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The Effect of Competition on Local Unemployment*

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Abstract

Competitive passenger rail can help workers access new or better jobs. This paper studies the wider economic impacts on local unemployment of the liberalized passenger rail between Ostrava, the third-biggest city in the Czech Republic, and Prague, its capital. The local impacts are estimated at the LAU 1 level (administrative districts) using the difference-in-differences method. The liberalization motivated the entry of two new private providers. The resulting competition in ticket prices, the number of connections, and service quality had a strong beneficial effect on labor market connectivity. It significantly reduced unemployment in the districts along the line compared with the control districts. The effect weakens with the level of urbanization of the treated district. It could partly transmit through higher firm entry and lower firm exit in the local market, as well as better skill matching on the back of higher inward and outward migration.

Keywords: Competition, difference in differences, districts, liberalization, local labor market, passenger transport, railways, unemployment, urbanization, EU country, OECD country.

JEL Classifications: J6, L4, R1, R4.

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1. Introduction

In 2010, RegioJet, a private provider of rail and other transportation services, reached an agreement with the Czech Railway Infrastructure Administration giving it the right to transport passengers on the Ostrava-Prague railway line. This agreement effectively initiated liberalization and competition (rivalry) on this line, the most lucrative in the country, which connects Ostrava, the third-biggest city in the northeast Czech Republic to Prague, its capital.[1] The Ostrava-Prague route is the busiest railway line in the Czech Republic, serving 1.4 million inhabitants in the environs of Prague, 0.8 million inhabitants in the environs of Ostrava, and many more residents of the numerous other cities and towns along the route. The natural competitive advantage of rail transport on this route persists even now because the direct highway connection between Ostrava and Prague involves a much longer travel time owing to its lengthy roundabout route via Brno (Tomeš et al., 2016).

After this initial step, RegioJet and then LeoExpress—also a private provider—entered the market on this route and began to compete with the state-owned firm, Czech Railways.[2] As a result of this open-access competition, ticket prices dropped, the frequency of available train connections increased, and the service quality on board improved—notably for commuters working in a mobile office (Tomeš et al., 2016). As a result, the overall labor market connectivity of the districts on the rail line increased—especially with Prague, an economic hub—and the local population in the districts along the rail line gained access to new and better jobs.

[1] Before 2010, most private providers focused on servicing rail lines that were subsidized, rather than competing on the unsubsidized Ostrava-Prague line.

[2] The vertical separation of Czech Railways in 2002 divided the company into two succession entities: Czech Railways (a joint-stock company and rail transport provider) and the Railway Infrastructure Administration (responsible for the ownership and management of the rail transport infrastructure).

This paper studies the effect of this liberalization on local labor markets, focusing on local unemployment. It uses the difference-in-differences (DID) methodology to estimate whether local unemployment in the district through which the Ostrava-Prague line runs decreased more than in other similar districts that were not served by this rail line. As the control group, we use the districts through which the rail line does not run and that do not neighbor any of the districts served by this line. The DID estimation is conducted within a regression framework in which we control for district-level confounding factors, such as the level of urbanization and industrialization. We also test for whether the effect of liberalization on local unemployment was largely uniform or whether it varied, depending on the level of initial urbanization.

Our paper fills an important gap in the literature. Based on a metanalysis of the existing literature on the wider economic impacts of transport corridors, Roberts et al. (2020) conclude that most studies on transport connectivity focus on highways, and the wider economic impacts of railways, especially modern rail in Europe, are understudied. In the context of the Czech Republic, the existing studies (Tomeš and Jandová, 2016; Tomeš et al., 2016; Jandová and Paleta, 2019; Tomeš and Fitzová, 2019) focus mostly on transport sector issues, such as vertical and horizontal separation, price competition, and customer choice and satisfaction. To our knowledge, none of the existing studies examine the wider economic impacts of railway passenger transport as we do in relation to the local labor market and unemployment. Moreover, our paper might be the first to apply the DID method in studying the impact of liberalization that reduces the cost of passenger transportation on existing railway infrastructure. Overall, our results suggest that competition in infrastructure service provision is an important policy issue to tackle in order to maximize the wider economic benefits from infrastructure investments—an important evidence for policymakers of other EU and OECD countries that consider liberalizing the passenger rail.

Specifically, we find that the liberalization of passenger rail on the Ostrava-Prague line reduced unemployment in the districts along this line significantly more than in the control districts (by about 1.5 percent). The permanent effect became significant in 2013 and strengthened further in the next two to three years. The beneficial effect in these districts did not spill over to the neighboring districts farther away. Moreover, the liberalization effect varied with the level of urbanization. Districts along this line with a larger population in cities experienced a proportionately lower reduction in unemployment—having about 1 percent more of the district population in cities than the average district reduced the beneficial effect of liberalization on local unemployment by one-tenth (or 0.15 percentage points). Exploring two possible transmission channels, we estimate that the beneficial effect of the passenger rail liberalization on unemployment can work through more firms entering and less firms exiting the local market. The growing numbers of local firms after the liberalization happens on the back of increased inward and outward migration in the district that could help with matching of skills needed by the local firms and thus reduce unemployment.

The remainder of the paper is organized as follows. Section 2 presents a literature review focused on the wider economic impacts of railways, heterogenous effects of transport connectivity, and liberalization of rail transport. Section 3 discuses liberalization of rail transport in the context of the Czech Republic and the Ostrava-Prague line, together with the related literature. Section 4 describes the data employed and the empirical methodology. Section 5 discusses the baseline estimation results and additional robustness checks. Section 6 concludes.

2. Literature Review

2.1. Wider Economic Impacts of Railways

Numerous studies estimate the wider economic impacts of highways, but less research of this kind has been conducted on railways, especially for modern rail in Europe. Based on a meta-analysis of 97 studies, Roberts et al. (2020) review the impact of transport corridor interventions on wider economic outcomes. On average, they find that transport connectivity improves local welfare and equity but worsens local environmental quality. Roberts et al. conclude that most studies on transport connectivity focus on highways, and the effect of railways, especially modern rail, is understudied.

Several papers study historical infrastructure projects to identify the wider economic benefits of railways (Lindgren et al., 2021; Donaldson, 2018; Donaldson and Hornbeck, 2016; Atack et al., 2010; Leunig, 2006). They focus mostly on freight, rather than passage transport. For instance, Donaldson (2018) examines the effects of railroad construction on commodity trade in colonial India. He finds that in colonial India railroads reduced the cost of trading and interregional price gaps, as well as increasing the local real income. Donaldson and Hornbeck (2016) estimate the aggregate impact of railroads on the US agricultural sector in 1890. They find that, without the railroad network, the total value of US agricultural land would be 60.2 percent lower, causing annual economic losses equal to 3.22 percent of the gross national product (GNP). Similarly, Atack et al. (2010) highlight that in the 1850s access to the railroad in the US increased the transformation of land to productive farmland by at least 25 percent. Focusing on passenger transport, Leunig (2006) studies the Victorian British railways and finds that railroads generated direct gains to workers thanks to social savings in time and money. Lindgren et al. (2021) use a large historical database spanning 1860-1917 to estimate the causal effect of new railroad on economic activity in Sweden. They find a large effect on non-agricultural real income of 130% over 30 years that can be driven by growth in industrial production and employment by about 100-300% rather than a reorganization of economic activity.

Studies documenting more recent experience include Pradhan (2019) and Baum-Snow et al. (2017). Pradhan (2019) consider five indicators of transport infrastructure for air and rail transport, including passenger transport by rail. He analyzes a cross-country data sample for the G-20 countries from 1961 to 2016 using a panel vector error-correction model and finds that transportation infrastructure stimulates economic growth in the long run. The short-run results are ambiguous and depend on the measure of transportation infrastructure employed. Baum-Snow et al. (2017) examine the effect of roads and railways on the decentralization of cities in China over the period 1990-2010. For railways, they find that radial railways decentralize industrial production. Each radial rail line decreases the growth of the central city's industrial gross domestic product (GDP) by 24 percent. Furthermore, it causes 35 percent of the central city's residents working in manufacturing to move or switch to other sectors, and almost 30 percent of the manufacturing jobs in cities are lost. The effect varies by the type of goods produced, with the largest effect from railroads relating to light manufacturing and the smallest to heavy industries. They suggest that the productivity in cities might increase when industrial production leaves the cities because the exodus frees space for more effective activities, such as for tradable services. Moreover, they highlight possible welfare gains. After the cost of accessing space due to new infrastructure decreases, commuting costs and the cost of living decline.

2.2. Heterogeneous Effects of Transport Connectivity

Several studies highlight the possibility of uneven, heterogeneous effects of transport infrastructure across space, depending on the proximity to the infrastructure and socioeconomic characteristics of regions. Using a theoretical framework, Redding and Turner (2015) review the literature on the relationship between transportation costs and the spatial distribution of economic activity. They conclude that employment density decreases significantly with growing distance to a railway and highway, if railways are the primary mode of transport. Other empirical studies find heterogenous effects in both developing and advanced economies. Although more studies document the heterogenous effects of highways (Chandra and Thompson, 2000; Michaels, 2008; Faber, 2014; Lin, 2017; Asturias et al., 2018; Lui et al., 2019; Melecky et al., 2019; He et al., 2020; Herzog, 2021; Terry et al., 2021), the heterogenous effects of railways have also been documented (Carbo et al., 2019; Wang and Wu, 2015; Chen and Haynes, 2017; Liu and Zhang, 2018; Meng et al., 2018).

The heterogenous effects of US interstate highways are highlighted, for instance, by Chandra and Thompson (2000), Michaels (2008), Herzog (2021), and Terry et al. (2021), who analyze historical data on the construction of interstate highways in the US. Chandra and Thompson (2000) find that highways affect the spatial allocation of economic activity, which increases in counties along a railway line and decreases in neighboring counties. Moreover, they identify that only some industries grow because of a decrease in transportation costs. Their estimates show the positive effects of new highways on nonmanufacturing industries, mostly positive but statistically inconclusive effects on manufacturing, and negative effects on farming.

Michaels (2008) identifies the heterogeneous impacts of highways on workers, depending on their skills. He concludes that highways had no effect on the demand in manufacturing for high-skilled workers relative to low-skilled workers. However, the differences in wage effects vary between these groups, depending on whether the skill is abundant or scarce in the county. Herzog (2021) studies the effects of the Interstate Highway System in 1950-2010 and finds that better connectivity and market access boosted employment but had only a small and delayed effect on wages. According to his results, states that built unplanned highways attracted employment from neighboring states. Herzog’s results imply heterogenous effects from new connectivity because building highways to improve lagging regions' market access might benefit newly connected regions at the expense of other regions. Terry et al. (2021) analyze the impact of interstate highways' stock on employment growth across counties in Texas during 1983-2012 and find a heterogenous impact in which counties with low initial levels of employment performed better than counties with high initial levels.

In the European context, Holl (2016) examines the effect of access to highways on firm-level productivity using micro-panel data for Spain in 1997-2007. He finds that highways can boost firm productivity through reduced transport costs and thus the costs of input and output, as well as broadening the market and creating higher competition. He identifies heterogeneity in the impact on firm productivity, with a larger positive effect on the productivity of firms in urban regions than on the productivity of firms in rural regions. The productivity effects can also vary across industries with significantly larger effects on manufacturing than other industries.

Also, in the developing country context, Farber (2014), Asturias et al. (2018), Lui et al. (2019), Melecky at el. (2019), and He et al. (2020) find evidence of the heterogenous effects of highways—focusing on Asia. Faber (2014) finds that massive investment in China's National Trunk Highway System had heterogeneous effects with respect to extant county characteristics. He concludes that large-scale interregional transport infrastructure can reduce the growth of output among connected peripheral regions relative to nonconnected regions. Lui et al. (2019) develop a theoretical model to examine how highway expansion influences productivity in China. They find that the decrease in transportation cost due to highway expansion induces stronger competition, which leads to lower markups and higher productivity. This impact is significantly stronger for private companies than state-owned companies. He et al. (2020) find heterogeneity in the effect of China's expressway system. Thanks to expressways, the GDP of poor rural counties grows more rapidly whereas that of rich rural counties grows more slowly than that of unconnected rural counties. These differences stem from prioritizing environmental quality over economic growth in developed regions, while applying the opposite strategy in poor regions.

Asturias et al. (2018) analyze the effect on welfare of the Golden Quadrilateral highway system in India. They highlight the role of allocative efficiency, which has been underresearched and can account for, on average, 7.4 percent of real income gains. However, this effect varies greatly across states and can reach as much as 18 percent in the largest Indian states, which had the lowest initial levels of allocative efficiency.[3] Melecky et al. (2019) discuss the wider economic impacts of transport corridor interventions in Pakistan based on the experience in India. They highlight that, on average, Pakistani households in newly connected districts can benefit from wider economic impacts, such as increasing consumption, better jobs, greater equity, and decreased air pollution. Their simulations for the planned China-Pakistan Economic Corridor, however, reveal that these impacts could vary substantially across Pakistani districts—both on and near the corridor—depending on the initial levels of output and input market development.

Turning specifically to the heterogeneous effects of railways, Wang and Wu (2015) study the causal effect of railway infrastructure on local economic development in China. They find that the Qingzang (Qinghai–Tibet) railway stimulated economic growth significantly in counties on the rail line, by about 33 percent on average. They also identify a nonlinear correlation between transportation infrastructure and the initial economic conditions in the region. The effect of new infrastructure appears stronger in underdeveloped regions and weaker in developed regions.

[3] This disproportion appears because low marginal costs in large states provide a cost advantage to local firms and enable them to charge high markups.

Estimating a panel data model for China in 2000-2014, Chen and Haynes (2017) find that high-speed rail decreased regional economic disparity by increasing economic convergence. They identify that the positive effects come from increased railway density and accessibility, with heterogenous impacts across regions. According to their results, growth was slower in urbanized regions with high population density.

Lin (2017) finds that connection to high-speed rail in China increases cross-city passenger flow by 10 percent and employment by 7 percent on average. Better connection with other cities driven by high-speed rail increases urban employment with elasticity of 2-2.5. The effect is larger in industries that require nonroutine cognitive skills.

Liu and Zhang (2018) study the impact of high-speed rail on 266 cities in China from 2006 to 2014. By estimating a gravity model, they identify the positive effects of high-speed rails on per capita productivity nationwide. However, the effects differ greatly across regions, as the sensitivity to the regional context reflects the local industrial structure, availability of skilled workers and resources, and developmental conditions.

Meng et al. (2018) construct a theoretical model to examine the effect of opening high-speed rail stations on local economic growth. By applying the model to Chinese data from 2006 to 2014, they find that the construction of high-speed rail can increase local economic growth by 14 percent, due to the resource redistribution effect. The radius of the effect differs with location (costal versus inland), and the resource redistribution negatively correlates with the market size of the surrounding counties.

Carbo et al. (2019) quantify spatial economic impacts arising from the introduction of high-speed rail between Madrid and Barcelona using DID and synthetic control analysis for two Spanish provinces on the rail line. They find that access to high-speed rail positively influences economic output (2.4% increase), the number of local companies (3.3% increase), and labor productivity (1.1% increase).

2.3. Liberalization of Rail Transport

Approaches to rail transport liberalization vary across countries and continents. McCullough (2006) compares the European and US rail systems and highlights three differences. First, the European system is publicly owned to a larger extent and, second, it is more focused than the US system on passenger transport. Third, the member states of the European Union (EU) share the rights with the EU whereas, in the US, the federal government controls the system. Bošković and Bugarinović (2015) summarize the four packages of directives that set the principles for restructuring the railway sector and liberalization of the railway market in the EU. They highlight the need for coordination based on compatible rules and institutions to build a single and efficient railway market in Southeast Europe. They argue that any liberalization should be done in phases so that all countries in the region can keep up with the pace of reform implementation. Only this sort of approach can bring gradual increase in competition, volume of services, and a modal share of the railways in the region. Nash (2010) highlights issues that appeared in the implementation of the railway packages in the EU. One main issue has been the insufficient independence of infrastructure managers from train operators because they often remain part of the same company or holding. Further issues include managers' lack of motivation to reduce costs, inability to establish an independent regulator with enough powers, and insufficient implementation of the charging framework.

The effects of railway liberalization and increased competition on regional development are underresearched. To date, most papers study the effects on transport efficiency. Tomeš (2017) analyzes 27 European countries in 1995-2013 and finds that vertical separation and competition did not significantly influence the modal shares of European railways. He proposes a focus on horizontal separation and privatization of freight transport, which might have larger effects than liberalization passenger transport. Bougna and Crozet (2016) do not find clear evidence that competition significantly influences productive efficiency because their statistical results remain inconclusive. They suggest that only a free and open approach will have clear and significant effects. They argue that liberalization and competition should not be the main goal of policy makers, who should instead prioritize wider economic benefits as the ultimate policy goal. Based on qualitative and quantitative analyses, Nash et al. (2014) find that no single rail structure fits all countries. The varying effects of vertical separation are driven by the density of railways and the proportion of freight traffic. They highlight the average cost savings from horizontal separation of freight and passenger service. The cost savings, however, appear to come from increased transparency, rather than competitiveness.

Cantos Sanchez et al. (2012) suggest combining vertical and horizontal reforms in the rail industry to achieve the best effects on transport efficiency. Cantos Sanchez et al. (2010) find that the entry of new freight operators has more pronounced effects than a similar tendering process in passenger services. Friebel et al. (2010) find that the result depends on the sequencing of reforms because adopting multiple reforms in one package could have negative effects. Policy makers should instead implement reforms sequentially to improve efficiency. Friebel (2007) highlights that it is hard to replicate improvements in productivity achieved in the EU via access to infrastructure and vertical separation in transition economies by following a similar restructuring process. This might stem from a high shadow price on government subsidies in transition economies.

Preston et al. (1999) discuss competitive strategies for a new entrant to the passenger rail industry, which include cream skimming (cherry picking), head on competition with and without price competition, product differentiation, and niche market entry. The latter strategy focuses on marginal consumer needs that the incumbent does not satisfy. Based on simulations of the entrant strategy and incumbent reactions, they suggest that competition for particular routes will increase economic efficiency only if cost reduction and product differentiation are implemented.

Our paper tries to connect the three strands of literature (wider economic effects, heterogenous effect, and competition effects of rail transport) and study the effect of horizontal railway liberalization and increased competition in passenger rail on a wider economic outcome: district-level unemployment. We do so, considering the potentially heterogeneous effects of competition on the liberalized railway route, based on the varying degree of district-level agglomeration/urbanization. Our paper fills an important gap in the literature and can help inform policy on rail transport in the EU.

3. The Czech Experience with Liberalizing Railway Passenger Transport on the Ostrava-Prague Route

The Czech Republic, a highly open and industrialized economy, is in the heart of Europe and has a tradition of rail transport going back to 1828. According to the UNECE (United Nations Economic Commission for Europe) Transport Statistics database, the Czech Republic has the highest railway density in Europe (121.4 kilometers of lines operated per 1,000 sq. km. in 2018). Nevertheless, the utilization of the infrastructure for passenger rail falls behind that of comparable countries, such as Austria, Poland, and Sweden, with 10.286 billion passenger-km in 2018 and only slowly developing high-speed rail. Czech legislation first established the Czech Railways in 1993 (Act of the Czech National Council No. 9/1992 Coll.). The Railway Act No. 266/1994 and its amendments defined the conditions for railway construction, operation, and transport. The Resolution of the Czech Republic No. 766 in 1995 divided the railways into national and regional rail. Parliament Act No. 77 from 2002 divided Czech Railways into two successor entities: Czech Railways (a joint-stock company) and the Railway Infrastructure Administration (RIA).[4] This act established vertical separation—that is, a formal

[4] In the process of path capacity allocation, RIA is obliged to prevent discrimination in track access for all carriers to reach maximum utilization of the path capacity.

separation of railway traffic from the ownership and management of transport infrastructure. The unbundling of infrastructure and services in 2003 enabled new operators to enter the market (Tomeš et al., 2014).

Actual competition in rail service first occurred in freight transport. Competing carriers in passenger rail operated on only a few regional routes before 2009. Because of business and financial risks, private operators were mainly interested in smaller subsidized regional lines, and even those lines attracted relatively few public tenders. Early on, the contracts were awarded directly to Czech Railways, which dominated the market (Tomeš and Jandová, 2016; Tomeš et al. 2016). Thus, RegioJet was the first company that bid for the unsubsidized main railway line, with the support of its parent company, Student Agency—which had the necessary experience in bus transport and flight ticket sales, as well as sufficient capital to withstand the business and financial risks.

In 2010, RegioJet concluded an agreement with the RIA concerning the transport operations on the national and regional rail owned by the Czech Republic (no. 192/10). After a short trial period, RegioJet began full operations on the Ostrava-Prague route in September 2011. Another private operator, Leo Express, was established in August 2011 and started regular service in January 2013 (the timeline is illustrated in Appendix Figure A1). The Ostrava-Prague route became the most important segment in competition and is the focus of our empirical analysis (see Appendix Figure 1 and Table A1).

[Figure 1 about here]

To date, only a few studies examine the effects of the liberalization of high-speed rail in the Czech Republic (Jandová and Paleta, 2019; Tomeš and Jandová, 2016; Tomeš et al., 2016; Tomeš and Fitzová, 2019). Tomeš and Jandová (2016) discuss the impacts of the direct competition on the Ostrava-Prague route and concur with Preston et al. (1999) that this type of competition leads to a price war—falling ticket prices, an increasing number of connections, improving service quality, and innovations to attract new customers.

Tomeš et al. (2016) highlight the slow liberalization of passenger rail in the Czech Republic until the two new private operators entered the market on the Ostrava-Prague route. These new entries led to open-access competition and a price war among the three operators, the two private ones and the publicly owned operator (Czech Railways). As a result, the second-class price declined, on average, 46 percent from September 2011 to September 2014.[5] The decrease in average prices was accompanied by increased price spreads and discrimination, improved on-board services, and a higher frequency of return trips per day (40 in 2013 versus 23 in 2010). The frequency of trains increased mostly between 4 am and 7 pm, in line with workers' demand, and the average capacity of a train decreased. The two new private operators both "stole" passengers from the incumbent provider and attracted new customers to rail transport by lowering prices and improved customer service. [6] In 2010-2014 the travel time stayed about the same on the fastest trains operated by the Czech Railways and decreased only slightly for the RegioJet trains, and in 2013-2014 it increased for the Leo Express trains because the operator began to service medium-size stations so as to reach additional passengers. To further document the ticket price dynamics, we use data presented in Jandová and Rederer (2013). We show the lowest normal fare available to the customers by carrier, with and without a loyalty program in Figure 2. An immediate decrease can be seen in the fares of the incumbent after the entry of the first competitor (RegioJet). Furthermore, at the end of 2012, RegioJet prices decreased markedly because of its new off-peak pricing strategy.

[5] The effect on the first-class tickets was smaller because travelers who purchase them are less price sensitive.

[6] Appendix Table A6 documents the number of passengers arriving from regions to Prague. Other than the Central Bohemian Region, the Moravian-Silesian and Olomouc regions contributed the most to the increase in arrivals of passengers by rail from regions to Prague between 2012 and 2014.

[Figure 2 about here]

Jandová and Paleta (2019) find that the competition on the same line in the Czech Republic stimulated demand for rail transport. However, because of increased compensation for state-imposed rebates, the competition did not improve the efficiency of public spending per train-km for long distances. The new competitors focused on the most lucrative routes, such as Ostrava-Prague, and the competition influenced final-price competition and service quality, rather than overall public spending efficiency.

Tomeš and Fitzová (2019) argue that the effect of opening the market on the Brno-Prague route is similar to that on the Ostrava-Prague line and include price drops, ridership increases, and numerous service innovations. On the Brno-Prague route, the incumbent (Czech Railways) employed its best rolling stock as a defense against the new entrant, RegioJet. Based on consumer surveys and moderated discussions, Tomeš and Fitzová (2019) find that the most important factor influencing the provider selection was departure time, not the price of the ticket, and that about 20 percent of travelers were new. The new providers attracted students, people who prefer high-quality services, low prices, and can work on the train (mobile office).

To our knowledge, none of the studies estimates the wider economic benefits of the liberalization or focuses on the local unemployment effects, as our study does.

4. Data and Methodology

Our empirical analysis uses district-level data for the Czech Republic. The country has 76 districts, which represent LAU 1 level administrative units, following the classification of the European Commission. In evaluating the wider economic impact of railway liberalization on the Ostrava-Prague line, we focus on annual changes in local labor markets—district-level unemployment—from 2005 to 2017. We select this time span because of data availability and comparability. The unemployment rate is available only as of 2005. The liberalization in transport effectively took place in 2010, followed by the entry of new private competitors in late 2011 and early 2013.[7] In 2018, the Czech government started massively subsidizing rail transport. Therefore, as of 2018, the data that influence the labor market are not directly comparable with those for the earlier period.

Because the number of districts did not change in 2005-2017, and there is no missing information in our dataset, we have a strongly balanced panel of 832 observations. The sources of our data are the databases of the Czech Statistical Office and the Czech Ministry of Labour and Social Affairs. Descriptive statistics for the data employed are in Appendix Table A2.

Our response variable is the total number of unemployed people. Two basic variables can be used to measure unemployment in the Czech Republic: the general unemployment rate based on survey data from the Czech Statistical Office and the rate of unemployed people based on the data from the Employment Offices registers. Because the general unemployment rate is not statistically representative at the level of districts, we use the latter variable to measure the labor market response to rail liberalization. The share of unemployed people is as follows:

$$\text{share of the unemployed} = \frac{\text{number of unemployed registered at Employment Offices}}{\text{total population, age } 15-64} \quad (1)$$

To avoid possible distortion of the estimated effects due to the denominator in the ratios, we consider only the numerator as our response variable ($unemployed$) and use the denominator as a control variable ($population$)—otherwise, we could not distinguish which part of the fraction is actually driving any changes. Additional district-level control variables include the average population living in the cities (calculated as the total population in the cities divided by the number of cities) denoted as $city$ and agricultural land as a percentage of total district land ($agri$) to proxy for the urbanization and industrialization rates, respectively. All

[7] This is the beginning of full operations. First LeoExpress trains first operated on this route in December 2012.

variables are the year-end values. Except for $agri$—which is expressed as a ratio—we transform all continuous variables ($unemployed$, $population$, $city$) into their natural logarithm. We also include district and time dummies to control for common factors across units and time.[8]

We construct our model based on the DID method as, for instance, in Melecky et al. (2019). For identification, we divide districts into three categories: (1) districts with a railway station on the Ostrava-Prague line (called "*on-track* districts");[9] (2) *neighboring* districts, which are adjacent to the *on-track* districts, and (3) *other* districts, which are not on the Ostrava-Prague line and are not adjacent. In our baseline estimation, we consider *on-track* districts as the treatment group and *other* districts as the control group. Here, we use the dummy variable $district\_1$ and neighboring districts are excluded from the estimation. We control for spillover effects by estimating the baseline model with district fixed effects and clustering standard error at the level of districts. As a robustness check, we also employ regional (NUTS 3) fixed effects or clustering of standard error at the regional level.

In addition, to explicitly highlight the possible spillover effects from the treatment and consider two more specifications of the treatment groups. First, the treatment group comprises both the *on-track* and *neighboring* districts to estimate the average direct and spillover effects on the two districts together—using the dummy variable $district\_2$. Second, we exclude *on-track* districts from the estimation to estimate the pure spillover effect on the *neighboring* district, which is considered as treated by the spillover in this case—using the variable

[8] We do not employ any specific variables for economic crises as the inclusion of time dummies controls for common cross-sectional shocks as a given time.

[9] At least one competitor to Czech Railways (RegioJet and LeoExpress) has a railway station in the on-track district.

$district\_3$. All $district$ dummy variables take a value of one for the treated districts and zero otherwise.

To divide the time span into the period before and after the liberalization was initiated, we use the post-treatment dummy variable $event$. To further capture the dynamics of the treatment effect, we make $event$ a vector of shift dummies. The first private competitor began to operate in late 2011, so we start the shift dummies in 2012.[10] Hence, the first shift dummy, $event\_2012$, takes a value of zero before 2012 and a value of one as of 2012. Similarly, the second (and third to sixth) shift dummy in the $event$ vector takes a value of zero before 2013 (2014, 2015, 2016, and 2017) and a value of one as of 2013 (2014, 2015, 2016, and 2017). Each of the shift dummies measures a marginal treatment effect as of the given year.

The baseline regression can be expressed by the following equation:

$$\begin{aligned} unemployed_{i,t} &= \beta_1 \cdot event'_t + \beta_2 \cdot district_i \cdot event'_t + \beta_3 \cdot city_{i,t-1} + \beta_4 \\ &\cdot district_i \cdot city_{i,t-1} + \beta_5 \cdot event'_t \cdot city_{i,t-1} + \beta_6 \cdot district_i \\ &\cdot event'_t \cdot city_{i,t-1} + \beta_7 \cdot population_{i,t} + \beta_8 \cdot agri_{i,t-1} + \gamma + \mu_i \\ &+ \tau_t + \varepsilon_{i,t} \end{aligned} \quad (2)$$

where the total number of unemployed in district i at time t ($unemployed_{i,t}$) is explained by the controls and dummy variables described above and by their interaction terms. γ denotes the constant, μ_i is an unobserved district fixed effect (district dummies), τ_t denotes an unobserved common time effect across districts (time dummies), and $\varepsilon_{i,t}$ is the idiosyncratic disturbance

[10] The inclusion of another 2011 shift dummy proved redundant and unnecessarily added to parametrization of the baseline model. Based on our estimations, the liberalization effect indeed appears significant only after 2012.

term (residual). To mitigate possible concerns about endogeneity with respect to the variables urbanization (*city*) and industrialization (*agri*), we lag these variables by one period.

The main estimated parameters of interest are β_2 and β_6. Specifically, β_2, which is associated with the interaction term $district_i \cdot event_t$ estimates the unemployment effect in the treated district after the rail liberalization. Therefore, β_2 is the average effect for the treated district group compared with the control district group. The policy intervention of rail liberalization stimulates the entry of new providers, promotes greater competition, decreases transportation prices, shortens the travel time, increases the number of connections, and ultimately increases the connectivity in the labor market and access to better or new jobs. Therefore, we expect β_2 to have a negative sign in the regression estimation—that is, the number of unemployed in the treated district should decrease after the liberalization, controlling for other confounding factors.

Coefficient β_6 estimates the effect of liberalization, depending on the level of urbanization (approximated by the average district population living in cities). More urbanized districts tend to have more developed infrastructure and better labor availability for and accessibility to job vacancies. Therefore, in the sense of diminishing marginal effects (returns), we assume that the effect of liberalization in more urbanized districts is weaker—that is, β_6 has a positive sign.

Even with the supporting evidence that the liberalization act was a surprise policy action (see Section 3), concerns about possible endogeneity could remain. For instance, that the city of Prague, the economic center of the Czech Republic, needed greater labor supply due to overheating labor market. The inclusion of Prague on the line may have influenced political decisions about the liberalization to improve labor supply to the labor market in Prague suffering from unmet demand. The positive effect of liberalization on the city's labor market thus might be biased because of potential endogeneity. For this reason, we need either to find a suitable instrumental variable to control for endogeneity or exclude Prague from the regression. According to Czech legislation, Prague has special status and is not considered a district. Therefore, the legislation determined the empirical strategy for us, and the city of Prague is excluded from our baseline dataset.

Furthermore, we exclude all districts in the Central Bohemia Region. Because this region surrounds Prague, the connectivity between districts in Central Bohemia Region and Prague was already strong before the liberalization—including improvements in local railway connectivity and the inclusion of local railways in the integrated transport system of Prague's metropolitan area. Liberalization of the Ostrava-Prague line did not influence the Central Bohemian labor market significantly because commuting from the Central Bohemia Region to Prague has been always driven by the local transport network, rather than by long-distance rail. Therefore, including the Central Bohemia Region would bias the results downward.

Before discussing our estimation results, we test the parallel-trend assumption for the pre-treatment periods on which the DID method relies. Figure 3 depicts the development of unemployment in on-track and other districts to support this hypothesis. The F-test of parallel trends in our baseline model does not reject the null hypothesis of parallel pre-treatment trends at the common 5 percent significance level. Still, the p-value of the F-test (0.076) is rather low ($F(1, 53) = 3.27$). Further inspection of the data reveals that any possible breach of the parallel trends assumption could only have occurred in the initial year, 2005. When we conduct the test separately for a pre-treatment period starting in 2006, we can be much more confident about the validity of the parallel-trend assumption with the p-value of 0.196 ($F(1, 53) = 1.71$). The parallel-trend assumption is further confirmed as of 2007 ($F(1, 53) = 0.44$, p-value = 0.511).

At the beginning of our sample, around 2005, a notable difference in urbanization (measured by the *city* variable) is found between on-track and other districts. In 2007, this difference was considerably lower. This structural aspect might also be a factor in the possible breaching of the parallel-trends assumption at the very beginning of the pre-treatment period. For this reason, we control for urbanization levels in all models by including the *city* variable both in levels and in the interaction with the treatment variables, so this initial difference should not influence our estimation results. Our estimation results are not materially changed by rerunning our regression as of 2006.

[Figure 3 about here]

5. Results and Discussion

5.1 Baseline Estimation Results

We estimate Equation 2 using the DID method in a regression that controls for confounding effects. Appendix Table A3 presents the detailed estimation results for this comprehensive regression model. Table 1 summarizes these estimation results for all three types of treatment groups examined.

[Table 1 about here]

First, we focus on the results for the on-track treatment group. On average, we find that liberalization of passenger rail on the Ostrava-Prague line significantly affected the local labor market of the district on the line. In particular, in 2013 liberalization significantly decreased unemployment in the on-track districts, then in 2014 the unemployment decreased further, and the beneficial effect on unemployment peaked in 2015.

The estimation results for the post-treatment dummies in Appendix Table A3 reveal that the number of unemployed workers fell in all districts during the post-treatment period.

However, this downward trend was significantly stronger in the on-track districts, through which the Ostrava-Prague line runs. The beneficial liberalization effect did not emerge immediately after all railway providers started to compete on the route in 2011. A gestation period of one to two years was necessary for the on-track competition after liberalization to generate wider economic impacts on local unemployment. The permanent effect first became significant in 2013 and then progressively strengthened in 2015. We see no further significant increase in the magnitude of the permanent effect as of 2016. This estimation result dovetails with our observations from the literature review: Because new providers entered the market one by one, higher competition resulted in a price war and services provided improved progressively (Tomeš et al., 2016; see Figure 2). By combining all the significant coefficients, we estimate that the liberalization permanently decreased the number of unemployed in the on-track districts by, on average, 1.5 percent compared with the control districts.

The estimation results for the interactive effect show that the liberalization effect depends on the level of urbanization. Our assumption was that more urbanized districts tend to have stronger connectivity to the labor market, and additional improvements in connectivity would not generate beneficial effects as significantly as in less urbanized districts. This assumption is confirmed by our estimation results, which show that, in on-track districts, when the initial urban population in cities is larger by 1 percent, the average permanent effect of liberalization on local unemployment decreases by 0.15 percentage points. This estimated interactive effect is significant even when controlling for the general effect of urbanization in the post-treatment period. As shown by the results in Appendix 2, more urbanized districts tend to have permanently higher unemployment beginning in 2014. Furthermore, the estimation results for the interactive effects confirm the length of a gestation period for the liberalization effect and its progressive strengthening from 2013 to 2015.

The estimation results in the next two columns (*on-track* + *neighboring* and *neighboring*) examine whether the liberalization effect had significant spillovers to a wider geographic area along the railway route. The results for the *on-track* plus *neighboring* districts follow patterns similar to those for the *on-track* districts but with lower significance. We estimate a significant permanent average effect—reducing unemployment—beginning in 2013. This permanent effect is comparable to the estimation results in the *on-track* regression, but the liberalization effect does not significantly increase in 2014-2015 and overall is substantially smaller than in the *on-track* districts. In addition, the estimated interactive effect confirms that the beneficial effect on unemployment is smaller in more urbanized districts.

The estimation results in the last column, which compares *neighboring* districts with the control group, are similar, but statistically weaker. They reconfirm that the effect of rail liberalization significantly lowered unemployment but also reveal that that the strength of the effect decreases with the distance from the rail line and is significantly smaller in more urbanized districts.

5.2 Alternative Regression Specification with Total Permanent Effects

Our baseline estimates provide the main empirical evidence about the beneficial effect of rail liberalization on the local labor market in districts on the Ostrava-Prague line. Next, we test whether these findings also hold in an alternative regression specification. Here, we do not put all the post-treatment dummies into the same equation and estimate regressions separately with each of the dummies. The alternative specification is more parsimonious in the estimated number of parameters and focuses on the total permanent effects over time, considering varying years in which the effect could have started. Table 2 summarizes the estimation results for all three treatment groups.

[Table 2 about here]

The estimation results for the *on-track* treatment group generally confirm our baseline findings. The rail liberalization permanently lowered unemployment in *on-track* districts compared with *control* districts that do not neighbor the *on-track* districts. This average effect weakens with the level of urbanization of the *on-track* districts. The estimated coefficients of all the post-treatment dummies (timed differently) are significant at the 1 percent level. The estimates do not imply that the liberalization effect became significant in 2012 (or earlier). Because *event_T* is a subset of the *event_T-1* dummy, and we do not control for all other correlated subsets (as we do in our baseline estimation), the significance of *event_2012* may be driven by later periods that we average. What we can infer, however, is that the coefficient magnitude rises, and the effect on local unemployment increases over time, reaching its maximum in 2015—one year later than suggested by our baseline estimates. At this maximum, the total average effect reaches -1.6 and is similar in magnitude to the total average effect in our baseline estimation of -1.5. The interactive effects follow a pattern similar to that of the average effects. They suggest that an on-track district with an urban population that is 1 percent larger than that of the average on-track district has about a 10 percent weaker permanent liberalization effect.

In contrast to our baseline estimates, the estimation results for the alternative specification suggest no significant effect of liberalization in the neighboring districts. Therefore, we can infer reliably only that the liberalization effect weakens with distance from the rail line. If neighboring districts experience some (spillover) effects, they are probably too weak, unstable, and observable only in a more detailed (parametrized) analysis, such as our baseline regression. Although the *on-track + neighboring* estimates indicate a significant liberalization effect, it is a consequence of merging districts in which the effect is significant (*on-track*) and in which it is not (*neighboring*).

5.3 Alternative Specification Accounting for a Lag in the Liberalization Effect

Thus far our results suggest that the effect of liberalization on local labor markets does not emerge immediately, but with a lag. One could argue that the inclusion of this lag in the DID estimation might bias the results downward because of the different possible trajectories in which the liberalization effect grows to its maximum. To address this possible bias, we exclude the years in the potential gestation period and reestimate the total permanent effect. The last competitor began to operate on the Ostrava-Prague line in late 2011, and the gestation lags considered are only from 2012 to 2016. In the reestimations without these various years, the post-treatment dummy is always set to one as of 2012. Table 3 summarizes the reestimation results excluding the potential gestation periods.

[Table 3 about here]

The reestimation results are generally in line with our earlier findings. The average and interactive effects of liberalization on unemployment in the *on-track* districts hold, and the coefficient magnitudes rise when the gestation period is excluded. The reestimation results confirm the stability of the interactive urbanization effect and its magnitude relative to the average effect (of about one-tenth). The reestimation also confirms the gradual strengthening of the average liberalization effect over time. In fact, it suggests that the average effect could have continued to increase until 2016.

The reestimation results for the other two treatment groups reconfirm earlier results that the liberalization effect weakens with distance from the line. The effect appears significant and robust only in the districts through which the line runs.

5.4 Inclusion of the Central Bohemia Region

As highlighted earlier, the Central Bohemia Region was excluded from the baseline regression because it has its own particular connectivity with Prague within the Prague metropolitan integrated transport system and as such is unsuitable for inclusion in both the treatment and the control groups. However, as another robustness check, we test whether the main findings from our baseline regression can withstand the inclusion of the twelve districts in the Central Bohemian Region in the regression. For simplicity, Table 4 reports only the results for the on-track treatment group.

[Table 4 about here]

The estimation results in Table 4 suggest that the effect of liberalization on local unemployment holds even with the inclusion of the districts from the Central Bohemian Region. Because of the superior labor market connectivity in the Central Bohemian Region (compared with other on-track, neighboring, and control regions), all the results are weaker in magnitude, and their statistical significance is lower. However, the liberalization effect in the on-track districts remains significant, and the basic patterns in the baseline results do not materially change. We do not report the estimation results for the other two treatment groups because all these results appeared insignificant.[11] The latter finding reconfirms the weak and unstable nature of the liberalization effect in districts that are farther from the line.

5.5 Further Robustness Tests

A specific regional policy on passenger transportation can influence the potential for workers to commute by train. These regional policies are common in all district of each region of the Czech Republic. As another robustness check, we thus reestimate all the regression specifications with robust standard errors clustered at the level of regions (NUTS 3 level). Even after we control for possible clustering (correlation) of disturbances at the level of regions, the statistical significance of all the results does not materially change, as is documented for our

[11] One exception is the regression for permanent effects. When *event_2016* is interacted with the *on-track* group and *neighboring* treatment group, the estimation results become significant at the 10% level.

baseline regression specification in Appendix Table A4. We also directly replace district FE with regional FE and reestimate the model, and all results hold (see Appendix Table A5).

In addition, we try to control for liberalization of the Brno-Prague line. However, because the first private provider initiated service on this line only in 2016, and most of the route is identical to that of the Ostrava-Prague line, this change influenced only a very small portion of our dataset (districts)—only two observations. As a result, all the reestimations remain nearly unchanged after liberalization of Brno-Prague line is controlled for.[12]

5.6 Discussion of Possible Transmission

Next, we discuss our conjectures about the liberalization of passenger rail, the ensuing increase in passenger transport, and in turn, the increase in labor market connectivity could be transmitted to affect local unemployment. Although microeconomic data to investigate this transmission in detail are not readily available, we try to qualitatively discuss this possible transmission with the support of the existing literature. We believe that there could be at least six transmission channels: (1) brain drain, (2) upward migration in local labor markets, (3) increased employment because of greater demand for local services, (4) increased labor demand because of firms' relocation to districts around the Ostrava-Prague rail line owing to the district's increased (labor market) connectivity, (5) possible positive effects of firm collocation and clustering on productivity, wage, and reduced employment, and (6) reinforcement of transmissions 1-5 through better access to higher-quality education.

1. Local skilled workers leave their jobs for better-paid positions in economic hubs along the rail line, such as Prague, Ostrava, and Olomouc. Typically they do not move to the new work location but rather pot for commuting. This choice may occur because of housing

[12] All estimation results not presented in the paper are available from the authors upon request.

supply shortages and high property prices in those hubs (especially in Prague), because only one family member moves to a better job, or because the family decides to take advantage of less expensive and locally affordable housing and other services. The higher elasticity of skilled labor migration to increased market connectivity is consistent with the findings of Lin (2017) and ICF Consulting Services (2018).

2. The local jobs given up by local skilled workers who travel to better jobs in economic hubs can be taken by less skilled (or unemployed) local workers who upgrade their skills through vocational training or by learning on the job. This spillover effect on local human capital development could cascade through intermediate-skill jobs and so on, and thus reduce local unemployment. For instance, Moretti (2011) finds, using data for the U.S., that an exogeneous shock—possibly also exogenous improvement in connectivity to labor markets—can increase local productivity and encourage workers to move up to locally available employment opportunities that are more productive. Kaplanis (2010) finds that, in the UK, an increase in high-skill residents in a locality increases the chance of local men with no qualifications to be employed. Importantly, if high-skill commuters later return to staff newly emerging firms or establish businesses in their district of origin they can help create job opportunities and positive knowledge externalities in the local labor market—the same way a brain drain can turn to a brain gain in the context of migration across countries (Khan and Islam, 2006; Chacko, 2007; Meyer, 2008).

3. At the same time, the commuting by skilled workers for better-paid jobs can increase local demand for quality services in their district of origin—on top of the services demanded by locals who remain—and ultimately the entire local demand. The heightened demand for local (non-tradable) services can further increase demand for labor and reduce local unemployment. Using data for the UK, Mulalic, Van Ommeren, and Pilegaard (2014) find that the increase in commuters wages could happen only with a lag of around three years.

But the rising wages for commuters will ultimately push up also the wages of non-commuters in the district and boost the entire local demand (Green et al. 2019). Moreover, commuting for better-paid jobs can help reduce wage differentials between the rich and the poorer regions and boost demand in lagging districts (Hazans, 2004).

4. Furthermore, the better connectivity of districts on the Ostrava-Prague line, which benefits from higher passenger and labor market connectivity, attracts more firms to relocate to these districts in order to benefit from more concentrated and competitive labor access (supply). This increases demand for labor in the local districts and reduces local unemployment. For instance, Audretsch et al. (2015) argue that connective infrastructure can boost local startup activity but that infrastructure facilitating connectivity and linkages among people can be relatively more conducive. They further argue that in some sectors broadband can be more conducive to startup activity than highways and railroads. Lee and Clarke (2019) find that, in the UK, local high-tech industries create a positive jobs multiplier, with each 10 new high-tech jobs creating around 7 local non-tradeable service jobs about 6 of which go to low-skilled workers—the jobs, however, are often poorly paid service work. By contrast, employment rates of mid-skilled workers do not increase, while their wages do. Moretti and Thulin (2013) find that when a local economy generates a new job by attracting a new business in the traded sector, a significant number of additional jobs are created in the local non-tradable sector. Moretti (2010; 2013) highlight the strong multiplication effect of high-tech industries on the local economy in the US by creating 4-5 new jobs in non-tradable services for each additional high-tech job. In an urban setting, Mejia-Dorantes et al. (2012) find that the location patterns of economic activities are related to accessibility and that agglomeration, through economies of scale, plays an important role in a firm's location choice.

5. Because firm density, collocation, and clustering increase, so do knowledge spillovers; labor matching becomes more effective locally, and labor productivity and wages rise. This improving ecosystem attracts more commuters and firms to better-connected districts and induces a virtuous cycle that reduces local unemployment until the price of labor and local productivity settle at a new equilibrium for a given level of passenger transport cost and labor market connectivity. This transmission can vary for locals with higher and lower skills. Combes et al. (2020) find that rural migrants face high competition, and therefore they benefit less from agglomeration and migration. However, more-skilled locals or local that can upgrade their skills might benefit greatly.
6. These types of transmission can be reinforced by better access to education and higher-quality education, which can help improve local skills among those who are (or will) be commuting for better jobs or intensify their upward progression to higher-skill jobs in the local labor market. In turn, this can motivate firms to locate in more connected districts and further boost co-location spillovers and local agglomeration effects. The latter could be the strongest in districts with the lowest initial level of agglomeration—as we also find. This potential type of transmission might be consistent with the findings of Hunt and Gauthier-Loiselle (2010): in the US, a one-percentage-point increase in the share of immigrant college graduates in the local population boosts patents per capita by 9-18 percent.

We leave a comprehensive empirical study of these detailed transmission channels using microeconomic data to future research.

Next, we explore just two potential transmission channels using additional district-level data. The first channel concerns the effect of the passenger rail liberalization on firm entry and exit, testing the hypothesis that greater labor market connectivity through more competitive passenger rail encourages new firms to enter the local market of the connected district (or relocate there) and thus provide new jobs. At the same time, the comparative advantage of existing firms located in the district with better labor market connectivity should reduce firm exit. Working in tandem, the second channel helps the local labor market better match the skills required by local firms because, after initial commuting period, over-skilled (and under-skilled) labor migrates out for better jobs and labor that matches the skill needs of local firms stays or migrates in (Brunello and Wruuck, 2019; Iammarino, Rodríguez-Pose and Storper, 2019; Chassamboulli, and Palivos, 2014; Mendes, Van Den Berg, and Lindeboom, 2010). To explore the two potential transmission channels, we estimate regressions with the log of the number of district-level firm entries and exits, as well as inward and outward migration of people in a DiD setup controlling for district-level fixed effects and time dummies. The results are reported in Table 5.

[Table 5 about here]

The estimation results suggest that, after the passenger rail liberalization, on-tract districts experienced higher entry of new firms into their local market compared with the districts not neighboring the on-tract districts, significant at the 1 percent statistical level. At the same time, after the passenger rail liberalization, the on-tract districts experienced a reduced firm—though significant only at the 10 percent level. In parallel, the on-tract districts experience higher inward migration (immigration) into the district, perhaps as outward migration freed up vacancies, and more firms created more new job opportunities. The higher inward and outward migration effects in on-tract districts are significant at the 5 percent level, with inward migration estimated slightly stronger than outward migration. These results support the hypothesis that rail liberalization triggered a significant firm entry and reallocation into treated districts and better matching in the local labor market that reduced local unemployment.

6. Conclusion

This paper studied the wider economic impact of liberalized passenger rail service on local labor markets in the Czech Republic. Because of the liberalization, two private providers entered the market for the highly lucrative Ostrava-Prague line in late 2011 and mid-2013 and began to compete with the state-owned incumbent. This competition lowered ticket prices, increased the frequency of train connections, and improved the quality of on-board services—notably for mobile-office workers/commuters.

This open-access competition significantly affected the local labor markets in districts through which the line runs but did not have robust and stable spillover effects on neighboring districts farther away from the line. In particular, the competition reduced local unemployment in the on-track districts significantly more than in the control districts that do not neighbor the on-track districts. This significantly greater reduction in local unemployment occurred first in 2013, and the effect progressively strengthened until 2015-2016. We estimated that the liberalization permanently decreased the number of unemployed workers in the on-track districts by, on average, 1.5 percent compared with the control districts.

Interestingly, this liberalization effect was weaker in more urbanized districts with a larger urban population, presumably because labor market connectivity in these districts was already adequate for the unemployed to find decent jobs. We estimate that a 1 percent larger urban population decreases the permanent effect of the liberalization on district unemployment by one tenth (or 0.15 percentage points). Therefore, the overall liberalization effect was even stronger than 1.5 percent in the on-track districts that were much less urbanized than the average district in the Czech Republic. Hence, rail liberalization has benefited the much less urbanized (lagging) districts disproportionately more and could be called an effective spatially-equitable policy.

We discussed several possible transmission channels through which the beneficial effect of passenger rail liberalization on the unemployment in on-tract districts could work through, referencing the existing literature. We explored two of these channels empirically using additional district-level data. We estimated that the beneficial effect of the passenger rail liberalization on local unemployment can work through more firms entering and fewer firms exiting the local market after the liberalization. The growing numbers of local firms after the liberalization happen on the back of increased inward and outward migration in the district that may help with the matching of skills needed by the local firms, and thus reduce unemployment.

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Tables in the Main Text

Table 1: Estimation Results for Equation 2 with Marginal Treatment Effects Over Time

Treatment group	On-track		(On-track + Neighboring)		Neighboring	
Effect	Average	Interactive	Average	Interactive	Average	Interactive
event_2012	0.0002	-0.002	0.341	-0.035	-0.239	0.030
	(0.429)	(0.046)	(0.359)	(0.039)	(0.715)	(0.079)
event_2013	-0.467***	0.048***	-0.372***	0.039***	-0.360**	0.038**
	(0.161)	(0.016)	(0.087)	(0.009)	(0.156)	(0.017)
event_2014	-0.344**	0.034**	-0.210	0.021	-0.290	0.031
	(0.149)	(0.015)	(0.150)	(0.016)	(0.285)	(0.030)
event_2015	-0.709***	0.071***	-0.451	0.046	-0.350	0.036
	(0.140)	(0.014)	(0.287)	(0.029)	(0.650)	(0.069)
event_2016	-0.301	0.033	-0.464	0.048	-0.298	0.028
	(0.279)	(0.028)	(0.353)	(0.037)	(0.770)	(0.082)
event_2017	-0.216	0.027	-0.024	0.006	0.479	-0.051
	(0.275)	(0.027)	(0.233)	(0.024)	(0.417)	(0.045)

Notes: The first number is the coefficient value. Robust standard errors are in parentheses. ***, **, and * statistical significance at the 1%, 5%, and 10% level, respectively. The column headings denote the type of treatment group and distinguish between the average effect and the interactive effect depending on the level of urbanization. The row labels indicate the timing of the post-treatment dummy used in the estimation. We estimate all effects together, thus there are only three estimated models in the table. The complete estimation results for all specifications are available in Appendix Table A3.

Table 2: Estimation Results for Model with Total Permanent Effects

Treatment group	On-track		(On-track + Neighboring)		Neighboring	
Effect	Average	Interactive	Average	Interactive	Average	Interactive
event_2012	-1.092***	0.110***	-0.496	0.052	-0.918	0.100
	(0.391)	(0.040)	(0.309)	(0.033)	(0.614)	(0.068)
event_2013	-1.325***	0.134***	-0.705**	0.073**	-1.041	0.112
	(0.377)	(0.037)	(0.338)	(0.035)	(0.701)	(0.076)
event_2014	-1.479***	0.150***	-0.850**	0.088**	-1.083	0.116
	(0.389)	(0.039)	(0.416)	(0.043)	(0.908)	(0.097)
event_2015	-1.626***	0.166***	-1.008*	0.105*	-1.080	0.114
	(0.418)	(0.042)	(0.525)	(0.054)	(1.171)	(0.124)
event_2016	-1.610***	0.166***	-1.098*	0.115*	-0.971	0.101
	(0.510)	(0.051)	(0.618)	(0.063)	(1.365)	(0.145)
event_2017	-1.561***	0.163***	-1.016	0.108	-0.584	0.061
	(0.576)	(0.058)	(0.641)	(0.066)	(1.374)	(0.146)

Notes: The first number is the coefficient value. Robust standard errors are in parentheses. ***, **, and * statistical significance at the 1%, 5%, and 10% level, respectively. The column headings denote the type of treatment group and distinguish between the average effect and the interactive effect depending on the level of urbanization. The row labels indicate the type of post-treatment dummy used in the estimation. We estimate all total effects separately, thus we run six regressions for each treatment group.

Table 3: Estimation Results for Model with Total Permanent Effects and Varying Gestation Periods

Treatment group	**On-track**		**(On-track + Neighboring)**		**Neighboring**	
Effect	Average	Interactive	Average	Interactive	Average	Interactive
2012	-1.329***	0.134***	-0.663*	0.069*	-1.079	0.117
	(0.414)	(0.042)	(0.347)	(0.037)	(0.710)	(0.077)
2012-2013	-1.554***	0.157***	-0.822*	0.085*	-1.225	0.131
	(0.442)	(0.044)	(0.415)	(0.044)	(0.888)	(0.095)
2012-2014	-1.807***	0.184***	-1.016*	0.105*	-1.344	0.143
	(0.482)	(0.048)	(0.517)	(0.054)	(1.145)	(0.122)
2012-2015	-1.954***	0.200***	-1.181*	0.123*	-1.335	0.141
	(0.581)	(0.058)	(0.626)	(0.065)	(1.395)	(0.148)
2012-2016	-2.093***	0.217***	-1.202*	0.128*	-0.985	0.104
	(0.671)	(0.067)	(0.711)	(0.074)	(1.514)	(0.161)

Notes: The first number is the coefficient value. Robust standard errors are in parentheses. ***, **, and * statistical significance at the 1%, 5%, and 10% level, respectively. The column headings denote the type of treatment group and distinguish between the average effect and the interactive effect depending on the level of urbanization. The first column is for the gestation period excluded from the estimation. We estimate all total effects separately, thus there are five estimated models for each treatment group.

Table 4: Estimation Results for On-track Districts Including the Central Bohemia Region

	Marginal Treatment Effect		**Total Permanent Effects**		**Total Permanent Effects (Varying Gestation Period)**	
Effect	Average	Interactive	Average	Interactive	Average	Interactive
event_2012	0.005	-0.001	-0.876*	0.090*	-1.073**	0.110**
	(0.404)	(0.043)	(0.458)	(0.046)	(0.491)	(0.049)
event_2013	-0.346**	0.036**	-1.071**	0.110**	-1.263**	0.130**
	(0.173)	(0.017)	(0.452)	(0.045)	(0.527)	(0.053)
event_2014	-0.305**	0.030**	-1.209***	0.124***	-1.473**	0.152**
	(0.131)	(0.013)	(0.457)	(0.045)	(0.577)	(0.058)
event_2015	-0.516**	0.053**	-1.332***	0.138***	-1.623**	0.169***
	(0.214)	(0.021)	(0.487)	(0.049)	(0.636)	(0.064)
event_2016	-0.358	0.039	-1.361**	0.142***	-1.752**	0.184**
	(0.260)	(0.027)	(0.524)	(0.053)	(0.705)	(0.070)
event_2017	-0.208	0.026	-1.323**	0.140**	NA	NA
	(0.239)	(0.024)	(0.568)	(0.057)		

Notes: The first number is the coefficient value. Robust standard errors are in parentheses. ***, **, and * statistical significance at the 1%, 5%, and 10% level, respectively. The column headings denote the model specification and distinguish between the average effect and the interactive effect depending on the level of urbanization. All estimation results are for the on-track districts as the treatment group. The row labels denote the type of post-treatment dummy used in the estimation. In the event of total permanent effects excluding the gestation period, the year denotes the end year of the excluded time span, which began in 2012.

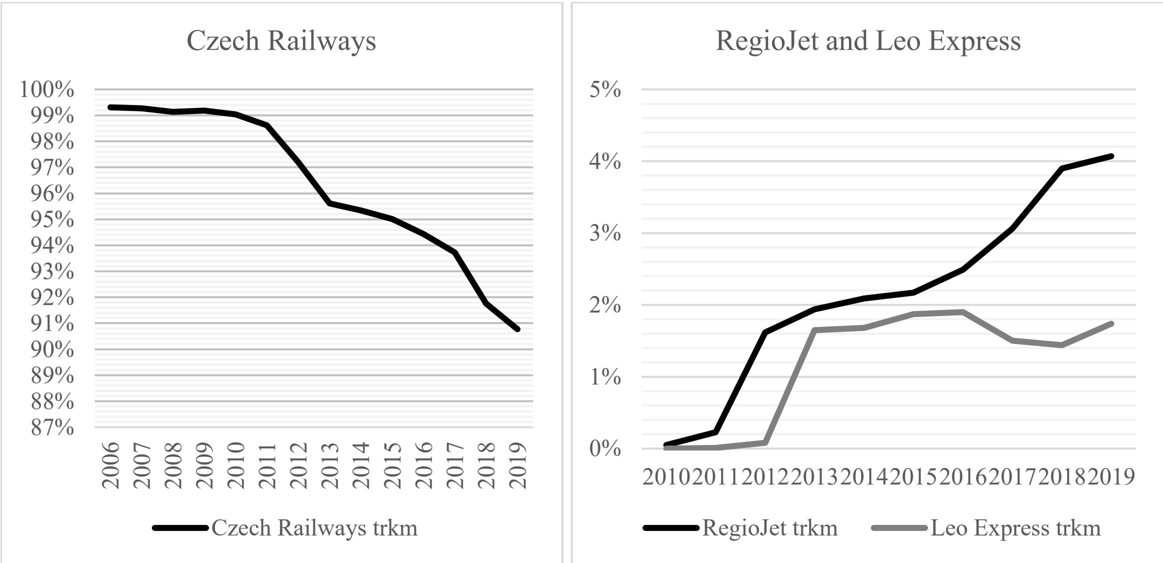
Table 5: The Effects of Rail Liberalization on Firms Entry and Exit, and Outward and Inward Migration

	New Firm Entry	Firm Closure/Exit	In-Migration	Out-Migration
Treatment (2012)	0.078***	-0.097*	0.090**	0.076**
	(0.022)	(0.054)	(0.040)	(0.030)
District FEs	YES	YES	YES	YES
Time Dummies	YES	YES	YES	YES
R^2	0.080	0.258	0.069	0.033
Number of Obs.	702	702	702	702

Notes: All columns represent a simple total permanent effects model (running for the period of 2005 – 2017) with post-treatment dummy starting in 2012. The response variables are the natural logarithms of the number of firms that entered and exited the district's market, and the number of people that emigrated from and immigrated to the district, respectively. In the DiD estimation, the control group are districts not neighboring with the on-tract (treated) districts. The first number is the estimated coefficient value. Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels.

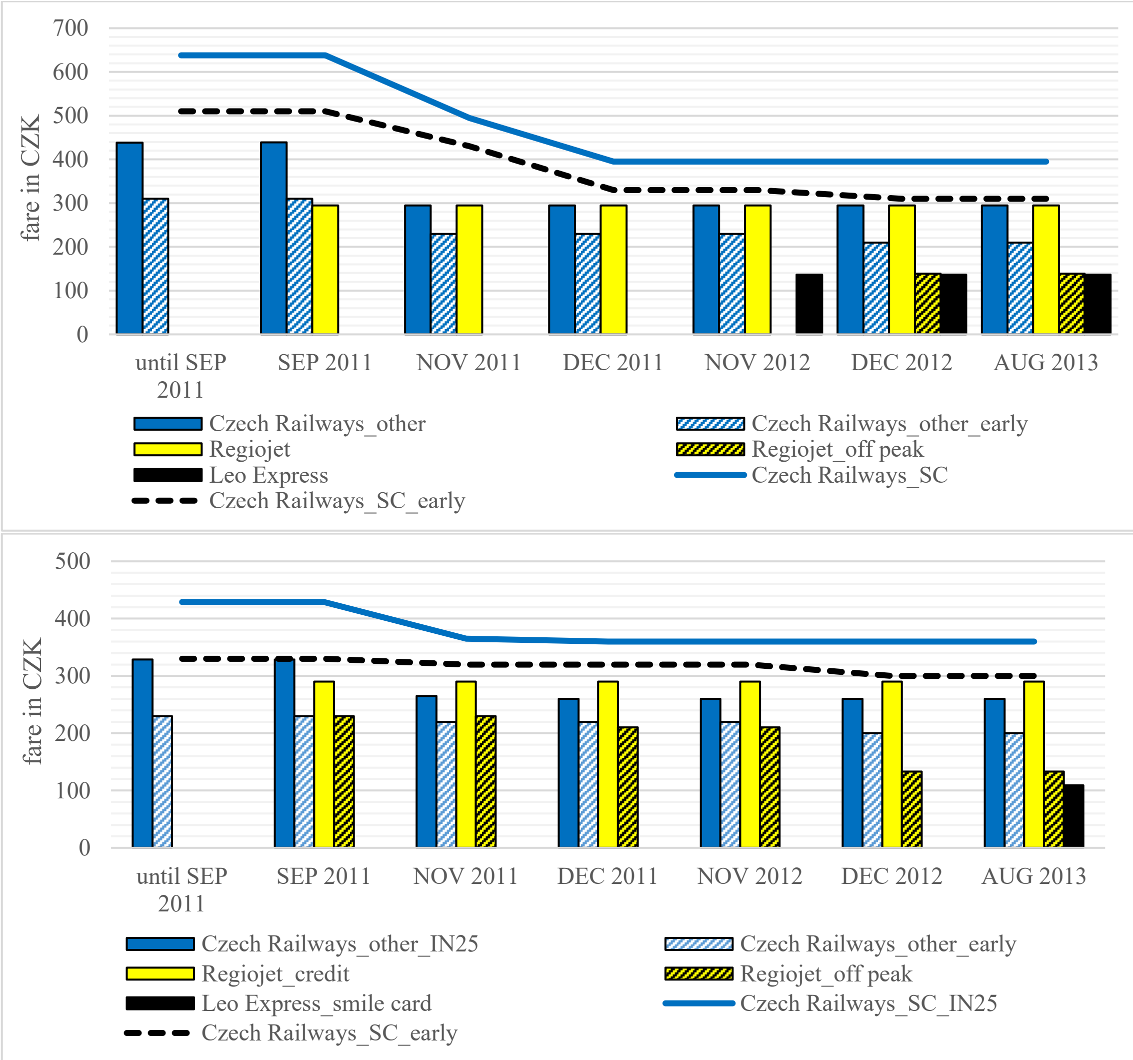
Figures in the main text

Figure 1: The Share of Competing Carriers on Passenger Rail Output (all lines in the Czech Republic)



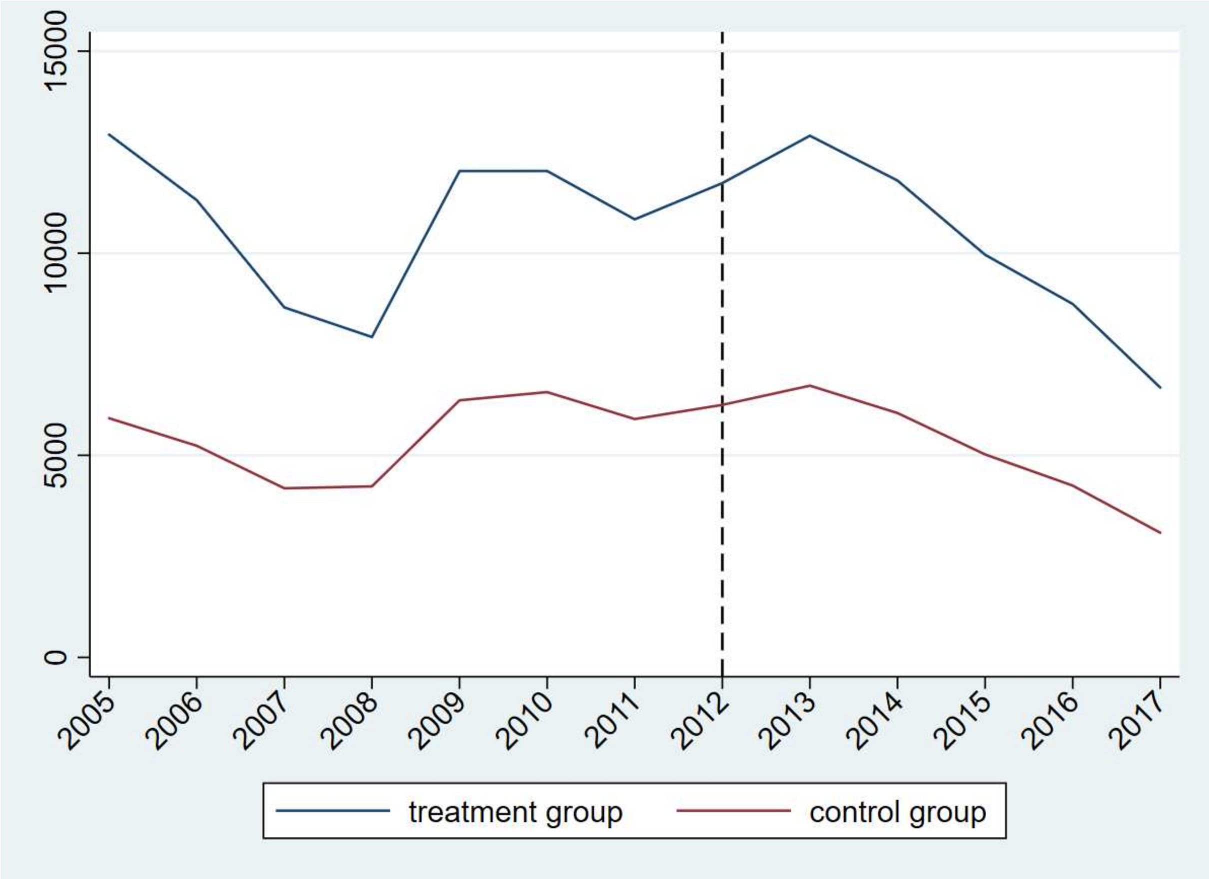
Source: Author calculations based on SŽDC Annual reports (2006-2019).
Notes: trkm = Train kilometers, meaning the distance traveled by train in kilometers.

Figure 2: The Lowest Normal Fare by Carrier, without a Loyalty Program (top panel) and with a Loyalty Program (bottom panel)



Notes: SC = Super city Pendolino trains (the highest quality service of the Czech Railways); early = early ticket; other = all trains on the track except of SC; Regiojet_credit = Topping-up credit online the booking must always be paid for from the credit; IN25 loyalty card of the Czech railways; Smile card = loyalty card of Leo Express. Does not include the prices of loyalty cards.

Figure 3: Developments in Unemployment for the On-track Districts (Treatment Group) and Other Districts (Control Group) Supporting the Pre-treatment Parallel-trend Assumption



Appendix

Tables in Appendix

Table A1: The Share of Competing Carriers on Passenger Rail Output (all lines)

		2006	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**	**2013**	**2014**	**2015**	**2016**	**2017**	**2018**	**2019**
Czech Railways	trkm	99.31	99.27	99.14	99.18	99.04	98.62	97.20	95.61	95.34	95.01	94.43	93.73	91.76	90.77
VIAMONT	*trkm*	*0.43*	*0.33*	*0.63*	*0.60*	*0.76*	*0.75*	*NA*	*NA*	*NA*	*NA*	*NA*	*NA*	*NA*	*NA*
OKD. Doprava	*trkm*	*0.13*	*0.12*	*0.12*	*0.10*	*NA*	*NA*	*NA*	*NA*	*NA*	*NA*	*NA*	*NA*	*NA*	*NA*
RegioJet	trkm	NA	NA	NA	NA	**0.05**	**0.23**	**1.62**	**1.94**	**2.09**	**2.17**	**2.49**	**3.06**	**3.90**	**4.07**
Die Länderbahn (Vogtlandbahn)	trkm	NA	NA	NA	NA	0.02	0.32	0.33	0.32	0.32	0.38	0.38	0.37	0.28	0.36
GW Train Regio	trkm	NA	NA	NA	NA	NA	0.75	0.70	0.34	0.35	0.35	0.36	0.67	1.83	1.88
Leo Express	trkm	NA	NA	NA	NA	NA	**0.01**	**0.08**	**1.65**	**1.68**	**1.87**	**1.90**	**1.50**	**1.44**	**1.74**
ARRIVA vlaky	trkm	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	NA	0.77

Source: Author calculations based on SŽDC Annual reports (2006-2019).
Notes: trkm = Train kilometers, meaning the distance traveled by train in kilometers. Providers are sorted by the date they started to appear in SŽDC reports.

Table A2: Descriptive Statistics for Variables Spanning 2005-2017

Continuous Variables

Variable	Number of Observations	Mean	Median	Standard Deviation	Minimum	Maximum
unemployed	832	8.530	8.569	0.624	6.620	10.187
city	832	9.196	9.037	0.811	7.976	12.845
agri	832	51.951	53.377	11.721	27.262	71.799
population	832	11.258	11.250	0.438	10.137	12.469

District Groups

Districts	On-track		(On-track + Neighboring)		Neighboring	
	Frequency	Percent	Frequency	Percent	Frequency	Percent
Treatment	117	16.67	247	29.69	130	18.18
Control	585	83.33	585	70.31	585	81.82
Total	702	100.00	832	100.00	715	100.00

Notes: All descriptive statistics refers to transformed variables directly entered into the estimation of the baseline model. As we use the lagged form of *city* and *agri*, descriptive statistics for these variables are calculated for the period of 2004–2016. Descriptive statistics for continuous variables are only for the (on-track + neighboring) treatment group specification. Initial data for *unemployed* were obtained from the database of the Czech Ministry of Labour and Social Affairs, available at: https://data.mpsv.cz/web/data/statistiky. All other initial data were obtained from the database of the Czech Statistical Office, available at: https://www.czso.cz/csu/czso/districts-of-the-czech-republic-2018.

Table A3: Estimation Results for Model with Permanent Effects: Full Results

		On-track		(On-track +Neighboring)		Neighboring	
A	*district · event_2012*	0.0002	(0.429)	0.341	(0.359)	-0.239	(0.715)
	district · event_2012 · city	-0.002	(0.046)	-0.035	(0.039)	0.030	(0.079)
	district · event_2013	-0.467***	(0.161)	-0.372***	(0.087)	-0.360**	(0.156)
	district · event_2013 · city	0.048***	(0.016)	0.039***	(0.009)	0.038**	(0.017)
	district · event_2014	-0.344**	(0.149)	-0.210	(0.150)	-0.290	(0.285)
	district · event_2014 · city	0.034**	(0.015)	0.021	(0.016)	0.031	(0.030)
	district · event_2015	-0.709***	(0.140)	-0.451	(0.287)	-0.350	(0.650)
	district · event_2015 · city	0.071***	(0.014)	0.046	(0.029)	0.036	(0.069)
	district · event_2016	-0.301	(0.279)	-0.464	(0.353)	-0.298	(0.770)
	district · event_2016 · city	0.033	(0.028)	0.048	(0.037)	0.028	(0.082)
	district · event_2017	-0.216	(0.275)	-0.024	(0.233)	0.479	(0.417)
	district · event_2017 · city	0.027	(0.027)	0.006	(0.024)	-0.051	(0.045)
B	*district · city*	-0.004	(0.126)	-0.086	(0.143)	-0.352*	(0.186)
	event_2012 · city	0.003	(0.009)	0.005	(0.010)	0.004	(0.010)
	event_2013 · city	0.009	(0.006)	0.009	(0.006)	0.009	(0.006)
	event_2014 · city	0.031***	(0.007)	0.031***	(0.007)	0.031***	(0.007)
	event_2015 · city	0.008	(0.007)	0.008	(0.007)	0.008	(0.007)
	event_2016 · city	0.006	(0.015)	0.006	(0.014)	0.006	(0.014)
	event_2017 · city	0.005	(0.009)	0.007	(0.009)	0.005	(0.009)
	event_2012	0.124	(0.093)	0.092	(0.094)	0.111	(0.095)
	event_2013	0.009	(0.056)	0.006	(0.056)	0.001	(0.058)
	event_2014	-0.390***	(0.063)	-0.393***	(0.061)	-0.396***	(0.061)
	event_2015	-0.248***	(0.067)	-0.250***	(0.067)	-0.255***	(0.065)
	event_2016	-0.217	(0.130)	-0.223*	(0.129)	-0.225*	(0.129)
	event_2017	-0.364	(0.085)	-0.378***	(0.084)	-0.365***	(0.090)
	city	0.226*	(0.125)	0.202	(0.124)	0.261**	(0.125)
	agri	0.005	(0.015)	-0.002	(0.015)	0.009	(0.018)
	population	1.413***	(0.297)	1.153***	(0.324)	0.951***	(0.321)
	drift	-9.699***	(3.099)	-5.918*	(3.533)	-4.393	(3.490)
	R^2	0.787		0.530		0.093	
	Number of Observations	702		832		715	

Notes: The first number is the coefficient value. Robust standard errors are in parentheses. ***, **, and * statistical significance at the 1%, 5%, and 10% level, respectively. The column headings denote the type of treatment group. Part A summarizes estimates of the average effect (*district · event*) and the interactive effect (*district · event · city*), which depends on the level of urbanization. Part B shows all other estimates of the regression equation, except for time dummies, whose results we do not report. We estimate all gradual effects together, thus there are only three estimated models in the table.

Table A4: Estimation Results for Equation 2 with Marginal Treatment Effects Over Time—Standard Errors Clustered at the Level of Regions

Treatment group	**On-track**		**(On-track + Neighboring)**		**Neighboring**	
Effect	Average	Interactive	Average	Interactive	Average	Interactive
event_2012	0.0002	-0.002	0.341	-0.035	-0.239	0.030
	(0.205)	(0.018)	(0.244)	(0.026)	(0.497)	(0.056)
event_2013	-0.467***	0.048***	-0.372***	0,039***	-0.360**	0.038**
	(0.151)	(0.015)	(0.048)	(0.005)	(0.151)	(0.016)
event_2014	-0.344**	0.034**	-0.210	0.021	-0.290	0.031
	(0.154)	(0.014)	(0.132)	(0.014)	(0.301)	(0.032)
event_2015	-0.709***	0.071***	-0.451*	0.046	-0.350	0.036
	(0.126)	(0.012)	(0.249)	(0.026)	(0.692)	(0.075)
event_2016	-0.301	0.033	-0.464	0.048	-0.298	0.028
	(0.298)	(0.030)	(0.340)	(0.035)	(0.562)	(0.059)
event_2017	-0.216	0.027	-0.024	0.006	0.479	-0.051
	(0.294)	(0.029)	(0.320)	(0.034)	(0.412)	(0.044)

Notes: The first number is the coefficient value. Robust standard errors are in parentheses clustered at level of 12 regions (NUTS 3). ***, **, and * statistical significance at the 1%, 5%, and 10% level, respectively. The column headings denote the type of treatment group and distinguish between the average effect and the interactive effect depending on the level of urbanization. The row labels indicate the timing of the post-treatment dummy used in the estimation. We estimate all effects together, thus there are only three estimated models in the table.

Table A5: Estimation Results for Equation 2 with Marginal Treatment Effects Over Time: Regional FE

Treatment group	**On-track**		**(On-track + Neighboring)**		**Neighboring**	
Effect	Average	Interactive	Average	Interactive	Average	Interactive
event_2012	0.116	-0.014	0.354	-0.036	-0.220	0.029
	(0.212)	(0.019)	(0.256)	(0.028)	(0.530)	(0.059)
event_2013	-0.469***	0.048***	-0.380***	0.040***	-0.375**	0.039**
	(0.152)	(0.015)	(0.052)	(0.005)	(0.153)	(0.017)
event_2014	-0.341**	0.033**	-0.212	0.021	-0.297	0.032
	(0.151)	(0.014)	(0.130)	(0.013)	(0.305)	(0.033)
event_2015	-0.703***	0.071***	-0.455*	0.047*	-0.359	0.037
	(0.122)	(0.012)	(0.248)	(0.026)	(0.688)	(0.075)
event_2016	-0.294	0.033	-0.465	0.048	-0.295	0.028
	(0.293)	(0.030)	(0.339)	(0.034)	(0.561)	(0.059)
event_2017	-0.204	.025177	-0.025	0.006	0.505	-0.054
	(0.290)	(0.028)	(0.321)	(0.034)	(0.415)	(0.044)

Notes: The first number is the coefficient value. Robust standard errors are in parentheses clustered at level of 12 regions (NUTS 3). ***, **, and * statistical significance at the 1%, 5%, and 10% level, respectively. The column headings denote the type of treatment group and distinguish between the average effect and the interactive effect depending on the level of urbanization. The row labels indicate the timing of the post-treatment dummy used in the estimation. We estimate all effects together, thus there are only three estimated models in the table.

Table A6: Arrival of Passengers by Rail to the City of Prague from Other Regions

(in Thousands of People)

Region	2005	2010	2011	2012	2013	2014
Central Bohemia	6 391.7	5 787.0	6 328.0	6 885.0	6 890.0	13 233.0
South Bohemia	500.5	393.0	411.0	441.0	400.0	419.0
Plzeň	350.0	319.0	317.0	364.0	392.0	429.0
Karlovy Vary	98.3	80.0	79.0	98.0	89.0	99.0
Ústí nad Labem	480.2	1 089.0	1 034.0	1 131.0	937.0	793.0
Liberec	59.5	67.0	74.0	74.0	70.0	67.0
Hradec Králové	382.4	272.0	297.0	332.0	349.0	374.0
Pardubice	687.5	724.0	811.4	893.8	901.2	905.0
Vysočina	209.9	198.0	211.0	227.0	213.0	210.0
South Moravia	262.9	237.0	305.0	370.0	406.0	458.0
Olomouc	382.9	423.0	509.0	643.9	732.9	843.1
Zlín	144.5	178.0	206.0	206.0	184.7	226.8
Moravian-Silesia	392.3	452.0	570.0	669.5	798.8	923.6
Total arrivals from regions	**10 342.5**	**10 219.0**	**11 152.4**	**12 335.2**	**12 363.6**	**18 980.4**

Source: Author calculations based on annual MD reports. The sharp increase in the number of passengers in the Central Bohemian Region between 2013 and 2014 was associated with the development of a regional integrated transport system. Other than the Central Bohemian Region, the Moravian-Silesian and Olomouc regions contributed the most to the increase in passenger arrivals by rail to the city of Prague from other regions from 2012 to 2014.

Figures in Appendix

Figure A1: The Timeline of Developments on the Prague-Ostrava Line