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From periphery to core: Measuring agglomeration effects using high-speed rail***

Abstract: We analyze the economic impact of the German high-speed rail (HSR) connecting Cologne and Frankfurt, which provides plausibly exogenous variation in access to surrounding economic mass. We find a causal effect of about 8.5% on average of the HSR on the GDP of three counties with intermediate stops. We make further use of the variation in bilateral transport costs between all counties in our study area induced by the HSR to identify the strength and spatial scope of agglomeration forces. Our most careful estimate points to an elasticity of output with respect to market potential of 12.5%. The strength of the spillover declines by 50% every 30 minutes of travel time, diminishing to 1% after about 200 minutes. Our results further imply an elasticity of per-worker output with respect to economic density of 3.8%, although the effects seem driven by worker and firm selection.

Keywords: Accessibility, agglomeration, density, high-speed rail, market potential, transport policy, productivity
JEL classification: R12, R28, R38, R48
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1 Introduction

"A major new high-speed rail line will generate many thousands of construction jobs over several years, as well as permanent jobs for rail employees and increased economic activity in the destinations these trains serve."

US President Barack Obama, April 16, 2009

One of the most fundamental and uncontroversial ideas in economic geography and urban economics is that firms and households benefit from access to economic markets due to various forms of agglomeration economies (Marshall, 1920). The mutually reinforcing effects of spatial density and productivity can theoretically account for the highly uneven distribution of economic activity between and within regions. The strong belief that economic agents benefit from an ease of interaction

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has always motivated large (public) expenditures into transport infrastructures, e.g., ports, airports, highways or railways. A striking example of an expensive, but increasingly popular transport mode is high-speed rail (HSR). The costs of implementing an HSR network in Britain, which mainly consists of a Y-shaped connection of London to Birmingham, Leeds, and Manchester of about 500km length are scheduled to amount to as much as £42 (about $63) billion at present (Storeygard, 2012). The US Department of Transportation (2009) has announced its strategic plan, which proposes the construction of completely new rail lines that will feature velocities of possibly up to 400km/h (250mph). The plan has already identified US$8 billion plus US$1 billion a year for five years in the federal budget just to jump-start a program that would only be comparable to the interstate highway program of the 20th century. The perhaps most spectacular HSR considered to date is a 7,000km line connecting the Russian and Chinese capitals Moscow and Beijing, currently estimated at 1.5 trillion yuan ($242 billion) (Phillips, 2015). The willingness to commit large amounts of public money to the development of HSR bears witness to the confidence that HSR will deliver a substantial economic impact.

The wider economic impacts such infrastructures deliver, however, naturally depend on the strength and the spatial scope of the agglomeration economies they enhance.[1] Estimating such agglomeration effects is empirically challenging. The density of economic activity and the productivity at a given location are not only potentially mutually dependent, but also potentially simultaneously determined by location fundamentals, such as a favorable geography or good institutions. The main challenge in estimating the strength and the spatial scope of agglomeration effects, therefore, is to find exogenous variation in access to the surrounding economic mass. While transport infrastructures, such as a new HSR, generate such variation in access to economic mass, the allocation of transport infrastructure is typically non-random, thus generating additional identification problems.

In this paper we provide causal estimates of the strength and the spatial scope of agglomeration effects using a variation of the market potential approach, which links economic outcomes at a given locality to economic activity at surrounding localities via a transportation network. To disentangle the effects of market potential from other determinants of economic outcomes our identification stems exclusively from changes in transport technology, which affect the effective distance between

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[1] The transport appraisal literature distinguishes between user benefits, which mainly capture the value of shorter travel times, and wider economic impacts, such as agglomeration benefits due to higher effective density, moves to more productive jobs, and output changes in imperfectly competitive markets (Department for Transport, 2014).

all pairs of locations in a region.[2] Specifically, we exploit the variation in bilateral transport costs between all counties located in the German federal states of North Rhine-Westphalia, Hesse, and Rhineland-Palatinate that was induced by the Cologne-Frankfurt HSR. With this research design we are able to control for unobserved time-invariant variation in location fundamentals and circumvent some of the typical challenges in estimating the effects of spatial density on economic outcomes. Given the particular institutional setting we argue that the HSR analyzed provides variation in bilateral transport costs that is credibly exogenous, creating a natural experiment with identifying variation that is as good as random.[3]

The Cologne-Frankfurt HSR was inaugurated in 2002. The line is part of the Trans-European Networks and facilitates train velocities of up to 300km/h. The HSR reduced travel time between both metropolises by more than 55% in comparison to the old rail connection and by more than 35% in comparison to the automobile. Along the HSR line, intermediate stops were created in the towns of Limburg, Montabaur, and Siegburg. It is noteworthy that given the small population of less than 25 and 15 thousand inhabitants in particular the stops in Limburg and Montabaur are unusual within the German if not European HSR network. Following the connection to the HSR line the two towns are within 40 minutes of Cologne and Frankfurt, which are the centers of the two largest German agglomerations, but also less than 10 minutes of each other.

The final routing of the line and the location of the intermediate stops were the result of a political bargaining process among the rail carrier, three federal states, and several business lobby and environmental activist groups that lasted almost 40 years. We argue that the institutional particularities, which we describe in more detail in Section 2, allow us to make the helpful identifying assumptions that the *routing* and the *timing* of the connection of Limburg, Montabaur, and Siegburg and the *timing* of the connection of all other stations are exogenous to the levels and trends of economic development.

Based on the exogenous variation provided, we are able to identify the causal impact of HSR on local economic development as well as the strength and the spatial scope of agglomeration economies

[2] Storeygard (2012) uses changes in oil prices interacted with static distance measures as a source of variation in transport cost over time.

[3] Complementary literature has modelled the mutual dependence of demand for and supply of transport infrastructure (Levinson, 2008; Xie & Levinson, 2010).

promoted by the line. In the first step, we assess the effect of the HSR on the local economies within the counties of the intermediate stops using program evaluation techniques. In the second step, we correlate the growth in effective density, which we express in market potential form (Harris, 1954), to the economic growth across counties within our study area. The market potential expresses effective density as the transport cost weighted sum of the GDP of all counties in the study area. The measure takes into account the effect of the HSR on bilateral transport costs between all counties in our study area. Since the HSR is used exclusively for passenger service we implicitly disentangle the effects of facilitated human interactions from the transport costs of tradable goods, i.e., the trade channel. The spillovers we capture thus include Marshallian externalities related to knowledge diffusion and labor market pooling and the effects of improved access to intermediated goods and consumer markets to the extent that the ease of communication reduces transaction costs, but not freight costs.

Theoretically, one effect of the HSR is to upgrade the accessibility of formerly peripheral areas, which at least during a transition period offer the benefit of relatively low land prices, and may thus become attractive to firms. Indeed, our results point to a positive economic impact of HSR. On average, six years after the opening of the line, the GDP in the counties of the intermediate stops exceeds the counterfactual trend established via a group of synthetic counties by 8.5%.[4] We find an elasticity of GDP with respect to effective density, i.e., market potential, of about 12.5% in our most conservative model. The elasticity of output per worker with respect to effective density is, at 10%, only marginally smaller. Because our measure of effective density is spatially smoothed the variance across counties is naturally lower than in conventional density measures. Normalized by the log ratio of the standard deviations of effective density over actual density our results imply an elasticity of productivity with respect to employment density of 3.8%, which is close to previous estimates derived from cross-sectional research designs (e.g. Ciccone & Hall, 1996).[5] The effect, however, seems to be driven to a significant extent by selection, i.e., a compositional change in industry and worker qualification (Combes, Duranton, & Gobillon, 2011; Combes, Duranton, Gobillon, Puga, & Roux, 2012). We further estimate that the strength of economic spillovers halves every 30 minutes of travel time and is near to zero after about 200 minutes. The spillovers we detect are significantly less localized than in pre-

[4] We create a synthetic equivalents for each treated county following Adabie and Gardeazabal (2003).

[5] Reviewing 729 estimates across 34 studies Melo et al. (2009) find a mean elasticity of 5.8%.

vious studies that have identified spillover effects from within-city variation (Ahlfeldt, Redding, Sturm, & Wolf, 2015; Ahlfeldt & Wendland, 2013; Arzaghi & Henderson, 2008), but are more localized than the scope of spatial interactions inferred from empirical NEG models with a stronger emphasis on trade costs (Hanson, 2005).[6]

Our research connects to a large and growing literature on the nature of agglomeration economies reviewed in detail in Duranton and Puga (2004) and Rosenthal and Strange (2004). A standard approach in this literature has been to regress economic outcome measures, such as wages, against some measure of agglomeration, typically employment or population density.[7] A smaller literature has exploited presumably exogenous variation in the surrounding concentration of economic activity. Rosenthal and Strange (2008) and Combes et al. (2010) use geology to instrument for density. Greenstone et al. (2010) analyze the effects of the openings of large manufacturing plants on incumbent plants. Another related strand has analyzed the impact of natural experiments such as trade liberalization (Hanson, 1996, 1997), wartime bombing (Davis & Weinstein, 2002), the decrease in the economic relevance of portage sites (Bleakley & Lin, 2014), and the Tennessee Valley Authority (Kline & Moretti, 2014) on the spatial distribution of economic activity.

At the intersection of both strands, Redding and Sturm (2008) have exploited the effects of the variation in access to the surrounding economic mass created by the division and unification of Germany on city growth. Ahlfeldt, et al. (2015) use the within-city variation in surrounding economic mass induced by the division and reunification of Berlin, Germany, to identify the strength and spatial scope of spillovers among residents and among firms as well as the rate at which commuting probabilities decline in time distance. Our main contributions to this literature are twofold. First, we estimate the agglomeration effects based on the variation in surrounding economic mass created by new transport infrastructures, which allows for a relatively robust separation of spillover effects from unobserved locational fundamental effects. Second, we contribute to a relatively small literature that has provided estimates of the rate of spatial decay in spillovers. The relatively strong spatial decay in spatial spillovers substantiates the intuition that moving people is more costly than moving goods.

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6 See Head and Mayer (2004) for a review of this literature. For an introduction into the theoretical and empirical literature on agglomeration, productivity and trade, see e.g. Ottaviano (2002), Behrens et al. (2014) or Sato and Zenou (2015).

7 Examples include Ciccone (2002), Ciccone and Hall (1996), Dekle & Eaton (1999), Glaeser and Mare (2001), Henderson, Kuncoro and Turner (1995), Moretti (2004), Rauch (1993), and Sveikauskas (1975).

Another growing strand in the literature to which we contribute is concerned with the economic effects of transport infrastructure. Overall, the evidence suggests that a well-developed transport infrastructure enhances trade (Donaldson, 2015; Duranton, Morrow, & Turner, 2013), promotes economic growth (Banerjee, Duflo, & Qian, 2012; Duranton & Turner, 2012), and, at a more local level, increases property prices (Baum-Snow & Kahn, 2000; Gibbons & Machin, 2005). There is also evidence of asymmetric impacts on labor markets, in particular, of a relative increase in demand for skilled workers in skill-abundant regions (Michaels, 2008). The evidence on the impact on the spatial distribution of economic activity is more mixed. Within metropolitan areas radial connections tend to facilitate suburbanization and, thus, benefit peripheral areas (Baum-Snow, 2007; Baum-Snow, Loren Brandt, Henderson, Turner, & Zhang, 2012; Kopecky & Suen, 2010).[8] However, there is also evidence that within larger regions reductions in trade costs between regions due to better road networks favor core regions at the expense of peripheral regions (Faber, 2014).

Empirically, the literature evaluating the economic effects of transport infrastructure has been concerned with the non-random allocation of transport infrastructure, which is usually built to accommodate existing or expected demand. Instrumental variables based on historic transport networks (Duranton & Turner, 2012), counterfactual least-cost networks (Faber, 2014) or straight-line connections among regional centers (Banerjee et al., 2012) have emerged as a standard approach to establishing a causal relationship. The alternative and potentially complementary strategy adopted here is the so-called inconsequential units approach (Redding & Turner, 2015). This approach rests on the assumption that the main purpose of a transport infrastructure is often to connect regional agglomerations and that the connection of localities along the way is not necessarily intended (Chandra & Thompson, 2000; Michaels, 2008). Our contribution to this line of research is, again, twofold. First, we provide evidence of the economic impacts of HSR, an increasingly important but empirically understudied transport mode, exploiting a source of exogenous variation. Second, we show that peripheral regions can benefit from a better connectivity to core regions if the cost of human interaction is reduced but trade costs remain unchanged.[9] This evidence complements the extant literature suggesting that HSR primarily benefits large cities, but not necessarily remote counties

[8] Such a tendency of decentralization in response to reductions in transport costs is in line with standard urban models in the spirit of Alonso (1964), Mills (1967), and Muth (1969).

[9] This finding is in line with evidence suggesting that well developed transport infrastructures are associated with less spatial concentration (Ramcharan, 2009)

(Lin, 2014; Qin, 2016; Zheng & Kahn, 2013). The evidence of positive effects emerging from Marshallian externalities is also complementary to the recent evidence of negative effects on peripheral regions operating through a trade channel (Faber, 2014). Our results similarly complement literature on suburbanization (Baum-Snow, 2007; Garcia-López, 2012; Garcia-López, Holl, & Viladecans-Marsal, 2015) by showing that a HSR is less likely to decentralize population to suburbs than a highway.

The next section introduces the institutional setting in more detail and discusses the data used. In Section 3 we conduct a program evaluation with a focus on the impact of HSR on the economies of the counties of the intermediate stations. In Section 4 we then exploit the full variation the HSR induced in bilateral transport costs between all counties in our study area to estimate the strength and spatial scope of agglomeration effects. The final section concludes.

2 Background and data

A – The Cologne–Frankfurt HSR Line

The HSR line from Cologne to Frankfurt/Main is part of the priority axis Paris-Brussels-Cologne-Amsterdam-London (PBKAL), which is one of 14 projects of the Trans-European Transport Network (TEN-T) as endorsed by the European Commission in 1994. In comparison to the old track alongside the river Rhine, the new HSR connects the Rhine/Ruhr area (including Cologne) and the Rhine/Main area (including Frankfurt) almost directly, reducing track length from 222km to 177km.[10] The new track is designed exclusively for passenger transport and allows train velocities of up to 300km/h. Due to both facts, travel time between the two main stations was reduced from 2h13 to 59min (Brux, 2002). Preparatory works for the construction of the HSR started in December 1995. The major construction work —on the various tunnels and bridges— began in 1998. The HSR line was completed at the end of 2001. After a test period the HSR line was put into operation in 2002. The total cost of the project was 6 billion Euros (European Commission, 2005, p. 17).

The broader areas of Rhine-Ruhr and Rhine-Main have long been considered to be the largest German economic agglomerations. The rail lines connecting the two centers along both Rhine riverbanks were among the European rail corridors with the heaviest usage. They had represented a traditional bottleneck since the early 1970s, when usage already exceeded capacity. The first plans for con-

[10] The straight-line distance between Cologne Main Station and Frankfurt Main Station is 152km.

structing an HSR line between Cologne and Frankfurt, consequently, date back to as far as the early 1970s. Since then, it has taken more than 30 years until the opening. A reason for the long time period was the complex evolution process of infrastructure projects in Germany. Several variants at the left-hand and right-hand side of the Rhine were discussed during the decades of negotiations. Taking into account the difficult geography of the Central German Uplands, it was ultimately decided to construct a right-hand side connection that would largely follow the highway A3 in an attempt to minimize construction and environmental costs as well as travel time between the major centers. These benefits came at the expense of leaving relatively large cities like Koblenz and the state capitals Wiesbaden (Hesse) and Mainz (Rhineland Palatinate) aside.

Due to the federal system of the Federal Republic of Germany the states (*Länder*) have a strong influence on infrastructure projects that affect their territories (Sartori, 2008, pp. 3-8). Three federal states were concerned with the subject project: North Rhine-Westphalia, Rhineland-Palatine, and Hesse. While Cologne lies in North Rhine-Westphalia and Frankfurt is located in Hesse, no stop was planned within the state of Rhineland-Palatine after the plans to connect Koblenz were abandoned in 1989. The announcement of the exact routing, however, suddenly opened opportunities for communities along the line to lobby in favor of their connection. Limburg, supported by Hesse, was the first city to make a case. Somewhat later in the process, the local political and economic actors in Montabaur also managed to convince the state authorities of Rhineland-Palatinate to support their case. It was argued that from Montabaur the hinterland of the state could be connected via an existing regional line. The case of Montabaur was facilitated by the decision to build the new Limburg station at the south-eastern fringe of the city in Eschhofen. The originally proposed site (Limburg Staffel) was significantly closer than Montabaur and, given the already short distance, would have made an additional stop in Montabaur almost impossible to justify. During a long lobbying process menacing a blockade of the planning and political decision process, the three federal states eventually negotiated three intermediate stops along the HSR line, one in each of the concerned federal states. While Bonn/Siegburg and Limburg represented the shares of North Rhine-Westphalia and Hesse, a new station in Montabaur ensured the connection of Rhineland-Palatinate.

At the end of this process, Montabaur, with a population of less than 20,000 – the by far smallest city on the German high-speed rail network – found itself within 40 minutes of the regional centers Cologne and Frankfurt and within 20 minutes of the international airports Frankfurt and Cologne-Bonn. Anecdotal evidence suggests that this exceptional upgrade in terms of accessibility improved the attractiveness of the city as a business location. A new congress center was opened and more than 50 firms settled in an industrial park built adjacent to the rail station;[11] 1&1, a leading provider of communication services, even moved their headquarters to that location. A number of local manufacturing companies in the wider catchment area expanded their capacities in response to the improvement in connectivity (Egenolf, 2008). Mainstream media reported a major success story as early as 2007, citing city officials who claim that the HSR brought at least 600 new jobs to the town (Sorge, 2007). Since then, the number has climbed to 1,800 (Hergert, 2015). Several newspapers cite executives of various local firms such as 1&1 (telecommunications), Emc^2 (consulting), Friedhelm-Loh (electrical engineering), ADG (congress centre), Itac (software) MTE Deutschland (milling machines) all of which suggest that the HSR was a major factor in their decisions to locate or expand business activity in Montabaur (Hergert, 2015; Rhein-Zeitung, 2012; Sorge, 2007). As an example, ADG representatives highlight that the new congress center would not have been viable without the fast connection (less than 30 minutes) to the major airports (Frankfurt and Cologne/Bonn). Among the major advantages reported were the ease of maintaining business relations and an improved access to a highly qualified labor pool. Various representatives, from Itac, 1&1, and others, stress that the HSR enables them to draw from a much larger labor market, which now includes the major agglomerations Cologne and Frankfurt. In selected firms, more than 80% of the managerial positions are held by in-commuters. As an example, two-thirds of the 1,600 employees working for 1&1 commute into town, most of them by HSR (Hergert, 2015). These newspaper reports align well with the mayor's HSR impact summary. According to Edmund Schaaf, employment has increased by 1,400 jobs (subject to social insurance contributions) over the past 15 years, which is almost 10 times the increase in population of 150.

Altogether, the anecdotal evidence suggests that the unexpectedly high passenger numbers of 3,000 per day, about 10 times the original forecasts (Müller, 2012), are driven by people working, not living, in the city. This pattern is also consistent with the numbers reported for Limburg, where over the same period the number of in-commuters increased from 13,000 to 17,500 (Hergert, 2015).

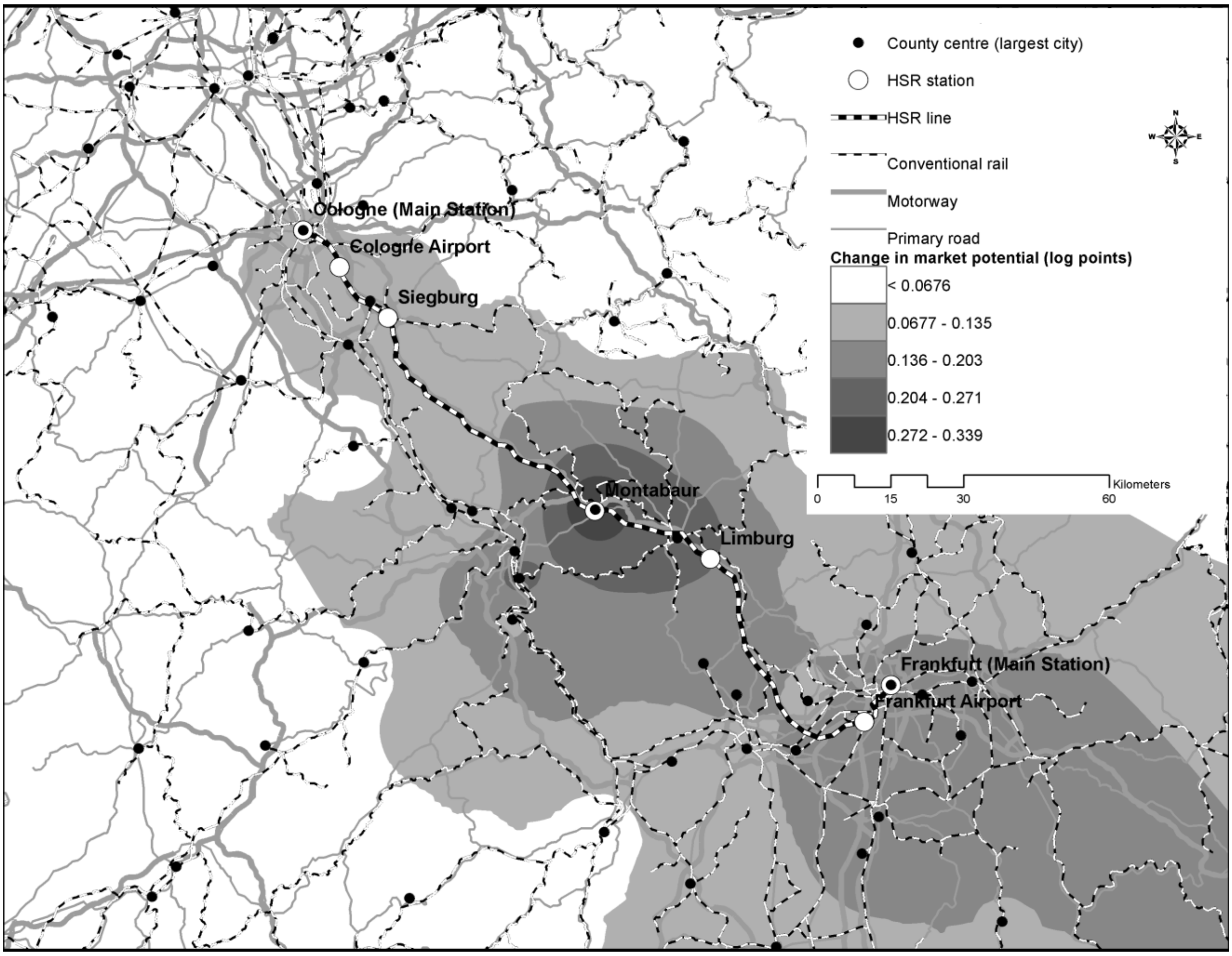
[11] Among them: Landesbetrieb Mobilität RLP Autobahnamt, Unternehmensberatung EMC^2, Industrie- und Handelskammer (IHK), Ingenieurgesellschaft Ruffert und Partner, Objektverwalter S.K.E.T, Cafe Latino, Kantine Genuss & Harmonie.

Notwithstanding this local impact, the intermediate stops have been very controversial in terms of their economic viability. The cities of Montabaur and Limburg only exhibit approx. 12,500 and 34,000 habitants. Furthermore, the distance between these two small cities is barely 20km and the high-speed ICE train needs only nine minutes between both stops, which is in contrast to the concept of high-velocity travelling that has its comparative advantages at much larger distances. The advantage of this institutional setting for our empirical analysis is that it is reasonable to assume that the routing of the track was exogenous in the sense that it was determined by geographical constraints, construction costs, and environmental concerns. Compared to the considered alternative route discussed in more detail in Section 3-C, the population living within the immediate catchment areas of the intermediate stops along the selected route amounts to less than a half.[12] The locations of the eventually chosen intermediate stops was constrained by the proposed routing and the need to accommodate three stops at reasonable distances in three different federal states, disentangling political lobbying for intermediate stops from other regional policies. Most importantly, the connection of the intermediate stations was not driven by existing or expected demand – in fact, these stations were heavily opposed by the operating rail carrier Deutsche Bahn.

Thus, we consider the resulting variation in accessibility provided by the rail line as exogenous to the economic outcomes we observe. Furthermore, it is reasonable to argue that the timing of the inauguration was exogenous to contemporary economic trends for the entire line. When the plans for a connection of Frankfurt and Cologne were first drafted in the 1970s it was virtually impossible to foresee *changes* in economic conditions in the late 1990s.

[12] Using GIS we find a 2002 population of 438,540 living within 10 km from Limburg, Montabaur, and Siegburg, while 1,020,474 residents lived within the same distance from Bonn, Koblenz and Wiesbaden. All distances are measured between municipality centroids as the crow flies.

Fig. 1 The transport infrastructure in the study area



Notes: Market potential based on Eq. (4-2) and the decay parameter estimate (δ_2) from Table 5, column (1).

B – Data and study area

Our study area comprises the German federal states Hesse, North Rhine-Westphalia, and Rhineland-Palatinate, to which the HSR connects. In 1996, six years before the opening of the HSR, the total population of the study area was about 28 million, thus somewhat less than California and about the size of Belgium and the Netherlands together. The share at the total German population was about 34%. The share at German GDP was slightly higher at 36%. For the 115 counties (NUTS3 regions) in the three federal states we collect data from various official sources: GDP, population, GVA by industry sectors from the German Federal Statistical Office;[13] number of in- and out-commuter, employment (at workplace and residence) and share of workforce holding an academic degree (at workplace) from the Federal Employment Agency. Municipality level population is obtained from The

[13] These data are available at the website www.regionalstatistik.de.

Federal Office for Building and Regional Planning. We use these data primarily to identify the most important cities within each county, which we define as their economic centers. We collected data from 1992–1995 (depending on data availability) to 2009. The average county in our study area in 1996 had a population of about 241k, which is significantly larger than the average county in the rest of the country (157k). In terms of output per worker, our study area is fairly similar to the rest of the country (€71.5k vs. €70.8). Also, the shares of various industries at the regional GVA are remarkably similar. Descriptive statistics are presented in Section 2 of the appendix, where we also present a map that illustrates the location of the study area and the HSR within Germany.

3 Program evaluation

The intermediate stops Limburg, Montabaur, and Siegburg on the Cologne-Frankfurt HSR were, as we argue, an accidental result of political bargaining and not rational transport planning. The new stations thus provide plausibly exogenous variation in transport services that can be exploited to detect economic impact using established program evaluation techniques. In this section we analyze the economic effects of the opening of the HSR – the *treatment* – on the economies of the counties of the intermediate stops, the *treated* counties. Specifically, we compare the evolution of various economic outcome measures in the treated counties to *control* counties that provide a counterfactual.

A – Treated vs. synthetic counties

We note that at this stage we ignore Cologne and Frankfurt because these regional centers are arguably major generators of transport demand, so the routing of the high-speed rail line cannot be considered exogenous to their economic performance. As these cities potentially benefit from improved transport services we also exclude them from the group of control counties. Besides, on the exogeneity of the treatment the credibility of a quasi-experimental comparison rests on the assumption that the treatment and control group would have followed the same trend in the absence of the treatment. To ensure a valid comparison we create a comparison group consisting of three synthetic counties, one for each of the treated counties in which the HSR stops Limburg, Montabaur, and Siegburg are located. With this approach, we avoid problems arising in conventional difference-in-differences analysis if the number of treated subjects is small relative to the number of control subjects (Colin Cameron & Miller, 2015). We follow the procedure developed by Abadie and Gardeazabal (2003), who define a synthetic region as a weighted combination of non-treated regions. The optimal combination of weights is determined by two objectives.

First, a synthetic county should match its treated counterpart as closely as possible in terms of the following economic growth predictors: GDP per worker, population density, ratio of out-commuters over in-commuters, the shares of construction, mining, services, retail, manufacturing, and finance at gross value added, and the share of workers holding a university degree in the workforce at workplace. Formally, this problem is defined as $\min_{W \in W}(X_1 - X_0W)'V(X_1 - X_0W)$, where W is a vector of non-negative weights of the non-treated counties in the synthetic county that must sum to one, X_1 is a vector of pre-opening values of k economic growth predictors for the treated county, X_0 is a matrix containing the same information for the non-treated counties, and V is a diagonal matrix with non-negative elements that determine the relative importance of the growth predictors.

The solution to this problem, the vector of optimal weights of non-treated counties W^*, depends on V, which leads to the second objective. We search for the optimal combination V^* which produces a synthetic control county that best matches the respective treated county in terms of the pre-construction growth trend. Formally, this second problem is defined as $V^* = \underset{V \in \nu}{\operatorname{argmin}}\big(Z_1 - Z_0W^*(V)\big)'(Z_1 - Z_0W^*(V))$, where Z_1 a is vector of pre-construction observations of an economic outcome measure Y for the treated county and Z_0 is a matrix with the same information for the non-treated counties.[14]

Table 1 summarizes the pre-treatment characteristics of the home counties of the intermediate HSR stops, the synthetic control counties and all other non-treated counties in the study areas. Each synthetic county is the result of a separate implementation of the procedure outlined above. In each case the economic outcome measure Y, used to find the optimal weights matrix W^* is the log of GDP. The pre-period covers all years prior to 1998, when the substantial construction works began and after – more than 25 years of negotiations – confidence was created so that the HSR would eventually materialize. The values for the k growth predictors for a given synthetic county are given by the vector $X_1^* = X_0W^*$, i.e., a weighted combination of non-treated counties. The treated counties (and the synthetic counties) are characterized by below-average productivity, tend to be residential locations, and have a low share of workers holding university degrees. With few exceptions, the synthetic counties resemble their treated counterparts closely in observable characteristics, certainly more closely than the average of the non-treated counties.

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14 We use the Stata ado file *synth* compiled by Hainmueller, Abadie, and Diamond to generate the synthetic control counties.

Tab. 1 Pre-treatment characteristics: Treated vs. synthetic controls

	Limburg-Weilburg (Limburg)		Westerwald-kreis (Montabaur)		Rhein-Siegkreis (Siegburg)		All non-treated counties	
Predictor variable	Treat	Synth	Treat	Synth	Treat	Synth	Mean	S.D.
GDP/worker (€)	63.8k	69.0k	64.9k	64.5k	74.9k	74.7k	69.3k	7.9k
Ratio out/in-commuting	0.51	0.30	0.51	0.50	0.36	0.38	1.09	1.00
Population/sq. km land area	227	424	193	178	464	463	771	813
Industry share: Const.	4.6%	4.6%	4.0%	4.0%	2.9%	2.9%	2.8%	1.1%
Industry share: Mining	9.2%	9.2%	14.1%	13.9%	10.8%	10.8%	13.7%	5.1%
Industry share: Services	36.2%	36.2%	31.9%	31.7%	36.2%	36.1%	33.5%	4.9%
Industry share: Retail	8.0%	8.9%	8.7%	8.7%	8.5%	8.5%	8.8%	2.1%
Industry share: Manufact.	13.8%	13.8%	18.1%	18.0%	13.8%	13.7%	16.5%	4.9%
Industry share: Finance	16.1%	15.9%	12.1%	12.0%	15.1%	15.0%	12.8%	2.8%
Share higher education	5.1%	4.7%	3.7%	3.6%	6.7%	6.7%	6.5%	3.1%

Notes: The reported values are means across all years prior to 1998 (when construction began) except for the share of workers (at workplace) holding a university degree, which refers to 1999, the earliest year for which data was available.

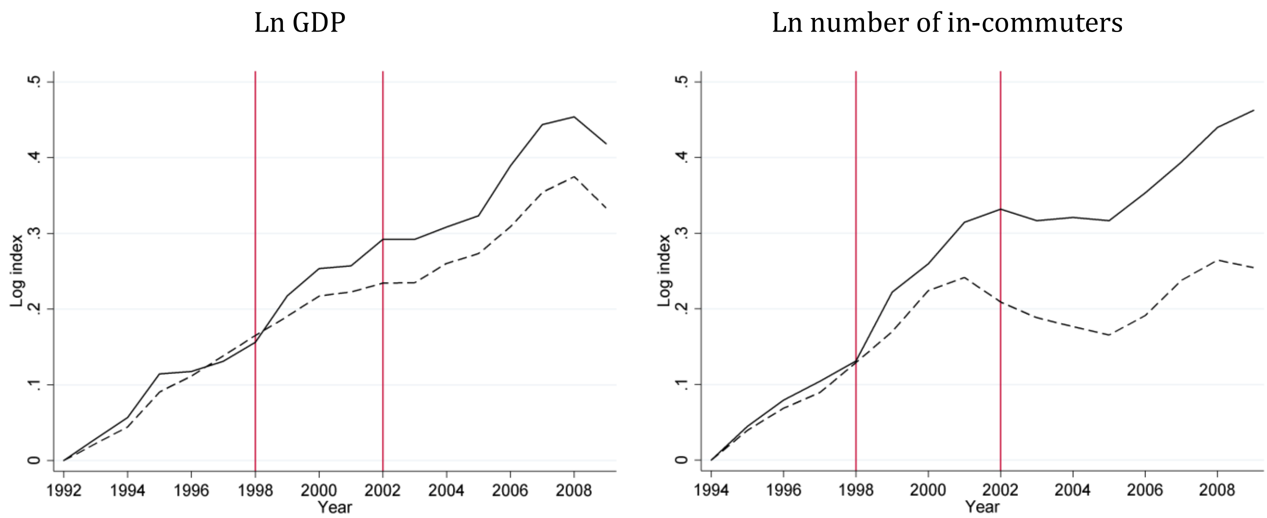
The weighting has achieved its first-order purpose of creating comparison counties that are more similar to the treated than the naïve control group of all non-treated counties. None of the synthetic counties depend solely on one county, nor are the important donors (with high weights) within a likely spillover range (see section 3-A in the appendix). We are thus ready to use the weights matrices (one for each treated county) to approximate vectors of counterfactual outcomes for the synthetic counties. We begin with Westerwaldkreis, home to the HSR stop Montabaur. As introduced in Section 2, Montabaur features particularly prominently in the media as an example of how communities can benefit from access to HSR. Using log GDP as an outcome variable, the left panel of Figure 2 compares the actual realizations (solid lines) to a vector of counterfactual values (dashed line) for the synthetic control county $Y_1^* = Y_0 W^*$, where Y_0 is a matrix containing the economic outcomes of all non-treated counties for all years. Both trend lines are normalized to zero in the first period. Up to 1998 the two lines follow each other closely, which indicates that the weighting also achieved the second-order purpose of equalizing pre-trends. After 1998 actual economic growth surpasses the counterfactual growth, in particular during the construction period. This pattern is indicative of some anticipation effects. Some firms moved to or expanded their businesses before the station was actually served, perhaps in an attempt to seek first-mover advantages and occupy the best possible spots in the business park close to the station.

To gain further insights into other dimensions of economic impact we have used the procedure outlined above to create synthetic control counties and counterfactual trends for each of the three treated counties and a number of alternative outcomes. The right panel in Figure 2 exemplarily illustrates the resulting trend lines for the actual and the counterfactual log number of in-commuters in Westerwaldkreis. This comparison substantiates the impression that the county was perceived as an economically more attractive location once it was clear that it would be connected to the HSR line.

Figure 3 provides an overview of the various comparisons between the actual and counterfactual trends we did for the three treated counties and six alternative outcome measures. In each panel we plot the differences between the trend lines (actual – counterfactual) for a different outcome measure. We further add an extrapolated linear trend fitted into the pre (before 1998) observations to allow for an intuitive comparison of the relative trends before and after construction began.

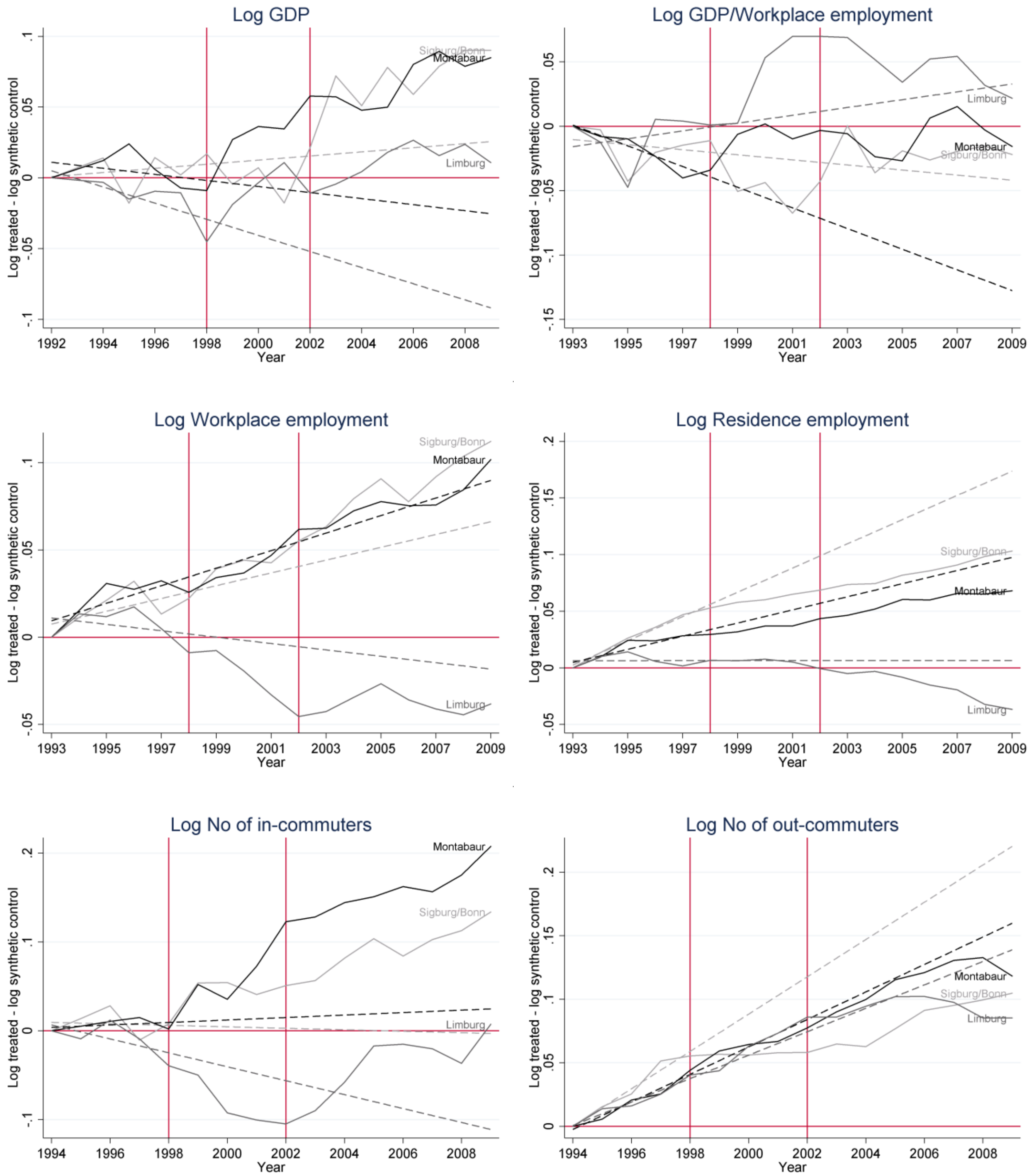
The positive impacts on economic activity suggested by Figure 2 for Montabaur seem to generalize to the two other intermediate stops. We find positive deviations from the relative pre-trend in GDP after the construction began (Limburg) or the line was completed (Siegburg). Similar positive turns in relative trends are evident in the share of in-commuters. The other outcome measures yield more mixed patterns and, in general, suggest that the HSR increased the attractiveness of the three affected counties as places to work rather than places to live.

Fig. 2 Westerwaldkreis (Montabaur) vs. synthetic control county



Notes: Solid (dashed) line shows the trend line for Westerwaldkreis where Montabaur is situated (the synthetic control county). Vertical lines indicate the period of substantial construction activity. Years up to 1997 were used in the construction of the weights matrix underlying the synthetic county.

Fig. 3 Relative trends for treated counties vs. synthetic control counties



Notes: Solid lines represent the differences between the trend lines for a treated county and the synthetic control county. Vertical lines indicate the period of substantial construction activity. Years up to 1997 were used in the construction of the weights matrices underlying the synthetic counties. Dashed lines are extrapolated linear fits using observations before 1998.

B – Econometric analysis

For a more formal test of the economic impact of the HSR on the group of treated counties we make use of the following difference-in-differences (DD) specification:

$$\log(Y_{it}) = \theta[T_i \times (t > 2002)_t] + \sum_{n=1998}^{2002} \theta_n [T_i \times (t = n)] + \vartheta[T_i \times (t - 2003)_t] + \vartheta^{P}[T_i \times (t - 2003)_t \times (t > 2002)_t] + \mu_i + \varphi_t + \varepsilon_{it} \quad (3\text{-}1)$$

, where i and t index counties (treated and non-treated) and years, T_i is a dummy variable that is one for the treated counties of Montabaur, Limburg, and Siegburg and zero otherwise, $(t > 2002)$ similarly indexes years after 2002, $(t = n)$ similarly indexes a year n, $(t - 2003)$ is a yearly trend taking a value of zero in 2003, and μ_i and φ_t are county and year fixed effects and ε_{it} is a random error term. This specification allows for a short-run impact on the level of the economic outcome variable $(\theta[T_i \times (t > 2002)_t])$ as well as a long-run impact on its trend $\left(\vartheta^{P}[T_i \times (t - 2003)_t \times (t > 2002)_t]\right)$ while controlling for heterogeneity in pre-trends across the treated and the control counties $(\vartheta[T_i \times (t - 2003)_t])$. The cumulated percentage impact in a given (post) year is defined as $\exp\left(\hat{\theta} + \hat{\vartheta}^{P} \times (t - 2003)\right) - 1$.[15] The new stations have provided transport services since 2002, but a high degree of confidence regarding the eventual completion of the line have existed since 1998 when the substantial construction works began. We therefore add a number of short-run DD terms $\sum_{n=1998}^{2002} \theta_n [T_i \times (t = n)]$ which absorb the effects during the construction period so that our treatment estimates are based on a comparison between the pre-construction (t<1998) to the post-completion period (t>2002). Essentially, the model produces empirical estimates of the cumulated effect (and its significance) which correspond to the differences between the solid and the dashed lines in Figure 3 during the post period. Standard errors are clustered on counties to account for serial correlation as recommended by Bertrand, Duflo, and Mullainathan (2004).

We begin with the presentation of the empirical results for the outcome measure log GDP in Table 2. We use the groups of all non-treated (1–3) as well as the synthetic counties (4–6) as control groups and, in each case, complement the presentation of the results of the full models (3) and (6) with simplified versions of the model. Columns (1) and (4) provide a simple mean comparison (conditional on county and year fixed effects) of the difference in log GDP across the groups of treated and non-

[15] The respective standard error is $\exp\left(var(\hat{\theta}) + (t - 2003)^2 \times \mathrm{var}(\hat{\vartheta}^{P}) + 2 \times (t - 2003) \times \mathrm{cov}(\hat{\theta}, \hat{\vartheta}^{P})\right) - 1$.

treated as well as the pre (before 2003) and post (from 2003 onwards) periods. Columns (2) and (5) control for effects during the construction years, but do not control for trends.

The results, relatively consistently point to a positive and significant impact of the HSR on GDP. Ignoring trends, GDP in the treated counties grew by about 7% more in the treated counties than in the remaining ones if the comparison is made between the periods before construction began and after construction ended (2). The effect is slightly larger than in the basic model (1), which is consistent with the anticipation effects found in the visual inspection of the trend lines. The effect is also roughly in line with the average differences between the actual relative trend (solid lines) and linearly extrapolation pre-trends (dashed lines) during the post-period in the upper-left panel of Figure 3. Once we control for relative trends, the treatment effect disappears. As there is no positive impact on (post) trends, the implication is that the model attributes the relative differences between the before and after period to heterogeneous trends that existed prior to the treatment.

Our preferred models, which compare the trends in the treated counties to the synthetic counties, yield a somewhat different picture. Consistently, all models (4–6) point to a GDP growth in the group of treated counties that exceeds the control group by about 5% in the short run. The full model (6) also suggests a positive long-run impact on the GDP trend, which is just about not statistically significant. The cumulated effects after three (2006) and six (2009) years, which are a combination of the short-run level and long-run trend effects amount to statistically significant effects of about 6.5–8.5% and are thus within the range of the effects suggested by Table 2, column (2) and Figure 3 (upper-left). The estimates reported in Table 2 are robust to using different donor pools (counties from which the algorithm can draw) and predictors (covariates the algorithm seeks to balance) (see section 3-B in the appendix).

Tab. 2 Treatment effect on GDP

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln GDP					
Control group	Non-treated counties			Synthetic counties		
T x (YEAR>2002)	0.057***	0.072***	-0.002	0.049**	0.051**	0.046*
[θ]	(0.006)	(0.008)	(0.011)	(0.014)	(0.016)	(0.018)
T x (YEAR>2002) x			-0.001			0.006
(YEAR-2003) [ϑ^P]			(0.003)			(0.003)
Cumulated effect			-0.003			0.066*
after 3 years			(0.017)			(0.027)
Cumulated effect			-0.005			0.084*
after 6 years			(0.024)			(0.036)
County Effects	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
Constr. years x T	-	YES	YES	-	YES	YES
T x (YEAR-2003)	-	-	YES	-	-	YES
r2	0.997	0.997	0.997	0.999	0.999	0.999
N	2034	2034	2034	108	108	108

Notes: Standard errors in parentheses are clustered on counties. *T* is a 0,1 indicator variable indexing the treated counties. Cumulated effects computed as $\exp\left(\hat{\theta} + \hat{\vartheta}^P \times (t - 2003)\right) - 1$. Cumulated standard errors computed as $\exp\left(var(\hat{\theta}) + (t - 2003)^2 \times \text{var}(\hat{\vartheta}^P) + 2 \times (t - 2003) \times \text{cov}(\hat{\theta}, \hat{\vartheta}^P)\right) - 1$. Constr, years x *T* indicates treatment *T* x year *n* interaction terms $\sum_{n=1998}^{2002} \theta_n [T_i \times (t = n)]$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In Table 3 we replicate the least (1) and most (6) demanding models from Table 2 separately for each of the treated counties. We find positive effects on each of the treated counties, which are roughly within the range of the effects derived from the pooled models. After six years, each of the treated counties exceeded its synthetic counterpart by about 7–10% in terms of GDP.

Table 4, applies the most demanding specification (comparison to synthetic control counties controlling for trends) to different outcome measures. We find a positive and statistically significant effect on per-worker GDP, which is roughly within the range of the GDP impact just discussed. Economic growth thus seems to have come at least in part, if not entirely, through an increase in productivity (of the labor force). It is noteworthy, however, that the naïve DD specification (used in Table 2, column 2) yields significantly positive effects on per-worker GDP and workplace employment of roughly similar magnitude (see appendix section 3-C). The naïve DD specification also adds to the results reported Table 4, column 5 in that the positive point estimate of the in-commuter effect is statistically significant. This is in line with the results of a complementary analysis of bilateral commuting flows within the study area summarized in Figure 4 and laid out in more detail in appendix section 3-E. Confirming the anecdotal evidence reported in section 2-A, the analysis shows that firms in Montabaur and Limburg have been drawing employees from a wider labor market since the opening of the HSR. The analysis also reveals that at the intensive margin (variation in reduction of commuting times), there seems to be a HSR effect on out-commuting.

Overall, the results of the econometric analysis support the key finding of the visual trend inspection that the HSR increased the attractiveness of the locations close to the intermediate stations as places to work, but not necessarily as places to live. A further breakdown by industry (in section 3-D in the appendix) shows that treatment effects are driven by financial services and other non-public services. This confirms anecdotal evidence citing the positive effects on firms specializing in consulting (Emc[2]), telecommunications (1&1), software (Itac) or events (ADG).

Tab. 3 Treatment effects on GDP by treated county

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln GDP					
	Limburg-Weilburg (Limburg)		Westerwaldkreis (Montabaur)		Rhein-Siegkreis (Siegburg)	
Control group	Non-treat.	Synthetic	Non-treat.	Synthetic	Non-treat.	Synthetic
TREAT x (YEAR>2002)	0.056***	0.033**	0.058***	0.049	0.057***	0.057**
[θ]	(0.006)	(0.010)	(0.006)	(0.030)	(0.006)	(0.023)
TREAT x (YEAR>2002)		0.005*		0.007*		0.005
x (YEAR-2003) [ϑ^{P}]		(0.002)		(0.004)		(0.003)
Cumulated effect		0.050***		0.073*		0.074**
after 3 years		(0.009)		(0.039)		(0.026)
Cumulated effect		0.067***		0.097*		0.089**
after 6 years		(0.013)		(0.049)		(0.031)
County Effects	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
Constr. years x T	-	YES	-	YES	-	YES
T x (YEAR-2003)	-	YES	-	YES	-	YES
r2	0.997	1.000	0.997	1.000	0.997	1.000
N	1998	36	1998	36	1998	36

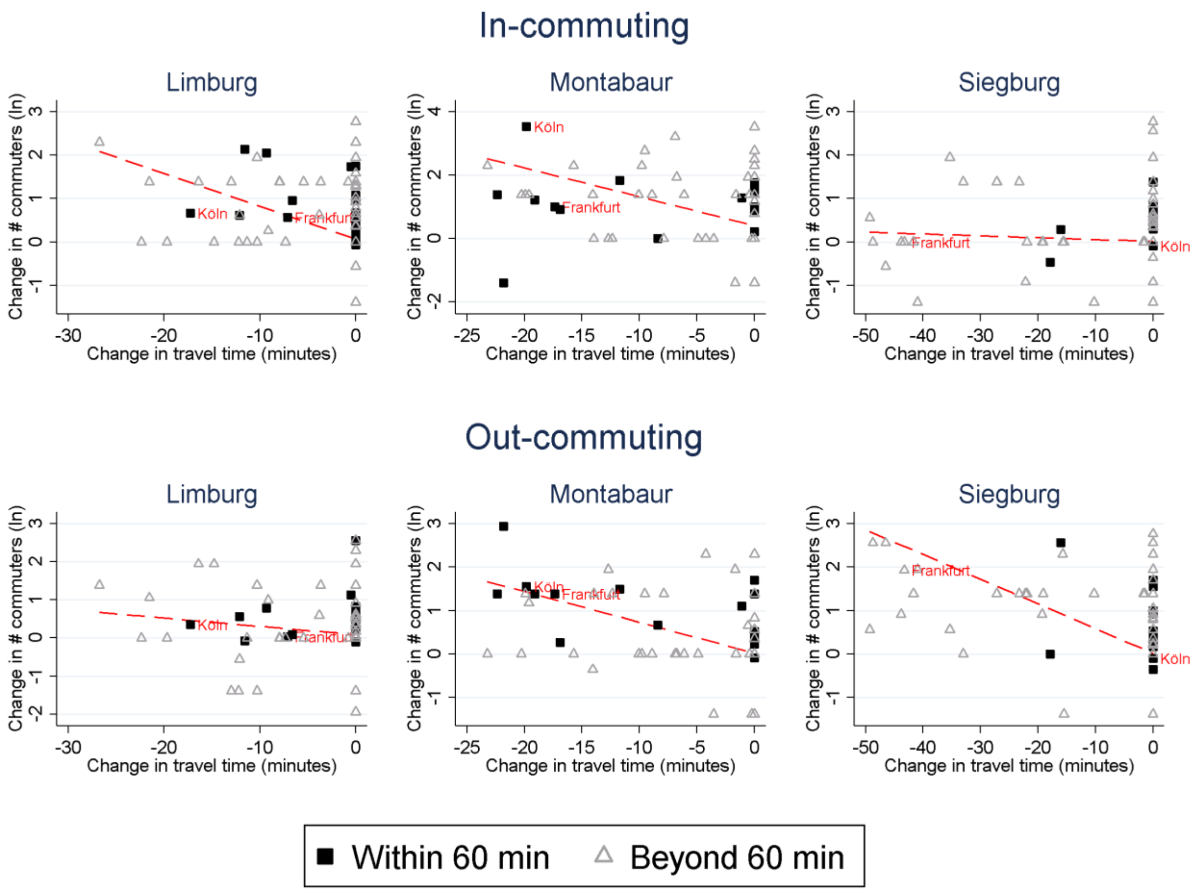
Notes: Standard errors in parentheses are robust in (2), (4) and (6) and clustered on counties in (1), (3), and (5). T is a 0,1 indicator variable indexing the treated counties. Cumulated effects computed as $\exp\left(\hat{\theta} + \hat{\vartheta}^{P}\times(t-2003)\right) - 1$. Cumulated standard errors computed as $\exp\left(var(\hat{\theta}) + (t-2003)^{2}\times \text{var}(\hat{\vartheta}^{P}) + 2\times(t-2003)\times\text{cov}(\hat{\theta},\hat{\vartheta}^{P})\right) - 1$. Constr, years x T indicates treatment T x year n interaction terms $\sum_{n=1998}^{2002}\theta_n[T_i\times(t=n)]$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Tab. 4 Treatment effect on other economic outcomes

	(1) Ln GDP/ worker	(2) Ln Work-place em-ployment	(3) Ln Resi-dence em-ployment	(4) Ln Population	(5) Ln No of in-commuters	(6) Ln No of out-commuters
Control group			Synthetic counties			
T x (YEAR>2002)	0.056^{***}	-0.020	-0.025	-0.009	0.030	-0.015
[θ]	(0.010)	(0.032)	(0.014)	(0.015)	(0.086)	(0.030)
T x (YEAR>2002) x	0.002	-0.001	-0.005^{***}	-0.004^{*}	0.010	-0.007
(YEAR-2003) [ϑ^{P}]	(0.004)	(0.006)	(0.001)	(0.002)	(0.011)	(0.004)
Cumulated effect	0.065**	-0.023	-0.040**	-0.022	0.062	-0.035
after 3 years	(0.021)	(0.050)	(0.014)	(0.021)	(0.123)	(0.039)
Cumulated effect	0.072*	-0.025	-0.055**	-0.034	0.095	-0.055
after 6 years	(0.034)	(0.069)	(0.015)	(0.026)	(0.158)	(0.049)
County Effects	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
Constr. years x T	YES	YES	YES	YES	YES	YES
T x (YEAR-2003)	YES	YES	YES	YES	YES	YES
r2	0.983	0.999	1.000	1.000	0.998	0.999
N	102	102	102	120	96	96

Note: Standard errors in parentheses are clustered on counties. T is a 0,1 indicator variable indexing the treated counties. Cumulated effects computed as $\exp\left(\hat{\theta} + \hat{\vartheta}^{P}\times(t-2003)\right) - 1$. Cumulated standard errors computed as $\exp\left(var(\hat{\theta}) + (t-2003)^2\times\text{var}(\hat{\vartheta}^{P}) + 2\times(t-2003)\times\text{cov}(\hat{\theta},\hat{\vartheta}^{P})\right) - 1$. Constr, years x T indicates treatment T x year n interaction terms $\sum_{n=1998}^{2002}\theta_n[T_i\times(t=n)]$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Fig. 4 HSR effects on bilateral commuting flows from and to HSR municipalities



Notes: In-commuter models use flows from all municipalities in the study area into intermediate HSR municipalities. Out-commuter models use flows from intermediate HSR municipalities to all municipalities in the study area. First difference in # of commuters is the difference between the average number of commuters from 1992–1995 and the average number of commuters from 2010–2012. First difference in travel time is based on the transport cost matrices summarized in Figure 4. Observations weighted by the average number of commuters (across all period).

C – Falsification

As with any program evaluation the key identification challenge in our empirical exercise is to find a credible counterfactual for the treated group. To ensure a valid comparison we have constructed a synthetic control group which resembles the treated counties in terms of observable characteristics and pre-treatment trends. In addition, we have made use of an econometric model that controls for heterogeneity in pre-trends between the treated and the control counties. We argue that this degree of sophistication helps to reduce the risk of erroneously attributing different macroeconomic trends that result from differences between the groups of treated and control counties to the HRS. But we acknowledge that there is, ultimately, no formal way of affirming that the true counterfactual trend has been established. What can be done is to evaluate the likelihood that our empirical design reveals a treatment effect where, in effect, there is no treatment. We summarize the results of three falsification tests here and refer to the appendix (sections 3-F, 3-G, 3-H) for a detailed discussion.

We begin with a classic "placebo" study. We apply our empirical strategy to an HSR which was considered during the planning stage but never built. The track would also have had three intermediate stops in each of the involved federal states and would have passed through the economically and politically relevant cities of Bonn (the former federal capital located in North Rhine-Westphalia), Koblenz (the largest city in northern Rhineland-Palatinate) and Wiesbaden (the state capital of Hesse). The results are easily summarized. The mean treatment effect on the GDP across the three cities is near to and not statistically different from zero in all specifications. The separate treatment estimates by treated county produce significant estimates with mixed signs in the naïve DD specification, but no significant cumulative effects using synthetic counties as comparisons (although Wiesbaden has a near to 10% significance level positive long-run effect). We don't find any significant effect of the other outcome measures either, although there are large and positive point estimates for per-worker GDP (but even larger standard errors).

Focusing on GDP as an outcome measure, we next conduct a placebo test in the spirit of Abadie, Diamond, and Hainmueller (2010). For every county in the donor pool (non-HSR counties), we use a synthetic county generated by the same method as in the baseline model to estimate a placebo treatment effect. We find that the actual average treatment effect (across the three intermediate towns) is outside the 90% confidence interval in the right tail of the distribution of placebo treatment effects (1,000 random combinations of three placebo-treated counties). The key insight from this placebo test is that the method employed is unlikely to yield treatment effects similar to the ones estimated for the HSR counties by chance. This, placebo test, however, could be favorable to the actual treated counties because these are not randomly located in space. Instead, the treated counties are within a relatively close distance (compared to the average distance between counties in the study area) located almost exactly along a straight line. It is therefore more likely that unobserved spatially correlated characteristics have a similar impact on economic trends of the actual treated counties than for the randomly selected placebo-treated counties.

To address this concern, we refine the selection process of placebo-treated counties to make the test more demanding. Again, we run a series of 1,000 similar models, however, this time requiring that the placebo-treated counties are connected by a placebo HSR. In each iteration of the placebo test, we first randomly select one county as one endpoint of a placebo HSR (the placebo Cologne). Second, we randomly select another endpoint (the placebo Frankfurt) from all counties within a 140–180km range (in terms of straight-line distances) of the first endpoint (the distance between Cologne and Frankfurt is 160km). Third, we pick the three counties whose economic center (the largest city) is closest to a straight line connecting the two endpoints and define them as the treated counties (the placebo intermediate stops). Fourth, we create synthetic comparison counties for each of the placebo-treated counties according to our standard procedure. Fifth, we estimate the naïve DD model (Table 2, column 3 model, which uses all non-treated control counties and does not control for trends) as well as our preferred model (Table 2, column 6 model, which uses synthetic control counties and controls for trends) and save the point estimates and significance levels. Of the 1,000 tests 8.4% (24%) deliver significant treatment effects after six years using our preferred (naïve) DD model. 5.6% (8.2%) iterations resulted in treatment effects that were significant (at the 10% level) and at least as large as our benchmark estimates. The mean of the point estimate is very close to zero. Notably, the standard deviation across placebo point estimates with 8.6% (5.4%) is relatively large compared to our 8.4% (5.7%) treatment estimate. We conclude that it is unlikely that our empirical specification delivers significant treatment effects that are spurious.

4 Agglomeration effects

Given the results presented so far it seems fair to conclude that the HSR has had a positive impact on the economies of the counties of the intermediate stops. This impact is in line with the idea that an increase in (market) accessibility should increase the attractiveness of a location as a place of production. In the next step we seek to model the change in accessibility pattern induced by the HSR more fully to gain insights into the strength and the spatial scope of agglomeration forces.

A – Empirical strategy

In our baseline empirical model we assume that the output in county i in year t denoted by Q_{it} depends on effective density D_{it} as well as arbitrary county effects c_i and year effects d_t.

$$\ln(Q_{it}) = \delta_1 \ln(D_{it}) + c_i + d_t + \varepsilon_{it} \quad (4\text{-}1)$$

, where δ_1 is the elasticity of output with respect to effective density for marginal changes in D and ε_{it} is a random error. We hypothesize that, all else equal, access to a larger economic mass should increase firm productivity and lead to higher economic output. We model effective density as a function of output across all counties *j* within reach and, thus, assume a black-box agglomeration force that depends on the productivity of all non-land inputs. Specifically, we allow for bilateral productivity externalities between all counties, assuming that the spillover effect declines exponentially in a measure of effective distance E_{ij} between regions *i* and *j*, which takes into account the availability of transport infrastructure. Our measure of effective density thus takes the market potential form (Harris, 1954), which is popular in the theoretical (Fujita & Ogawa, 1982; Lucas & Rossi-Hansberg, 2002) and empirical (Ahlfeldt et al., 2015; Ahlfeldt & Wendland, 2013) agglomeration economics literature. Similar measures have been used in the empirical NEG literature (Hanson, 2005; Redding & Sturm, 2008).

$$D_{it} = \sum_j Q_j e^{-\delta_2 E_{ij}} \tag{4-2}$$

, where δ_2>0 determines the rate of spatial decay of the productivity effect in effective distance between two regions *i* and *j*.[16] The strength of the market potential formulation is that it effectively allows the productivity effect of spatial externalities to vary in effective distance to the surrounding economic mass without imposing arbitrary discrete classifications. Instead of assuming that externalities operate within the administrative borders of a region or contiguous groups of regions, our measure of effective density also accounts for externalities across such borders.

Estimating the parameters of interest δ_1 and δ_2 is challenging for a variety of reasons. Firstly, it is difficult to control for all location factors subsumed in c_i , which impact on productivity and are potentially correlated with the agglomerations measure. Secondly, there is a mechanical endogeneity problem because the dependent variable output (Q_{it}) also appears in the market potential of regions *i=j*. Unobserved shocks to output can therefore lead to a spurious correlation between the outcome measure and effective density. The problem is non-trivial given that internal effective distance $E_{ij=i}$ is typically short so that the $Q_{ij=i}$ receives a relatively high weight. Thirdly, it is likely that shocks to outputs are spatially correlated so that the same problem also applies to nearby areas *i* and *j*.

[16] Our internal effective distance $E_{ij=i}$ depends on the land area of county *i* so that our measure corresponds to a standard density measure for the within county externalities.

The first problem can be addressed by estimating equation (4-1) in differences so that unobserved time-invariant location factors are differentiated out as, for example, in Hanson (2005). Informed by the program evaluation results, we take long differences over the construction period from 1998 to 2002 in our baseline estimation, but we consider alternative end dates in an alternative specification. The second problem, in principle, can be mitigated by aggregating right-hand side areas *j* to larger spatial units (e.g. Hanson, 2005) or replacing $Q_{ij=i}$ with imputed values (Ahlfeldt & Wendland, 2013). Both strategies come at the cost of losing information. The third problem is even more difficult to address since shocks to output at nearby regions are likely correlated not only in levels but also in trends.

Our empirical strategy addresses the abovementioned problems by exploiting the variation in bilateral transport times created by the HSR. We set the output levels at all locations *j* to $Q_{jt=1998}$ in both periods, so that the identification comes exclusively from changes in effective distance. Our estimation equation thus takes the following form:

$$\ln\left(Q_{i,t=2002}\right) - \ln\left(Q_{i,t=1998}\right) = \delta_0 + \delta_1 \begin{bmatrix} \ln\left(\sum_j Q_{j,t=1998} e^{-\delta_2 E_{ij,t=2002}}\right) \\ - \ln\left(\sum_j Q_{j,t=1998} e^{-\delta_2 E_{ij,t=1998}}\right) \end{bmatrix} + \Delta\varepsilon_i \qquad (4\text{-}3)$$

We stress that this specification differs from a conventional first-difference approach in that the first-difference in the market potential is driven by changes in the travel time, but not output. Specification (4-3) is estimated using a non-linear least squares estimator to simultaneously determine both parameters of interest (δ_1 and δ_2). With the estimated parameters it is then possible to express the effect of an increase in economic mass at *j* by one unit of initial market potential of county *i* on the outcome of county *i* as a function of the bilateral effective distance:

$$\frac{\partial \log(Q_i)}{\partial (Q_j)} \times \left(\sum_j Q_j e^{-\delta_2 E_{ij}}\right) = \widehat{\delta_1} \exp(-\widehat{\delta_2} E_{ij}) \qquad (4\text{-}4)$$

Similar increases in economic mass are expected to benefit a county more if it happens in a county within a shorter effective distance.

We consider several alterations of specification (4-3) for the purposes of validation, falsification, and evaluation of robustness. We estimate equation (4-3) using the GVA in various industry sectors as an outcome variable. We consider a grid search over a large parameter space (δ_1, δ_2) to evaluate whether the agglomeration and spillover parameters are credibly separately identified. We contrast our results with those derived from a market potential specification that allows for more flexibility in the spatial decay. We allow for trends correlated with initial sectorial composition, workforce qualification, and exposure to agglomeration. We also control for trends pre-existing the construction of the HSR and explore the temporal pattern of adjustment using an alternative panel specification. Importantly, we use instrumental variables to restrict the identifying variation to the portion that is not only exogenous with respect to the timing, but also with respect to the routing of the HSR. For falsification, we make use of the placebo HSR, which was considered but never built, and public sector GVA as an outcome, which we expect not to respond to the HSR, at least in the short run. Finally, we replicate the main stages of the analysis using per-worker GDP as a dependent variable to connect more closely to the literature on the productivity effects of density. In this alternative specification we will also control for changes in the industry sector structure and workforce qualification to address selection effects.

B – Approximation of effective distance

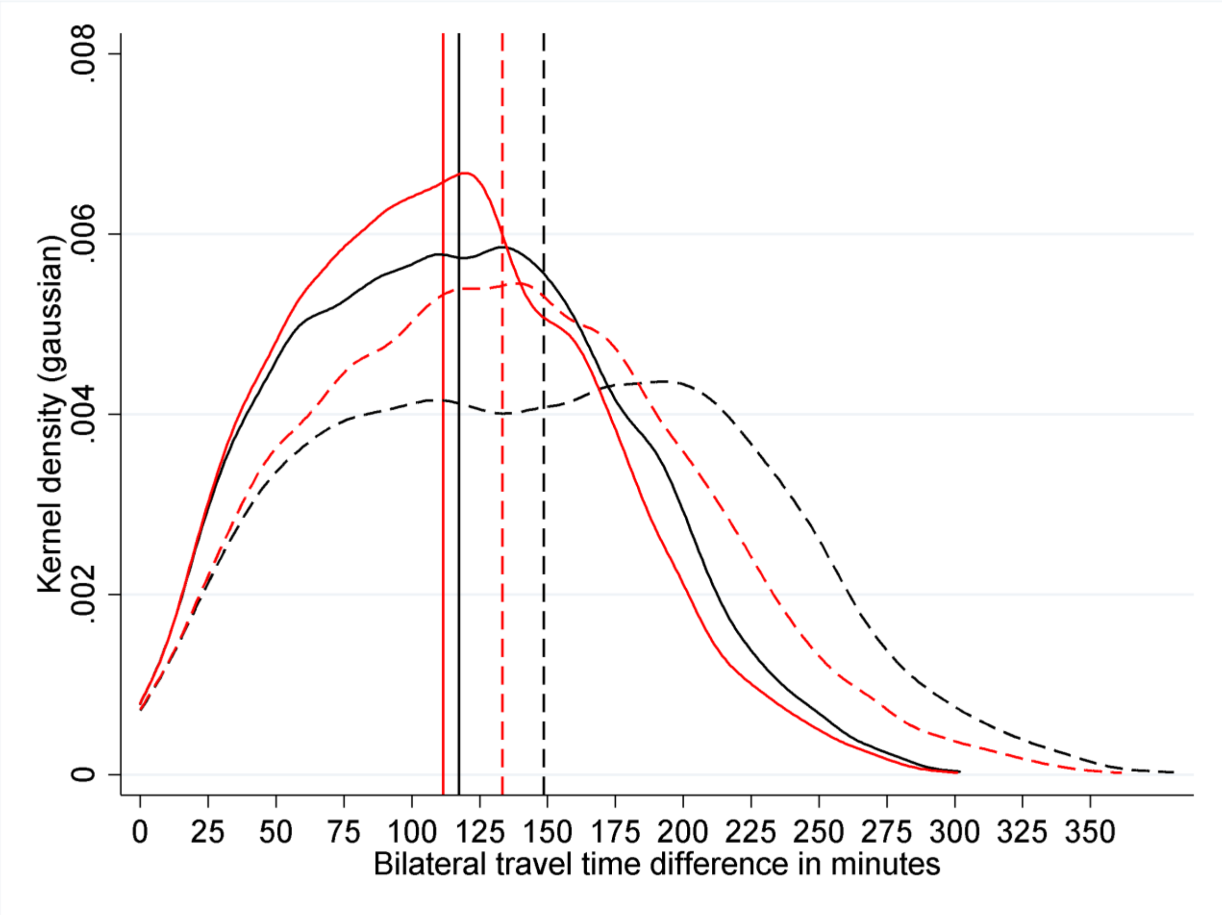
To implement the empirical strategy laid out above we require empirical approximations of the bilateral travel costs between each pair of counties in the study area, the effective distance. To compute our measures of effective distance we make use of a Geographic Information System (GIS) and the information on transport infrastructure displayed in Figure 1. In connecting two counties we refer to the largest cities within the pair of counties as the respective centers of economic mass (the black dots in Figure 1). In computing the effective distance we assume that transport costs are incurred exclusively in terms of travel time and that route choice is based on travel time minimization. The identifying variation stems exclusively from the HSR line in question. We abstract from any other change in transport technology.

To solve for the least-cost matrix connecting all potential origins and destinations we assign travel times to each fraction of the transport network, which are based on the network distance and the following speeds: 160km/h for HSR, which is roughly in line with the 70min journey along the 180km Cologne-Frankfurt HSR line; 80km/h for conventional rail, which is roughly in line with the 140min journey along the 205km conventional rail line; 100km/h for motorways and 80km/h on the other primary roads. In combining these transport modes we experiment with different procedures. In our benchmark cost matrix we allow travellers to change from roads to conventional rail in any city (they all have rail stations) and from any mode to HSR at the dedicated HSR stations (white circles in Figure 1) if in the respective period HSR is available. For robustness checks we compute travel times according to two alternative decision rules. In one version travellers can choose either the automobile or rail, including HSR if available, but they are not allowed to switch mode during a journey. In a further alternative we eliminate the automobile altogether. Since the automobile is typically the more competitive mode the resulting change in travel time reflects an upper bound of the true accessibility gain.

In each case, we approximate the average internal travel time within a region i=j as the travel time that corresponds to a journey at 80km/h (primary road) along a distance that corresponds to two-thirds of the radius of a circle with the same surface area.

Figure 5 summarizes the distribution of travel times across the 115^2=13,225 county pairs in the situations with and without HRS according to the baseline decision rule and the rail-only alternative. Evidently, the introduction of HSR had a significant impact on the competitiveness of the rail network as reflected by the major shift in the distribution of rail travel times (dashed lines) toward the distribution of road travel times (black solid line). Prior to HSR, the road network offered faster connections for almost all county pairs so that the road travel time matrix effectively describes the least-cost matrix (black solid lines). As expected, adding HSR as a potential mode that can be combined with the automobile reduces travel times significantly on a number of routes, especially on those that would otherwise take 50min or more (red solid line).

Fig. 5 Distributions of bilateral travel times



Notes: Black (red) solid line shows the distribution of bilateral travel times on roads (the fastest combination of car and rail including HSR). Black (red) dashed line shows the distributions for rail excluding (including) HSR. Vertical lines denote the respective means of the distributions.

C – Market potential effects on output: Baseline results

Column (1) in Table 5 summarizes the results of estimating the model given by Eq. (4-3) using ln regional GDP as the economic outcome. The estimates point to positive spillover effects, which decay in distance. Given an 18.5% elasticity of output with respect to market potential, a doubling in market potential implies an increase in GDP by 20% (=exp(0.185)-1). The strength of spillovers decays by 2.3% every minute, which corresponds to a half-life travel time of about 30 minutes. It takes about 200 minutes before the strength of the spillovers diminish to around 1%. The black line in Figure 6 illustrates the implied productivity effect of an increase in economic mass at location *j* by one unit of total market potential at location *i*. Based on this estimated spatial decay, we illustrate the change in market potential in Figure 1. Not surprisingly, Montabaur (the primary town in its county) experiences the largest accessibility gain from HSR. Combining the change in market potential by 0.34 log-points with the estimated market potential elasticity the predicted increase in GDP for Montabaur is about 6%, which is close to the cumulated effect after three years detected in the program evaluation section.

The remaining columns in Table 5 present results according to Eq. (4-3) replacing regional GDP with the GVA of various industry sectors as the outcome variable. The estimated spillover effects are visualized in Figure 6 as gray lines. The estimates are generally within the range of column (1). For some sectors the parameters are, however, estimated less precisely. The results also suggest that the market potential elasticity estimated in column (1) is brought down somewhat by sectors that are apparently less susceptible to agglomeration benefits, namely services other than financial services. For construction, mining, manufacturing, and financial services the elasticity of output with respect to market potential is relatively large.

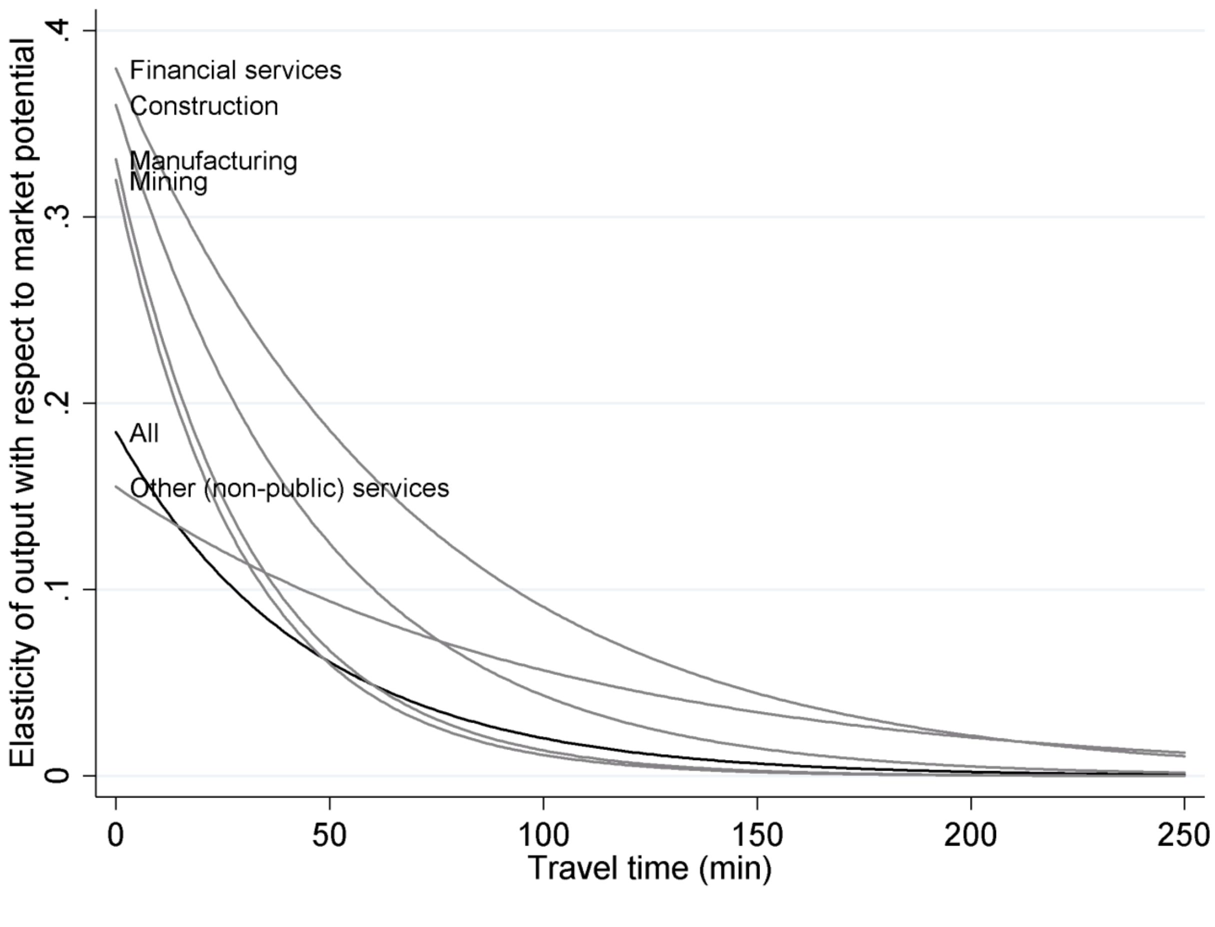
As the HSR line is used exclusively for passenger transport, we expect to capture Marshallian externalities related to human interactions. Candidates are knowledge spillovers due to formal and informal meetings, improved labor market access and matching, as well as improved access to intermediated goods and consumer markets to the extent that the ease of communication reduces transaction costs but not freight costs. Our results are thus principally comparable to Ahlfeldt et al. (2015) and Ahlfeldt and Wendland (2013) who have estimated the effects of spillovers on productivity from within-city variation. These studies have found spillover effects that are significantly more localized. The spillover effect in these studies decay to near to zero within about half a kilometer, which is in line with Arzaghi and Henderson (2008) who also focus on within-city variation. Compared to these studies the lower spatial decay suggests that we are capturing different types of spatial externalities. While the steep spillover decay in the within-city studies points to a dominating role of face-to-face contacts that purposely or accidently happen at high frequency in the immediate neighborhood (Storper & Venables, 2004), our results suggest that the HSR effects operate at an intermediate range and through the benefits of shared inputs and labor pools, labor market matching or increases in consumer and producer market access. This interpretation is also in line with the significantly lower spatial decay found in an empirical NEG studies with an emphasis on trade costs (Hanson, 2005).

Tab. 5 Market potential effects on output by sectors

	(1) Δln GDP 1998-2002	(2) Δln GVA 1998-2002	(3) Δln GVA 1998-2002	(4) Δln GVA 1998-2002	(5) Δln GVA 1998-2002	(6) Δln GVA 1998-2002
Sector	All	Construction	Mining	Manufacturing	Financial services	Other services
Δln Market potential (δ_1)	0.185***	0.360**	0.320**	0.331***	0.379***	0.155
	(0.051)	(0.167)	(0.124)	(0.118)	(0.116)	(0.094)
Decay (δ_2)	0.022**	0.021	0.033	0.032*	0.014	0.010
	(0.011)	(0.014)	(0.022)	(0.018)	(0.013)	(0.022)
r2	0.054	0.036	0.030	0.037	0.050	0.021
N	115	115	115	115	115	115

Notes: Estimation method is nonlinear least squares in all models. Robust standard errors (in parentheses) of the market potential coefficient δ_1 are heteroscedasticity robust and computed in separate OLS regressions holding the decay parameters (δ_2) constant at the levels estimated in the NLS models reported in the table. The market potential of region *i* is the transport cost weighted sum of output in all regions *j*. The change in market potential is driven by changes in travel cost between regions exclusively. Regional output is held constant at 1998 level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Fig. 6 Market potential effect on output by effective distance

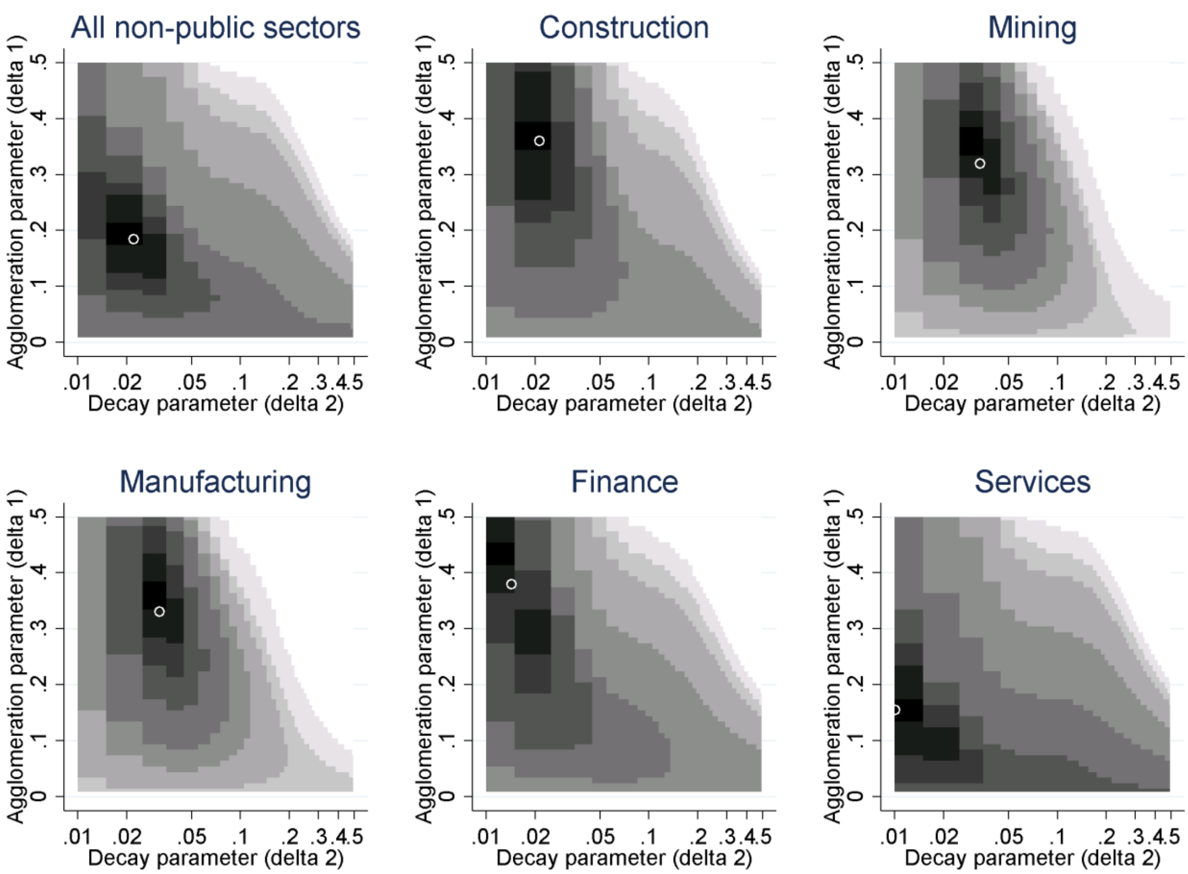


Notes: The figure shows the effect of a hypothetical increase in output at county *j* by one unit of initial market potential at county *i* on log output of county *i*. The figure illustrates agglomeration spillover effects as defined as in Eq. (4-4). Estimates of δ_1 and δ_2 from Table 2.

D – Market potential effects on output: Validation, robustness, and falsification

As in any market potential equation, the elasticity and decay parameters are not necessarily separably identified. In fact, it is only the (ad-hoc) functional form of the spatial decay imposed in the market potential formulation (4-2) that allows us to separately estimate the market potential elasticity (δ_1) and the decay parameter (δ_2). In general terms, a larger decay parameter δ_2 implies that more distant regions enter the market potential with a lower weight, reducing the degree of implicit spatial smoothing. The resulting larger variation in the market potential normally implies a lower estimate of the elasticity parameter δ_1.

Fig. 7 Market potential effect on output: Grid searches over parameter space



Notes: Dark shades indicate a low root sum of square error in "predicted change log output" – "actual change log output", where predicted change in log output and actual change in log output are normalized to have a zero mean. A more detailed description of the objective function is in the appendix (section 4-A). Output is measured in GDP for *all non-public sectors*, and GVA for all other sectors. Services exclude financial services (*Finance*) and public services. Class thresholds correspond to the 1st, 5th, 10th, 25th, 50th, 75th, 90th, 95th and 99th percentile in the distribution within the parameter space delta 2 = {0,0.1}. White circles denote NLS point estimates from Table 5. X-axis in ln scale.

As there could be multiple combinations of these critical parameters that fit the data we have run a grid-search over 500 possible values of δ_1 and δ_2(0.001 to 0.5) resulting in 250,000 parameter combinations for each of the models reported in Table 5. For each parameter combination, we compute the root sum of the square deviations between the observed and predicted changes in regional output. As illustrated in Figure 7, we find relatively clearly defined global minima, supporting the parametric estimates presented in Table 5 and Figure 6. This robustness check using an alternative approach to identifying the elasticity and decay parameters increases our confidence in the point estimates of the decay parameters estimated with large standard errors reported in Table 5. A more detailed description of the objective function is in the appendix (section 4-A).

In Table 6 we present a series of alterations of the baseline model in column (1) of Table 5. We fix the decay parameter to the value estimated in the baseline model (Table 5, column 1) so the market potential elasticity remains comparable across alternative models. In columns (1–3) we control for trends that may be correlated with but are economically unrelated to the change in market potential and potentially confound the estimates. The purpose of these models is, thus, similar to the matching on observables we imposed in the construction of synthetic counties in the program evaluation section. In model (4) we additionally control for the (1992 to 1997) pre-trend in log GDP to account for the possibility that unobserved county characteristics determine long-run growth trends.[17] This control serves a similar purpose to the matching on pre-trends in the construction of the synthetic counties and the control for heterogeneity in pre-trends in the program evaluation DD model. The market potential elasticity decreases somewhat but remains significant and within the range of the baseline estimate.

In model (5) we exploit that the timing and the routing of the HSR line can be assumed to be exogenous for the intermediate stops (Limburg, Montabaur, Siegburg) while "only" the timing (and not the routing) is exogenous for the endpoints Cologne and Frankfurt. To restrict the variation in change in market potential to the fraction that is most plausibly exogenous we instrument the change in market potential with three indicator variables, each denoting one of the counties in which the intermediate stops are located. The market potential elasticity remains significant, but decreases somewhat

[17] We take the lagged log GDP long-difference over the period 1992–1997 instead of 1992–1998 to avoid a mechanical endogeneity problem that would arise if the 1998 log GDP was entered on both sides of the equation.

further to about 12.5%. The instruments are strong (F-Stat > 10) and while we prefer to justify the instruments on theoretical grounds (they restrict the identification to plausibly exogenous variation), we note that a Sargan-Hansen test does not reject validity.

In model (6) we use GVA in the public sector instead of total GDP as the left-hand side measure of output. We view this model as a placebo test because the spatial distribution of this sector is unlikely to be determined by economic agglomeration forces, at least in the short run. In line with this interpretation we find a non-statistically significant near to zero agglomeration effect.

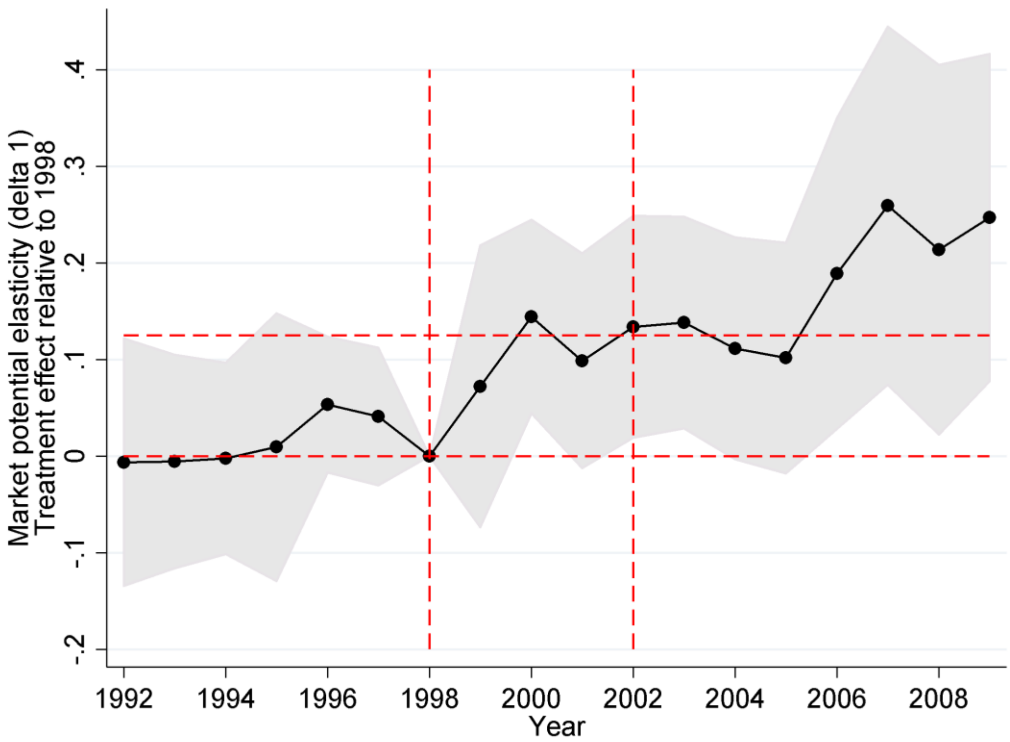
Tab. 6 Agglomeration effects: expanded models

	(1)	(2)	(3)	(4)	(5)	(6) Δln GVA Public services1998-2002
			Δln GDP all sectors 1998-2002			
	OLS	OLS	OLS	OLS	2SLS	OLS
Δln Market potential (δ_1)	0.149*** (0.048)	0.154*** (0.046)	0.154** (0.066)	0.138** (0.068)	0.125** (0.054)	-0.014 (0.081)
Industry shares	YES	YES	YES	YES	YES	-
Degree share	-	YES	YES	YES	YES	-
Agglomeration effects	-	-	YES	YES	YES	-
Δln GDP all sectors 1992-1997	-	-	-	YES	YES	-
F-Stat (Cragg-Donald)	-	-	-	-	23.95	-
Hansen J (p-value)	-	-	-	-	0.33	-
r2	0.123	0.145	0.220	0.235	0.235	0.000
N	115	115	115	115	115	115

Notes: Robust standard errors in parentheses are heteroscedasticity robust. Δln Market potential (δ_1) is based on Eq. (4-2) and the decay parameter (δ_1) from Table 5, column (1). Industry shares are shares at total 1998 GVA in the following sectors: Construction, manufacturing, mining, financial services, and other services. Degree share is the share for the workforce (at place of work) holding a university degree in 1998. Agglomeration effects include the 1998 market potential, population density, the 1997 log GDP as well as straight-line distances to Frankfurt and Cologne. Instrumental variables in column (5) are three indicator variations, each denoting one of the counties in which the intermediate stops Limburg, Montabaur, and Siegburg are located. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

So far we have estimated the agglomeration effects induced by the HSR line assuming that economic adjustments took place between 1998 to 2002. This choice is based on the results presented in the program evaluation section, where we find that each of the counties of the intermediate stops experienced a substantial impact over this period. To evaluate the temporal pattern of the adjustment and to empirically substantiate the chosen adjustment period, we estimate a time-varying treatment effects model such as in Ahlfeldt and Maennig (2015), where the treatment measure is the change in market potential used in Table 6. With this model, we estimate a series of market potential elasticities, each of which is identified from a comparison between long-differences in log GDP and log market potential taken over a treatment year n and the base year 1998. We set up the model such that the identifying variation corresponds to our most conservative long-difference model in Table 6, column (5), i.e., we control for trends correlated with observables and restrict the identifying variation to the intermediate stops using instrumental variables. The exact details of the specification are in the notes to Figure 8, which presents the resulting estimated market potential elasticity series.

Fig. 8 Market potential elasticity: Time-varying estimates



Notes: The figure is based on the following panel specification: $\ln(Q_{it}) = \sum_{n\neq 1998}[\delta_{1,n}\Delta\ln(D_i)\times(t=n)] + X_{it}b_t + c_i + d_t + \varepsilon_{it}$. where Q_{it} is the output measured as GDP of county i in year t, n indexes treatment years from 1992 to 2009, excluding the base year 1998, $\Delta\ln(D_i)$ is the change in market potential assuming the decay parameter estimated in Table 5, column (1), X_{it} is a vector year effects interacted with a vector of the following variables: industry shares at total 1998 GVA (construction, manufacturing, mining, financial services, and other non-public services), the share of the workforce (at place of work) holding a university degree in 1998, the 1998 market potential, population density, the 1997 log GDP as well as straight-line distances to Frankfurt and Cologne. b_t is a matrix of coefficients for each variable-year combination. c_i and d_t are county and year effects as in equation (4-1). We instrument the vector of change in market potential × year interaction terms $\Delta\ln(D_i)\times(t=n)$ using a full set of interaction terms between year effects and three indicator variations, each denoting one of the counties in which the intermediate stops Limburg, Montabaur, and Siegburg are located. Black dots represent point estimates of $\delta_{1,n}$ and the gray shaded area denotes the 95% confidence intervals (standard errors clustered on the counties). Vertical dashed lines frame the period over which long-difference are taken in Table 5 and 6. The upper horizontal dashed lines indicates the market potential elasticity estimated in Table 6, column (5) model, which in terms of the identifying variation is comparable to the model presented.

As expected, we find no significant response in the spatial distribution of economic activity to the market potential shock for treatment years $n<1998$, while the estimates of the market potential elasticity converge to the estimate in Table 6, column (5) relatively quickly for treatment years $n>1998$. By 2000, still in anticipation of the opening of the line in 2002, the spatial economy seems to have adjusted to the market potential shock as the time-varying estimates of the elasticity then remain relatively stable for a number of consecutive years. This pattern is suggestive of an impact of the HSR on the level, but not the trend of economic activity. In 2006, however, we observe a further relative shift in economic activity in regions which benefited from the HSR. Looking at the overall trend in the economic adjustment, this shift seems somewhat detached from the market potential shock, and it remains ultimately difficult to assert whether or not this shift is causally related to the HSR.

We have conducted a number of further alterations of our baseline model, which we briefly discuss in the remainder of this subsection. A more detailed discussion can be found in the appendix.

The exponential functional form of the spatial decay in spillovers, while popular in the theoretical and empirical literature (e.g. Ahlfeldt et al., 2015; Fujita & Ogawa, 1982; Lucas & Rossi-Hansberg, 2002), is ad-hoc and other functional forms are theoretically imaginable. We have estimated an alternative version of our benchmark model in which the market potential is captured as the total GDP within several mutually exclusive 20-minute travel time bins (e.g., 0–20 min, 20–40 min, etc.). For each travel time bin a separate market potential elasticity is estimated, thus allowing for a more flexible pattern in the spatial decay. When comparing the predicted effects of the change in market potential on GDP in this alternative model to our baseline model, we find an approximately linear relationship, suggesting that our results are not driven by an inappropriate functional form of the spatial decay (see appendix section 4-B). On a related note, we have addressed the insignificant sectoral market potential decay parameters by estimating the market potential elasticity imposing the decay parameter estimated across all sectors, which is highly significant. With the exception of "other non-public services," whose effect is already not significant in Table 5, the market potential estimates remain within close range of the results reported in Table 5 (see appendix section 4-B).

We have experimented with alternative travel choice models underlying the construction of travel times. In one alternative choice model, we disallow switching from train to automobile or vice versa along a journey. In another alternative choice model, we disallow the use of the automobile altogether. The results remain qualitatively unchanged and quantitatively within the range of the results presented here (see appendix section 4-C).

We have also experimented with alternative instrumental variables to restrict the identification to variation in the change in market potential to the fraction that arises from the intermediate transport stations. In particular, we consider the log straight-line distance to Montabaur as well as indictor variables for the counties of the intermediate stops as well as the adjacent counties as alternatives. The results remain close to those reported in Table 6 (see appendix section 4-D).

Finally, we have also replicated the main stages of our analysis replacing the actual HSR with the considered but never built placebo HSR, which we introduced in the previous section. We find no robust evidence of HSR effects in this falsification exercise (see appendix section 4-E).

D – Market potential effects on productivity

As discussed in the introduction a large literature has analyzed agglomeration effects by regressing a measure of productivity against a measure of density. In order to connect to this literature and to assess to which extent the market potential (effective density) effect on GDP discussed above is attributable to an increase in productivity of the labor force (rather than an expansion of the labor force), we replicate our baseline model using the ratio of GDP over the total employment (at workplace) as a dependent variable. The empirical specification used shared similarities with the nominal wage equation estimated in the NEG literature (e.g. Hanson, 2005) . In Table 7 we present the results of three OLS (1–3) and three 2SLS (4–6) estimations. In each case, we present unconditional correlations between per-worker GDP growth and change in log market potential, a version using the same controls as in Table 6, column (4), and one version where we additionally account for changes in the industry structure and the skill composition of the workforce. The instrumental variables used are the same as in Table 6, column (5).

The preferred results in columns (2) and (5) suggest that the increase we find in GDP is driven by an increase in worker productivity, rather than an expansion of the workforce, as the estimated elasticity is within the range of the models in Table 6. In comparing these results to the literature on the productivity effects of density it is important to acknowledge that unlike conventional density measures, our market potential takes into account the economic activity in surrounding regions, albeit with a lower weight. As a measure of effective density the market potential therefore introduces a spatial autocorrelation, which reduces variation in effective density across counties. It turns out that the standard deviation in the 1998 log market potential across counties in our data is almost three times the standard deviation in the 1998 log employment density. Our elasticity of productivity with respect to effective density is, therefore, not directly comparable to the majority of estimates in the agglomeration economics literature as a 1% increase in market potential, on average, implies a much larger percentage increase in density. Normalized by the log ratio of the standard deviations of effective density (market potential) over density (employment per area) our results imply a 3.8% elasticity of productivity with respect to employment density, which is close to previous estimates derived from cross-sectional research designs (e.g. Ciccone, 2002; Ciccone & Hall, 1996).

Once we control for changes in the industry sector and skill composition the productivity effect is substantially reduced and is no longer significantly different from zero (columns 3 and 6). One interpretation is that the increase in per-worker output is driven by a relative expansion of, on average, more productive and skill-intensive sectors, which benefit particularly from HSR. This may suggest that the economic adjustments are primarily due to selection instead of agglomeration effects (Combes et al., 2012). Another interpretation is that our controls for sector and skill composition are endogenous and we may be over-controlling, a *bad control* problem as discussed by Angrist and Pischke (2009).

Tab. 7 Productivity effects

	(1)	(2)	(3)	(4)	(5)	(6)
	ΔLn (GDP/Employment (workplace)) 1998-2002					
	OLS	OLS	OLS	2SLS	2SLS	2SLS
Δln Market potential (δ_1)	0.066	0.132**	0.009	0.170***	0.108*	0.042
	(0.059)	(0.058)	(0.048)	(0.055)	(0.062)	(0.059)
Industry shares	-	YES	YES	-	YES	YES
Degree share	-	YES	YES	-	YES	YES
Agglomeration effects	-	YES	YES	-	YES	YES
Δln GDP/Employment (workplace) 1992-1997	-	YES	YES	-	YES	YES
Composition effects	-	-	YES	-	-	YES
r2	0.007	0.148	0.495	-	0.147	0.487
N	115	115	115	115	115	115

Notes: Robust standard errors in parentheses are heteroscedasticity robust. Δln Market potential (δ_1) is based on Eq. (4-2) and the decay parameter (δ_1) from Table 5, column (1). Industry shares are shares at total 1998 GVA in the following sectors: Construction, manufacturing, mining, financial services, and other services. Degree share is the share for the workforce (at place of work) holding a university degree in 1999. Agglomeration effects include the 1998 market potential, population density, the 1997 log GDP as well as straight-line distances to Frankfurt and Cologne. Composition effects are 1998 to 2002 changes in industry shares and 1999 to 2002 degree share. Instrumental variables in columns (4–6) are three indicator variations, each denoting one of the counties in which the intermediate stops Limburg, Montabaur, and Siegburg are located. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5 External validity

Before we draw conclusions in the next section, some words are due on the external validity of our findings. As with most attempts to improve identification, our variant of the inconsequential units approach (Redding & Turner, 2015) provides local treatment effect estimates as we infer a causal effect from a large accessibility shock on small towns. If marginal agglomeration benefits were concave in city size, HSR effects could be smaller for the typically connected large cities. It is not possible for us to test for such a concave relationship since our inconsequential units approach is not suited for large metropolitan areas that are connected purposely (implying a potential for reverse causality). That said, the fact that the implied elasticity of productivity with respect to a density of 3.8% (previous subsection) is close to the average of comparable estimates in the literate (Melo et al., 2009) is at least suggestive that our results have some generalizability.

As with every case study, some factors are specific to our context and are worth considering before transferring conclusions to other contexts. As mentioned in section 2-A, the Cologne-Frankfurt HSR was built parallel to a highway (A3) and the presence of such a substitute may affect the effects of the HSR as much as the technology of the HSR itself. If one is willing to accept that our estimated parameters hold some external validity, our market potential approach can be used to compute counterfactual outcomes as follows:

$$\Delta\widehat{\ln(Q_i)} = \hat{\delta}_1 \left[\ln\left(\sum\nolimits_j Q_{j,t=1998} e^{-\hat{\delta}_2 E_{ij,t=post}} \right) - \ln\left(\sum\nolimits_j Q_{j,t=1998} e^{-\hat{\delta}_2 E_{ij,t=pre}} \right) \right] \tag{7-1}$$

, where $\hat{\delta}_1$ is our preferred estimate of the elasticity of output with respect to effective density and $\hat{\delta}_2$ is our preferred estimate of the spatial decay. All variables are defined as in equation (4-3). It is possible to compare predicted outcomes $\Delta\widehat{\ln(Q_i)}$ across different scenarios by solving equation (7-1) assuming different speeds on the rail and road network in the transport matrices $E_{ij,t=(\text{pre},post)}$ before (*pre*) and after (*post*) the HSR opening. In Table 8, we first increase the speed on the HSR (1-4, 2 is the actual scenario) before we eliminate the highway (5) and increase the speed of the highway (6). Without the highway (5), the effects on the three intermediate towns would have been even larger and roughly as large the effect of an HSR operating at a 40km/h (25%) higher average speed (assuming there are no complementarities between the HSR and the highway). In keeping with intuition, increasing the speed on the highway by 20km/h (6) has roughly the same effects as reducing the speed of the HSR by about 20km/h.

Tab. 8 Counterfactual scenarios

	(1)	(2)	(3)	(4)	(5)	(6)
HSR speed (km/h)	140	160	180	200	160	160
Highway speed (km/h)	100	100	100	100	0	120
Limburg GDP effect (%)	3.16	4.55	5.73	6.75	6.62	2.37
Montabaur GDP effect (%)	4.85	6.40	7.65	8.71	9.14	4.02
Siegburg GDP effect (%)	1.63	2.25	2.79	3.27	3.29	1.33
Total GDP effect (€, bn.)	5.16	7.90	10.46	12.88	10.99	4.53
Extra GDP effect in % of actual (2)	-34.74	0	32.41	63.03	39.01	-42.67

Notes: Table illustrates the relative and aggregate GDP effects of the HSR under different average speeds of the two modes (first two lines). Counterfactual outcomes are computed as $\Delta\widehat{\ln(Q_\iota)} = \hat{\delta}_1\widehat{\Delta D_\iota}$, where $\hat{\delta}_1$ is our preferred estimate of the elasticity of output with respect to effective density. In computing the transport matrixes used to compute $\widehat{\Delta D_\iota}$ we assume that individuals strictly choose the fastest route between any pairs of counties. They can switch between rail and car at any rail station.

Another feature of the analyzed institutional context are complementary investments into infrastructure. Minor upgrades were made to the Siegburger Bahn (new overhead system, accessibility at selected stations), a light rail connecting Bonn to Siegburg (one of the intermediate stops). Station terminal buildings were newly constructed or expanded in Limburg, Montabaur, and Siegburg, including some improvements to existing facilities, such as bus stops (Roggendorf & Schmidt, 1999). Much of the new or expanded business activity discussed in section 2-A is located in new business parks developed close to the new HSR stations. It is difficult to create a counterfactual describing the situation with the HSR, but without such complementary investments, because the anecdotal evidence overwhelmingly suggests that these measures would have had little impact without the HSR. We believe that the economic effects are best understood as originating from the combination of a substantial accessibility benefit and complementary measures that facilitate firms taking advantage of it. Our results, therefore, generalize to other contexts to the extent that similar complementary policies usually accompany the rollout of HSR.

A final word concerns the entrepreneur Ralph Dommermuth, who founded one of the world's largest web hosting companies (1&1) in his birthplace Montabaur. He is also an important investor involved in the development of the business park close to the Montabaur HSR station. Like the other stakeholders cited in section 2, he stresses the role of the HSR as a critical ingredient for economic development, in particular the access to a wider labor market pool the HSR offers (Ferdinand, 2015). Yet, it seems possible that personal attachments to his birthplace partially motivated his strong engagement as an investor, implying that without a similarly wealthy and engaged citizen the HSR effects in Montabaur could have been more moderate or, at least, delayed.

6 Conclusion

We analyze the economic effects of the Cologne-Frankfurt HSR in Germany, which connects the two major economic core regions in Germany and a number of peripheral regions along the way. Due to the institutional setting the HSR represents one of the rare occasions where transport improvements provide plausibly exogenous variation in access to surrounding economic mass. We find that the average GDP in the counties of the intermediate stops six years after the opening of the line exceeds a counterfactual trend by 8.5%. We make further use of the quasi-experimental variation provided by the HSR to contribute to a literature that has focused on estimating the strength and scope spatial scope of agglomeration effects. We find an elasticity of output with respect to effective density, i.e., market potential, of about 12.5% in our most conservative model. Our results further imply an elasticity of productivity with respect to density of 3.8%, which is well within the range of existing cross-sectional estimates. The strength of spillovers halves every 30 minutes of travel time and is near to zero after about 200 minutes. The spillovers we detect are significantly less localized than in previous studies that have identified similar spillover effects from within-city variation, but more localized than those found in the empirical NEG literature with an emphasis on trade cost. The benefits HSR has delivered to the peripheral regions operate through knowledge diffusion and labor market pooling and the effects of improved access to intermediated goods and consumer markets to the extent that the ease of communication reduces transaction costs, and thus, *Marshallian externalities* (Marshall, 1920). Our results complement recent evidence suggesting that improved transport linkages can benefit core regions at the expense of peripheral regions through a *trade channel* (Faber, 2014).

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Appendix: From periphery to core: Measuring agglomeration effects using high-speed rail

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1 Introduction

This technical appendix complements the main paper by providing material not reported in the main paper for brevity. The appendix partially replicates fractions of the text in the main paper to improve readability. It is not designed to stand alone or to replace the reading of the main paper.

2 Study area

Our study area comprises the German federal states Hesse, North Rhine-Westphalia, and Rhineland-Palatinate, out of a total of 16 federal states. As is evident from Figure A1 these three states are located in the west of Germany. In 1996, six years before the opening of the HSR, the total population of the study area was about 28 million, thus somewhat less than the size of California and about the size of Belgium and the Netherlands together. The share of the total German population was about 34%. The share of the German GDP was slightly higher at 36%.

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Fig. A1. Study area

Notes: Own illustration.

Table A1 compares characteristics within 115 counties within the study area to the characteristics of 287 counties in the other federal states. The average county in our study area in 1996 had a population of about 241k, which is significantly larger than the average county in the rest of the country (157k). In terms of output per worker, our study area is fairly similar to the rest of the country (€71.5k vs. €70.8). Also, the shares of various industries at the regional GVA are remarkably similar.

Tab. A1. County characteristics: Study area vs. rest of Germany

	Study area		Rest of Germany	
1996	Mean	S.E.	Mean	S.E
Population	240,594	14,552	157,219	7,427
GDP (1000 €)	5,787,372	549,684	3,533,259	267,128
GDP/Worker (€)	71,456	740	70,751	546
Share GVA: Construction	2.7%	0.1%	3.7%	0.1%
Share GVA: Mining	13.6%	0.5%	13.2%	0.3%
Share GVA: Other services	33.7%	0.5%	33.1%	0.3%
Share GVA: Retail	9.0%	0.2%	8.8%	0.1%
Share GVA: Manufacturing	16.3%	0.5%	16.9%	0.3%
Share GVA: Financial services	12.9%	0.3%	12.0%	0.1%
Share GVA: Public services	11.8%	0.3%	12.3%	0.2%

Notes: Study area includes 115 counties (NUTS3 regions) in the federal states North Rhine-Westphalia, Hesse, and Rhineland-Palatinate. Rest of Germany includes 287 counties.

3 Program evaluation

This section complements the program evaluation section in the main paper by providing additional detail on the synthetic counties, further robustness checks, and greater details on the falsification exercises.

A – Synthetic counties

The synthetic control algorithm generates the synthetic counties summarized in Table 1 in the main paper as weighted combinations of non-treated counties (the donor pool). The table below shows the donors (included in the synthetic counties) and their corresponding weights for each of the synthetic counties. One relevant insight is that none of the synthetic counties depend solely on one donor. Another important insight is that none of the donor counties with sizable weights are located near to the synthetic county it contributes to. It is therefore unlikely that the counterfactual is contaminated by spillover effects from the HSR onto counties entering the synthetic controls.

Tab. A2. Synthetic counties: Weights table

Limburg-Weilburg (Limburg)		Westerwaldkreis (Montabaur)		Rhein-Siegkreis (Siegburg)	
County	Weight	County	Weight	County	Weight
Ludwigshafen	0.468	Borken	0.245	Kusel	0.323
Heinsberg	0.445	Altenkirchen	0.239	Rheingau-Taunus-Kr.	0.184
Hochtaunuskreis	0.047	Suedwestpfalz	0.204	Main-Taunus-Kreis	0.154
Odenwaldkreis	0.034	Bitburg-Prüm	0.129	Ahrweiler	0.108
Herne	0.004	Trier-Saarburg	0.071	Südliche Weinstraße	0.094
Kaiserslautern	0.002	Neuwied	0.022	Mainz-Bingen	0.052
		Birkenfeld	0.019	Herne	0.05
		Olpe	0.019	Kaiserslautern	0.031
		Rhein-Hunsrück-Kreis	0.004	Rhein-Lahn-Kreis	0.001
		Herne	0.003		
		Vogelsbergkreis	0.002		
		Ahrweiler, Alzey-Worms, Bad Dürkheim, Bad Kreuznach, Cochem-Zell, Coesfeld, Darmstadt-Dieburg, Donnersbergkreis, Euskirchen, Fulda, Heinsberg, Hersfeld-Rotenburg, Höxter, Kaiserslautern, Kleve, Lahn-Dill-Kreis, Landau in der Pfalz, Lippe, Main-Kinzig-Kreis, Mainz-Bingen, Mayen-Koblenz, Neustadt an der Weinstraße, Paderborn, Recklinghausen, Rhein-Lahn-Kreis, Rheingau-Taunus-Kreis, Schwalm-Eder-Kreis, Soest, Steinfurt, Südliche Weinstraße, Unna, Viersen, Warendorf, Werra-Meißner-Kreis, Wesel, Wetteraukreis	all 0.001		

Notes: Table shows the donor counties and their weights in the synthetic counties summarized in Table 1 in the main paper.

B – Varying donor pools and predictor

To provide a valid counterfactual a control group must not be affected by a treatment. In the present context, the main concern is that there might be a spatial spillover effect from the HSR onto counties in the donor pool, in which case the treatment effect could be downward biased. Table A2 suggests that the counties from which the synthetic controls are generated are generally not near to the treated counties. As a further robustness check, we replicate the synthetic control analysis using donor pools (from which the algorithm can draw) that exclude counties located adjacent to an intermediate stop county or adjacent to any HSR county.

Another concern with the synthetic control method is that the counterfactual might be sensitive to the set of predictor variables, which the algorithm seeks to balance in constructing a synthetic control. Since the method balances not only the predictors but also trends (treated vs. synthetic control) prior to an analyzed intervention, using different sets of predictors should result in different weights matrices (different combinations of donor counties in a synthetic control county), but similar post-intervention trends as long as relative pre-trends (treated vs. other non-treated trends) are a good approximation for relative counterfactual trends. The latter assumption cannot be tested directly. However, it is informative to replicate a synthetic control analysis using a different set of predictor variables. If the assumption is plausible (pre-trends are a good approximation of counterfactual trends), the resulting treatment effect should not be too sensitive to the choice of predictors.

In the table below we replicate the analyses combining different donor pools (non-treated counties from which the algorithm can draw) with different sets of predictors (balancing covariates). All combinations yield positive and statistically significant estimates that are reasonably close to the baseline estimate (column 1), but there is some variation. Excluding counties adjacent to the intermediate stops has virtually no effect on the estimated treatment effects (2). This is the expected result because none of the adjacent counties carry a high weight in any of the synthetic controls (see Table A2). Excluding counties from the donor pool that are adjacent to any HSR county has a larger effect (3). However, the estimated treatment effect is not larger, as would be expected if there was a spillover problem in the baseline, but is somewhat smaller than in the baseline.

Using predictors that describe the economic activity within counties (GDP per capita, the ratio of out-commuters over in-commuters, and population density) yields a larger treatment effect than using predictors describing the composition of industries and workers (seven industry shares and the share of the population holding an academic degree) (4-5). Combining these subsets of predictors with the most restricted donor pool (3), however, brings the treatment effects again closer to the baseline.

It is reassuring to see that alterations of the baseline model do not lead to mutually reinforcing departures from the baseline estimate (in contrast, the effects of different alterations cancel each other out). Our main takeaway is that the treatment estimate is unlikely driven by specification issues of the synthetic control method and, instead, is a genuine feature of the data as already suggested by the unweighted difference-in-differences analyses.

Tab. A3. Varying synthetic control predictors and donor pools

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log GDP	Log GDP	Log GDP	Log GDP	Log GDP	Log GDP	Log GDP
T x (YEAR>2002)	0.049**	0.050**	0.036***	0.075***	0.040***	0.062***	0.045***
	(0.014)	(0.013)	(0.006)	(0.004)	(0.008)	(0.013)	(0.009)
County Effects	YES	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES	YES
Synth predictors	Economic activity & composition	Economic activity & composition	Economic activity & composition	Economic activity	Composition	Economic activity	Composition
Synth donor pool	All non-HSR station counties	Non-HSR station counties excluding counties adjacent to intermediate HSR stops	Non-HSR station counties excluding counties adjacent to any HSR stop	All non-HSR station counties	All non-HSR station counties	Non-HSR station counties excluding counties adjacent to any HSR stop	Non-HSR station counties excluding counties adjacent to any HSR stop
r2	0.999	0.999	0.999	1.000	0.999	0.999	0.999
N	108	108	108	108	108	108	108

Notes: Standard errors clustered on counties. Donor pool is the set of counties from which the synthetic control algorithm can draw. Predictors is the set of covariates that are balanced over the treated and the synthetic control. T is a 0/1 indicator variable indexing the treated counties. Economic activity is a vector of variables including GDP per capita, the ratio of out-commuters over in-commuters, and population density. Composition is a vector of variables including seven industry shares and the share of the population holding an academic degree. To ease comparison, Table 2, column (4) model is replicated in column (1). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C – Treatment effect on other outcomes

Table 4 in the main paper uses our most demanding program evaluation specification (Table 2, column 6 in the main paper) to estimate HSR treatment effects on various economic outcomes. In the table below, we replicate the analysis using the standard DD model from Table 2, column (1).

The results confirm the significantly positive per-worker GDP effect from Table 4 in the main paper. However, the percentage effect, at 2.5%, is smaller than in the more demanding synthetic control specification controlling for pre-trends (5.6% after the opening, 7.2% after six years). Moreover, there is a significantly positive workplace employment effect of 2.9%, suggesting that the HSR effect on GDP is partially driven by an expansion of the labor force and partially driven by an increase in productivity. The standard DD specification also reveals a relative increase in residence employment and population. The HSR effect on the number of commuters is positive and statistically significant.

The econometric specification used in Table 4 in the main paper is our preferred specification due to the presumed strength of the counterfactual (synthetic control counties) and its control for pre-trends. However, given the relatively small number of pre-treatment workplace employment and commuting observations, there is some risk of over-controlling. To reconcile the evidence, it is, therefore, informative to consult Figure 3 in the main paper, which plots the relative trends (treated – synthetic control) over the study period. From Figure 3 it seems reasonable to conclude that the positive population effect revealed by the simple DD specification (Table A4) is likely attributable to trend heterogeneity since there are positive relative trends over the entire study period (which flatten rather than increase after the completion of the HSR). In contrast, the relative pre-trends in in-commuting are relatively flat and there is a notable (positive) shift over the adjustment period for any of the considered intermediate stops. Thus, the simple DD results revealing a significantly positive effect on commuting (Table A4, column 5) seem credible. It is further reassuring that the more demanding specification (Table 4, column 5) yields a large and positive point estimate whose statistical insignificance is driven by large standard errors. The relative temporal trends in workplace employment are more ambiguous, which makes it difficult to distinguish between the origins of the GDP effect (increase in employment vs. productivity).

Tab. A4. Treatment effect on other economic outcomes: Standard DD

	(1) Log GDP/Workplace employment	(2) Log Workplace employment	(3) Log Residence employment	(4) Log population	(5) Log No of in-commuters	(6) Log No of out-commuters
T x (YEAR>2002)	0.024** (0.012)	0.029** (0.012)	0.043*** (0.012)	0.042*** (0.015)	0.059*** (0.014)	0.019 (0.019)
County Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.932	0.997	0.998	0.999	0.996	0.997
N	1,921	1,921	1,921	2,260	1,808	1,808

Notes: Table replicates Table 4 from the main paper using the standard DD model from Table 2, column (1) in the main paper. Number of observations vary due to different lengths of the time-series. Standard errors clustered on counties. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D – Breakdown by industries

The table below replicates the synthetic control baseline model (Table 2, column 4) using gross value added (GVA) by sectors as a dependent variable. For each model, we create synthetic counties using the same method as in the baseline model (using GDP). We find that the positive output effect that comes with the HSR is driven by financial services and other non-public services. This result confirms anecdotal evidence citing the positive effects on firms specializing in consulting (Emc²), telecommunications (1&1), software (Itac) or events (ADG). The result is also in line with theoretical expectations because the HSR reduces the cost of moving people, but not the cost of shipping goods.

Tab. A5. Treatment effect by industry sector

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log GDP	Ln GVA	Ln GVA	Ln GVA	Ln GVA	Ln GVA	Ln GVA	Ln GVA
Sector	All	All	Construction	Mining	Manufacturing	Financial services	Other non-public services	Public services
T x (YEAR>2002)	0.036** (0.013)	0.031* (0.014)	-0.027 (0.052)	0.012 (0.032)	0.010 (0.025)	0.067* (0.027)	0.025* (0.011)	0.015 (0.017)
County effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
r2	0.999	0.999	0.995	0.996	0.997	0.998	1.000	1.000
N	60	60	60	60	60	60	60	60

Notes: Standard errors in parentheses clustered on counties. T is a 0,1 indicator variable indexing the treated counties. Observation period is 1996 to 2005 in all models due to data limitations (changing sector classifications). To ease comparison, Table 2, column (4) model is replicated in column (1). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E – Commuting patterns

The anecdotal evidence reviewed in section 2-A in the main paper suggests that firms benefited from the HSR due to an improved access to labor markets. In this section, we use a confidential data set from the Institute for Employment Research to analyze how the improved accessibility provided by an HSR impacts on commuting flows between residence and workplace locations.[1] The data set contains the number of in-commuters and out-commuters commuting into and from the intermediate HSR stop municipalities (Limburg, Montabaur, Siegburg) along with the respective destinations and origins. The starting point of our analysis is a gravity model of commuting:

[1] We gratefully acknowledge Per Kropp from the Institute for Employment Research for facilitating the access to these data.

$$\ln C_{ijt} = \beth E_{ijt} + (\upsilon_i \times \chi_j) + \varrho_t + \epsilon_{ijt}$$

, where C_{ijt} is the number of commuters commuting from location *i* to location *j* at period *t*, E_{ijt} is the travel time between *i* and *j* at period *t*, $(\upsilon_i \times \chi_t)$ is an origin-destination-specific effect, $\varrho_t$is a period effect and ϵ_{ijt} is a random error term. We estimate this model in first differences:

$$\Delta \ln C_{ij} = \aleph + \beth \Delta E_{ij} + \Delta \epsilon_{ij}$$

, where $\Delta \ln C_{ij}$ is the ln change in the number of commuters from the before-HSR to the after-HSR period. We use the average number of commuters from 1992 to 1995 for the before-HSR period and the average number of commuters from 2010 to 2012 for the after-HSR period (the HSR opened in 2002). ΔE_{ij} is the change in bilateral travel times due to the HSR as in equation 4-3 in the main paper.

We note that this empirical model provides a strong control for unobserved heterogeneity. Recent cross-sectional applications of commuting gravity models (Ahlfeldt, Redding, Sturm, & Wolf, 2015; Ahlfeldt & Wendland, 2016) control for push and pull factors via origin and destination fixed effects $(\upsilon_i + \chi_j)$. Because our analysis focuses on the effects of a change in infrastructure, we can control for interactions of origin and destination fixed effects $(\upsilon_i \times \chi_j)$.

The results are in Table A6. Because the commuting matrix is not symmetric (three HSR stops vs. all locations in the study area) it is useful to use the number of in-commuters and out-commuters as dependent variables to test the internal validity. We also consider the two predominant functional forms of the spatial decay in commuting probabilities, namely the semi-log and the log-log form. We find that a one-minute reduction in travel time induced by the HSR increases the number of commuters on an origin-destination route by 5%. The respective elasticity is 3.7. These estimates are roughly within the range of existing commuting gravity estimates, although the estimated semi-elasticity is somewhat smaller and the estimated elasticity is somewhat larger compared to recent estimates for Germany (Ahlfeldt & Wendland, 2016). In the context of this study it is notable that our estimated decay parameter in the market potential (see Table 5 in the main paper) is closer to the commuting decay parameter reported in Table A6 than to recent estimates of decay parameters capturing the spillover effects between firms (Ahlfeldt et al., 2015) or decay parameters in NEG models capturing trade costs (Hanson, 2005).

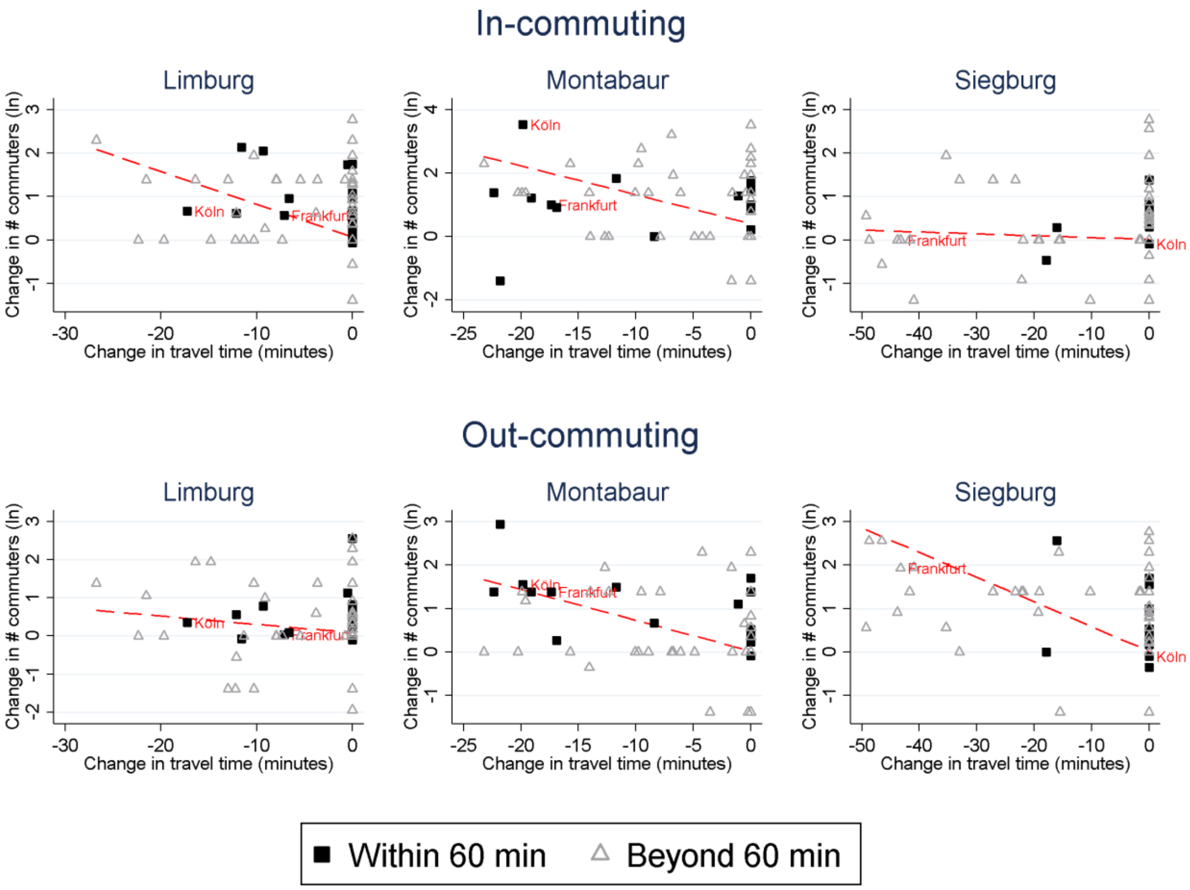
Tab. A6. Commuting effects

	(1) ΔLn # in-commuter	(2) ΔLn # in-commuter	(3) ΔLn # out-commuter	(4) ΔLn # out-commuter
Δ Travel time (minutes)	-0.048***		-0.048***	
	(0.012)		(0.011)	
Δ Ln travel time		-3.793***		-3.662***
		(0.934)		(0.884)
Observations	206	206	206	206
R^2	0.065	0.080	0.074	0.086

Notes: Robust standard errors in parentheses. In-commuter models use flows from all municipalities in the study area into intermediate HSR municipalities (Limburg, Montabaur, Siegburg). Out-commuter models use flows from intermediate HSR municipalities to all municipalities in the study area. First difference in # of commuters is the difference between the average number of commuters from 1992–1995 and the average number of commuters from 2010–2012. First difference in travel time is based on the same transport cost matrices as used in the baseline models. Observations weighted by the average number of commuters (across all period). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In Figure A2 and Table A7 we differentiate the HSR effects by HSR municipality. In line with the general trend in Table A6, the number of in-commuters from areas that are now in closer to reach due to HSR increased in Limburg and Montabaur. This is in line with the anecdotal evidence pointing to firms benefiting from access to wider labor markets. We do not find a similar effect for Siegburg. This does not necessarily imply that the HSR had no impact at all on commuting. Possibly, the HSR is used by few highly-skilled workers commuting from farther distances, which are critical for firm competitiveness and productivity, but hard to detect empirically. Still, it appears that the role of improved labor market access is less obvious for Siegburg than for the other towns, making it more likely that the improved attractiveness to firms materialized through other channels. Consistently across all three cities, we find that the HSR promotes out-commuting to areas that are now within closer reach. We conclude that HSR had a significant impact on commuting patterns in the study area and that, at least in Montabaur and Limburg, firms have been drawing employees from a wider labor market since the opening of the HSR.

Fig. A2. HSR effects on commuting from and to HSR municipalities



Notes: In-commuter models use flows from all municipalities in the study area into intermediate HSR municipalities. Out-commuter models use flows from intermediate HSR municipalities to all municipalities in the study area. First difference in # of commuters is the difference between the average number of commuters from 1992–1995 and the average number of commuters from 2010–2012. First difference in travel time is based on the transport cost matrices summarized in Figure 4 in the main paper. Observations weighted by the average number of commuters (across all period).

Tab. A7. Commuting effects by HSR city

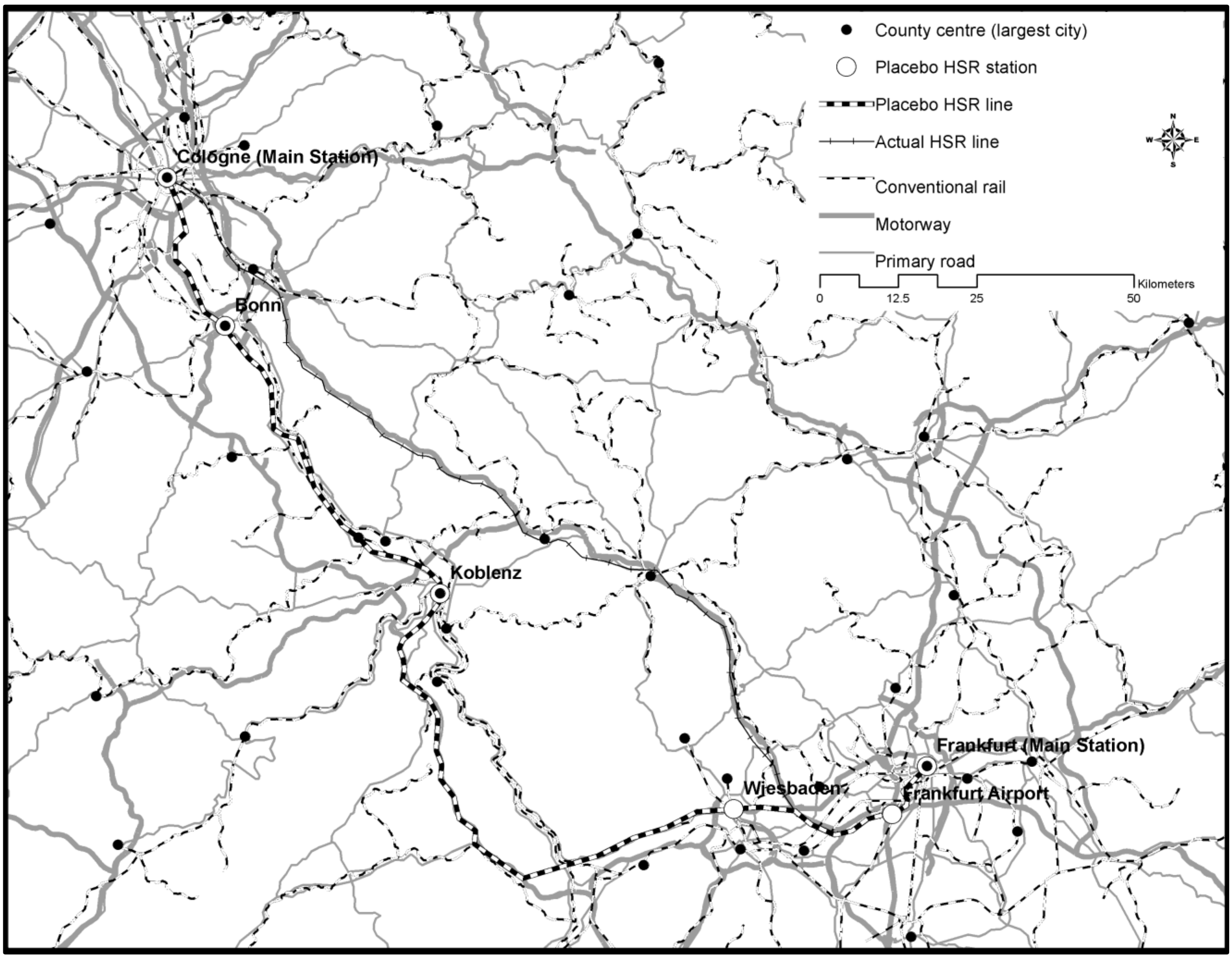
	(1) ΔLn # in-commuter	(2) ΔLn # in-commuter	(3) ΔLn # in-commuter	(4) ΔLn # out-commuter	(5) ΔLn # out-commuter	(6) ΔLn # out-commuter
ΔTravel time (min)	-0.076***	-0.089***	-0.005	-0.022	-0.069***	-0.055***
	(0.015)	(0.024)	(0.007)	(0.015)	(0.012)	(0.019)
City	Limburg	Montabaur	Siegburg	Limburg	Montabaur	Siegburg
N	81	55	70	81	55	70
r2	0.153	0.152	0.001	0.012	0.186	0.112

Notes: Robust standard errors in parentheses. First difference in # of commuters is the difference between the average number of commuters from 1992–1995 and the average number of commuters from 2010–2012. First difference in travel time is based on the same transport cost matrices as used in the baseline models. Observations weighted by the average number of commuters. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

F – Falsification I

Our first falsification exercise is a classic "placebo" study. We use exactly the same empirical design as in the baseline estimations to estimate the economic impact of an alternative HSR route that was considered during the planning stages but never built. The track would have had three intermediate stops in each of the involved federal states and would have passed through the economically and politically relevant cities of Bonn (the former federal capital located in North Rhine-Westphalia), Koblenz (the largest city in northern Rhineland-Palatinate) and Wiesbaden (the state capital of Hesse). The exact location of these intermediate stops is evident from Figure A3 below. A detailed discussion of alternative routes can be found in Kandler (2002).

Fig. A3. Placebo HSR



Notes: Own illustration. The route of the placebo HSR is based on Kandler (2002).

Table A8 summarizes the pre-treatment characteristics of the counties of the placebo intermediate HSR stops, synthetic control counties constructed for comparison to the placebo-treated counties using the same procedure as in the main paper, and all other non-treated counties in the study area. The placebo-treated counties are economically significantly more potent than the treated counties (in the main paper), which supports our notion that the HSR treatment at the intermediate stops was exogenous to their economic strength. Perhaps with the exception of Wiesbaden, the synthetic counties look very similar to their placebo counterparts.

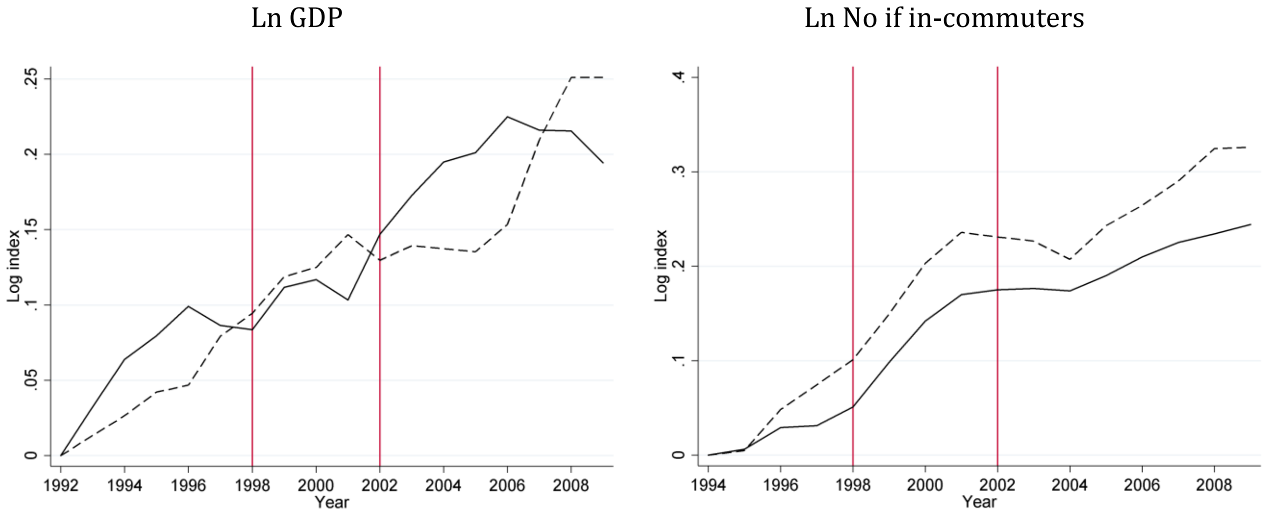
Tab. A8. Pre-treatment characteristics: Treated vs. synthetic controls

	Bonn		Koblenz		Wiesbaden		All non-treated counties	
Predictor variable	Treat	Synth	Treat	Synth	Treat	Synth	Mean	S.D.
GDP/worker (€)	80.3	79.4	74.1	76.7	78.3	70.0	69.0	7.9
Ratio out/in-commuting	1.92	2.06	4.32	2.83	3.21	1.05	1.03	0.93
Population / sqm land area	1.31	1.10	1.02	1.84	2.09	0.50	0.75	0.81
Industry share: Const.	1.8%	1.8%	1.6%	1.6%	1.1%	2.7%	2.8%	1.1%
Industry share: Mining	7.2%	7.2%	7.4%	7.4%	4.2%	5.1%	13.9%	5.0%
Industry share: Services	41.1%	41.0%	41.0%	41.0%	44.7%	42.3%	33.3%	4.7%
Industry share: Retail	7.4%	9.7%	10.7%	10.8%	8.1%	13.0%	8.8%	2.1%
Industry share: Manufact.	8.9%	8.9%	9.0%	9.0%	5.3%	7.7%	16.7%	4.7%
Industry share: Finance	20.5%	20.4%	12.1%	15.7%	14.0%	14.7%	12.7%	2.8%
Share higher education	11.8%	11.8%	7.4%	11.6%	15.4%	5.8%	6.6%	3.1%

Notes: The reported values are means across all years prior to 1998 (when construction began) except for the share of workers (at workplace) holding a university degree, which refers to 1999, the earliest year for which data was available.

Figure A4 corresponds to Figure 2 in the main paper. The left panel of Figure A4 compares the GDP trend of Koblenz to its synthetic counterpart. Koblenz as a potential stop on the placebo HSR line roughly corresponds to Montabaur in that it is located in Rhineland-Palatinate and is a comparable distance from Cologne and Frankfurt. Notably, Koblenz is located about 25km to the west of Montabaur, so that positive – or negative – spillovers from the Montabaur station could exist, in principle. The two trended lines follow a relatively similar long-run trend. There is no or at least no persistent divergence in the trends after the placebo HSR opened. Similarly, there is no evident break in the relative trends in the number of in-commuters (right panel).

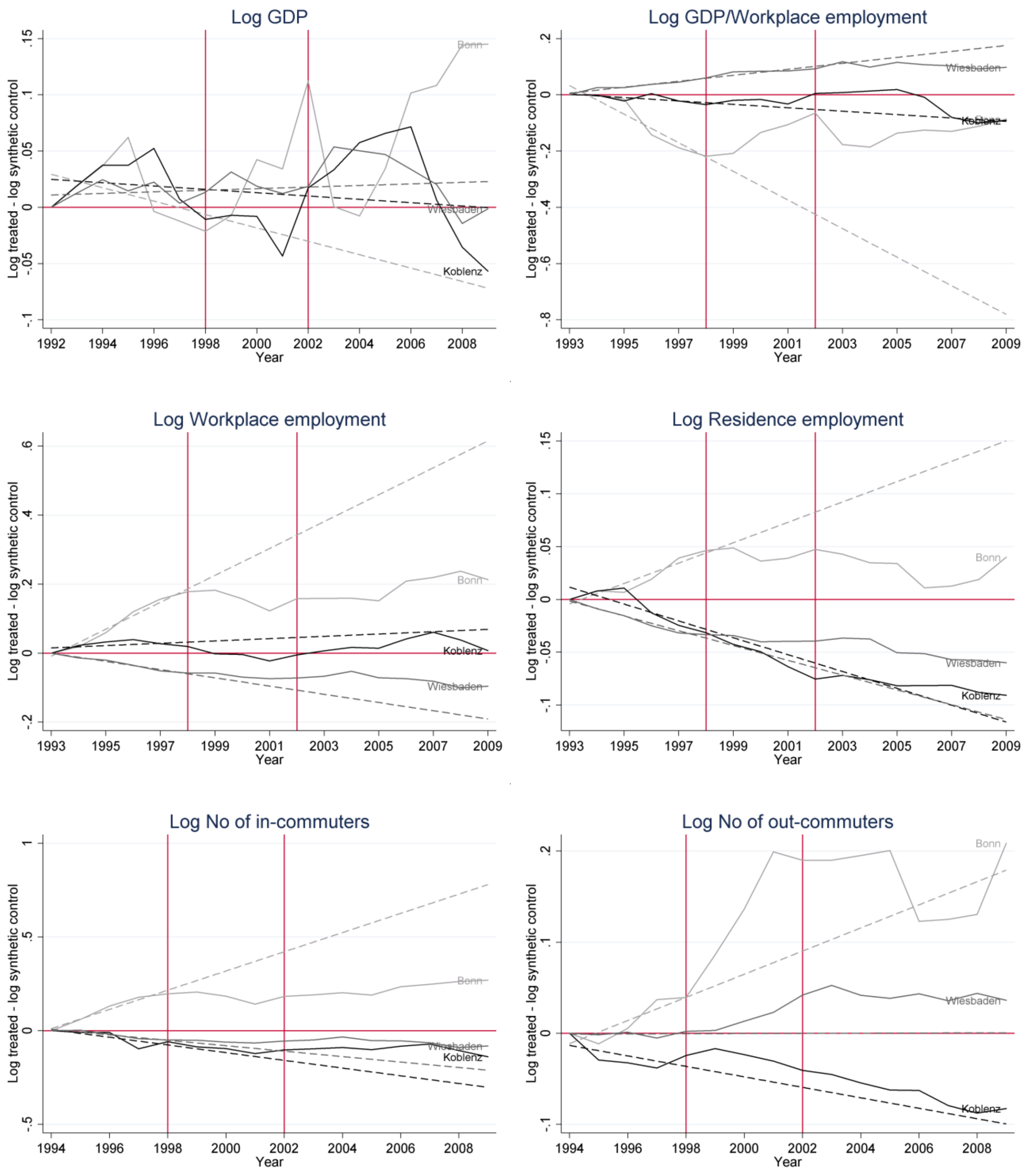
Fig. A4. Koblenz vs. synthetic control county



Notes: Solid (dashed) line shows the trend line for Koblenz (the synthetic control county). Vertical lines indicate the period of substantial construction activity. Years up to 1997 were used in the construction of the weights matrix underlying the synthetic county.

Figure A5 summarizes the comparisons between the placebo counties and the synthetic counterparts for different outcome variables (corresponding to Figure 3 in the main paper). There is, in general, little support for positive impacts on economic outcomes for either of the three placebo counties. The exception is perhaps the GDP and GDP/workplace trend in Bonn. It needs to be noted, though, that the Bonn mainline station is located within only 12km of Siegburg. There is even a light rail line connecting the main line station in Bonn to the Siegburg HSR station (it takes about 25min). Hence, there could be spillovers from Siegburg to Bonn.

Fig. A5. Relative trends for placebo-treated counties vs. synthetic control counties



Notes: Solid lines represent the differences between the trend lines for a placebo-treated county and the synthetic control county. Vertical lines indicate the period of substantial construction activity. Years up to 1997 were used in the construction of the weights matrices underlying the synthetic counties. Dashed lines are extrapolated linear fits using observations before 1998.

Tables A9–A11 replicate Tables 2–4 in the main paper for the three placebo HSR stations. The mean treatment effect on GDP across the three cities is near to and not statistically different from zero in all specifications. The separate treatment estimates by treated county produce significant estimates with mixed signs in the naïve DD specification, but no significant cumulative effects using synthetic counties as comparisons (although Wiesbaden is close to borderline significance after six years). We do not find any significant effect for the other outcome measures either, although there are large and positive point estimates for GDP/Workplace (but even larger standard errors).

Tab. A9. Treatment effect on GDP

	(1)	(2)	(3)	(4)	(5)	(6)
	Log GDP					
Control group	Non-treated counties			Synthetic counties		
T x (YEAR>2002)	-0.018	-0.036	-0.007	0.025	0.022	0.018
[θ]	(0.026)	(0.039)	(0.014)	(0.048)	(0.073)	(0.013)
T x (YEAR>2002) x			0.005			-0.001
(YEAR-2003) [ϑ^P]			(0.012)			(0.015)
Cumulated effect			0.007			0.015
after 3 years			(0.030)			(0.036)
Cumulated effect			0.022			0.012
after 6 years			(0.065)			(0.081)
County Effects	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
Constr. years x T	NO	YES	YES	NO	YES	YES
T x (YEAR-2003)	NO	NO	YES	NO	NO	YES
r2	0.997	0.997	0.997	0.997	0.997	0.997
N	1980	1980	1980	108	108	108

Notes: Standard errors in parentheses are clustered on counties. T is a 0,1 indicator variable indexing the placebo-treated counties. Cumulated effects computed as $\exp\left(\hat{\theta} + \hat{\vartheta}^P \times (t-2003)\right) - 1$. Cumulated standard errors computed as $\exp\left(var(\hat{\theta}) + (t-2003)^2 \times \text{var}(\hat{\vartheta}^P) + 2 \times (t-2003) \times \text{cov}(\hat{\theta}, \hat{\vartheta}^P)\right) - 1$. Constr, years x T indicates treatment T x year n interaction terms $\sum_{n=1998}^{2002} \theta_n [T_i \times (t=n)]$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tab. A10. Treatment effects on GDP by treated county

	(1)	(2)	(3)	(4)	(5)	(6)
	Log GDP					
	Bonn		Koblenz		Wiesbaden	
Control group	Non-treat.	Synthetic	Non-treat.	Synthetic	Non-treat.	Synthetic
TREAT x (YEAR>2002)	0.041***	0.039	-0.051***	0.017	-0.043***	-0.003
[θ]	(0.006)	(0.030)	(0.006)	(0.073)	(0.006)	(0.068)
TREAT x (YEAR>2002)		-0.013**		-0.022*		0.032***
x (YEAR-2003) [ϑ^{P}]		(0.004)		(0.010)		(0.008)
Cumulated effect		0.002		-0.049		0.097
after 3 years		(0.041)		(0.092)		(0.091)
Cumulated effect		-0.035		-0.110		0.207
after 6 years		(0.052)		(0.116)		(0.115)
County Effects	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
Constr. years x T	NO	YES	NO	YES	NO	YES
T x (YEAR-2003)	NO	YES	NO	YES	NO	YES
r2	0.997	1.000	0.997	1.000	0.997	1.000
N	1998	36	1998	36	1998	36

Notes: Standard errors in parentheses are robust in (2), (4) and (6) and clustered on counties in (1), (3), and (5). T is a 0,1 indicator variable indexing the placebo-treated counties. Cumulated effects computed as $\exp\left(\hat{\theta}+\hat{\vartheta}^{P}\times(t-2003)\right)-1$. Cumulated standard errors computed as $\exp\left(var(\hat{\theta})+(t-2003)^{2}\times\text{var}(\hat{\vartheta}^{P})+2\times(t-2003)\times\text{cov}(\hat{\theta},\hat{\vartheta}^{P})\right)-1$. Constr, years x T indicates treatment T x year n interaction terms $\sum_{n=1998}^{2002}\theta_n[T_i\times(t=n)]$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tab. A11. Treatment effect on other economic outcomes

	(1) Log GDP/worker	(2) Log Workplace employment	(3) Log Residence employment	(4) Log population	(5) Log No of in-commuters	(6) Log No of out-commuters
Control group	Synthetic counties					
T x (YEAR>2002)	0.126	-0.083	-0.008	0.011	-0.037	0.068
[θ]	(0.068)	(0.070)	(0.030)	(0.014)	(0.142)	(0.049)
T x (YEAR>2002) x	0.012	-0.009	-0.001	0.000	-0.005	-0.005
(YEAR-2003) [ϑ^{P}]	(0.022)	(0.008)	(0.005)	(0.003)	(0.018)	(0.007)
Cumulated effect	0.176	-0.103	-0.011	0.012	-0.050	0.056
after 3 years	(0.139)	(0.096)	(0.045)	(0.022)	(0.213)	(0.065)
Cumulated effect	0.218	-0.126	-0.014	0.013	-0.064	0.041
after 6 years	(0.215)	(0.120)	(0.060)	(0.031)	(0.279)	(0.082)
County Effects	YES	YES	YES	YES	YES	YES
Year Effects	YES	YES	YES	YES	YES	YES
Constr. years x T	YES	YES	YES	YES	YES	YES
T x (YEAR-2003)	YES	YES	YES	YES	YES	YES
r2	0.921	0.999	1.000	1.000	0.997	0.998
N	102	102	102	120	96	96

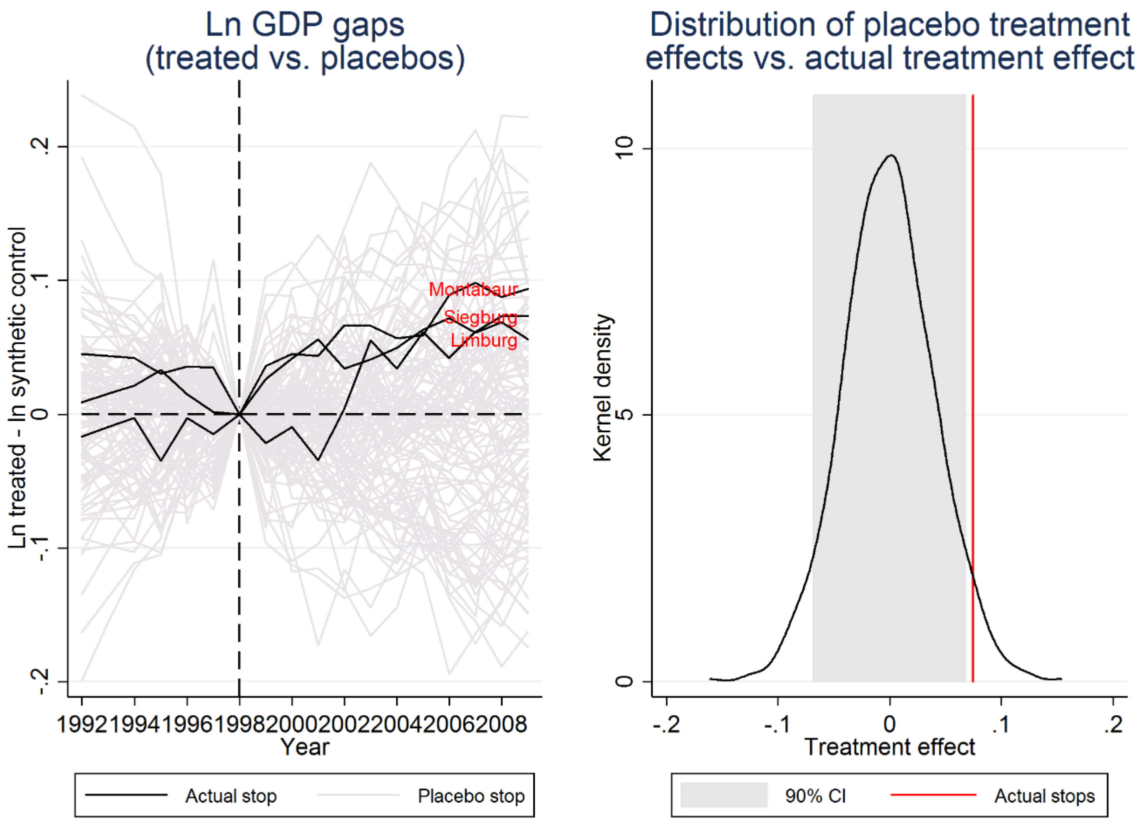
Notes: Standard errors in parentheses are clustered on counties. T is a 0,1 indicator variable indexing the treated counties. Cumulated effects computed as $\exp\left(\hat{\theta}+\hat{\vartheta}^{P}\times(t-2003)\right)-1$. Cumulated standard errors computed as $\exp\left(var(\theta)+(t-2003)^{2}\times\text{var}(\hat{\vartheta}^{P})+2\times(t-2003)\times\text{cov}(\hat{\theta},\hat{\vartheta}^{P})\right)-1$. Constr, years x T indicates treatment T x year n interaction terms $\sum_{n=1998}^{2002} \theta_n [T_i \times (t = n)]$. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

G – Falsification II

As a more general placebo test, Abadie, Diamond, and Hainmueller (2010) propose replicating the synthetic control analysis using subjects in the donor pool (potential controls) as treated subjects. The idea of the test is to evaluate the likelihood of obtaining a spurious result with the implemented method. In the left panel of Figure A6 we illustrate the result of a replication of the synthetic control analysis, assigning each of the non-treated counties (no HSR stop) as a treated county. We illustrate the ln GDP gap, which is the difference between the ln GDP of the (placebo) treated county and the corresponding synthetic control county in a given year, normalized by the 1998 difference. As expected, the trends in ln GDP gaps of the placebo counties (gray lines) appear to be random in the sense that they are centred on the zero line with some notable variation. Unlike for the placebo stops, the ln GDP gaps for the actual stops show a systematic pattern as the treatment effect is positive and roughly within the same range in each case.

We can now ask the question of how likely it is that a random combination of placebo stops will yield an average treatment effect that is at least as large as the one found for the three actually treated counties. To answer the question, we randomly draw 1,000 combinations of placebo-treated counties. For each combination, we compute the average placebo treatment effect as the difference in the mean ln GDP gap over the period 2003–2009 and 1992–1998 (to exclude the adjustment period). The distribution of the resulting placebo treatment effects is depicted in the right panel of the figure below. The analogically calculated treatment effect for the actual stops (vertical line) is in the right tail of the distribution of placebo treatment effects, outside the 90% confidence interval. We can thus conclude that it is unlikely that a random combination of counties in our study area will yield a similarly large treatment effect as we find for the actually treated counties.

Fig. A6. GDP gaps for actual stops vs. placebo stops



Notes: Ln GDP gaps in left panel are defined as the differences between ln GDP values of any county and their synthetic control county normalized by the 1998 value. Placebo stops are counties without an HSR stop (in the donor pool of the synthetic control counties of the actual stops). Treatment effects in the right panel are defined as the difference in mean ln GDP gaps across the periods 1992–1998 and 2003–2009 (which excludes the adjustment period). The black line gives the distribution of placebo treatment effects for 1000 random combinations of three placebo stops (in each case the mean across the three individual treatment effects). The vertical line indicates the same for the three actual stops (the counties of Limburg, Montabaur, and Siegburg).

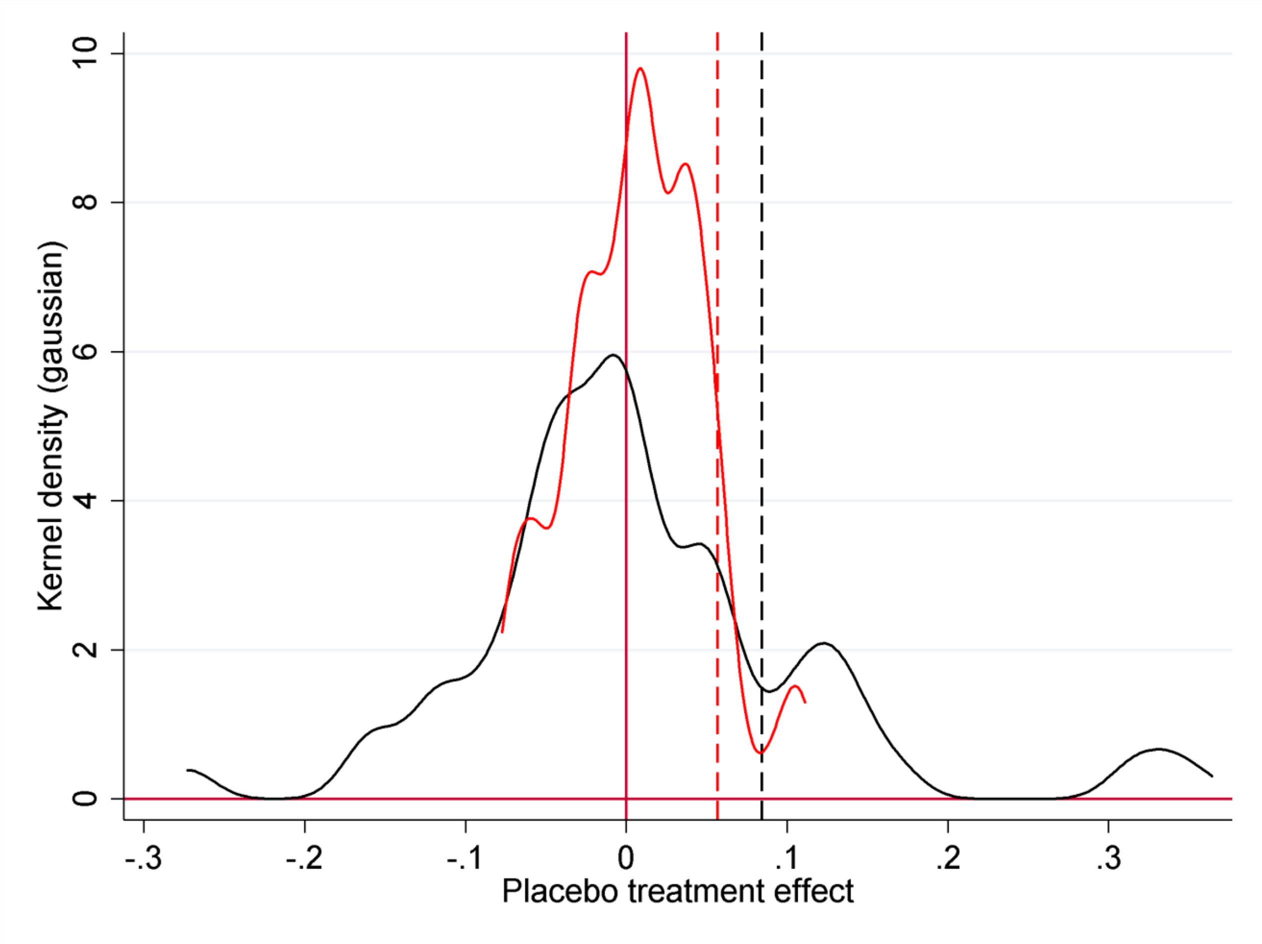
H – Falsification III

It can be argued that the test in the spirit of Abadie et al. (2010) conducted in the previous subsection is favorable to the actually treated counties because these are not randomly located in space. Instead, the treated counties are within a relatively close distance (compared to the average distance between counties in the study area), located approximately along a straight line. It is therefore more likely that unobserved spatially correlated characteristics have a similar impact on economic trends for the actually treated counties than for the randomly selected placebo-treated counties. To address this concern, we refine the selection process of placebo-treated counties to make the test more demanding. To make the treatment estimates directly comparable to the baseline, we replicate the baseline identification strategy.

As before, we run a series of 1,000 placebo tests, in each case drawing three counties (potential control counties) as placebo-treated counties. The difference compared to the previous test is that in each draw we generate a placebo HSR to which the placebo-treated counties have to be connected. In each iteration of the placebo test, we first randomly select one county as one endpoint of an artificial line (the artificial Cologne). Second, we randomly select another endpoint (the artificial Frankfurt) from all counties within a 140–180km range (in terms of straight-line distances) of the first endpoint (the distance between Cologne and Frankfurt is 160km). Third, we pick the three counties whose economic center (the largest city) is closest to a straight line connecting the two endpoints and define them as the treated counties (the artificial intermediate stops). Fourth, we create synthetic comparison counties for each of the placebo-treated counties according to our standard procedure. Fifth, we estimate the naïve DD model (Table 2, column 3 model, which uses all non-treated control counties and does not control for trends) as well as our preferred model (Table 2, column 6 model, which uses synthetic control counties and controls for trends) and save the point estimates and significance levels.

Figure A7 and Table A12 summarize the resulting treatment effects after six years. Of the 1,000 test 8.4% (24%) deliver significant treatment estimates using our preferred (naïve) DD model. 5.6% (8.2%) iterations resulted in treatment effects that were significant (at the 10% level) and at least as large as our benchmark estimates. The means of the point estimates is very close to zero. Notably, the standard deviation with 8.6% (5.4%) is relatively large compared to our 8.4% (5.7%) treatment estimate.

Fig. A7. Distribution of placebo treatment estimates on GDP



Notes: The black (red) solid line plots the distribution of the placebo treatment estimates after six years according to our preferred (naïve in Table 2, column 3) DD specification in Table 2, column 6). The vertical black (red) dashed line indicates the benchmark point estimate.

Tab. A12. Distribution of artificial treatment effects on GDP

Control Group	All non-treated counties			Synthetic counties		
	Coeff.	p-value	p-value<0.1	Coeff.	p-value	p-value<0.1
Obs	1,000	1,000	1,000	1,000	1,000	1,000
Mean	0.000	0.371	0.240	-0.007	0.501	0.084
Std. Dev.	0.047	0.306	0.428	0.096	0.277	0.278
Min	-0.117	0.000	0.000	-0.262	0.001	0.000
Max	0.150	0.996	1.000	0.512	0.999	1.000

Notes: Artificial treatment estimates after six years based on Table 2, column (3) and (6) specifications.

4 Agglomeration effects

This section complements Section 4 of the main paper by providing additional results not reported in the main paper for brevity.

A – Grid search over parameter space

As in any market potential equation, the elasticity and decay parameters are not necessarily separably identified. In fact, it is only the (ad-hoc) functional form of the spatial decay imposed in the market potential formulation (equation 4-2 in the main paper) that allows us to separately estimate the market potential elasticity (δ_1) and the decay parameter (δ_2). In general terms, a larger decay parameter δ_2 implies that more distant regions enter the market potential with a lower weight, reducing the degree of implicit spatial smoothing. The resulting larger variation in the market potential normally implies a lower estimate of the elasticity parameter δ_1.

As there could be multiple combinations of these critical parameters that fit the data, we run a grid-search over 500 possible values of δ_1 and δ_2(0.001 to 0.5) resulting in 250,000 parameter combinations for each of the models reported in Table 5 in the main paper. Our objective is to find the combination of parameters that minimizes the root mean squared error (RMSE) defined as:

$$RMSE = \sqrt{\frac{1}{N}\sum_{i}^{N}\left(\overline{\Delta\ln(Q_i)} - \overline{\widehat{\ln(Q_i)}}\right)^2}$$

, where $\Delta\ln(Q_i) = (Q_{i,t=2002}) - \ln(Q_{i,t=1998})$ is observed in the data, upper bars indicate that a variable is normalized to have a zero mean, and $\widehat{\ln(Q_i)}$ is computed as

$$\widehat{\ln(Q_i)} = \delta_1 \begin{bmatrix} \ln\left(\sum_j Q_{j,t=1998} e^{-\delta_2 E_{ij,t=2002}}\right) \\ -\ln\left(\sum_j Q_{j,t=1998} e^{-\delta_2 E_{ij,t=1998}}\right) \end{bmatrix}$$

The RMSE for all combinations of δ_1 and δ_2 by industry are illustrated in Figure 7 in the main paper. There are clearly defined global minima (white circles). This robustness check using an alternative approach to identify the elasticity and decay parameters increases our confidence in the point estimates of the decay parameters estimated with large standard errors reported in Table 5.

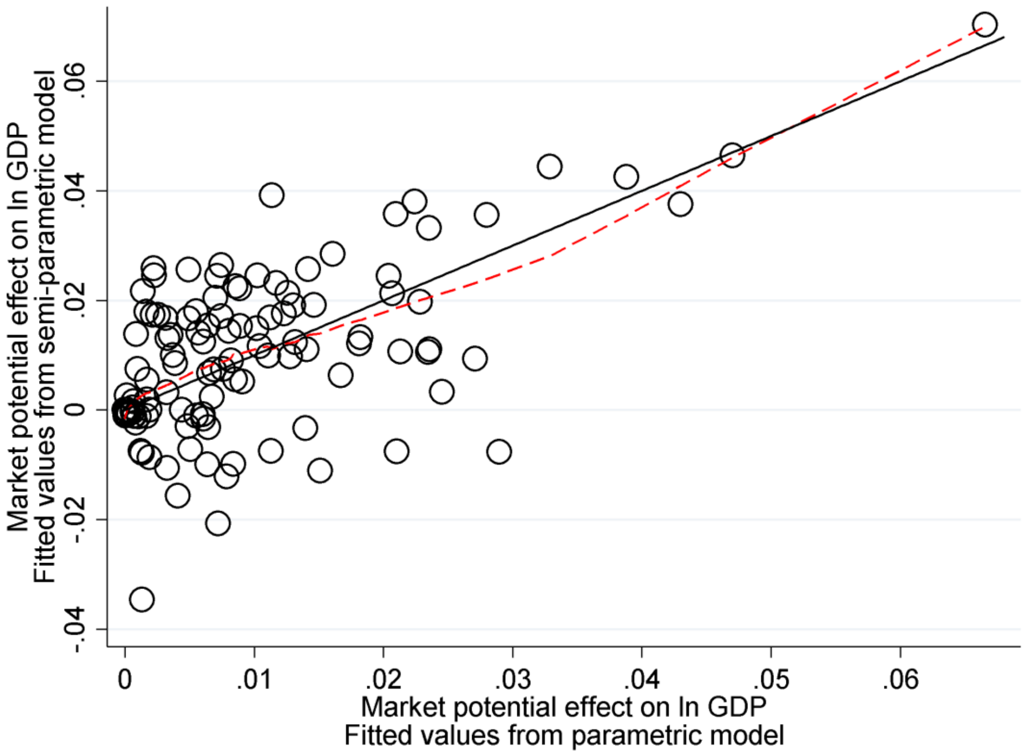
B – Alternative spatial decay in market potential

The exponential functional form of the spatial decay in spillovers, while popular in the theoretical and empirical literature (e.g. Ahlfeldt et al., 2015; Fujita & Ogawa, 1982; Lucas & Rossi-Hansberg, 2002), is ad-hoc and other functional forms are theoretically imaginable. To evaluate the sensitivity of the results to the chosen functional form of the spatial decay in the market potential we estimate an alternative to specification (4-3).

$$\ln(Q_{i,t=2002}) - \ln(Q_{i,t=1998}) = \delta_0 + \sum_m \delta_{1,m}(B_{m,i,2002} - B_{m,i,1998}) + \Delta\varepsilon_i$$

, where $B_{m,i,t}$ indicates the total GDP in all counties within a certain effective distance ring m in year t={1998,2002}. We group counties with mutually exclusive 20-minute travel time bins (e.g., 0–20min, 20–40min, …, 140–160min). For each travel time bin a separate market potential elasticity $\delta_{1,m}$ is estimated, thus allowing for a more flexible pattern in the spatial decay. In Figure A8 we compare the predicted effects of the change in market potential on GDP in this alternative model to our baseline model. We find an approximately linear relationship, suggesting that our results are not driven by an inappropriate functional form of the spatial decay.

Fig. A8. Parametric vs. semi-parametric fitted values



Notes: Dashed line is the lowess fitted line (bandwidth of 0.8). Solid line is the 45 degree line.

Another concern related to the spatial decay in the spillover effects is that for several sector-specific models in Table 5 the decay parameter is not statistically distinguishable from zero. It is important to note that an insignificant decay parameter estimate does not necessarily invalidate a significant market potential elasticity estimate. An insignificant decay parameter estimate implies that we cannot reject that spillovers decline in space. However, the alternative hypothesis is not that there are no spillover effects, but, instead, that spillover effects have an infinite range. Smaller decay parameters reduce the spatial variation in the market potential and, hence, the market potential elasticity. If anything, a smaller decay parameter along with positive and statistically significant market potential elasticities implies strong spillovers. Large, standard errors, however, imply larger uncertainty surrounding the true spatial decay. Since the market potential elasticity and the decay parameter are not independent, as discussed in section 4-D in the main paper, it is useful to evaluate how the market potential elasticity interacts with the decay parameter. For this purpose, we replicate the sectoral GVA models from Table 5 in the main paper imposing the decay parameter estimated from the GDP across all sectors (Table 5, column (1)), which is estimated relatively precisely. This is a substantial variation in the decay parameter as the imposed decay parameter is about two-thirds of the estimated decay parameter for mining and manufacturing and about twice as large as the estimated decay parameter of financial services and other services.

The table below reports the results. Except for other services, whose market potential elasticity is already not significant in Table 5, the estimates are within close range of the results in Table 5. This insensitivity of the market potential elasticity to variations in the decay parameter is reassuring because it suggests that a bias of the market potential elasticity estimate resulting from imperfect approximations of the decay parameter will likely be limited.

Tab. A13. Agglomeration effects by sectors: Fixed decay parameter

	(1) ΔLn GVA 1998-2002	(2) ΔLn GVA 1998-2002	(3) ΔLn GVA 1998-2002	(4) ΔLn GVA 1998-2002	(5) ΔLn GVA 1998-2002
Sector	Construction	Mining	Manufacturi ng	Financial Services	Other Services
Δln Market potential (δ_1)	0.354** (0.165)	0.353** (0.162)	0.362** (0.145)	0.300*** (0.087)	0.092 (0.059)
r2	0.035	0.025	0.033	0.046	0.016
N	115	115	115	115	115

Notes: Market potential is constructed using the decay parameter from column (1) in Table 5 in the appendix. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C – Alternative travel time matrices

To approximate the effective change in accessibility induced by HSR in our study area we have made a number of assumptions regarding the effective travel costs on different parts of the transport network and the way travelers are able to combine these segments. In this section, we experiment with different travel choice decision rules to evaluate the degree to which our results depend on the assumptions made. In our benchmark model, which for the purpose of comparison we replicate in Table A14, column (1), we allow for a great degree of choice in finding the optimal route through the combined road and rail network. Travelers are allowed to switch mode at HSR stations, i.e., they can use their cars to travel to an HSR station, then switch to the HSR train, and then switch back to the road networks (e.g., using a taxi or a rental car).

In column (2) we replicate the model with a more restrictive transport decision model. Travelers are assumed to choose the least-cost mode for a given origin-destination combination in any given period, but they are not allowed to switch mode during a journey. An HSR trip can only be combined with a conventional rail trip, but not the road network. As road trips are typically faster than conventional rail trips (because of higher speeds on highways), forcing HSR passengers onto the conventional rail network for the remaining parts of their journey results in a lower impact of HSR on travel times within our study area. Based on this more restrictive transport choice and the parameter estimates in column (2) the implied maximum impact on market potential (for Westerwaldkreis, the home county of the Montabaur stop) is 0.27 log points, which is less than the 0.34 effect in model (1). Not surprisingly, given the smaller variation in change in market potential we find a larger market potential elasticity than in the baseline model. For Montabaur, the estimated market potential elasticity and the change in market potential implies a predicted effect on GDP of 6%, which is virtually identical to the benchmark model.

In column (3) we ignore the road network altogether and force travelers on the rail network for all trips in any period. As this change slows travelers down on virtually all trips that do not benefit from HSR, the impact of HSR on travel times becomes significantly larger. The increase in market potential for Montabaur amounts to 0.65 log points, which is almost twice as much as in the baseline model. While the market potential elasticity is lower than in the baseline model as expected, the estimated parameters in column (3) along with the changes in bilateral travel cost imply a predicted GDP impact of 8%, which is only slightly higher than in the baseline model. In column (4) we extend model (3) by a market potential measure that only incorporates the road network (we restrict the decay parameter to be the same as in the change in rail market potential) to disentangle the accessibility benefits induced by HSR from trends correlated with road accessibility. The results remain almost unchanged.

Finally, we use a mix of the transport matrices used in (2) and (3) in model (5). Making the heroic assumption that 50% of travelers use either the road or the rail network in any period and on every route we compute the average of the road and rail travel times. While the maximum accessibility effect of 0.32 log points is almost identical to the benchmark estimate the market potential elasticity is notably larger, implying a larger (maximum) predicted effect on GDP.

A notable finding that emerges from the comparison of the estimates presented in Table A14 is that the decay parameter is reassuringly stable. The comparison across these models further reassures our decision to use model (1) as a benchmark model because it not only allows for relative flexible transport choices, but the model also delivers a combination of relatively high explanatory power and a conservative estimate of the market potential elasticity.

Tab. A14. Agglomeration effects by transport cost matrix

	(1)	(2)	(3)	(4)	(5)
	Δln GDP 1998-2002				
Δln Market potential (δ_1)	0.185***	0.225**	0.126***	0.129***	0.251***
	(0.051)	(0.069)	(0.031)	(0.030)	(0.063)
Decay (δ_2)	0.022*	0.023*	0.020**	0.021***	0.021**
	(0.011)	(0.013)	(0.008)	(0.007)	(0.008)
Ln market potential 1998 (automobile)				-0.016	
				(0.010)	
Combination of modes allowed	Auto, rail, rail & HSR, auto & HSR	Auto, rail, rail & HSR	Rail, rail & HSR	Rail, rail & HSR	50% rail or rail & HSR, 50% auto
Spillover elasticity (β)	0.028	0.034	0.019	0.019	0.038
N	115	115	115	115	115
r2	0.054	0.037	0.059	0.085	0.059

Notes: Estimation method is nonlinear least squares in all models. Robust standard errors (in parentheses) of the market potential coefficient δ_1 are heteroscedasticity robust and computed in separate OLS regressions holding the decay parameters (δ_2) constant at the levels estimated in the NLS models reported in the table. The market potential of region *i* is the transport cost weighted sum of output in all regions *j*. The change in market potential is driven by changes in travel cost between regions exclusively. Regional output is held constant at 1998 level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D – Alternative 2SLS models

At the heart of our identification strategy we argue that the timing of the construction and opening of the HSR is exogenous to the economic trends during the analyzed adjustment period as planning had been initiated several decades prior to then. We also argue that the timing and the routing is plausibly exogenous for the intermediate stops as the purpose of the line was to connect the endpoints and the intermediate stops resulted from political bargaining rather than economic reasoning. As such, the arguably most credible identification stems from counties that lie in-between Cologne and Frankfurt and benefit from improved access through the intermediate stops at Limburg, Montabaur, and Siegburg.

In Table A15 we therefore restrict the information used to identify the market potential effect to this most plausibly exogenous fraction using a 2SLS estimation strategy. We consider three different sets of instrumental variables: the log of the straight-line distance to Montabaur station, which is the location that experienced the largest accessibility gain (see Figure 1 in the main paper), three indicator dummy variables for the counties of the three intermediate stops (used in column 5 of Table 6 in the main paper), and the same plus six additional indicator variables for the adjacent counties. We use for each of these instrumental variable strategies, excluding (1–3) and including (4–6), the covariates used in Table 6 in the main text. We find that any of the 2SLS models produces results that are similar to the baseline model (Table 5, column 1) when we exclude the covariates. Including the covariates we get results that are within the range of the most conservative model reported in the main paper (Tab. 6, column 5), which we also add to Table A15 (column 5) for comparison. Only in the 2SLS model using the log distance to Montabaur as an IV and controlling for all county characteristics do we not find a significant market potential effect due to large standard errors.

Tab. A15. Market potential effects: alternative 2SLS models

	(1)	(2)	(3)	(4)	(5)	(6)
	Δln GDP all sectors 1998-2002					
Δln Market potential (δ_1)	0.174*** (0.055)	0.213*** (0.023)	0.188*** (0.044)	0.108 (0.078)	0.125** (0.054)	0.118** (0.056)
Instrumental Variables	Log straight-line distance to Montabaur	Dummies for intermediate stops	Dummies for intermediate stops and adjacent counties	Log straight-line distance to Montabaur	Dummies for intermediate stops	Dummies for intermediate stops and adjacent counties
Industry shares	-	-	-	YES	YES	YES
Degree share	-	-	-	YES	YES	YES
Agglomeration effects	-	-	-	YES	YES	YES
Δln GDP all sectors 1992-1997	-	-	-	YES	YES	YES
r2	0.054	0.053	0.054	0.217	0.235	0.234
N	115	115	115	115	115	115

Notes: Robust standard errors in parentheses are heteroscedasticity robust. Δln Market potential (δ_1) is based on Eq. (4-2) and the decay parameter (δ_1) from Table 5, column (1). Industry shares are shares at total 1998 GVA in the following sectors: Construction, manufacturing, mining, financial services, and other services. Degree share is the share for the workforce (at place of work) holding a university degree in 1998. Agglomeration effects include the 1998 market potential, population density, the 1997 log GDP as well as straight-line distances to Frankfurt and Cologne. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

E – Falsification

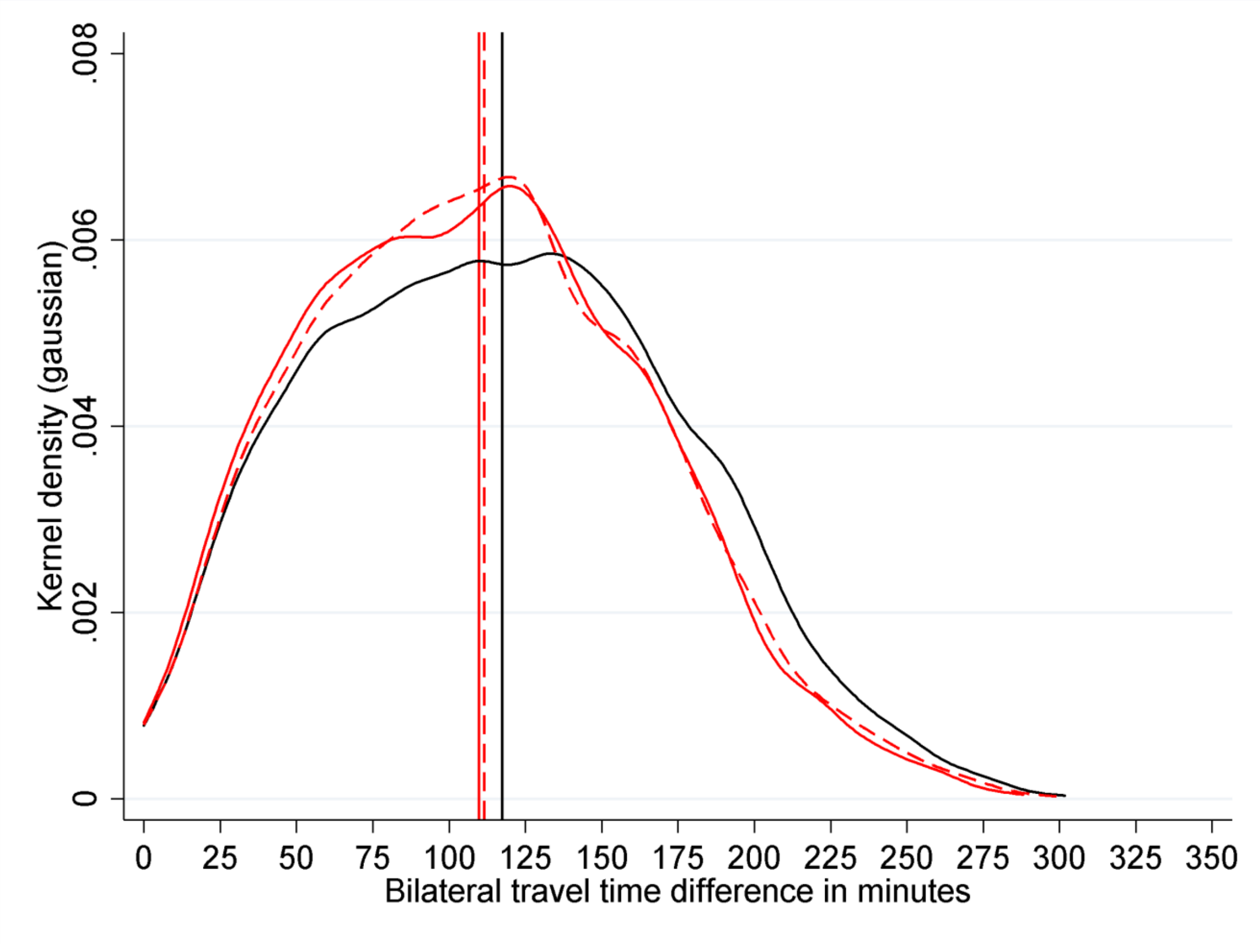
As a further test of whether our estimated agglomeration effects we attribute to the HSR are spurious, we conduct a placebo study using the placebo HSR track that was considered in the planning stages, but never built. We make use of the same placebo HSR as in the first falsification exercise in the program evaluation section discussed in Section 3 of the main text and Section 3 of this appendix. The HSR would have had three intermediate stops in each of the involved federal states and would have passed through the economically and politically relevant cities of Bonn (the former federal capital located in North Rhine-Westphalia), Koblenz (the largest city in northern

Rhineland-Palatinate), and Wiesbaden (the state capital of Hesse). Unlike in the program evaluation section it is not sufficient here to describe the placebo track by the locations of the tree intermediate stops. To replicate the analysis of agglomeration effects we require a full matrix of the bilateral travel times that incorporates the alternative HSR routing.

As the exact routing of this line was never finalized we approximate a plausible route based on the information released during the planning stages and the decision rules employed in the design of the actual line. The alternative line was intended to largely follow the river Rhine, but it was clear that for a section south of Koblenz the terrain was too mountainous to allow for very high speeds (Kandler, 2002). We therefore construct the placebo track parallel to the Rhine north of Koblenz and parallel to the highways A61, A66, and A3 south of Koblenz. As discussed in the main text, a similar approach was followed with the actual HSR, which in large part runs parallel to the A3. The routing of the placebo HSR is illustrated in Figure A2. In constructing bilateral travel times we employ exactly the same travel cost parameters and decisions rules as in the baseline transport decision model discussed in section 4-B in the main text.

In Figure A9 we compare the resulting distribution of bilateral travel times (solid red line) to the distribution of travel times on the road network (pre-HSR, black solid line) and the distribution of combined network times including the actual HSR (red dashed line). Not surprisingly, given the fact that the placebo HSR connects the same endpoints at the same speed (albeit along a slightly longer route), the resulting travel time distribution resembles the one of the actual HSR scenario quite closely.

Fig. A9. Distributions of bilateral travel times: Placebo HSR



Notes: Black (red) solid line shows the distribution of bilateral travel times on roads (the fastest combination of car and rail including placebo HSR). Red dashed lines show the fastest combination of car and rail including the actual HSR line. Vertical lines denote the respective means of the distributions.

In Table A16 we replicate a number of agglomeration models using the travel time matrix computed for the placebo HSR scenario instead of the actual HSR scenario travel times. Column (1) reports the baseline NLS model, which corresponds to Table 5, column (1) in the main text. While we find a borderline significant market potential elasticity effect, the decay parameter is estimated very imprecisely. Yet, the positive coefficients together indicate some accessibility benefits, which runs counter to the idea of the falsification exercise. It is important to note, however, that because of the placebo stops Bonn and Koblenz are relatively close to Siegburg and Montabaur, the placebo effect may be capturing an effect of the actual line, discounted by the time distance between the actual and the placebo stops. The 2SLS estimator using indicator variables for the placebo intermediate stops as instrumental variables mitigates this problem as it identifies the market potential effect only from the placebo intermediate stops exclusively. As before, we fix the decay parameter at the value estimated in (1) in the remainder of the Table A16 to ensure that the estimates of the market potential elasticity are comparable. Once we restrict the identifying variation to the intermediate placebo stops in (2), the market potential effect is essentially zero. The market potential effect also becomes insignificant once we add controls for correlated trends (3–6). With the largest set of controls the effect is close to zero (6), and even negative (and insignificant) if we combine all controls with a 2SLS estimation strategy (7). All in all, it seems fair to conclude that the placebo test is passed comfortably and that the effects reported in the main text are primarily driven by the intermediate stops on the actual line which, as we have argued, provide plausibly exogenous quasi-experimental variation.

Tab. A16. Agglomeration effects with placebo HSR track

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Δln GDP 1998-2002						
	NLS	2SLS	OLS	OLS	OLS	OLS	2SLS
Δln Market potential (δ_1)	0.136*	0.004	0.119	0.114	0.066	0.042	-0.030
	(0.074)	(0.067)	(0.076)	(0.076)	(0.081)	(0.083)	(0.132)
Decay (δ_2)	0.012						
	(0.022)						
Industry shares	-	-	YES	YES	YES	YES	YES
Degree share	-	-	-	YES	YES	YES	YES
Agglomeration effects	-	-	-	-	YES	YES	YES
Δln GDP all sectors 1992-1997	-	-	-	-	-	YES	YES
N	115	115	115	115	115	115	115
r2	0.018	0.001	0.103	0.123	0.204	0.221	0.218

Notes: Robust standard errors in parentheses are heteroscedasticity robust. δ_2 is fixed to the estimated value in (1) in models (2-7) to maintain comparability of the market potential elasticity (δ_1) Estimate. Industry shares are shares at total 1998 GVA in the following sectors: Construction, manufacturing, mining, financial services, and other services. Degree share is the share for the workforce (at place of work) holding a university degree in 1998. Agglomeration effects include the 1998 market potential, population density, the 1997 log GDP as well as straight-line distances to Frankfurt and Cologne. Instrumental variables in column (2) and (7) are three indicator variations, each denoting one of the counties in which the placebo intermediate stops in Bonn, Koblenz, and Wiesbaden are located. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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