^{1} \underbrace{\mathbb{I}[LDD_i = d]}_{\text{LDD dummy}} \\
& \times \left\{ \underbrace{\beta_{-4}^{d} \mathbb{I}\left[\left\lfloor \frac{t_i - t_{0i}}{12} \right\rfloor \le -4\right]}_{\substack{\text{Long-run pre-} \\ \text{intervention} \\ \text{equilibtium}}} + \underbrace{\sum_{s=-3}^{s=3} \beta_s^d \mathbb{I}\left[\left\lfloor \frac{t_i - t_{0i}}{12} \right\rfloor = s\right]}_{\substack{\text{Adjustment period} \\ \text{around the intervention}}} \right. \\
& \left. + \underbrace{\beta_4^d \mathbb{I}\left[\left\lfloor \frac{t_i - t_{0i}}{12} \right\rfloor \ge 4\right]}_{\substack{\text{Long-run post-} \\ \text{intervention} \\ \text{equilibtium}}} \right\} + \underbrace{\sum_{k=1}^{k=6} \gamma_k t^k}_{\substack{\text{Economy-wide} \\ \text{trend}}} + u_{it},
\end{aligned} \tag{1}
$$

where Δy_{it} is the first-differenced unemployment rate in district i observed in month t; α_i are district-specific fixed effects, $\mathbb{I}[\cdot]$ is the indicator function, LDD_i is a dummy taking the value of one if district i has been classified as one of the “least developed districts” in Slovakia and zero otherwise, $\lfloor \cdot \rfloor$ indicates rounding down to the nearest integer, t_{0i} is the time of the largest intervention occurring in district i, u_{it} is the unobserved residual (unpredictable unemployment shock), and, finally, the coefficient β_s^1 shows the average difference between a treated LDD district and a treated non-LDD district s years from the intervention, while β_s^0 is the average difference between a treated non-LDD district and a control non-LDD district (also s years from the intervention). As a result, to convert the coefficients into effects on unemployment level in t-th year after the intervention, we compute $12 \times (\sum_{s=1}^{s=t} \beta_s^1 - \beta_s^0)$ for LDDs and $12 \times (\sum_{s=1}^{s=t} \beta_s^0)$ for non-LDDs. In this manner, changes in the monthly unemployment rate growth are converted into the difference in unemployment rate levels that would have accumulated over the years in the absence of an intervention. Consequently, the total months of unemployment saved are $12 \times [\sum_{s=1}^{s=t} (1 + t - s) \beta_s^1 - \beta_s^0]$ for LDDs and $12 \times [\sum_{s=1}^{s=t} (1 + t - s) \beta_s^0]$ for non-LDDs.

The rationale for using only the largest intervention aimed at district i is threefold: (a) it obviates the need to impose arbitrary assumptions on the potential synergetic effects of multiple interventions, (b) it leads to conservative estimates of the impacts, since the control group contains some treated districts that received smaller interventions, and (c) this simplification affects a minority of districts: out of the 79 districts, 50 received either zero or one intervention. In the robustness checks, we show that including smaller interventions strengthens our results, if anything.

This is the baseline model to be estimated by OLS. The departure from the customary differences-in-differences approach is the inclusion of the interaction between the LDD status of a district and the intervention dummies. As a consequence, model (1) accounts for potential treatment effect heterogeneity between LDDs and the other districts in Slovakia. The rationale for using the unemployment rate in first differences as opposed to levels stems from the need to avoid the heavy autocorrelations in the residuals, which appear when this 3D model is fitted on the levels (see Table 7 in the appendix). Differencing appears to sever most of these autocorrelations and reduce the residual variance substantially, making the data more amenable to the spatial QMLE models, which allow for spatial correlations only. As a result, the interpretation of the coefficients revolves around the effect of the interventions on the changes in unemployment, rather than on unemployment itself. As a robustness check, we include OLS models on levels, which lead to the same conclusion albeit with noisier estimates. Inasmuch as the first-differencing fails to eliminate temporal autocorrelations completely, we contend that the inclusion of spatial lag of the dependent variable and the spatial lag of the residuals captures these remaining autocorrelations adequately. The persistence in the unemployment series in linked districts is a likely consequence of their shared labour market conditions. Therefore, including the spatial lag of differenced unemployment and its residual from linked districts is a natural way to model persistence in the time series in a given district.

It might be worth pointing out that Monras (2019) does not include coefficients for the long-run pre-treatment equilibrium (β_{-4} in our specification) or the long-run post-treatment equilibrium (β_4 above). Instead, in his estimation, periods further than three years from the intervention period are simply dropped from the sample. In our case, dropping observations will not be feasible since this plan would result in an unbalanced panel, which precludes the estimation of spillover effects across districts. Since one of the objects of interest in the present study is to quantify cross-border effects of unemployment, we have opted for the specification above, which preserves the further-removed periods, but at the same time it accommodates the possibility that such observations may be influenced by other, unobserved factors. Thus, separate fixed effects are estimated for the long-run pre-treatment equilibrium and the long-run post-treatment equilibrium.

Since our data are at the monthly frequency, we estimate the average yearly effects by creating intervention dummies at the yearly frequency as shown above. By using the data at the original monthly level, we avoid the loss of information caused by collapsing the time series to yearly frequency (e.g. Rossana & Seater, 1995) but at the same time, we avoid the problem of over-fitting, by using yearly dummies only. In other words, while the customary form of a differences-in-differences model (e.g. Angrist & Krueger, 1999) would include a dummy for each period in the dataset, we capture the economy-wide trend in unemployment by a sixth-order polynomial. In addition to the benefit of avoiding over-fitting, this approach significantly simplifies the numerical optimisation required to fit the quasi-maximum likelihood models used in the present analysis. In fact, fitting the typical form of the differences-in-differences model proved infeasible by numerical optimisation of the quasi-likelihood function in this case.

4.2 Spatial extension

Under the presence of spillover effects between districts, the OLS estimates of the intervention effects are expected to be attenuated, i.e. the OLS estimates of β are biased towards zero if the direct and indirect effect have equal sign. This is due to the unidirectional contamination of the control group. In other words, the districts which did not receive treatment themselves might still be affected by the spillover effect from a linked district that did receive treatment. Formally:

$$\hat{\beta}_s^{\text{OLS}} = \mathbb{E}\left[\underbrace{Y_s^0 + TE_1}_{\text{Treated gr.}} |\Theta\right] - \mathbb{E}\left[\underbrace{Y_s^0 + TE_0}_{\text{Control gr.}} |\Theta\right] = \mathbb{E}[TE_1 - TE_0|\Theta], \tag{2}$$

where $\mathbb{E}$ is the expectation operator, $Y_s^0$is the outcome without intervention in period s, TE_1 is the treatment effect on the treated group, TE_0 is the spillover treatment effect on the control group, and Θ is the conditioning set, which in this case contains district-specific fixed effects and the economy-wide time trend. If the treatment effects have the same sign, i.e. $TE_1 \times TE_0 > 0$, then there is a clear attenuation bias:

$$\left|\underbrace{\mathbb{E}[TE_1 - TE_0|\Theta]}_{\equiv\widehat{\beta}^{\mathrm{OLS}}}\right| < |\mathbb{E}[TE_1|\Theta]|\,. \tag{3}$$

Thus, the nominally "control" group becomes a "partially treated" group instead. Recognising this potential influence of the treated district on the controls, we augment the OLS specification by including the spatial lags of unemployment, unemployment (shocks) and of the intervention variables. This way, the model takes into consideration the linkages between districts that may serve as channels for spillover effects. As a result, the estimated treatment effect is no longer polluted by the attenuation bias. Formally:

$$\Delta y_{it} = \mathbf{X}\theta + \sum_j w_{ij}\left(\sum_{d=0}^{1}\mathbb{I}[LDD_i = d]\left\{\beta_{-4}^{d*}\mathbb{I}\left[\left\lfloor\frac{t_i - t_{0i}}{12}\right\rfloor \leq -4\right] + \sum_{s=-3}^{s=3}\beta_s^{d*}\mathbb{I}\left[\left\lfloor\frac{t_i - t_{0i}}{12}\right\rfloor = s\right]\right.\right.$$
$$\left.\left. + \beta_4^{d*}\mathbb{I}\left[\left\lfloor\frac{t_i - t_{0i}}{12}\right\rfloor \geq 4\right]\right\} + \lambda_1 y_{jt} + \lambda_2 u_{jt}\right) + \varepsilon_{it}, \tag{4}$$

where $\mathbf{X}\theta$ collectively denotes all the regressors and coefficients in the OLS specification (1), w_{ij} are pre-specified weights indicating the strength of the link between districts i and j (by definition $w_{ii} \equiv 0$ otherwise the model would not be estimable). Consequently, the newly-added coefficients β_s^* indicate the mean impact of the intervention in the linked districts on the unemployment in district i (weighted by the strenghts of the individual links w_{ij}) *conditional* on the spatial lags of the dependent variable and the residuals. Similarly, the parameters λ_1 and λ_2 respectively indicate the association between the unemployment and the unemployment shocks in the linked districts with the unemployment level in district i. This specification follows the model due to Manski (1993), which allows for, in his terminology, (a) exogenous effects in which regressor values in linked districts influence the outcome in district i, (b) endogenous effects, when the outcome variable in the linked district (y_{jt}) influences y_{it}, and (c) the correlated effect, whereby the unobserved component (u_{jt}) can predict the outcome. Estimation of the model is carried out by the Gaussian quasi-maximum likelihood model with fixed effects due to Lee and Yu (2010). Just like OLS, this model is robust to violations of normality of the residuals. To economise on notation, it will be convenient to think of the weights w_{ij} collectively as a single weighting matrix $\mathbf{W}$ containing the weight for any combination of i and j.

The estimation of the spatial model requires taking a stance on the weighting matrix $\mathbf{W}$, since this object is not identifiable directly from the data in the absence of restrictive assumptions (e.g. Bhattacharjee & Jensen-Butler, 2013; Seya, Yamagata, & Tsutsumi, 2013). Practical experience suggests that the spatial models show some degree of robustness to the misspecification of $\mathbf{W}$ (Davenport, 2017; LeSage & Pace, 2014) but out of caution we prefer to use several versions of the weighting scheme and check the resulting fit for the data in order to gauge the appropriateness of the chosen $\mathbf{W}$ following the work of Mur and Angulo (2009). This methodology uses information criteria in an attempt to improve fit for the data, while penalising model complexity (cf. LeSage & Polasek, 2008 for a similar approach). The intuition for opting for this approach is that if the spatial dependence is strong, selecting $\mathbf{W}$ on the basis of data fit is more likely to lead to the best approximation of the true data-generating process. On the other hand, if the spatial dependence is weak, then selection of the "best" weighting matrix is less certain but in such circumstances, the need for spatial models is much weaker and OLS will provide quite good results (Elhorst, 2010).

Several reasonable candidates for $\mathbf{W}$ suggest themselves and have been used in the literature:

Contiguity weighting matrix assigns equal linkage to neighbouring districts and rules out links for non-neighbouring districts. A variant of this weighting matrix might also include second-order neighbours with a potentially different weight. Both equally-weighted second-order contiguity matrix and unequally-weighted second-order contiguity matrix (using half weight for second-order neighbours) have been used (see Appendix for comparison of results). It bears noting that contiguity matrices do not rule out spillover effects between non-neighbouring districts. Under this weighting matrix, an unemployment shock in district i may spill over to a neighbouring district j, and, subsequently to yet another district k, which is contiguous with j but may not be neighbouring the original district i. Therefore, the contiguity weighting matrix may not be as restrictive as might be imagined at first glance. While it precludes direct spillovers from between non-contiguous districts, it allows spillovers to be mediated by districts located between non-neighbouring districts.

Inverse distance weighting matrix simply uses the distance between the districts (in our case, between the centroids thereof) as a proxy for the strength of the linkage between districts. In contrast to the contiguity weighting matrix, inverse distance matrix allows direct spillovers between any two districts but the strength of the spillover effect is being dampened by the distance between them.

"**Residual**" weighting matrix follows the intuition of Meen (1996) that predictability of the outcome variable in one district by the outcome in another district (conditional on other factors, such as each district's own lagged outcome) can be used to construct $\mathbf{W}$. The rationale for this approach is as follows: the hypothesis of spillover effects from one district to another implies that some part of the variation of the dependent variable in district i is caused by impulses from another (linked) district. Therefore, there will be some unexplained (or "residual") component of the unemployment in district i even after accounting for all of the relevant regressors within district i (unless the regressors within district i are perfectly correlated with the predictors from the linked districts). To construct a weighting matrix along these lines, we utilise a vector-autoregressive (VAR) model, in which we predict the first differences of unemployment in district i using its own six lags, dummies for each month of the year (in order to remove seasonality). In addition to these "own" predictors, we wish to add the contemporaneous difference in unemployment and six lags from the other districts. However, this model would be infeasible since we have only 257 monthly observations for each district and including seven parameters for unemployment differences from the other 78 districts would call for 546 degrees of freedom. To find a feasible specification, we follow Chernozhukov, Hansen, and Spindler (2015) and utilise LASSO to identify the subset of the available predictors, which is most strongly associated with the unemployment differences in district i. The resulting OLS regression takes the form:

$$\Delta y_{it} = \alpha_m + \sum_{s=1}^{s=6} \delta_{is} \Delta y_{i,t-s} + \sum_{j \neq i} \zeta_{js} \Delta y_{j,t-s} + \zeta_{0s} \Delta y_{j,t} + u_{jt}, \tag{5}$$

where most of the coefficients ζ are constrained to nullity by the preceding LASSO. Significance of those parameters ζ_{js}, which were not constrained is taken as an estimate of the spatial weight. In case multiple lags from the same district j were selected as predictors, we employ the most significant one, i.e. $w_{ij}^R = \max_s \{ |\hat{\zeta}_{js} / \mathrm{sd}(\hat{\zeta}_{js})| \}$, where sd stands for the standard deviation of the estimated coefficient.[5]

Migration-based matrix – in recent literature (e.g. Metulini, Sgrignoli, Schiavo, & Riccaboni, 2018), migration flows have been taken as natural proxies for the latent spatial linkages. This weighting scheme has a notable intuitive appeal since migration is arguably an observable manifestation of the latent linkages between districts. To the extent that the migration decisions are facilitated by the degree of interconnectedness between the districts such as family ties, professional contacts, infrastructure (roads, railways etc., which boost communication across district borders) or compatibility between the labour markets within the districts (a specialised school in one district may be producing graduates who are readily employable in another

[5] The term "residual matrix" refers to Meen's approach of first running regressions outcome variables on the regions' own predictors and subsequently correlating residuals from different region-specific models. This plan is open to the objection that the first-stage regressions are biased due to the omission of effects from linked districts. Our approach disarms this objection as it accounts for regions' own predictors as well as for the other regions' influences simultaneously by including them all in a single model. The term "residual matrix" is nevertheless applicable here, too, since we are constructing the weighting matrix on the basis of the leftover variation of the dependent variable that is not explicable by the district's own predictors.

district), the migration ought to be a valid proxy for these underlying (poorly observable) linkages across districts. To implement the migration-based matrix, we use data from the Statistical Office of the Slovak Republic (SOSR) on the changes of the permanent place of residence from 1996 until 2019 and the weights are defined as $w_{ij}^{M} = \sum_{t=1996}^{2019} m_{ijt}$. That is, we sum all of the migration flows into district i originating from district j to construct the migration-based measure of district linkage.

Three points are worth noting regarding the computation of the migration-based matrix. First, this matrix need not be (and is not in the present application) symmetric since migration flows originating in district i bound for district j might be different than the reverse, e.g. a large regional capital may be receiving significant inflows from the surrounding areas but there might be relatively small out-migration from the city into the outlying areas. Secondly, there is a case to be made that the weighting matrix constructed from the changes in permanent place of residence might be underestimating the true linkages since labour markets in different districts are not affected solely by persons moving across district lines, but also by temporal migration, which is not visible in the data. Insofar as the measurement error is just a rescaling of the observed migration flows by a multiplicative constant, there is no problem for the spatial model since only relative strengths of the linkages are required. Under a different data-generating process for the measurement error, however, the migration weighting might lead to distortions of the results. The last point that merits discussion is the summation of migration flows from 1996 up to 2019. This is a consequence of the relative rarity of the changes in the permanent place of residence and therefore we need to gather more observations per each cell of the migration matrix in order to estimate linkages between districts. Had we relied on migration data for one year only, our results might have erroneously marked several district pairs as unconnected, simply because that selected year has not seen a realisation of the rare event of changing the permanent place of residence.

The inclusion of spatial models can help address questions regarding cross-district migration for work. Suppose, for instance, that a district “i" receives treatment but residents in neighbouring districts get hired by the firm that received investment incentives. On the assumption of no second-order effects (e.g. treated firm's suppliers based in district i hiring more workers), there would be no direct effect of the treatment on district i, however, we would observe spillover effect into the neighbouring districts. Of course, one may envision a more complex scenario, in which workers from the treated district i, who used to work in neighbouring districts, decide to leave their jobs to work closer to home for the treated firm. Their former positions in neighbouring districts are thus free to be taken by the job seekers living in those districts, which would also appear as a spillover effect on unemployment in linked districts.

5 Results

5.1 Exogeneity check

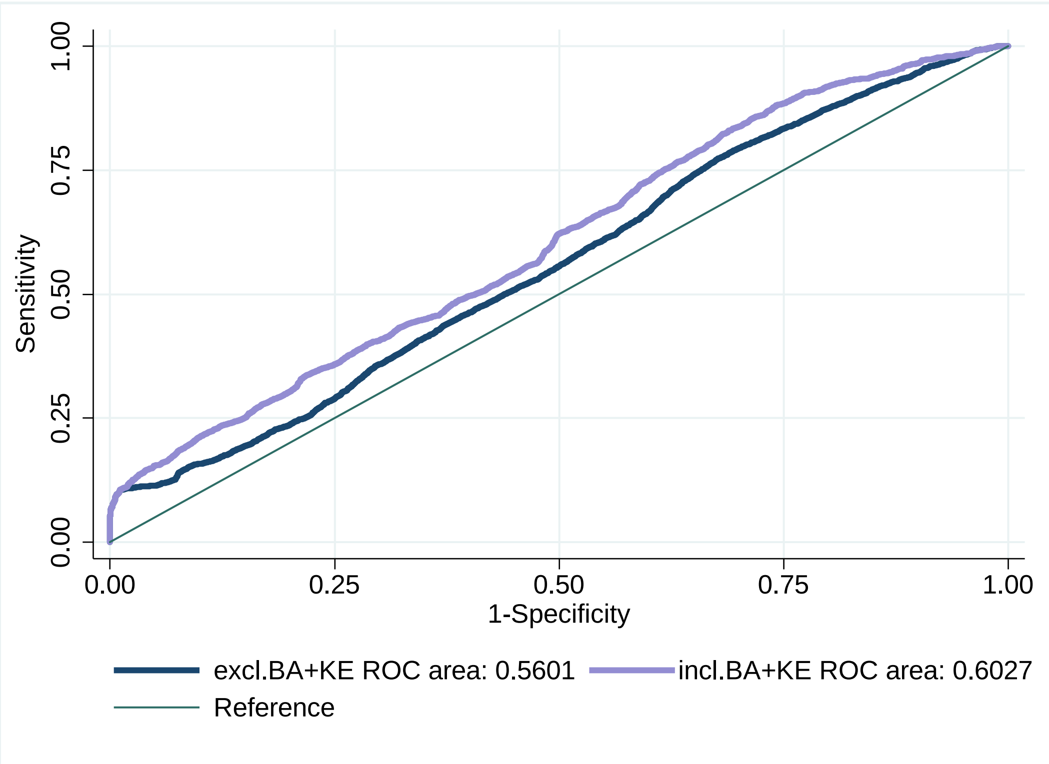
Inasmuch as the 3D estimates from OLS or the spatial panel models admit any causal interpretation whatsoever, it is necessary to check for signs of endogenous assignment of the regressor of interest, in this case the granting of the investment stimulus. Specifically, differences-in-differences models are sensitive to the selection on pre-existing time-trends. To take an extreme example, if the investment incentives had been granted only to districts with the fastest upward trends in unemployment and the intervention had zero effect, a differences-in-differences model would have found that investment incentives sped up the rise in unemployment.

Therefore, we check to what extent is the assignment of the investment incentives predictable from the temporal trend in unemployment in a given district. This is accomplished by a flexibly specified logistic regression taking the form:

$$\mathbb{E}[T_i|\Theta] = \Lambda\left(\sum_{d=0}^{1} \mathbb{I}[LDD_i = d]\left\{\alpha_r^d + \theta_1^d y_{it} + \theta_2^d \Delta y_{it} + \theta_3^d m_r(y_{it}) + \theta_4^d m_r(\Delta y_{it})\right\}\right),$$

where T_i is an indicator whether district i received an investment stimulus (treatment dummy), Θ is the conditioning set, $\Lambda(\cdot)$ is the logistic function, $\mathbb{I}[LDD_i = d]$ is a dummy for the least developed districts, $\alpha_r^d \equiv \mathbb{I}[t \in \text{year}_r]$ are yearly dummies, y_{it} and Δy_{it} are the unemployment rate and its first difference respectively, and $m_r(x_{it}) \equiv \max_t\{x_{it} \times \mathbb{I}[t \in \text{year}_r]\}$ is the yearly maximum of x_{jt} within district i. This specification takes into account both the levels of unemployment as well as their growth rates, accounts for possible structural changes by including yearly dummies and for possible shocks by taking the maxima of unemployment levels and growth rates.

Figure 2: Receiver operating characteristic (ROC) curve showing the predictability of the treatment status on the basis of the trend in unemployment. Area under the curve is 0.603 under the exclusion of Bratislava (BA) and Košice (KE) and falls to 0.56 if these regions are included $\chi^2(1)$ statistic for test of equality of these areas = 49.31 (p-value < 0.001).

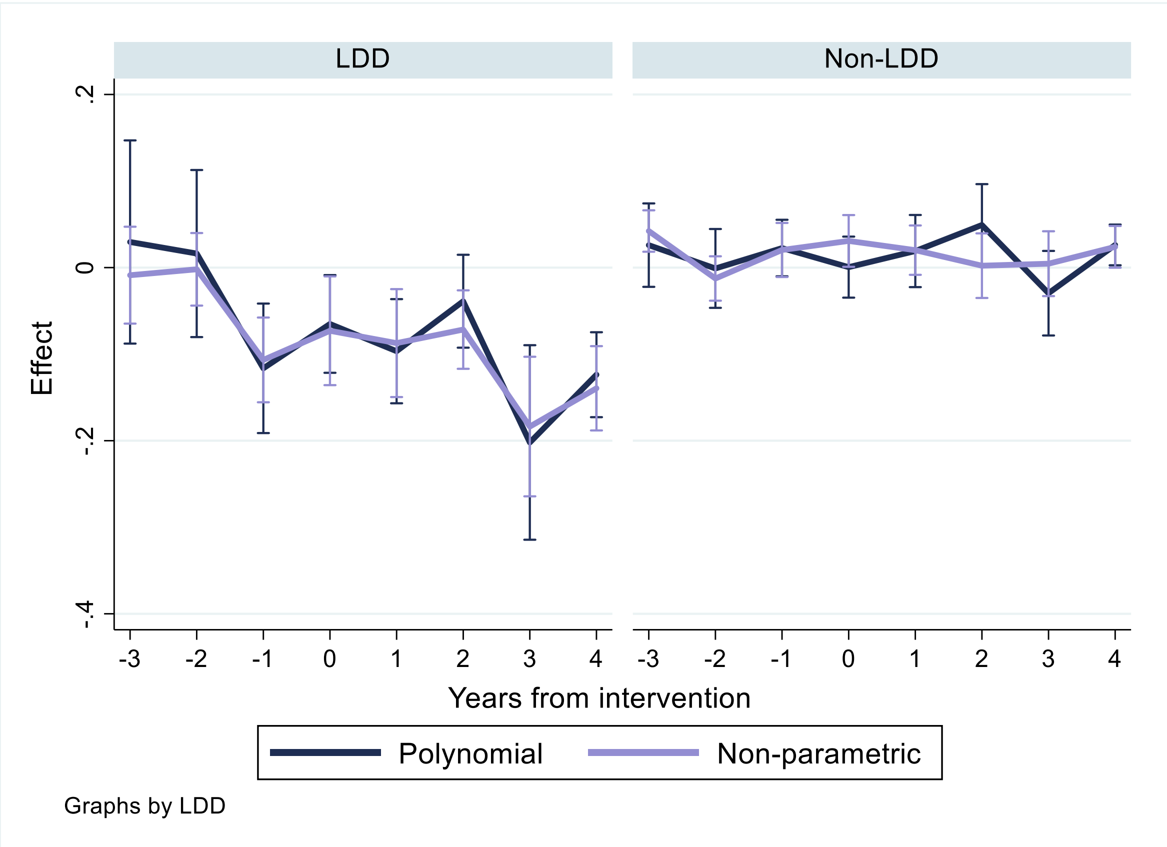


As Figure 2 makes plain, there is little predictability of the treatment status from the trend in unemployment. In fact, whether or not we include the two metropolitan areas of Bratislava and Košice, the ROC curves are very close to the 45-degree line which indicates no predictability whatsoever. In spite of the richness and flexibility of the underlying logistic model, the areas under the ROC curves are both around 0.6, which is well below the 0.7 for an "acceptable" model (Hosmer, Lemeshow, & Sturdivant, 2013, p. 177). Hence there is a reasonable warrant in the data for assumption that the treatment was not assigned on the basis of the trend in unemployment, as required by the differences-in-differences framework.

5.2 Baseline model: Oridinary Least Squares (OLS) estimates

Figure 3 reports the results for the standard 3D model estimated by OLS. The lines marked as “polynomial” belong to the more parsimonious version of the model, which uses a sixth-order polynomial in order to control of the secular time-trend. Despite the relative generality of the sixth-order polynomial, this specification may be seen as objectionable in cases of a very volatile time series with abrupt breaks. For this reason, the polynomial specification is compared to a model with a fully non-parametric trend specification, in which a dummy is used for every month separately. Comparison of the results across specifications reveals very minor differences in the estimated coefficients and confidence intervals, which suggests that the polynomial is a reasonable approximation for the economy-wide trend in unemployment changes. This finding is important since the numerical optimisation necessary to fit the quasi-maximum likelihood models with spatial dependence proved infeasible with the full set of monthly dummies and, as a consequence, spatial models will only use the polynomial specification.

Figure 3: Plot of coefficients from (1) alongside 95% confidence intervals computed on the basis of a robust variance-covariance matrix clustered by district (70 clusters; 17,920 observations). Horizontal axis represents years from the intervention while the vertical axis is the estimated effect on unemployment differences (in percentage points per month). Four years from the intervention on the horizontal axis represents the long-run equilibrium 4 years and more after the intervention.



In both specifications, there is some evidence of an Ashenfelter dip (Ashenfelter, 1978) in the unemployment differences in the year right before the intervention (cf. Heckman & Smith, 1999 for discussion).[6] This finding may suggest anticipatory effects, since the decisions to grant investment incentives scarcely occur in a vacuum. Rather, the firm’s intention to invest has to be declared well in advance of the decision to grant incentives. Therefore, it is plausible that a part of the economic boost takes place earlier than the intervention.

[6] Strictly speaking, we are observing an „inverse Ashenfelter dip” since the original concept referred to an unfavourable development among the treated group, which preceded the intervention.

This interpretation is buttressed by the conspicuous absence of any such anticipatory dip in the non-LDD sample. Had it been the case that districts that were likely to experience a reduction in unemployment were more likely to get the stimulus (a proposition contradicted by the results from the preliminary logit regression above), we would expect to see an Ashenfelter dip in the non-LDD sample as well. Since none is to be found there, the more plausible explanation of this result is an anticipatory effect of the intervention. In other words, there is very little evidence to suggest that the stimulus recipients would have made their investment in the absence of intervention. One difference between the (otherwise very close) results from polynomial and non-parametric OLS specifications is a marginally significant positive effect in non-LDDs occurring 2 years from the intervention in the polynomial specification. No such effect is found in the more flexible model with monthly dummies and even in the polynomial specification, the positive effect disappears during the third year after treatment. This suggests that while the polynomial approximation is generally very close to the dummy-based model, some rare instances of more abrupt changes in the data are better captured by monthly dummies. In contrast to this transient and specification-sensitive effect, the impact measured in LDDs is both robust to the choice of specification and stable over time, which warrants confidence in the conclusion that the treatment was beneficial for the labour markets within LDDs.

Taken at face value, the model indicates that over the adjustment period of three years following the intervention, there is no discernible effect on non-LDDs, but the unemployment differences from month-to-month decline in LDDs by about 0.1pp during the same period, which would indicate a reduction of unemployment rate by 1.2pp per year compared to the counterfactual.[7] To convert this number into a more concrete measure, one may approximate the size of the labour force within a district by the population aged 15 – 64, which is about 15,000 for the smallest LDDs.[8] Therefore, 0.1pp reduction in monthly unemployment rate growth saves about $15 \times (36 + 24 + 12) = 1080$ person-months of unemployment in the first three years following the intervention. Under the assumption that these persons would earn minimum wages, the income taxes and contributions would generate an inflow of about 334 EUR per person a month into the treasury in 2021. Assuming that nearly all of the workers' incomes are consumed, VAT taxes collected would amount to additional 100 EUR per person monthly. Thus, one arrives at monetary returns of about $1080 \times 434 = 468{,}720$ EUR following the intervention. It should be noted that these savings are calculated conservatively using the smallest LDDs and minimum wages and thus the actual monetary savings might be larger. If median-sized (or average-sized) LDD had been used for computation, 2880 person-months of unemployment would have been saved, generating returns of approximately 1,250,000 EUR in three years. These results compare favourably with the minimum- and median-sized projects in LDDs in terms of the size of the incentives, which are about 80,000 and 1,660,000 EUR respectively. We do not include any savings of unemployment benefits as not all unemployed persons are entitled to them. Furthermore, these are only direct returns, which do not include indirect costs of unemployment such as diminished well-being (e.g. Devos & Rahman, 2018) or erosion of skills. In other words, smaller-sized projects in LDDs may pay for themselves already within the first year following the provision of the investment aid, while median-sized projects in median-sized LDDs recover about 75% of their costs within the first three years after the intervention in direct savings and returns only.

[7] Importantly, although we only focus on the three-year post-treatment period as the basis of our evaluation of the impact of investment incentives on the regional unemployment levels in order to avoid possible contamination of the results by unrelated factors that might affect our dependent variable (risk of which inevitably increases when examining a longer post-treatment period), the levels of unemployment in treated LDDs remain lower compared to the pre-treatment period even after the third year.

[8] We use data on the number of persons by age (series om7009rr from the Slovak Statistical Office for the period 2018-2019) because data on the size of the labour force are results of interpolations. Encouragingly though, our regression results are similar whether the unemployment rate is computed by the (interpolated) size of the labour force or by this approximation, see Robustness Checks. The use of the smallest LDDs (Poltár and Sobrance) is to obtain a conservative estimate. The average and median size of the number of persons aged 15-64 in Slovak LDDs over the sampled period is about 40,000 persons.

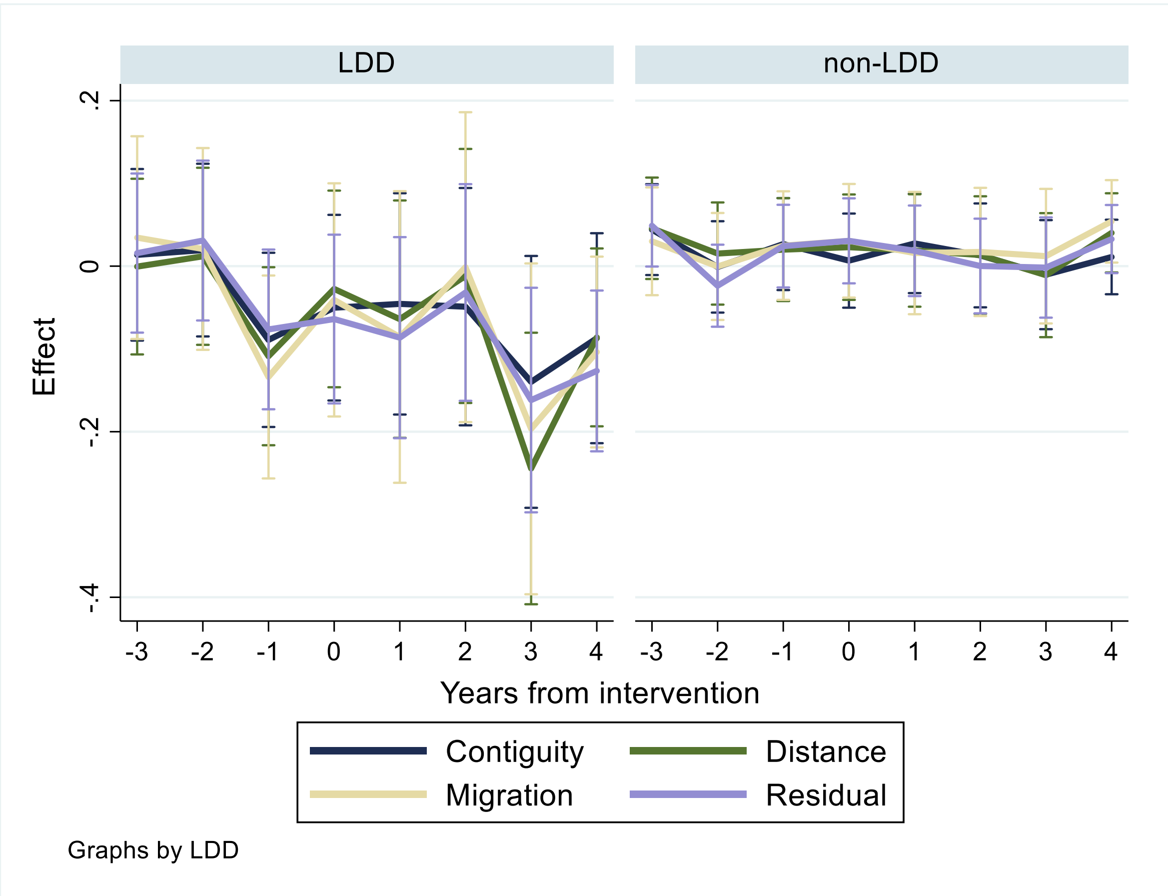
isp@employment.gov.sk

The long-term equilibrium for non-LDDs is essentially the same as in the pre-treatment period, while for LDDs, we observe a notable reduction in the unemployment growth, although the uncertainty attached to this coefficient is quite substantial. However, the further from the introduction, the more difficult the causal inference becomes. It is quite possible that the long-term equilibrium is influenced by unobserved effects that correlate with the treatment assignment. For this reason it bears paying attention to the adjustment period and accounting for the spatial dependence between the observations.

5.3 Spatial models

The results from spatial models agree in substance with the baseline OLS as seen in Figure 4, which reports the average discrete change of the outcome variable (differenced unemployment) with respect to the intervention variable. It might be noted in passing that in spatial models, the average derivatives of the dependent variable with respect to a regressor are, in general, different from the coefficients (LeSage & Pace, 2009, sec. 2.7), unlike in the case of OLS and the same holds for discrete impacts (the coefficients are reported in Table 5 in the appendix). There is the same Ashenfelter dip in the LDD sample, although it is less significant than in the case of the OLS model.

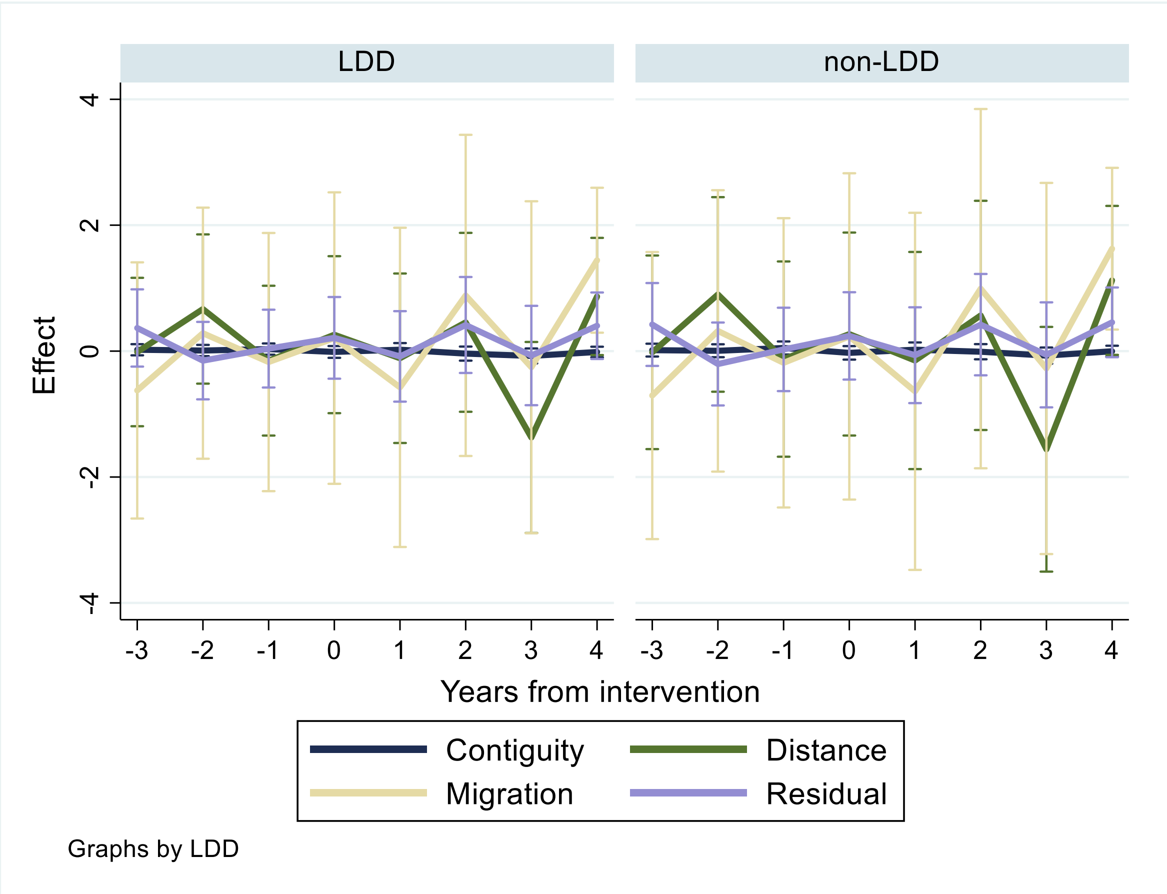
Figure 4: Direct effects (average discrete impacts of the intervention on the dependent variable) estimated from models with different spatial weighting matrices. Horizontal axis represents years from the intervention while the vertical axis is the estimated effect on unemployment differences (in percentage points per month). Four years from the intervention on the horizontal axis represents the long-run equilibrium 4 years and more after the intervention.



Crucially, the non-LDD sample shows the same lack of effect as in the OLS model. For this reason, we may rule out the suggestion that the flat response from OLS is due to attenuation caused by a contamination of the control group due to the positive spillover effects. Since none of the spatial models, all of which take into account these spillovers, has detected an effect on non-LDDs, there is a robust support for the OLS results.

The estimates of the indirect effects are rather noisy (Figure 5), but they are all centred around zero, with possibly the only exception of the inverse-distance weighting matrix, which shows a dip three years from the intervention. The conclusion to be drawn here is that the data do not show a particularly strong evidence of spillover effects. On the other hand, it is impossible to rule out quite substantial spillover effects due to the wide confidence intervals. From the results of our spatial models, the point estimates of indirect effects 3 years after the intervention are -0.06pp at the low end of the estimated spectrum (residual matrix), which would amount to about 0.7pp reduction of unemployment via the spillover effect per year. At the other side of the spectrum, the distance matrix would indicate -1.56pp spillover effect, which would imply the quite implausible 18.72pp spillover effect per year (although the corresponding 95% CI ranges from -4.6pp to +42pp). On the basis of model fit, however, the residual matrix is to be preferred (see Table 5 in the Appendix) since it shows the lowest AIC and BIC by far. Therefore, the dip in the distance matrix is likely to be a consequence of the imprecision of the distance model rather than a signal from the data.

Figure 5: Indirect effects (average discrete impacts of the intervention in linked districts on the dependent variable) estimated from models with different spatial weighting matrices. Horizontal axis represents years from the intervention while the vertical axis is the estimated effect on unemployment differences (in percentage points per month). Four years from the intervention on the horizontal axis represents the long-run equilibrium 4 years and more after the intervention.

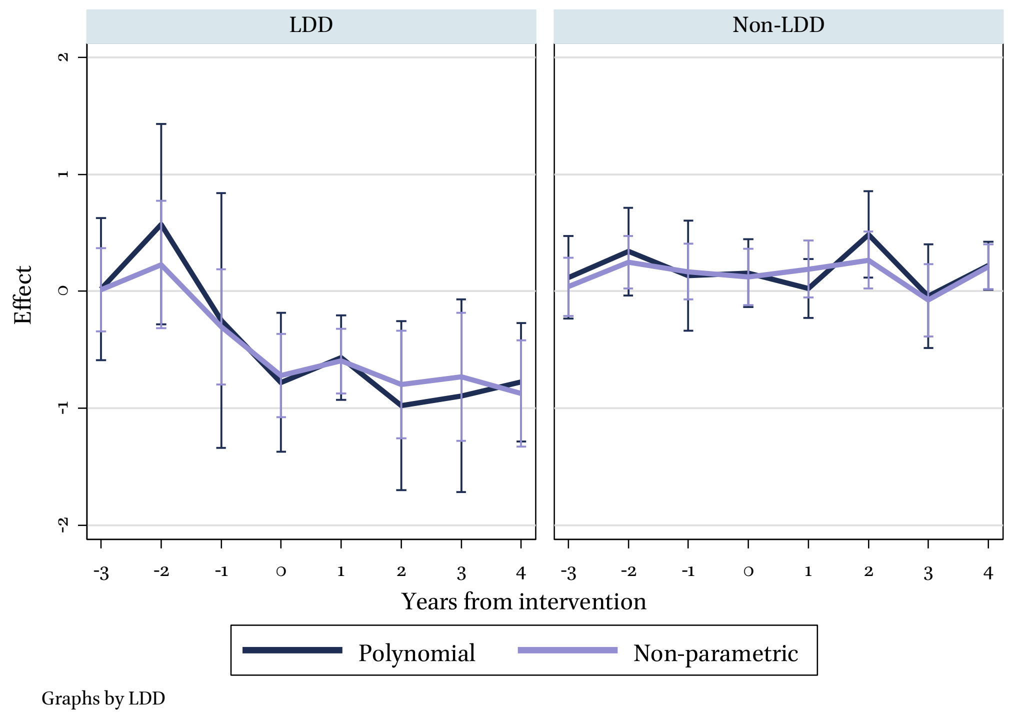


Therefore, while it is possible that there might be some substantial spillover effects, the large confidence intervals do not admit drawing any firm conclusions. Hence, there is no sound basis for policy proposals relying on the possible spillover effects from the treated district into the non-treated linked districts. While these spillover effects may exist, identification of their magnitude remains uncertain. The most precise estimates of the spillover effects come from the model with contiguity matrix, which are all very close to zero and the confidence intervals are all tighter than +/- 0.2pp. Taking these most informative estimates of the indirect treatment effects, it would have to be concluded that the magnitude of these effects has been estimated as a very precise zero. On the other hand, since AIC and BIC favour the residual matrix, greater uncertainty around the estimated effects ought to be entertained. Despite this variation in the precision, there is a broad agreement across all spatial models that the most likely values of the indirect effects appear near zero, which is in line with the experience that the choice of weighting matrices does not lead to substantial changes in the estimates. In terms of the substance of the results, our estimates match those found by Fidrmuc, Hulényi, and Zajkowska (2020), who found statistically significant effects of the EU funds on treated region in their baseline model. However, their spatial model found indirect effects that are only narrowly significant at the 10% level.

5.4 Robustness checks

As a first robustness check, we re-estimate the model on yearly data using the number of unemployed divided by the population aged 15 – 64. This specification answers two potential concerns with our preferred model: (a) since the original specification uses monthly data but estimates yearly effects to avoid over-parametrisation, it is reasonable to align the data frequency with the resolution of the treatment effect estimates; (b) the size of the labour force is measured semi-annually and therefore the denominator in unemployment rates has to be interpolated. Using yearly data avoids this problem.[9]

Figure 6: 3D model estimated on yearly data using the number of unemployed divided by the population aged 15 – 64.



[9] Similar results are found using raw numbers of unemployed persons although the estimates are less precise.

Figure 6 shows that the results are comparable to our preferred specification. If anything, it strengthens our results since the Ashenfelter dip, which appears to be present in our baseline model, is not present here. The lack of Ashenfelter dip is consistent with the exogeneity checks above, and, therefore, the crucial parallel trend assumption does not seem to be in conflict with the data.

To bolster the case for the low importance of trend heterogeneity, we re-estimate the baseline OLS model on *levels* rather than on differences. Table 1 reports the results for estimation on unemployment levels sans Bratislava and Košice. Here, treated non-LDD districts do display slightly lower unemployment rates compared to the controls (roughly by 0.4pp), while the opposite is true for the LDDs. However, joint tests of the significance of coefficients in the pre-treatment periods indicate that the null hypothesis cannot be rejected and thus treated and control districts are ex ante quite similar as in the case of models on differenced series. There is a suggestive trend in the coefficients for LDDs: while they exhibit slightly higher unemployment rates pre-treatment than the controls (conditional on the fixed effects and trend), the point estimates decline notably in the post-intervention period. In fact, in the long-run post intervention equilibrium (four years and more) the estimated treatment effect is about 4pp reduction in the unemployment in LDDs.

Table 1: OLS estimates of the triple-differenced model of unemployment rate in levels with either a polynomial control for time-trend or a dummy for each month (non-parametric). LDD = Least-developed district. Standard errors clustered at the district level are reported in the parentheses. Significance codes *, **, * correspond to significance at the 10%, 5% , and 1% level respectively. Joint test of pre-trend tests the null hypothesis that coefficients for periods -3, -2, and -1 are all zero, while test for Effect tests whether coefficients for periods 1, 2, and 3 are all zero.**

Specification:	Polynomial time-trend		Non-parametric time trend	
LDD:	No	Yes	No	Yes
Years from intervention:				
-3	-0.436	0.124	-0.554	0.347
	(0.407)	(0.903)	(0.385)	(0.883)
-2	-0.507	0.801	-0.508	0.538
	(0.402)	(1.003)	(0.422)	(0.937)
-1	-0.430	0.353	-0.629	0.242
	(0.451)	(1.079)	(0.433)	(1.029)
0	-0.446	-0.747	-0.370	-0.653
	(0.492)	(1.087)	(0.460)	(1.089)
1	-0.572	-0.728	-0.306	-0.948
	(0.479)	(1.400)	(0.487)	(1.475)
2	-0.216	-1.473	-0.391	-1.325
	(0.519)	(1.842)	(0.526)	(1.861)
3	-0.472	-2.366	-0.587	-2.184
	(0.580)	(1.468)	(0.554)	(1.485)
≥4	-0.323	-4.043***	-0.335	-4.075***
	(0.769)	(0.828)	(0.786)	(0.816)
Joint tests (p-values)				
Pre-trend	.623	.13	.179	.435
Effect	.136	.608	.804	.898
Summary statistics				
R^2	0.27	–	0.29	–

Observations	17,990	—	17,990	—
Districts	70	—	70	—
District FEs	Yes	—	Yes	—

These estimates show a very pronounced decline in the unemployment levels in LDDs vis-à-vis a flat trend in the non-LDDs, just as in the case of our baseline estimates. However, the results on unemployment levels are much noisier than those obtained from models fitted on the differenced unemployment series. This is likely a consequence of two unfavourable features of the unemployment levels time series: (a) it is highly autocorrelated, even after conditioning on fixed effects and time trend (cf. Table 7 in the Appendix), and (b) it has much larger residual variance, which inflates the standard errors. Therefore, the differenced model is much more appropriate in this context.

Table 2: OLS estimates of the triple-differenced model of unemployment rate in levels with either a polynomial control for time-trend or a dummy for each month (non-parametric). No districts were excluded from the sample. LDD = Least-developed district. Standard errors clustered at the district level are reported in the parentheses. Significance codes *, **, * correspond to significance at the 10%, 5% , and 1% level respectively. Joint test of pre-trend tests the null hypothesis that coefficients for periods -3, -2, and -1 are all zero, while test for Effect tests whether coefficients for periods 1, 2, and 3 are all zero.**

Specification	Polynomial time-trend		Non-parametric time trend	
LDD:	No	Yes	No	Yes
Years from intervention:				
-3	-0.506	-0.056	-0.621	0.160
	(0.412)	(0.925)	(0.389)	(0.901)
-2	-0.610	0.572	-0.590	0.315
	(0.411)	(1.039)	(0.432)	(0.987)
-1	-0.613	0.127	-0.740	-0.030
	(0.457)	(1.140)	(0.456)	(1.099)
0	-0.680	-0.941	-0.637	-0.823
	(0.492)	(1.140)	(0.461)	(1.144)
1	-0.871*	-0.842	-0.629	-1.042
	(0.482)	(1.468)	(0.484)	(1.533)
2	-0.633	-1.547	-0.742	-1.459
	(0.521)	(1.913)	(0.521)	(1.933)
3	-0.918	-2.507	-1.085*	-2.276
	(0.572)	(1.516)	(0.557)	(1.542)
≥4	-0.663	-4.478***	-0.665	-4.511***
	(0.789)	(0.849)	(0.805)	(0.839)
Joint tests (p-values)				
Pre-trend	.492	.143	.098	.423
Effect	.129	.547	.518	.83
Summary statistics				
R^2	0.23	—	0.25	—
Observations	20303	—	20303	—
Districts	79	—	79	—
District FEs	Yes	—	Yes	—

Table 2 shows that the exclusion of the metropolitan areas of Bratislava and Košice causes only minor changes (compare also Table 5 and Table 6 in the Appendix). However, due to the problems with measurement error, the smaller sample is preferred, since the behaviour of models under non-classical measurement error is poorly understood even in the case of OLS and in spatial models even less so.

Table 3 reports results of 4D regressions which account for both the LDD status and the magnitude of the investment project supported by the incentive scheme. Partitioning the sample into two groups with above-median investment (about 13.5 mil EUR in our sample) and below-median investment shows that LDDs receive larger benefits from larger investment, which is in line with standard economic intuitions. However, this model is likely to suffer from small-sample problems due to the double partitioning of the sample and therefore it is not our preferred specification. A probable consequence of the small sample problems is the estimated increase in unemployment in non-LDDs receiving below-median incentives. This suggests that the asymptotic approximations needed for cluster-robust inference are inaccurate in this context and that the standard errors are probably somewhat understated. With this caveat in mind, we note that the 4D model does agree in substance with the more parsimonious 3D version, namely, that benefit from investment incentives accrues to LDDs but not to non-LDDs. Here it is also worth pointing out that tests for systematic pre-intervention differences between treated and control districts are never significant at 1% and only once at 5%, which strongly suggests that there are no systematic differences among treated and control districts that are not accounted for by district-specific fixed effects and economy-wide time trend. This is especially relevant with regard to the fact that these tests are probably over-rejecting the null due to under-stated standard errors. This, combined with the evidence from logit model in Section 5.1 indicates that our estimates correspond to a causal effect of the intervention and not to other, unobserved, factors.

Table 3: OLS models accounting for treatment intensity. Est = point estimate, SE = cluster-robust standard error, p-val = p-value for the null hypothesis of no effect. Reported point estimates represent total difference between treated group and control group across 3 year period before treatment and 3 year period after treatment.

	Polynomial			Non-parametric		
Pre-treatment period:	Est.	SE	p-val	Est.	SE	p-val
non-LDD, < 13.5 mil EUR	0.865	1.322	0.513	0.569	0.862	0.509
non-LDD, ≥ 13.5 mil EUR	0.497	0.448	0.268	0.616	0.340	0.071
LDD, < 13.5 mil EUR	-0.420	0.964	0.663	-1.283	0.620	0.039
LDD, ≥ 13.5 mil EUR	-1.728	0.983	0.079	-1.673	1.179	0.156
Post-treatment period:						
non-LDD, < 13.5 mil EUR	1.567	0.652	0.016	1.335	0.356	0.000
non-LDD, ≥ 13.5 mil EUR	0.243	0.514	0.637	0.105	0.413	0.799
LDD, < 13.5 mil EUR	-3.690	0.367	0.000	-3.757	0.465	0.000
LDD, ≥ 13.5 mil EUR	-5.776	0.553	0.000	-5.755	0.551	0.000

Finally, we modify the model in order to allow multiple treatment episodes within one district. The indicator dummy in (1) is modified such that pre-treatment periods are defined for the first intervention only and the post-treatment periods are reset with each subsequent intervention, i.e. if there are two interventions two years apart, then the indicator takes values $\{\leq -4, -3, -2, -1, 0, 1, 0, 1, 2, 3, \geq 4\}$. Furthermore, we utilise a dynamic definition of the treatment intensity, such that "high" treatment intensity is defined as above-median cumulative investment supported within a given year. Thus we account for the possibility that accumulated investment may benefit more than a district with just one-off investment project.

Table 4 shows that accounting for dynamic and potentially synergetic effects of investment incentives agrees with previous conclusions, and, if anything, it strengthens them. It is encouraging that none of the pre-treatment tests reject the null hypothesis at 1%, indicating the crucial trend homogeneity. To the extent that Ashenfelter dip might be present, as indicated by the rejection at 5% in the non-parametric specification, the trend heterogeneity is likely small, since the polynomial specification fails to reject the null at 60%. Due to the dynamic specification of the investment intensity, it is not possible to differentiate pre-trends between high and low intensity regimes as the intensity variable is always zero in the pre-treatment period. In previous models that considered only one investment episode, the time-invariant intensity indicator could have been used to separate the pre-treatment sub-groups. In sum, post-intervention effects for LDDs are very similar to the static specification in Table 3. For non-LDDs, we observe smaller, but marginally significant effects indicating a reduction in unemployment, which is more intuitively appealing than the previous result. Thus, it may be concluded that the effect of investment incentives has diminishing marginal returns, witch incentives being most helpful in the most under-developed regions.

Table 4: OLS models accounting for synergetic treatment intensity. Est = point estimate, SE = cluster-robust standard error, p-val = p-value for the null hypothesis of no effect. Reported point estimates represent total difference between treated group and control group across 3 year period before treatment and 3 year period after treatment.

	Polynomial			Non-parametric		
	Est.	SE	p-val	Est.	SE	p-val
Pre-treatment period:						
non-LDD	0.671	0.463	0.147	0.048	0.373	0.898
LDD	-0.388	0.769	0.614	-1.152	0.502	0.022
Post-treatment period:						
non-LDD, Low	-1.416	0.486	0.004	0.103	0.913	0.91
non-LDD, High	-1.276	0.8	0.111	-1.501	0.731	0.04
LDD, Low	-3.147	0.677	0.000	-2.787	0.763	0.000
LDD, High	-7.581	0.91	0.000	-5.911	0.908	0.000

6 Conclusion

This paper has evaluated investment incentives in Slovakia at the district level in a differences-in-differences-in-differences (3D) framework. By comparing districts, in which a firm has successfully applied for investment incentives to those which had no successful applicants, we show that the effect of these incentives on regional unemployment varies by the level of development of the recipient district. While no significant effects have been found in most Slovak districts, investment incentives directed into one of the twelve "least developed districts" (LLDs) which have been so designated since 2015 shows significant improvements in unemployment within the treated LDDs compared to the treated non-LDDs. Our baseline OLS model indicates that treated LDD will experience a reduction in unemployment by 4.06pp (SE = 1.05) over the course of three years following the intervention when compared to controls. By contrast, the baseline model found an effect of 0.83pp (SE = 0.44) increase in unemployment in non-LDDs over three years since the intervention when compared to control districts but this result is not statistically significant at 5%. We find some evidence that larger investment projects lead to larger declines in unemployment, especially in LDDs.

Our 3D methodology filters out persistent differences between districts and therefore our results are not driven by differences in unemployment levels in different parts of Slovakia. Moreover, we also eliminate economy-wide trend, thus ensuring that the results are not affected by effects of business cycle. Furthermore, using two different procedures to check for local trend differences (auxiliary logit and tests for pre-trend), we find compelling evidence that unemployment trends in treated and control districts are very similar before the intervention, which suggests that our estimates are causal effects of the intervention. Therefore, no reduction of unemployment in LDDs would have been observed in the absence of treatment.

One potential objection to the finding of no significant effects in non-LDDs might be that the baseline OLS model fails to capture potential spillover effects from the treated districts into the control group. As a response, we estimated a battery of spatial models that explicitly model such spillovers and the zero effect remained intact. Therefore, the OLS results do not appear to be biased due to spillover effects. Direct estimates of the spillover effects of intervention are very noisy under most specifications of the spatial weights, although the most precisely estimated ones indicate that the magnitude of spillover effects is close to zero.

Consequently, the policy relevance of this work is threefold:

(a) There is evidence that investment incentives into LDDs were helpful in reducing unemployment in those districts that received help.

(b) There is limited evidence that investment incentives into non-LDDs are effective in reducing unemployment in those districts. Of course, there might be other compelling reasons to direct incentives to non-LDDs, such as creation of jobs with higher value added. These considerations are worth further investigation.

(c) There is little evidence to suggest that the effect of investment incentives on unemployment spills over into linked districts. In fact, there is some limited evidence to contradict this proposition.

As a by-product, this paper has confirmed the empirical and simulation experience, which indicates that spatial quasi-maximum likelihood estimators are relatively robust to the choice of spatial weighting matrix. Our trials of several specifications of the weighting matrices all yielded very similar results, with the only notable difference among the models considered being the estimated variance of the indirect impacts. While most models yielded very noisy estimates of the indirect impact of the intervention on linked districts, contiguity matrix placed these indirect effects at a very precise zero. However, the remaining models also produced estimates centred at zero, so they clearly agreed in substance with those obtained using contiguity matrix.

In sum, the empirical findings of this paper contradict the hypothesis that investment incentive schemes are mechanisms by which firms merely extract concessions from the government without any corresponding benefit for the economy as a whole. To the contrary, we found substantial returns of the incentive scheme in LDDs in terms of the reduction in unemployment within those districts. Furthermore, it appears that larger projects reduce unemployment more strongly. On the other hand, investing in non-LDDs has not been shown to be effective in reducing unemployment in those districts and there do not seem to be spillover effects into other linked (non-treated) districts. These findings are consistent with the economic intuition of diminishing marginal returns, which would indicate that investing into districts that had enjoyed more investment and development in the past will not be as impactful as investing into districts with less investment in the past. At the same time our results also support the intuition that, at the level of state, returns on valued outcomes such as unemployment rates may not follow the logic of agglomeration and regional economies of scale, but rather respond to targeted measures aimed at corrections of potential self-reinforcing trends. However, it bears noting that unemployment may not be the only relevant variable for granting the investment incentives. Attracting higher-paying employers or employers with higher value added can be sound policy objectives even if they do not yield reductions in the unemployment rate.

An intriguing possibility for further work might be evaluation of spatial effects of other types of interventions or detection of the effects of investment incentives on other outcome variables. Another direction well worth pursuing is investigating treatment effect heterogeneity across different types of investment projects in a firm-level study along the lines of Hanousek and Madzharova (2020). For instance, incentivising "green-field" projects might lead to different outcomes from incentivising "brow-field" projects. Testing this null hypothesis could yield new insights into firm responsiveness to investment stimuli.

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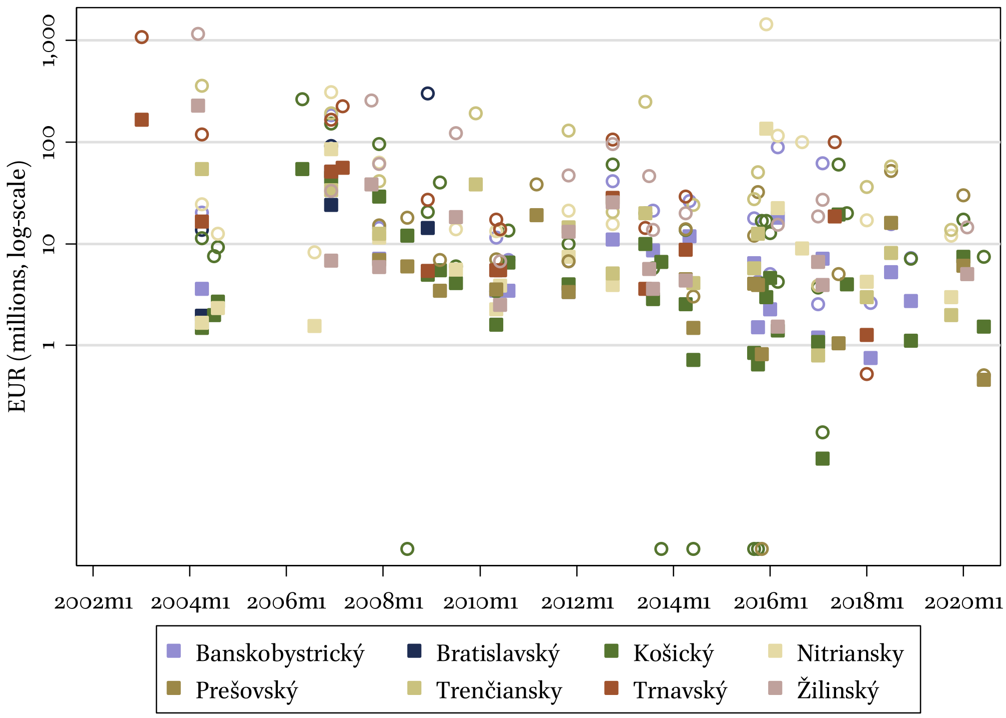
Appendix A: Additional data characteristics

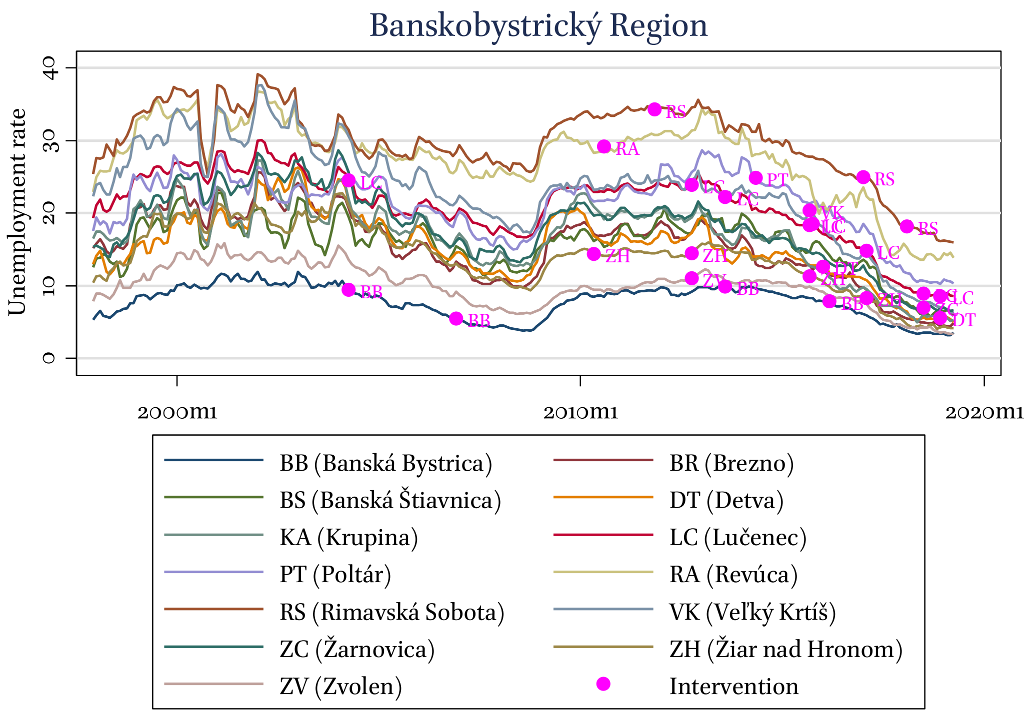
Here we include a more detailed overview of the characteristics of the dataset used in the empirical analysis above. Figure 7 shows the inflow of planned investment into Slovak NUTS regions by month for the entirety of the observed time series. The 221 observed values of the total approved investment incentives vary widely from the mean value of 8.9 million EUR with standard error of about 20 million EUR with median at 3.8 million EUR. The total sum of approved incentives consists of direct subsidies, contributions for creation of new jobs, contributions towards re-qualification costs, tax shields, and sale of government property at reduced prices. The mean investment costs were 46.8 million EUR (SE = 141, median = 13.9). Most of the intervention episodes occurred in late 2010's but there are instances observed in the period 2000 – 2010, which is important for our analysis given that our differences-in-differences model estimates treatment effects in the first three years following the intervention. Hence, intervention episodes too close to the end of the observed period cannot be used to compute treatment effects.

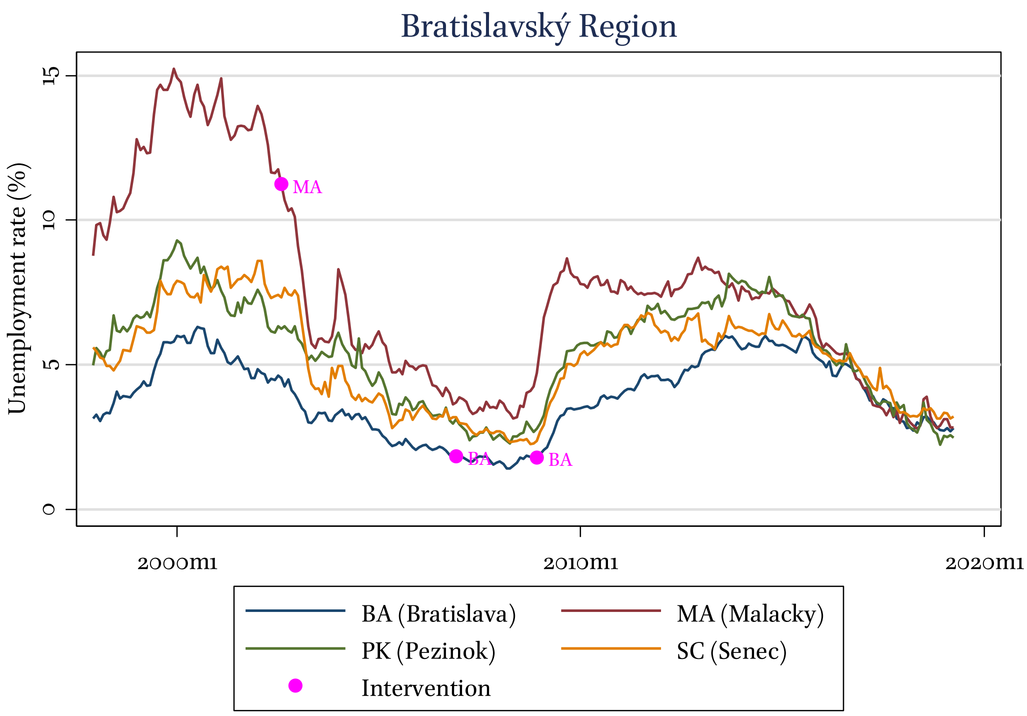
Figure 7: Magnitude of the planned investment (hollow circles) and the total investment incentives approved (solid squares) by month and NUTS region.



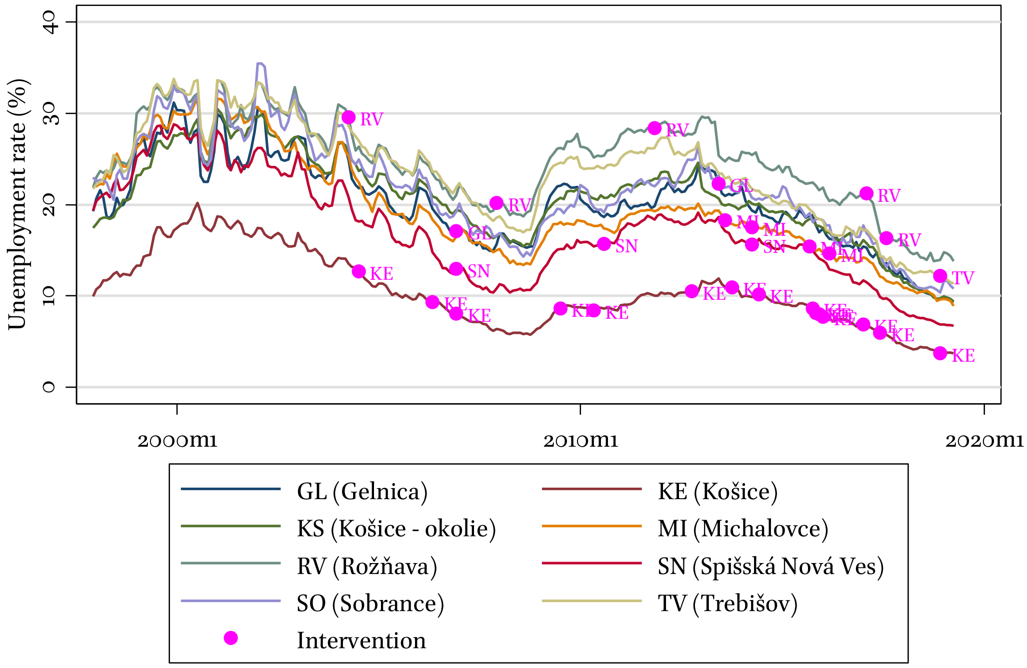
Examining the trends in unemployment in Figure 8 below shows that district-specific unemployment rates move largely in parallel to each other, exhibiting a period of high but decreasing unemployment in the early 2000s, which is then followed by an abrupt increase in unemployment following the 2008 financial crisis. Crucially for our purposes, interventions do not seem to be preceded by a district-specific change in the unemployment trend. Instead, treated districts appear to be on the same path as the control ones, which confirms the conclusion from the logistic model in the main text that the treated and control group seem comparable.

Figure 8: Timeline of the district-specific unemployment rates by NUTS regions. Interventions marked by solid circles.

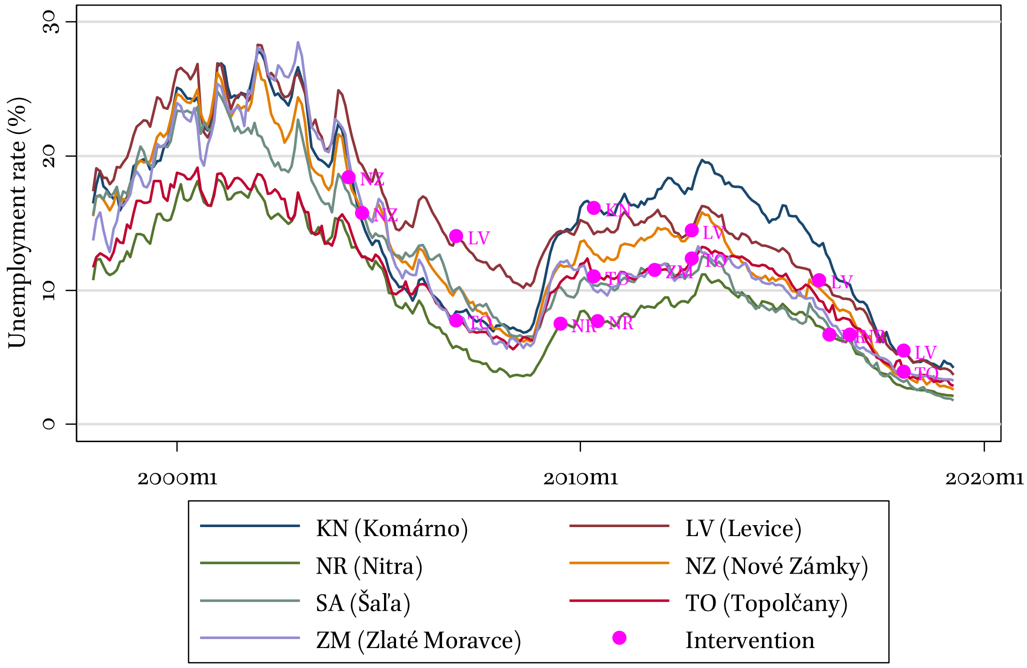




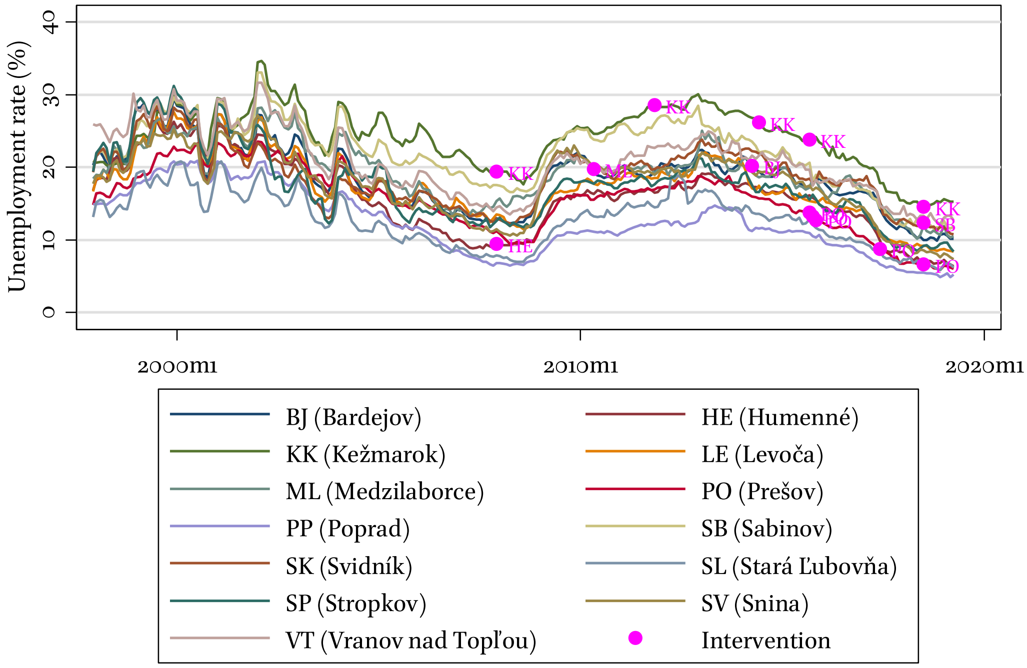
Košický Region



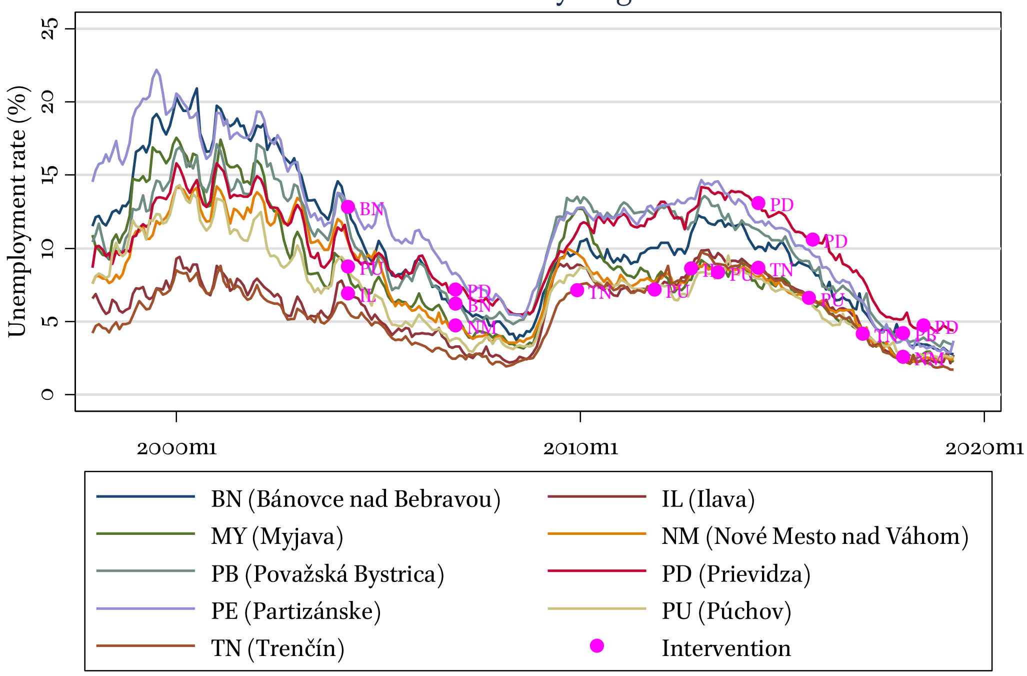
Nitriansky Region

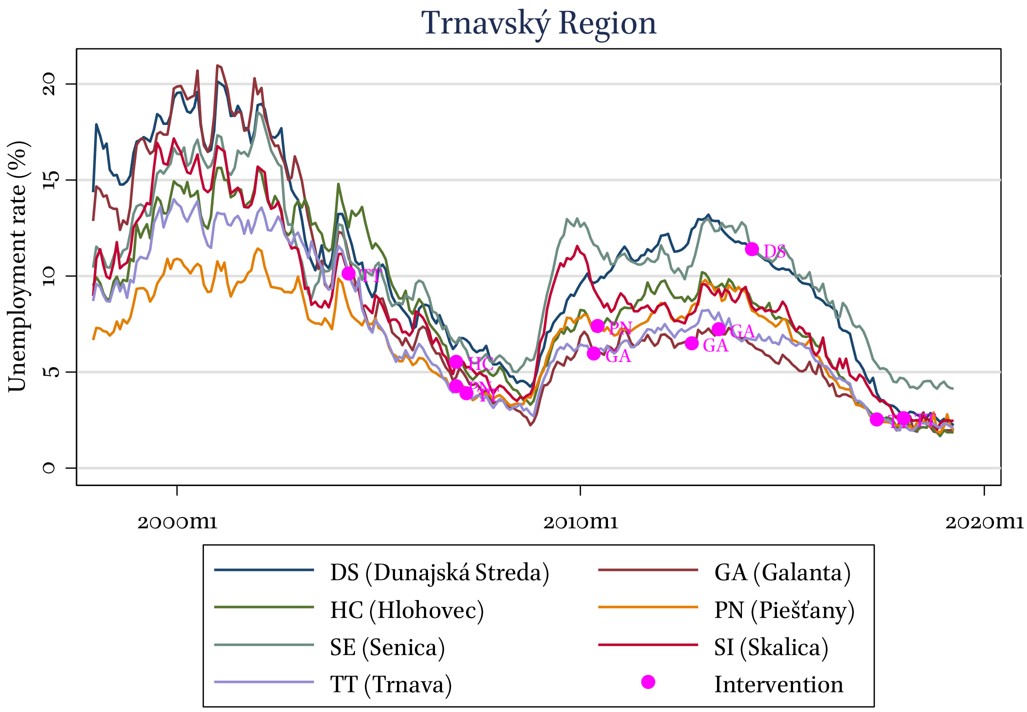


Prešovský Region

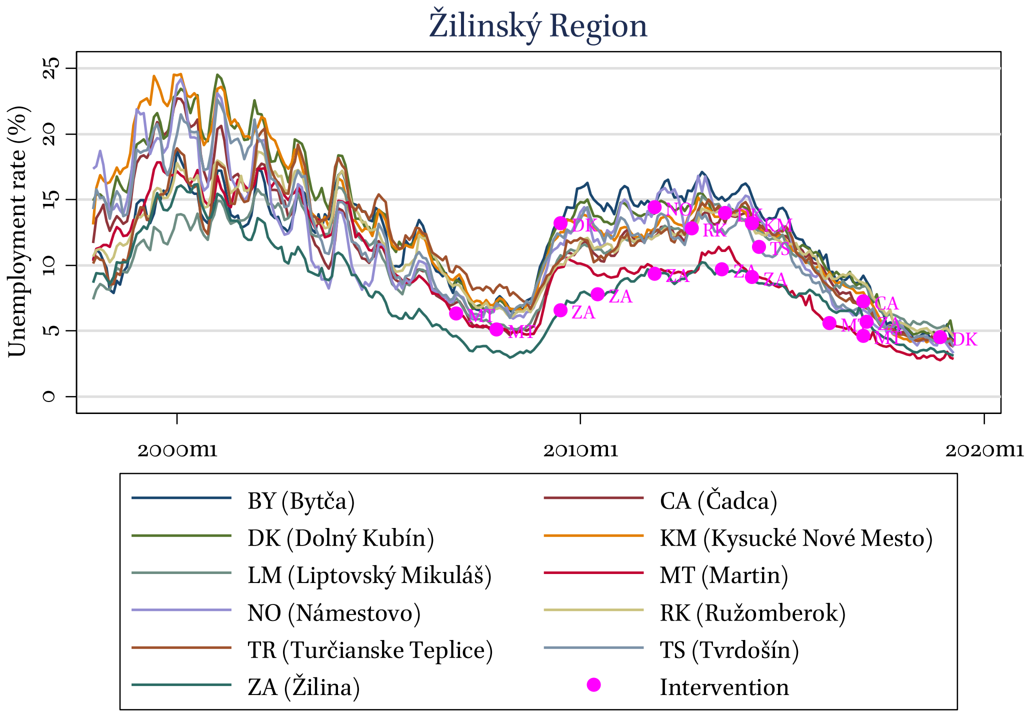


Trenčiansky Region





Trnavský Region
Unemployment rate (%)
0
5
10
15
20
2000m1
2010m1
2020m1
DS (Dunajská Streda)
GA (Galanta)
HC (Hlohovec)
PN (Piešťany)
SE (Senica)
SI (Skalica)
TT (Trnava)
Intervention



Žilinský Region
Unemployment rate (%)
0
5
10
15
20
25
2000m1
2010m1
2020m1
BY (Bytča)
CA (Čadca)
DK (Dolný Kubín)
KM (Kysucké Nové Mesto)
LM (Liptovský Mikuláš)
MT (Martin)
NO (Námestovo)
RK (Ružomberok)
TR (Turčianske Teplice)
TS (Tvrdošín)
ZA (Žilina)
Intervention

Appendix B: Full estimation results

Table 5: Estimated coefficients for sample sans Bratislava and Košice for the 3D model in differences of the unemployment. The column Parameter includes dummies for years from the intervention interacted by the LDD status. Dep. Var = indirect effect of the dependent variable in linked districts; Error term = indirect effect of the residual in linked districts.

	W matrix:	None (OLS)	None (OLS, monthly dummies)	Contiguity	Contiguity, incl. 2nd order neighbours	Contiguity, incl. 2nd order neighbours, equally weighted	Distance	Migration	Residual
	Parameter:								
Direct effects	-3	0.026	0.042***	0.045	0.057*	0.053*	0.046*	0.041	0.042*
		(0.025)	(0.012)	(0.031)	(0.029)	(0.029)	(0.025)	(0.027)	(0.024)
	-2	-0.001	-0.013	-0.001	-0.000	0.002	0.001	-0.005	-0.019
		(0.023)	(0.013)	(0.031)	(0.030)	(0.029)	(0.025)	(0.027)	(0.024)
	-1	0.023	0.020	0.032	0.026	0.019	0.018	0.020	0.025
		(0.017)	(0.016)	(0.031)	(0.030)	(0.029)	(0.025)	(0.028)	(0.024)
	0	0.001	0.031**	0.003	0.012	0.018	0.022	0.027	0.027
		(0.018)	(0.015)	(0.032)	(0.031)	(0.030)	(0.026)	(0.029)	(0.025)
	1	0.019	0.020	0.029	0.031	0.033	0.021	0.017	0.018
		(0.021)	(0.015)	(0.033)	(0.032)	(0.032)	(0.028)	(0.031)	(0.026)
	2	0.049**	0.002	0.014	0.020	0.023	0.007	0.007	-0.003
		(0.024)	(0.019)	(0.035)	(0.034)	(0.033)	(0.029)	(0.032)	(0.028)
	3	-0.030	0.005	-0.016	-0.022	-0.016	0.001	0.007	-0.002
		(0.025)	(0.019)	(0.036)	(0.035)	(0.035)	(0.031)	(0.034)	(0.029)
	≥4	0.026**	0.024*	0.012	0.025	0.025	0.024	0.032	0.026
		(0.012)	(0.012)	(0.024)	(0.024)	(0.024)	(0.021)	(0.023)	(0.020)
	-3×LDD	0.004	-0.051	-0.027	-0.049	-0.048	-0.046	-0.022	-0.024

		(0.064)	(0.031)	(0.064)	(0.061)	(0.060)	(0.053)	(0.058)	(0.050)
	-2×LDD	0.017	0.011	0.023	0.013	0.008	0.009	0.030	0.040
		(0.054)	(0.024)	(0.064)	(0.061)	(0.060)	(0.053)	(0.058)	(0.051)
	-1×LDD	-0.139***	-0.127***	-0.121*	-0.107*	-0.098	-0.113**	-0.123**	-0.107**
		(0.040)	(0.028)	(0.065)	(0.061)	(0.060)	(0.053)	(0.058)	(0.051)
	0×LDD	-0.066**	-0.104***	-0.052	-0.059	-0.064	-0.065	-0.065	-0.090*
		(0.033)	(0.035)	(0.068)	(0.065)	(0.063)	(0.057)	(0.062)	(0.053)
	1×LDD	-0.116***	-0.107***	-0.071	-0.066	-0.090	-0.084	-0.078	-0.095
		(0.036)	(0.033)	(0.079)	(0.077)	(0.075)	(0.067)	(0.074)	(0.061)
	2×LDD	-0.088**	-0.074**	-0.077	-0.085	-0.076	-0.028	-0.013	-0.043
		(0.035)	(0.030)	(0.083)	(0.082)	(0.080)	(0.072)	(0.079)	(0.066)
	3×LDD	-0.172***	-0.188***	-0.136	-0.163*	-0.159*	-0.190**	-0.168**	-0.154**
		(0.061)	(0.044)	(0.088)	(0.086)	(0.084)	(0.077)	(0.084)	(0.068)
	≥4×LDD	-0.150***	-0.163***	-0.104*	-0.127**	-0.134**	-0.121**	-0.117**	-0.152***
		(0.025)	(0.025)	(0.063)	(0.060)	(0.058)	(0.059)	(0.057)	(0.049)
Indirect Effects	-3			0.034	0.166	0.146	-0.039	-0.530	0.188
				(0.092)	(0.190)	(0.187)	(0.246)	(0.532)	(0.136)
	-2			0.003	0.041	0.116	0.296	0.202	-0.154
				(0.093)	(0.191)	(0.188)	(0.246)	(0.523)	(0.136)
	-1			0.097	0.109	0.004	0.018	0.217	-0.042
				(0.093)	(0.191)	(0.188)	(0.248)	(0.533)	(0.136)
	0			-0.050	-0.037	0.039	-0.002	0.134	0.104
				(0.096)	(0.195)	(0.192)	(0.254)	(0.561)	(0.142)
	1			0.041	0.015	0.003	-0.062	-0.070	0.020
				(0.102)	(0.210)	(0.206)	(0.273)	(0.609)	(0.151)
	2			0.025	0.177	0.227	0.137	0.432	0.098
				(0.107)	(0.217)	(0.214)	(0.289)	(0.631)	(0.158)
	3			-0.096	-0.327	-0.237	-0.243	0.230	0.015
				(0.114)	(0.226)	(0.224)	(0.307)	(0.662)	(0.165)

≥4			0.015	0.144	0.141	0.310*	0.970***	0.201**
			(0.078)	(0.128)	(0.135)	(0.177)	(0.316)	(0.102)
-3×LDD			0.062	-0.106	-0.225	0.034	1.297	-0.256
			(0.206)	(0.394)	(0.404)	(0.600)	(1.310)	(0.283)
-2×LDD			0.053	0.015	-0.193	-0.229	-0.425	0.376
			(0.205)	(0.393)	(0.404)	(0.603)	(1.292)	(0.285)
-1×LDD			-0.138	-0.180	-0.193	-0.445	-1.715	0.223
			(0.206)	(0.395)	(0.404)	(0.605)	(1.299)	(0.285)
0×LDD			0.065	0.186	0.090	0.554	-0.290	-0.090
			(0.215)	(0.415)	(0.425)	(0.670)	(1.499)	(0.297)
1×LDD			0.006	0.289	0.040	0.078	-1.127	-0.180
			(0.250)	(0.507)	(0.508)	(0.856)	(1.949)	(0.348)
2×LDD			-0.310	-0.518	-0.364	0.226	-0.193	0.268
			(0.273)	(0.554)	(0.555)	(0.916)	(2.142)	(0.372)
3×LDD			-0.202	-0.154	-0.028	-1.542	-1.935	-0.054
			(0.287)	(0.587)	(0.585)	(0.958)	(2.320)	(0.388)
≥4×LDD			-0.155	0.098	0.077	0.134	-1.864	-0.130
			(0.206)	(0.332)	(0.318)	(0.538)	(1.449)	(0.232)
Dep. Var.			-0.445***	0.087**	-0.035	0.714***	0.599***	0.635***
			(0.020)	(0.039)	(0.036)	(0.019)	(0.033)	(0.019)
Error term			0.879***	0.836***	0.869***	0.739***	0.691***	0.649***
			(0.005)	(0.012)	(0.009)	(0.018)	(0.026)	(0.019)
Log-lik.	-18134	-11353	-13543	-12831	-12877	-11531	-12788	-10794
AIC	36312.53	22845.37	27167.87	25743.92	25836.06	23143.88	25658.88	21669.31
BIC	36483.99	23383.13	27487.41	26063.46	26155.60	23463.42	25978.42	21988.85
R^2	0.04	0.55	0.04	0.04	0.04	0.04	0.04	0.04
Observations	17920	17920	17920	17920	17920	17920	17920	17920

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Table 6: Estimated coefficients for sample with the inclusion of Bratislava and Košice for the 3D model in differences of the unemployment. The column Parameter includes dummies for years from the intervention interacted by the LDD status. Dep. Var = indirect effect of the dependent variable in linked districts; Error term = indirect effect of the residual in linked districts.

	W matrix:	None (OLS)	None (OLS, monthly dummies)	Contiguity	Contiguity, incl. 2nd order neighbours	Contiguity, incl. 2nd order neighbours, equally weighted	Distance	Migration	Residual
	Parameter:								
Direct effects	-3	0.026	0.037***	0.042	0.053*	0.050*	0.043*	0.046*	0.038*
		(0.023)	(0.012)	(0.028)	(0.027)	(0.027)	(0.024)	(0.027)	(0.023)
	-2	-0.008	-0.013	-0.002	-0.006	-0.003	0.003	-0.002	-0.022
		(0.022)	(0.013)	(0.029)	(0.028)	(0.027)	(0.024)	(0.027)	(0.023)
	-1	0.014	0.012	0.022	0.018	0.013	0.015	0.016	0.016
		(0.016)	(0.014)	(0.029)	(0.028)	(0.027)	(0.024)	(0.027)	(0.023)
	0	-0.010	0.012	-0.003	0.003	0.008	0.012	0.003	0.011
		(0.017)	(0.018)	(0.029)	(0.028)	(0.028)	(0.025)	(0.028)	(0.023)
	1	0.004	0.014	0.020	0.016	0.019	0.020	0.004	0.011
		(0.020)	(0.014)	(0.031)	(0.030)	(0.030)	(0.026)	(0.030)	(0.025)
	2	0.037	-0.012	0.006	0.003	0.004	-0.001	-0.008	-0.010
		(0.023)	(0.018)	(0.032)	(0.031)	(0.031)	(0.028)	(0.031)	(0.026)
	3	-0.038*	-0.010	-0.017	-0.028	-0.026	-0.005	-0.015	-0.011
		(0.022)	(0.018)	(0.033)	(0.032)	(0.032)	(0.029)	(0.033)	(0.027)
	≥4	0.021*	0.020*	0.017	0.022	0.018	0.029	0.036*	0.021
		(0.012)	(0.012)	(0.022)	(0.021)	(0.021)	(0.020)	(0.021)	(0.019)
	-3×LDD	-0.002	-0.049	-0.031	-0.053	-0.057	-0.042	-0.025	-0.019
		(0.065)	(0.033)	(0.061)	(0.058)	(0.057)	(0.052)	(0.058)	(0.049)

	-2×LDD	0.017	0.006	0.018	0.008	0.001	0.003	0.031	0.038
		(0.054)	(0.024)	(0.061)	(0.058)	(0.058)	(0.052)	(0.058)	(0.050)
	-1×LDD	-0.138***	-0.127***	-0.119*	-0.108*	-0.101*	-0.117**	-0.125**	-0.101**
		(0.040)	(0.027)	(0.061)	(0.059)	(0.058)	(0.052)	(0.058)	(0.050)
	0×LDD	-0.063*	-0.091**	-0.052	-0.057	-0.065	-0.059	-0.053	-0.077
		(0.034)	(0.037)	(0.065)	(0.062)	(0.061)	(0.056)	(0.062)	(0.052)
	1×LDD	-0.111***	-0.112***	-0.070	-0.061	-0.089	-0.091	-0.080	-0.097
		(0.035)	(0.033)	(0.075)	(0.074)	(0.072)	(0.066)	(0.074)	(0.060)
	2×LDD	-0.088**	-0.069**	-0.081	-0.082	-0.075	-0.022	0.003	-0.047
		(0.035)	(0.029)	(0.080)	(0.079)	(0.077)	(0.071)	(0.079)	(0.064)
	3×LDD	-0.177***	-0.186***	-0.149*	-0.165**	-0.159**	-0.193**	-0.143*	-0.157**
		(0.060)	(0.044)	(0.084)	(0.083)	(0.080)	(0.076)	(0.084)	(0.067)
	≥4×LDD	-0.157***	-0.170***	-0.121**	-0.136**	-0.138**	-0.134**	-0.131**	-0.159***
		(0.026)	(0.026)	(0.060)	(0.057)	(0.055)	(0.058)	(0.056)	(0.048)
Indirect Effects	-3			0.012	0.137	0.136	-0.000	0.102	0.112
				(0.086)	(0.179)	(0.176)	(0.231)	(0.606)	(0.130)
	-2			-0.014	-0.030	0.052	0.250	0.268	-0.212
				(0.087)	(0.179)	(0.177)	(0.231)	(0.591)	(0.129)
	-1			0.054	0.089	0.030	0.060	0.400	-0.100
				(0.088)	(0.179)	(0.177)	(0.232)	(0.600)	(0.129)
	0			-0.079	-0.098	-0.016	-0.238	-0.395	0.041
				(0.090)	(0.183)	(0.180)	(0.237)	(0.616)	(0.134)
	1			-0.008	-0.087	-0.034	-0.071	-0.681	-0.045
				(0.096)	(0.196)	(0.193)	(0.253)	(0.680)	(0.143)
	2			-0.019	0.039	0.119	0.145	0.146	-0.032
				(0.100)	(0.202)	(0.200)	(0.265)	(0.719)	(0.149)
	3			-0.119	-0.371*	-0.284	-0.329	-0.392	0.018
				(0.106)	(0.209)	(0.209)	(0.279)	(0.749)	(0.155)
	≥4			0.005	0.062	0.029	0.258	1.241***	0.199**

			(0.074)	(0.119)	(0.123)	(0.163)	(0.321)	(0.095)
-3×LDD			0.038	-0.064	-0.194	0.150	1.120	-0.252
			(0.196)	(0.384)	(0.394)	(0.649)	(1.480)	(0.272)
-2×LDD			0.032	0.042	-0.142	-0.114	0.035	0.235
			(0.196)	(0.384)	(0.394)	(0.652)	(1.456)	(0.273)
-1×LDD			-0.139	-0.162	-0.160	-0.626	-2.061	0.272
			(0.197)	(0.386)	(0.396)	(0.653)	(1.458)	(0.273)
0×LDD			0.067	0.243	0.154	0.763	-0.425	-0.048
			(0.205)	(0.404)	(0.414)	(0.719)	(1.694)	(0.285)
1×LDD			0.040	0.368	0.063	-0.059	-1.174	-0.178
			(0.239)	(0.493)	(0.491)	(0.917)	(2.202)	(0.333)
2×LDD			-0.283	-0.474	-0.336	0.340	0.537	0.310
			(0.261)	(0.535)	(0.537)	(0.981)	(2.420)	(0.354)
3×LDD			-0.192	-0.123	0.017	-1.778*	-0.854	-0.029
			(0.275)	(0.567)	(0.566)	(1.025)	(2.642)	(0.368)
≥4×LDD			-0.132	0.121	0.095	0.264	-2.275	-0.066
			(0.198)	(0.323)	(0.309)	(0.590)	(1.653)	(0.218)
Dep. Var.			-0.453***	0.017	-0.154***	0.700***	0.544***	0.625***
			(0.019)	(0.036)	(0.034)	(0.018)	(0.036)	(0.017)
Error term			0.875***	0.847***	0.883***	0.734***	0.705***	0.632***
			(0.005)	(0.010)	(0.007)	(0.017)	(0.025)	(0.017)
Log-lik.	-19594	-12860	-14520	-13733	-13779	-12912	-14541	-11945
AIC	39231.42	25876.58	29122.11	27547.33	27640.80	25905.64	29164.39	23971.72
BIC	39405.55	26493.92	29446.61	27871.83	27965.30	26230.14	29488.89	24296.22
R^2	0.04	0.51	0.04	0.04	0.04	0.04	0.04	0.04
Observations	20224	20224	20224	20224	20224	20224	20224	20224

Table 7: Comparison of the model in levels of unemployment and in first differences (Bratislava and Košice are omitted). Standard errors clustered by districts are reported in parentheses.

	Dependent variable:	Y	Y	Y	ΔY	ΔY	ΔY
	Specification:	Polynomial	Monthly dummies	Polynomial	Polynomial	Monthly dummies	Polynomial
	Estimator:	OLS	OLS	MLE	OLS	OLS	MLE
Years from intervention	-3	-0.436	-0.554	-0.011	0.026	0.042***	0.030
		(0.407)	(0.385)	(0.081)	(0.025)	(0.012)	(0.023)
	-2	-0.507	-0.508	-0.058	-0.001	-0.013	-0.004
		(0.402)	(0.422)	(0.136)	(0.023)	(0.013)	(0.023)
	-1	-0.430	-0.629	-0.114	0.023	0.020	0.018
		(0.451)	(0.433)	(0.204)	(0.017)	(0.016)	(0.018)
	0	-0.446	-0.370	-0.118	0.001	0.031**	0.004
		(0.492)	(0.460)	(0.245)	(0.018)	(0.015)	(0.019)
	1	-0.572	-0.306	-0.103	0.019	0.020	0.022
		(0.479)	(0.487)	(0.298)	(0.021)	(0.015)	(0.023)
	2	-0.216	-0.391	-0.091	0.049**	0.002	0.039*
		(0.519)	(0.526)	(0.330)	(0.024)	(0.019)	(0.023)
	3	-0.472	-0.587	-0.122	-0.030	0.005	-0.023
		(0.580)	(0.554)	(0.366)	(0.025)	(0.019)	(0.024)
	≥4	-0.323	-0.335	-0.145	0.026**	0.024*	0.025*
		(0.769)	(0.786)	(0.395)	(0.012)	(0.012)	(0.013)
	-3×LDD	0.124	0.347	-0.113	0.004	-0.051	-0.014
		(0.903)	(0.883)	(0.122)	(0.064)	(0.031)	(0.065)
	-2×LDD	0.801	0.538	0.017	0.017	0.011	0.009
		(1.003)	(0.937)	(0.208)	(0.054)	(0.024)	(0.053)
	-1×LDD	0.353	0.242	0.103	-0.139***	-0.127***	-0.138***
		(1.079)	(1.029)	(0.268)	(0.040)	(0.028)	(0.044)
	0×LDD	-0.747	-0.653	0.092	-0.066**	-0.104***	-0.034
		(1.087)	(1.089)	(0.340)	(0.033)	(0.035)	(0.030)
	1×LDD	-0.728	-0.948	-0.112	-0.116***	-0.107***	-0.119***

		(1.400)	(1.475)	(0.362)	(0.036)	(0.033)	(0.036)
	2×LDD	-1.473	-1.325	-0.534	-0.088**	-0.074**	-0.070
		(1.842)	(1.861)	(0.389)	(0.035)	(0.030)	(0.052)
	3×LDD	-2.366	-2.184	-1.155***	-0.172***	-0.188***	-0.187***
		(1.468)	(1.485)	(0.420)	(0.061)	(0.044)	(0.072)
	≥4×LDD	-4.043***	-4.075***	-1.337***	-0.150***	-0.163***	-0.152***
		(0.828)	(0.816)	(0.438)	(0.025)	(0.025)	(0.027)
	Error variance			4.926			0.443***
				(25.434)			(0.035)
	Error autocorrelation			0.954			0.314***
				(0.24)			(0.014)
Summary statistics:	Log-lik.	-39484	-36864	-18112	-18134	-11354	-17212
	AIC	79012	73866	36275	36313	22845	34475
	BIC	79183	74404	36470	36484	23383	34670
	Observations	17990	17990	17990	17920	17920	17920