

# Transport Investment and Economic Growth Elasticities

## Technical Annex II: UK-Specific Estimate

Department for Transport

30 March 2026



**FINAL REPORT**

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## 1. INTRODUCTION

The Department for Transport (DfT) has commissioned CEPA to provide a robust, policy-relevant assessment of the macroeconomic impacts of transport capital investment. The overall study comprises two separate strands of analysis:

- **Meta-analysis:** a meta-analysis of elasticity estimates, pooling evidence across studies to generate generalisable benchmarks and identify systematic patterns.
- **UK regional regression:** a UK-specific econometric analysis of regional data, providing a bespoke causal estimate tailored to current policy needs and filling an identified gap in the literature.

This **technical annex** sets out the details of the UK regional regression, providing the methodological detail and academic framing needed to assure readers of the robustness of our approach. The headline findings of this study, along with the findings from the meta-analysis workstream, are synthesised in the main report, provided separately.

### 1.1. PURPOSE OF THIS STUDY

Transport infrastructure investment is often justified by claims of contributions to economic growth. However, there is wide cross-country variation in published elasticities, highlighting limits to **external validity**: effects identified elsewhere may not apply in the UK. To inform investment decisions, DfT requires an elasticity that better reflects the UK context. Yet, our literature review for the meta-analysis workstream found no UK studies passing our quality screen,<sup>1</sup> underscoring the need for a robust, up-to-date UK-focused causal estimate of the elasticity of economic output with respect to transport investment.

This workstream develops a regionally disaggregated econometric analysis of the short-term impact of transport capital investment on UK economic growth. We assemble a subnational panel at the ITL1 level linking real transport investment to gross value added (GVA). To address endogeneity in investment, we employ an instrumental-variable (IV) approach using a Bartik (shift-share) instrument, which exploits differential local exposure to nationally driven funding shocks and path dependence from historical transport investment. We then disaggregate by mode (road/rail) and assess potential spillovers to neighbouring regions.

The analysis produces a UK-specific causal elasticity of approximately 0.07, implying a 10% increase in transport investment per capita is associated with an average 0.7% contemporaneous rise in regional GVA per capita. This estimate is best interpreted as a short-run effect driven primarily by the demand-side stimulus.

Although the estimated elasticity remains positive when London is excluded, the specification becomes weakly identified and substantially less precise. This indicates that London provides important identifying variation and anchors the preferred model. We therefore have much less confidence that the exact same relationship can be inferred to apply outside London.

There is evidence that rail investment has a positive and statistically significant effect on regional GVA, with an estimated elasticity of 0.11. Weaker evidence exists for the effect of other transport investment, where instruments are very weak and effects are not well identified. The results provide the first empirically grounded, reproducible UK estimate of the output elasticity of transport investment.

The outputs are policy-facing and transparent. We report headline and mode-specific elasticities, quantify spillovers, and present extensive diagnostics (first-stage strength and over-identification tests such as Sargan) alongside robustness checks (excluding outlier regions; estimating in levels versus per capita to explore scale

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<sup>1</sup> Using the What Works Centre's modified Maryland Scientific Methods Scale (SMS), we consider a high-quality study to be SMS  $\geq 3$ , e.g., well-developed panel data methods, difference-in-difference (DiD) designs, IVs, and randomised control trials (RCTs). For more details on the scale please visit <https://whatworksgrowth.org/resource-library/the-maryland-scientific-methods-scale-sms/>.

effects). All estimates are reproducible from a scripted pipeline. Together, these results provide DfT with transparent, UK-calibrated parameters to inform policy decisions and to cross-check against the international evidence synthesised in the meta-analysis.

## **1.2. STRUCTURE**

The remainder of this annex is structured as follows. Section 2 situates our study within the academic literature. Section 3 sets out our methodology, covering our identification strategy, instrument construction and validity, and the estimation approach. Section 4 describes the dataset and presents key descriptive statistics. Section 5 reports our main results: per-capita elasticities, mode-specific elasticities, and results from the spatial models controlling for spillover effects. Additional results are reported in Appendix A. Section 6 interprets the findings in the context of prior UK and international evidence and our meta-analysis. Section 7 concludes.

## 2. LITERATURE REVIEW

### 2.1. ECONOMIC IMPACTS OF TRANSPORT INVESTMENT

A substantial literature links transport investment to productivity and growth via lower travel costs and improved market access (Laird and Johnson, 2021).<sup>2</sup> In neoclassical models (e.g., the Solow-Swan framework), a transport upgrade induces a level shift in productivity and output; growth then reverts to its long-run rate as capital and labour adjust. By contrast, endogenous growth and new economic geography emphasise that lower transport costs can strengthen regional agglomeration and learning effects, through denser labour markets, input-output linkages, and knowledge spillovers, potentially altering the long-run growth path. Transport shocks can re-centralise activity in some places and disperse it in others, generating reallocation and possible displacement alongside aggregate gains (Krugman, 1991).<sup>3</sup> These frameworks provide the conceptual basis for estimating elasticities while recognising that impacts are place, mode, and horizon dependent.

Much of the empirical literature estimates Cobb-Douglas production functions using aggregate country- or state-level data, yielding elasticities that describe the responsiveness of output to changes in the transport capital stock. However, our findings from the meta-analysis indicate that most studies lack credible causal identification: many rely on time-series or cross-sectional designs with limited controls and no strategy to address endogeneity, making causal interpretation difficult. Notably, no recent UK-based studies passed our quality screen. These gaps motivate the use of IV and quasi-experimental approaches, such as the Bartik style shift-share design adopted here, to isolate exogenous variation in investment and recover causal elasticities.

Recent causal studies generally find that improved transport access raises economic activity, although magnitudes vary by setting, mode, and timeframe. Quasi-experimental evidence from major rail expansions shows positive effects on regional income, trade, and structural change (e.g., Donaldson, 2018).<sup>4</sup> For roads, micro-evidence highlights strong reallocation effects, with improved accessibility inducing the entry and expansion of more transport-intensive sectors, shaping realised productivity gains (e.g., Gibbons et al., 2019).<sup>5</sup> Meta-analyses (e.g., Melo et al., 2013),<sup>6</sup> including our recent study, likewise indicate positive but moderate output elasticities, and highlight that estimates are sensitive to identification quality, region, and to the transport mode considered.

Most macro-infrastructure studies estimate elasticities using public capital stocks constructed through perpetual-inventory methods.<sup>7</sup> Methodological review (e.g., Romp and de Haan, 2005)<sup>8</sup> highlight conceptual and measurement challenges associated with stock series – particularly for sectors such as transport with long-lived assets. In contrast, more recent causal designs typically exploit investment flows (e.g., Ramey, 2011; Nakamura and Steinsson, 2014),<sup>9</sup> which respond directly to exogenous spending shocks. Focusing on US federal highway grants,

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<sup>2</sup> Laird, J. and Johnson, D. (2021). “The GDP Effects of Transport Investments: The Macroeconomic Approach.” In: VICKERMAN, R. (Eds) et al. *Encyclopedia of Transportation*. Elsevier.

<sup>3</sup> Krugman, P. (1991). “Geography and Trade.” MIT Press, Cambridge, MA.

<sup>4</sup> Donaldson, D. (2018). “Railroads of the Raj: Estimating the Impact of Transportation Infrastructure.” *American Economic Review*, 108 (4-5): 899-934.

<sup>5</sup> Gibbons, S., Lyytikäinen, T., Overman, H., and Sanchis-Guarner, R. (2019). “New road infrastructure: The effects on firms.” *Journal of Urban Economics*, 110: 35-50.

<sup>6</sup> Melo, P., Graham, D., and Brage-Ardao, R. (2013). “The productivity of transport infrastructure investment: A meta-analysis of empirical evidence.” *Regional Science and Urban Economics*, 43 (5): 695-706.

<sup>7</sup> See for example Aschauer, D. (1989) “Is Public Expenditure Productive?” *Journal of Monetary Economics* or Munnell, A. (1992) “Infrastructure Investment and Economic Growth” *Journal of Economic Perspectives*

<sup>8</sup> Romp, W. and de Haan, J. (2005) “Public Capital and Economic Growth: A Critical Survey.” *EIB Papers*

<sup>9</sup> Ramey V. (2011) “Identifying Government Spending Shocks: It’s All in the Timing”. *Quarterly Journal of Economics* and Nakamura, E. and Steinsson, J. (2014) “Fiscal Stimulus in a Monetary Union” *American Economic Review*

LeDuc and Wilson (2013)<sup>10</sup> show that an unexpected 1% increase in the expected present discounted value of current and future highway funding raises state GDP by around 1–1.5% on impact, with a larger medium-run response of roughly 2–3% emerging six to eight years after the shock. We build on this strand of literature.

## 2.2. CAUSAL IDENTIFICATION IN TRANSPORT SETTINGS

A common challenge in estimating the economic effects of transport investment is endogeneity. Transport provision is not randomly allocated: investment may respond to anticipated economic growth, political priorities, or unobserved regional characteristics. When such confounders cannot be fully observed or controlled for directly. Standard regression approaches risk bias due to selection on unobservables. An established solution is to use instrumental variables (IVs). If a valid instrument exists, one that affects economic outcomes only through its effect on transport investment, it can be used to isolate the exogenous component of investment and identify its causal impact.

In transport economics, a prominent strand of literature exploits historical transport networks as sources of exogenous variation. A canonical example is Duranton and Turner (2012),<sup>11</sup> who estimate the causal effect of road capacity on traffic in U.S. cities using a two-stage least squares design. Their instruments include planned interstate highway kilometres from 1947 and railroad route kilometres from 1898. These predetermined measures predict modern road provision through path dependence in network development, satisfying relevance. Exogeneity is argued on the basis that, conditional on geography and historical population, nineteenth-century rail alignments or mid-twentieth century highway plans are unlikely to affect contemporary vehicle traffic except via their influence on later road supply. This approach, using historical network exposure to instrument for contemporary transport infrastructure, has been highly influential and is widely regarded as setting the benchmark for causal identification in transport settings.<sup>12</sup>

Our approach is closely related in spirit but differs methodologically. Rather than using historical network measures directly as instruments, we employ a Bartik (shift-share) design (Bartik, 1991; Blanchard and Katz, 1992),<sup>13</sup> which interacts predetermined historical transport investment with aggregate shocks to transport budgets. In our setting, historical rail and road density measures the exposure, while changes in national transport investment act as the shocks. Identification comes from differential regional responses to common national investment fluctuations, driven by path dependence in where infrastructure spending is most likely to occur. This structure allows us to exploit both cross-sectional variation in historical exposure and time variation in national transport budgets, while controlling for region and year fixed effects.

Bartik-style designs are widely used in regional and public economics, most notably in the fiscal multiplier literature. Nakamura and Steinsson (2014), for example, instrument regional government spending using pre-period military spending shares interacted with aggregate defence expenditure, an identification strategy that closely parallels our own. Recent methodological work formalises such designs as valid shift-share instruments provided the exposure

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<sup>10</sup> Leduc, S. and Wilson, D. (2012) “Roads to Prosperity or Bridges to Nowhere? Theory and Evidence on the Impact of Public Infrastructure Investment,” *NBER Macroeconomics Annual* 27, pp. 89–142.

<sup>11</sup> Duranton, G. and Turner, M. (2011). “The Fundamental Law of Road Congestion: Evidence from US Cities.” *American Economic Review*, 101(6): 2616–52.

<sup>12</sup> Another prominent example is Banerjee et al., (2020), who draw straight lines between historically important Chinese cities and “Treaty Ports” to predict future transport investment and investigate the effect on long term economic growth:

Banerjee, A., Duflo, E., and Qian, N. (2020). “On the road: Access to transportation infrastructure and economic growth in China.” *Journal of Development Economics*, 145: 102442.

<sup>13</sup> Bartik, T. J. (1991). “Who Benefits from State and Local Economic Development Policies?” Kalamazoo, Michigan: W.E. Upjohn Institute for Employment Research and Blanchard, O. and Katz, L. (1992). “Regional Evolutions.” *Brookings Papers on Economic Activity*, 1: 1–75.

is plausibly exogenous and the aggregate shocks are not themselves driven by region-specific shocks (Goldsmith-Pinkham et al., 2019; Borusyak et al., 2020).<sup>14</sup>

There is a growing set of recent studies that apply explicit shift-share designs in transport settings. Particularly relevant is a working paper by Garcia-López et al. (2024),<sup>15</sup> who study Great Britain and use a shift-share instrument interacting exposure to the historical railway network (circa 1870) with growth in the modern highway network to identify the causal effect of road capacity on traffic and population outcomes. Related applications include Garcia-López et al., (2016),<sup>16</sup> who use historical postal routes and railways interacted with modern infrastructure expansion to study suburbanisation in European cities, and Santagata (2022),<sup>17</sup> who adopts a Bartik-style approach using ancient Roman roads as exposure in the study of regional trade and transport infrastructure in Italy. We build upon these methodologies to isolate the instrument-induced, exogenous component of transport investment and thereby estimate its causal effect on economic growth.

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<sup>14</sup> Goldsmith-Pink, P., Sorkin, I. and Swift, H. (2020) “Bartik Instruments: What, When, Why, and How” *American Economic Review* and Borusyak, K., Hull, P. and Jaravel, X. (2022) “Quasi-Experimental Shift-Share Research Designs” *Quarterly Journal of Economics*

<sup>15</sup> Garcia-López, M.-À., Gómez-Hernández, P. and Sanchis-Guarner, R. (2024). “Highway Traffic in Britain: The Effect of Road Capacity Changes.” CEP Discussion Paper No. 2034, Centre for Economic Performance, London School of Economics.

<sup>16</sup> Garcia-López, M.-À., Pasidis, I. and Viladecans-Marsal, E. (2016). “Express Delivery to the Suburbs: The Effects of Transportation in Europe’s Heterogeneous Cities.” CESifo Working Paper No. 5699.

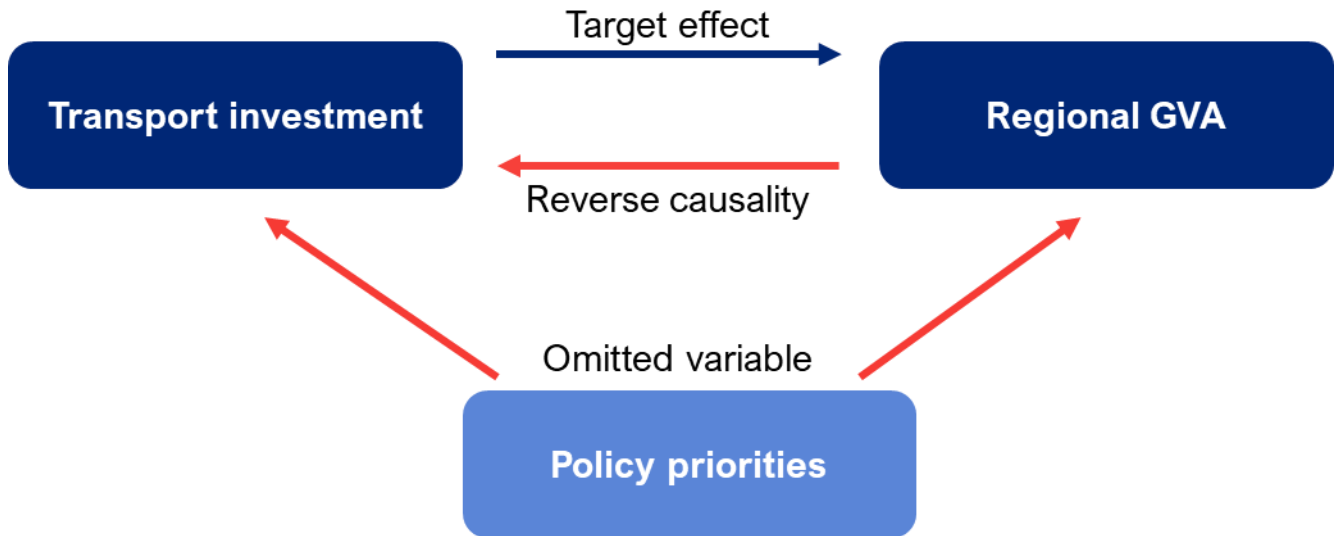
<sup>17</sup> Santagata, S. (2022). “Roads and Intra-National Trade: Evidence from Italian Regions.” *Journal of Transport Economics and Policy*, 56(3): 257–286.

### 3. METHODOLOGY

#### 3.1. THEORETICAL MODEL

To identify the **causal** effect of transport investment on economic growth, we start from a simple causal framework. A directed acyclic graph, shown in the Figure 3.1 below, summarises the key relationships: nodes represent relevant variables (e.g., transport investment, output, policy priorities), and arrows represent causal links and their direction.

Figure 3.1: Causal graph of transport investment on economic growth



Source: CEPA analysis

In the figure, the dark-blue arrow denotes the target effect: the causal impact of transport investment on economic output (GVA). The red arrows highlight pathways that render transport investment endogenous. The main concerns are:

- **Omitted variable bias:** contemporaneous policy priorities, institutional changes, or local shocks can influence both transport investment and GVA. If these factors are not fully observed or controlled, the estimated effect conflates the target effect with confounding influences.
- **Reverse causality:** regional output performance can shape investment allocations. If capital is steered towards faster-growing regions, naive estimates overstate the elasticity (upward bias). Conversely, if allocations favour lagging regions for rebalancing, naive estimates understate the elasticity (downward bias).

These channels violate the exogeneity condition, so an OLS regression of GVA on transport investment will be biased and inconsistent. Section 3.2 sets out the baseline empirical specification (OLS) to frame the problem, and Section 3.3 presents our identification strategy and IV approach, which isolates variation in investment uncorrelated to these confounding pathways.

#### 3.2. BASELINE EMPIRICAL SPECIFICATION

Our baseline empirical specification is a two-way fixed effects (TWFE) panel regression model:

$$y_{rt} = \beta \ln(TI_{rt}) + \alpha_r + \gamma_t + \varepsilon_{rt}$$

Where  $y_{rt}$  denotes the log of output in region  $r$  and year  $t$ ,  $\ln(TI_{rt})$  is the log of regional transport investment,  $\alpha_r$  are region fixed effects controlling for time-invariant heterogeneity, and  $\gamma_t$  are year fixed effects absorbing macroeconomic shocks common across regions. Standard errors are clustered at the regional level to account for

within-region serial correlation. The coefficient of interest is  $\beta$ . The log-log specification means  $\beta$  is an elasticity: it approximates the percentage change in GVA associated with a 1% change in transport investment.

Under this specification, the coefficient  $\beta$  is not likely to give a causal estimate of the impact of transport investment on economic growth due to (i) omitted variables that co-move with investment and output, and (ii) reverse causality in allocation of public capital. These concerns motivate the IV strategy set out in Section 3.3.

### 3.2.1. Choice of investment flows rather than capital stock

A methodological choice in this study is to use annual transport investment flows as the key explanatory variable rather than an estimate of the transport capital stock. Much of the earlier macro-infrastructure literature estimates production functions using public capital stocks constructed via the perpetual inventory method. These capital stock series are not observed and instead require assumptions on depreciation rates, asset lifetimes, historical investment allocation, and price deflators. Reviews of the infrastructure literature consistently highlight that public capital stocks constructed using the perpetual inventory method can be sensitive to these assumptions and may contain substantial measurement error, particularly for sectors such as transport where asset lives are long and valuation is uncertain.

In the UK context, these challenges are amplified by data limitations. As far as we are aware, there is no robust, continuous series of transport-specific capital stocks at the ITL1 regional level; constructing them would require retroactively allocating decades of historical investment across regions and modes using unverifiable assumptions. Because measurement error in the capital stock regressor can bias IV estimates towards OLS, relying on imputed stock measures would undermine the credibility of our causal identification strategy.

Using flows is also conceptually aligned with our Bartik shift-share identification. In a Bartik design, identification arises from the interaction of:

- **Predetermined regional exposure (shares)**, here historical rail or road density; and
- **Time-varying national shocks (shifts)**, here annual changes in national transport capital budgets.

These national shocks operate directly on current investment flows, not on the accumulated capital stock. Capital stocks evolve slowly through depreciation and past investment, so year-to-year changes in stocks are small and mechanically related to earlier flows. Instrumenting a slow-moving capital stock with annual national budget shocks would therefore deliver a weak first stage. This is consistent with the modern shift-share literature, which emphasises that identification is strongest when the endogenous regressor responds meaningfully to the common shock. Investment flows meet this requirement while constructed capital stocks do not. Hence, using flows maximises first-stage relevance and makes the exclusion restriction more credible: national budget shocks affect regional output only via their effect on contemporaneous investment, not through the slowly evolving accumulated capital base.

We expect that elasticities using flows will generally be larger than those based on capital stocks. Stocks adjust only gradually, so large investments translate into small changes in the stock, flattening stock elasticities. Flows, by contrast, primarily capture demand-side stimulus from construction activity and supply chains, rather than the longer-run supply-side gains that emerge once infrastructure is built and operational. This generates a stronger measured relationship with output but means the resulting elasticity should be interpreted as a short-run effect driven primarily by the stimulus associated with investment spending.

Flow- and stock-based elasticities are related under the standard perpetual inventory method as the capital stock is the cumulative, depreciated sum of past investment flows. It is appropriate to interpret our estimates as capturing the **marginal productivity of new investment**, rather than the average productivity of the accumulated capital base. This distinction aligns with the focus of many recent causal studies and with contemporary fiscal-multipliers literature, both of which analyse the effects of government spending shocks rather than the stock of public capital.

From a policy perspective, this choice **strengthens interpretability**. Policy makers have control over annual investment budgets, not capital stocks. A flow-based elasticity therefore provides a directly relevant measure of the expected economic impact of marginal changes in public investment plans. While stock-based elasticities remain

useful for structural growth modelling, flow-based elasticities are better suited for evaluating short-run economic impacts of public transport investment decisions.

However, the choice to use changes in investment flows does mean that the elasticity estimate must be considered distinct from estimates of economic impacts derived from transport appraisals; the latter excluding demand-side stimulus effects, while attempting to capture longer-term effects.

### 3.3. IDENTIFICATION STRATEGY

To address endogeneity concerns, we adopt a **Bartik-style shift-share IV strategy**. In this section, we first explain why an IV approach is required in this context, then set out the 2SLS framework and the construction of our instrument (time-invariant exposures  $\times$  lagged national shocks). Finally, we state the identification conditions (relevance, exogeneity, exclusion) and discuss potential threats to validity and how we mitigate them.

#### 3.3.1. Summary and intuition

We employ an IV framework to purge transport investment of endogenous variation arising from confounding and reverse causality. Identification rests on three validity conditions: relevance (the instrument is correlated with investment), exogeneity (the instrument is uncorrelated with the structural error term, conditional on fixed effects and controls), and the exclusion restriction (the instrument affects economic output only through its impact on investment). When these conditions hold, two-stage least squares (2SLS) yields a consistent estimate of the causal elasticity.

Formally, the **first stage** projects transport investment on the instrument, fixed effects, and any controls:

$$\ln(TI_{rt}) = \delta IV_{rt}^{(k)} + \alpha_r + \gamma_t + \mu_{rt}$$

This regression produces fitted values  $\ln(\widehat{TI}_{rt})$  that reflect only instrument-induced movements in transport investment.

The **second stage** relates economic output to the exogenous fitted transport investment:

$$y_{rt} = \beta \ln(\widehat{TI}_{rt}) + \alpha_r + \gamma_t + \varepsilon_{rt}$$

Under the relevance, exogeneity, and exclusion conditions,  $\beta$  identifies the causal elasticity of output with respect to transport investment. Intuitively, the first stage purges investment of its endogenous component, retaining only the instrument-induced, exogenous variation. The second stage then relates output to this purified measure, isolating the causal effect.

#### 3.3.2. Constructing the Bartik instrument

Building on Duranton and Turner (2011) and Banerjee et al (2020), we employ a Bartik-style instrument that interacts time-invariant regional exposure to transport investment, proxied by historical infrastructure density, with time-varying national shocks to transport budgets. In settings like ours, such designs are well-suited: historical networks embed path dependence that predicts where investment flows when national budgets expand, while aggregate budget shocks are determined at the national level and are plausibly uncorrelated with short-run local growth shocks once fixed effects are included.

Formally, for mode  $k \in \{rail, road\}$ , the instrument takes the form

$$IV_{rt}^{(k)} = E_r^{(k)} \times \Delta S_{t-1}$$

Where  $E_r^{(k)}$  represents the standardised exposure of region  $r$  (e.g., rail density), and  $\Delta S_{t-1}$  denotes the change in national transport budget, lagged by a year. We use the lag  $t - 1$  to reduce simultaneity with current investment allocations and outcomes.

### Box 1: Identifying variation and intuition of the Bartik instrument

**The interaction between fixed historical exposure and common national budget movements creates quasi-random variation in transport investment intensity across regions, which we use to study the causal effect on growth.**

For example, if the government were to ramp up investment to upgrade old rail lines nationwide, regions with greater historical rail density would predictably receive a larger boost simply because there is more track to renew and more junctions to connect. Conversely, regions with sparse historical networks would receive less. In effect, a region's exposure to modern transport investment is determined by how rail-heavy it was in the past, multiplied by the national wave of investment today.

The key idea is that our instrument captures the part of investment that is driven by maintaining and upgrading legacy transport assets when national budgets change, rather than by local economic conditions or policy targeting. Historical rail and road density is fixed in time and its direct effect on regional growth is fully absorbed by region fixed effects. National budget shocks, meanwhile, affect all regions at the same time and therefore do not reflect local growth conditions. By interacting these two components, the instrument assigns each region a different "share" of the national shock based solely on how much legacy infrastructure it inherited – not on how fast it is growing today.

This means that the fitted values of investment coming from the instrument reflects renewal intensity where assets already exist, not decisions to invest more in high-growth or low-growth regions. The instrument therefore gives us the part of transport investment that is independent of current local growth, policy priorities, or shocks, allowing the second stage to recover a genuine causal effect.

### Box 2: Why the instrument breaks the link between current growth and current investment

#### Step 1: Historical networks are fixed

Rail and road densities from 1950/1890s cannot be influenced by growth in the 21<sup>st</sup> century. Region fixed effects remove any level differences associated with these historical patterns.

#### Step 2: National budget shocks are common to all regions

When the national transport budget increases, every region is exposed to the same shift. Year fixed effects absorb the overall national cycle and macroeconomic trends.

#### Step 3: The identifying variation only comes from the interaction

Regions with more legacy track or road mileage receive relatively more renewal investment when budgets rise. This mechanism reflects engineering needs, not today's growth performance.

#### Step 4: Why this removes reverse causality

Current regional growth cannot influence how much legacy rail a region had in 1950, nor can it influence the size of the national transport budget. Therefore, the instrumented part of investment is driven by where old infrastructure exists, not by whether a region is growing faster or slower today.

#### Result

The instrumented variation in transport investment is plausibly exogenous – it is the part unrelated to local economic conditions, policy targeting, or reverse causality.

### 3.3.3. Instrument validity

Goldsmith-Pinkham et. al. (2020)<sup>18</sup> set out the conditions under which a Bartik design identifies a causal effect. They argue that, while shifts (the common time-varying shocks) primarily deliver first-stage strength, the shares (the predetermined exposures) are typically the source of exogenous variation. Thus, when identification relies on

<sup>18</sup> Goldsmith-Pinkham, P., Sorkin, I. and Swift, H. (2020). "Bartik Instruments: What, When, Why, and How." *American Economic Review*, 110(8), 2586–2624.

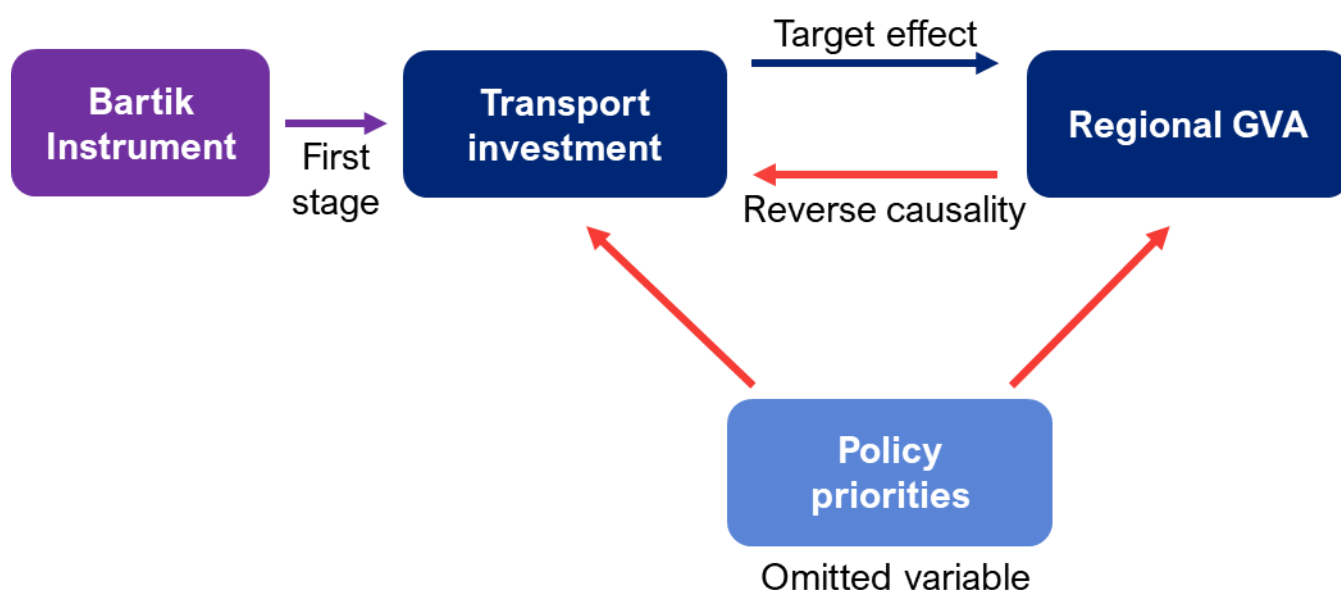
differential exposure to common shocks, the plausibility of exogeneity rests chiefly on the initial shares rather than the shifts.

The three conditions required to ensure IV validity are:

- **Relevance:** historical network density predicts where new spending occurs.
- **Exogeneity:** historical networks predate the sample and are uncorrelated with current shocks.
- **Exclusion:** past networks affect GVA only via new investment.

Below, we set out and justify in detail why our Bartik instrument satisfies the three required validity conditions. Figure 3.2 presents a causal graph incorporating the instrument and illustrates each identification assumption in turn.

Figure 3.2: Causal graph with Bartik instrument



Source: CEPA analysis

**First, instrument relevance.** The interaction between exposure and shocks must be strongly correlated with contemporary regional transport investment. This condition is plausible in our setting because historical infrastructure density captures long-run path dependence. Regions with denser rail or road networks are more likely to attract subsequent investment as national budgets expand, reflecting both engineering feasibility and political inertia in maintaining and upgrading existing networks. In the UK, these legacies are particularly strong: rail investment remains concentrated along corridors defined in the nineteenth century, while the motorway system overlays pre-existing road patterns. Empirically, we test relevance through the first-stage regression. In Figure 3.2, the first-stage is shown by the purple arrow.

**Second, instrument exogeneity.** Conditional on region and year fixed effects, the interaction between historical infrastructure and national shocks must be uncorrelated to unobserved determinants of contemporary regional output. This is the key condition for causal interpretation. In our context, exogeneity is plausible for two reasons. Historical transport densities are pre-determined, reflecting past investment decisions that are unlikely to be influenced by current growth dynamics. At the same time, national shocks to transport budgets are driven by aggregate fiscal conditions and political priorities, which are common across all regions. Their interaction therefore generates quasi-random variation across space and time. By including region fixed effects, we net out any persistent differences in levels of output associated with historical infrastructure, while year fixed effects absorb contemporaneous macroeconomic shocks. The remaining identifying variation reflects only the interaction of pre-determined exposure with common national shocks, which is credibly exogenous to local growth trends.

**Third, the exclusion restriction.** Beyond their impact on regional transport investment, the instruments must not exert an independent effect on output. Put differently, once fixed effects are included, the only channel through which the interaction term can affect GVA is via actual transport investment. This assumption is plausible because historical rail or road density by itself does not generate additional shocks to regional output in the study period; it is only through new investment flows that these historical differences become relevant. The inclusion of region fixed effects further strengthens this exclusion restriction by absorbing all time-invariant regional characteristics – such as geography, industrial legacy, or long-run productivity differentials – that could otherwise create a spurious link between historical network density and contemporary output. Consequently, identification relies solely on within-region changes over time arising from national investment shocks interacting with predetermined historical exposures. The use of lagged national shocks further insulates against the possibility that contemporaneous growth in certain regions influences national allocations in ways correlated with exposures. In Figure 3.2, the exclusion restriction is shown by the fact that the instrument only impacts regional GVA via its effect on transport investment, and not through any other channels.

### 3.3.4. Spillovers

Economic effects of transport investment can extend beyond administrative boundaries via improved connectivity, agglomeration, and displacement. Because the analysis is conducted at the ITL1 level, and these are large geographic and population units, any spillovers operating within an ITL1 region are absorbed into the local effect and cannot be separately identified. To allow for such cross-regional spillovers, we augment the baseline with a spatial term so that GVA in region  $r$  may depend on transport investment in neighbouring regions. We construct a row-normalised spatial weights matrix  $W$  ( $w_{ij} = 1/n_i$  if regions  $i$  and  $j$  share a boundary, 0 otherwise; rows sum to 1;  $n_i$  is the number of regions bordering region  $i$ ). Row normalisation means  $W \ln(TI_{rt})$  is the average of neighbours' transport investment. We estimate specifications that include both local investment  $\ln(TI_{rt})$  and neighbouring investment  $W \ln(TI_{rt})$ .

Because neighbouring transport investment is also endogenous, we instrument the spillover term using the spatial lag of the Bartik instrument,  $W IV_{rt}^{(k)}$ , alongside the local instrument  $IV_{rt}^{(k)}$ . This yields two first-stage equations:

$$\begin{aligned}\ln(TI_{rt}) &= \delta IV_{rt}^{(k)} + \alpha_r + \gamma_t + \mu_{rt} \\ W \ln(TI_{rt}) &= \pi W IV_{rt}^{(k)} + \alpha_r + \gamma_t + v_{rt}\end{aligned}$$

The second stage relates output to the fitted local and fitted neighbouring transport investment:

$$y_{rt} = \beta_1 \widehat{\ln(TI_{rt})} + \beta_2 \widehat{W \ln(TI_{rt})} + \alpha_r + \gamma_t + \varepsilon_{rt}$$

Here,  $\beta_1$  captures the within-region elasticity, holding neighbour investment constant, while  $\beta_2$  captures spillover effects from adjacent regions.

Including the spillover term helps isolate the impact of investment in region  $r$  on output in region  $r$ . Evidence of spillovers would be indicated by  $\beta_2 \neq 0$ , implying that models omitting  $W \ln(TI_{rt})$  risk omitted-variable bias.

### 3.3.5. Threats to identification

Naturally, there are potential threats to identification. Historical infrastructure density could be correlated with persistent agglomeration dynamics or long-run growth trajectories that are imperfectly captured by fixed effects. For instance, areas, such as the North West, with high historical rail density may also be regions with enduring industrial or institutional advantages, which could bias estimates if those characteristics influence contemporary growth in ways unrelated to current transport spending. This concern is particularly salient in the case of London, which combines exceptionally high historical infrastructure density with deep-rooted structural advantages as the UK's administrative and financial centre. In this context, part of the estimated relationship between rail exposure and output could, in principle, reflect persistent regional characteristics correlated with both past and present infrastructure rather than purely exogenous variation in investment.

We judge this threat to be limited but cannot fully rule it out. Exposures are measured using deep-historical networks (well before the study window), making it defensible that most level and trend effects associated with early

rail geography were realised long ago. Region fixed effects remove all time-invariant advantages, while year fixed effects purge common national cycles, so identification comes from the interaction of fixed, pre-determined exposure with intertemporal national budget movements, not from persistent cross-sectional differences. In the appendix, we estimate models that include regional time trends, allowing each ITL1 region to follow its own underlying long-run growth path. By controlling for these region-specific trends, the model relies only on deviations from each region's long-run trajectory, tightening the exclusion restriction and reducing the risk of bias from correlated long-run structural forces. The estimated elasticities remain positive and statistically significant in our preferred model, and are not statistically different from our estimates without regional time trends.

However, the robustness test excluding London underscores that the capital region carries substantial leverage in the identification strategy. When London is omitted, estimated elasticities remain positive but become statistically weak, reflecting the sharp loss of first-stage strength. This pattern suggests that much of the identifying variation originates from London's combination of high exposure and sustained investment intensity, which raises the possibility that the instrument may partly capture London-specific persistence. We therefore acknowledge this as a residual identification risk: while the fixed-effects structure and historical depth of the exposures mitigate bias, some degree of correlation between historical infrastructure and long-run regional characteristics, particularly in London, cannot be entirely excluded. Any resulting violation of the exclusion restriction would most likely bias the estimated elasticity upward.

Another consideration with our identification strategy is potential weak instrument bias, where the 2SLS estimator with weak instruments is biased in small samples. Bound, Jaeger, and Baker (1995)<sup>19</sup> show that the bias of 2SLS centres on the OLS bias as the weakness of the instrument grows. Following Angrist and Pischke (2009),<sup>20</sup> we can write the bias as a function of the OLS bias  $\frac{\sigma_{\varepsilon\mu}}{\sigma_{\mu}^2}$  and the first-stage F statistic:

$$E[\hat{\beta}_{2SLS} - \beta] \approx \frac{\sigma_{\varepsilon\mu}}{\sigma_{\mu}^2} \frac{1}{1 + F}$$

As relevance increases ( $F \rightarrow \infty$ ) the bias tends to zero. Alternatively, as the instrument weakens ( $F \rightarrow 0$ ) the 2SLS bias tends to the OLS bias. This is intuitive. If there is no first stage ( $\delta = 0$ ), then any variation in the fitted value of transport investment  $\ln(\widehat{TI}_{rt})$  in the sample just comes from the first stage error  $\mu_{rt}$ . So, the variation in  $\ln(\widehat{TI}_{rt})$  is no different from the variation in  $\ln(TI_{rt})$ , and hence OLS and 2SLS will have to estimate the same quantity on average.

This underscores the importance of reporting first-stage results and F-statistics. Consistent with standard practice, we treat  $F > 10$  as a rule of thumb indicating that estimates are unlikely to suffer materially from weak-instrument bias.

To increase confidence in our results, we implement several robustness checks. First, we re-estimate models in levels (totals) rather than per capita to assess sensitivity to scale. Second, we account for differences in regional long-run trajectories by including regional time trends. Third, we examine spatial spillovers by allowing neighbouring regions' investment to affect local GVA. Finally, we exclude influential regions (London or Scotland) as a stress test.

We estimate both just-identified (single-instrument) and over-identified specifications (more instruments than endogenous regressors). For each second-stage regression, we report the corresponding first-stage results and assess instrument strength using the Kleibergen-Paap Wald F-statistic (robust to heteroskedasticity and clustering). Where appropriate, we also report over-identification tests – the Sargan test under homoskedasticity and the Hansen J test under heteroskedasticity/cluster-robust settings. These tests evaluate whether the full set of instruments is jointly consistent with instrument exogeneity, recognising that such tests can be low-powered.

<sup>19</sup> Bound, J., Jaeger, D. and Baker, R. (1995). "Problems with Instrumental Variables Estimation When the Correlation Between the Instruments and the Endogenous Explanatory Variable Is Weak." *Journal of the American Statistical Association* 90 (430).

<sup>20</sup> Angrist, J., and Pischke, J. (2009). *Mostly Harmless Econometrics*. 1st ed. Princeton University Press.

## 4. DATA SUMMARY

We assemble a balanced panel at the ITL1 level covering 12 regions over 23 years (2001-2023). The dependent variables are the natural logarithm of regional GVA and the natural logarithm of GVA per capita. Prices for both GVA and transport investment are constant (2024 prices); level variables are reported in £ million, and per-capita variables in £ per person per year.

Population counts and GVA series are drawn from the Office for National Statistics (ONS). Public transport investment is taken from HM Treasury's Country and Regional Analysis (CRA). We construct two principal regressors: (i) total transport capital expenditure (log, £ million) and (ii) per-capita transport capital expenditure (log, £ per person). For mode-specific analysis, we further disaggregate investment into rail and road components, again in both levels (log, £ million) and per-capita (log, £ per person) terms. The log-log specification permits interpretation of coefficients as elasticities and reduces skewness arising from scale differences across regions.

To implement the Bartik shift-share instrument, we combine historical exposure measures with national budget shocks. Historical exposure is proxied by legacy network densities for rail and road, sourced from the Cambridge Group for the History of Population and Social Structure.<sup>21 22</sup> National shocks are constructed as changes in aggregate transport (mode-specific) capital budgets from the CRA, expressed in £ million and lagged by one year to mitigate simultaneity.

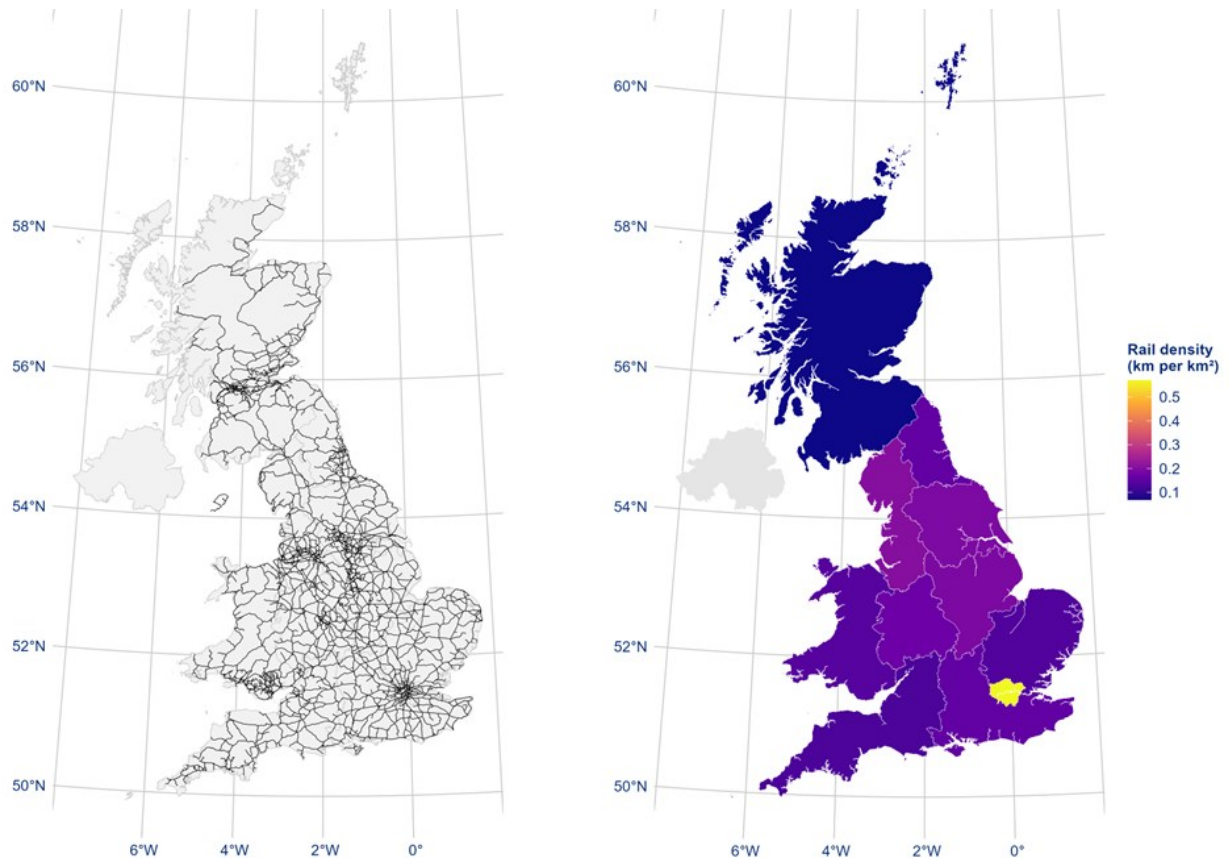
Descriptive mapping (Figure 4.1) illustrates the 1950 rail network and the corresponding regional average rail density; analogous figures are provided for roads. The spatial pattern is as expected: London exhibits very high historical rail density, whereas Scotland is comparatively sparse. These legacy patterns underpin the exposure component of the instrument used in the empirical analysis.

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<sup>21</sup> Martí-Henneberg, J., Satchell, M., You, X., Shaw-Taylor, L., 'Railway Lines of England and Wales 1807-1994 dynamic GIS shapefile (2017)' This is an enhanced version of Martí-Henneberg, J., Tapiador, F., Satchell, M., Shaw-Taylor, L., and Wrigley, E.A., 'Railway Lines of England and Wales 1807-1994 dynamic GIS shapefile' (2008). The Martí-Henneberg et al dataset is a GIS digitisation of the rail lines in M.H. Cobb, The Railways of Great Britain, a Historical Atlas at the scale of 1 inch to 1 mile, 2 vols., (Shepperton, Ian Allan Publishing, 2005).

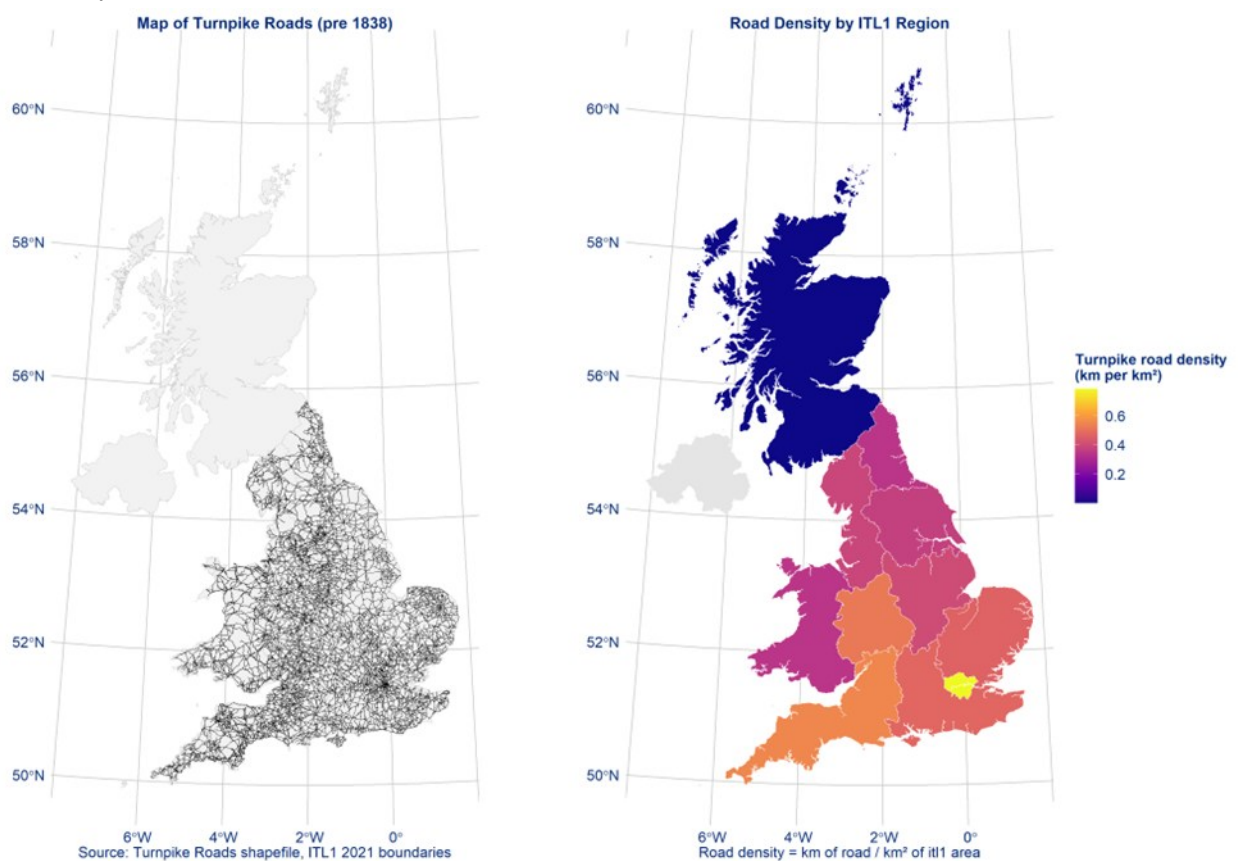
<sup>22</sup> Rosevear, A., Satchell, M., Bogart, D., Shaw Taylor, L., Aidt, T. and Leon, G., 'Turnpike roads of England and Wales, 1667-1892', 2017. This dataset was created with funding from the Leverhulme Trust (RPG-2013-093), the NSF (SES-1260699), and the British Academy (SG121870). A description of the dataset can be found in Bogart, D, Rosevear, A. and Satchell, M., 'Turnpike roads of England and Wales 1667-1892 GIS shapefile documentation' available at: <http://www.geog.cam.ac.uk/research/projects/occupations/datasets/documentation.html>

Figure 4.1: Rail network in 1950



Source: Cambridge Group for the History of Population and Social Structure, Rail shapefile (1950), ITL1 2021 boundaries

Figure 4.2: Turnpike road network in c1892



Source: Cambridge Group for the History of Population and Social Structure, Turnpike Road shapefile, ITL1 2021 boundaries

#### 4.1. SUMMARY STATISTICS

Table 4.1 reports descriptive statistics for the balanced ITL1 panel (12 regions, 2001-2023). Monetary variables are real (2024 prices); level variables are expressed in £ million and per-capita variables in £ per person per year. Rail density is defined as route-kilometres of railway per km<sup>2</sup> of ITL1 area<sup>23</sup> in 1950, and road density as route-kilometres of historical turnpike roads<sup>24</sup> per km<sup>2</sup> in 1892.

Table 4.1: Summary statistics

| Variable                             | Mean       | SD         | Min       | Max        |
|--------------------------------------|------------|------------|-----------|------------|
| GVA (£m)                             | 179,847.00 | 119,450.52 | 41,624.40 | 598,018.42 |
| Transport investment (£m)            | 1,657.14   | 1,469.43   | 154.93    | 8,601.02   |
| Rail investment (£m)                 | 734.24     | 1305.04    | 0.00      | 7,895.01   |
| Road investment (£m)                 | 524.33     | 459.09     | 0.00      | 2,346.64   |
| GVA per capita (£)                   | 31,553.66  | 9,169.73   | 22,122.10 | 66,852.74  |
| Transport investment per capita (£)  | 284.62     | 151.18     | 91.74     | 969.78     |
| Rail investment per capita (£)       | 111.64     | 150.83     | 0.00      | 890.18     |
| Road investment per capita (£)       | 99.19      | 74.35      | 0.00      | 397.65     |
| Rail density (km / km <sup>2</sup> ) | 0.19       | 0.12       | 0.07      | 0.57       |
| Road density (km / km <sup>2</sup> ) | 0.42       | 0.18       | 0.00      | 0.79       |

Source: ONS Regional Gross Value Added (GVA); ONS Population Estimates (Population); HMT Country and Regional Analysis (transport Investment, by type); Cambridge Group for the History of Population and Social Structure (rail and road density)

GVA averages £179.8bn per region-year (SD £119.5bn), with substantial dispersion reflecting differences in regional scale. Average transport investment is £1.66bn (SD £1.47bn), or £285 per person (SD £151), again indicating marked cross-sectional and intertemporal variation. By mode, rail investment displays the widest range (up to £7.90bn), consistent with large, lumpy programmes. Historical rail and road density indices exhibit meaningful variation across regions (means 0.19 and 0.42 respectively), providing the cross-sectional exposure required for the shift-share instrument.

There are a few data limitations. Historic rail network data is not available for Northern Ireland, and historical turnpike road data is not available for Northern Ireland and Scotland. These constraints reduce the effective sample for instrumented specifications and mean that the rail-based instrument (with better historical coverage) is likely to yield the most reliable first-stage strength. An in-depth summary of the data, including transformations and sources, is provided in the Appendix.

<sup>23</sup> Based on ITL 2021 boundaries. ITL1 boundaries have not changed over the reference period.

<sup>24</sup> A turnpike road is a toll road, managed by a trust, that charged fees to fund road maintenance. They rose in popularity during the 17<sup>th</sup>, 18<sup>th</sup> and 19<sup>th</sup> centuries and form the basis of the modern highway network.

## 5. RESULTS

This section reports the headline elasticity estimates. We begin with per-capita specifications, showing the baseline two-way fixed effects ordinary least squares (TWFE-OLS) results for reference and the IV estimates using the rail, road, and combined Bartik instruments, including versions with lagged shocks. For all IV models we report first-stage strength and, where applicable, Sargan/Hansen tests of over-identifying restrictions. We then examine mode-specific regressions to compare rail and road elasticities. The section concludes with the spatial IV specification, which incorporates spillovers by allowing neighbouring regions' investment to affect local GVA per capita.

### 5.1. MAIN RESULTS

Table 5.1 reports estimates of the elasticity of regional GVA per capita with respect to transport capital investment per capita, comparing TWFE-OLS (model 1) with IV specifications using rail, road, and combined instruments. All models include region and year fixed effects, with standard errors clustered by region.

We work in per-capita terms to identify the responsiveness of output per person to investment per person, abstracting from differences in population size across regions. Normalising both sides by population is preferable to including population as a right-hand-side control while leaving variables in levels: the latter risks conflating scale effects (bigger places spend and produce more) with intensity effects (spend per person). By contrast, per-capita specifications yield elasticities that are comparable across regions of different sizes and are more transparent for policy questions on distributional impacts and the relative productivity of investment.

Table 5.1: Elasticity of regional GVA per capita with respect to transport investment per capita

| In(GVA pc)                        | (1) OLS          | (2) IV: Rail        | (3) IV: Rail<br>2 <sup>nd</sup> Lag | (4) IV:<br>Road   | (5) IV:<br>Lagged Rail | (6) IV: Rail +<br>Road |
|-----------------------------------|------------------|---------------------|-------------------------------------|-------------------|------------------------|------------------------|
| <b>Second stage</b>               |                  |                     |                                     |                   |                        |                        |
| In(TI pc)                         | 0.007<br>(0.018) | 0.071**<br>(0.017)  | 0.078**<br>(0.022)                  | 0.066<br>(0.039)  | 0.084***<br>(0.017)    | 0.067+<br>(0.032)      |
| <b>First stage:</b>               |                  |                     |                                     |                   |                        |                        |
| Rail Density<br>× Nat Budget (L1) |                  | 0.023***<br>(0.004) |                                     |                   | 0.019***<br>(0.003)    | 0.007<br>(0.009)       |
| Rail Density<br>× Nat Budget (L2) |                  |                     | 0.020***<br>(0.004)                 |                   | 0.014**<br>(0.003)     |                        |
| Road Density<br>× Nat Budget (L1) |                  |                     |                                     | 0.030*<br>(0.011) |                        | 0.023<br>(0.020)       |
| <b>Model diagnostics</b>          |                  |                     |                                     |                   |                        |                        |
| Observations                      | 276              | 231                 | 220                                 | 210               | 220                    | 210                    |
| R Squared (second<br>stage)       | 0.992            | 0.994               | 0.995                               | 0.995             | 0.995                  | 0.995                  |
| Weak-IV F                         | N/A              | KP F = 24.37        | KP F = 24.87                        | KP F = 6.76       | CD F = 4.07            | CD F = 2.50            |
| Sargan J                          | N/A              | N/A                 | N/A                                 | N/A               | p = 0.907              | p = 0.905              |

Notes: Two-way fixed effects (region and year) in all columns; standard errors clustered at the region level in parentheses. KP = Kleibergen-Paap Wald F; CD = Cragg-Donald F. Sargan J is an overidentification test; N/A indicates not applicable for exactly-identified models.

Significance: +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

Table 5.1 presents estimates of the elasticity of regional economic output with respect to transport investment per capita. The baseline TWFE OLS specification reveals no systematic relationship between transport investment and economic growth. The estimated coefficient on log transport investment per capita is small and statistically insignificant (0.007, s.e. 0.018), indicating that once region and year fixed effects are included, there is no detectable contemporaneous association between investment intensity and regional GVA growth. This result is

consistent with the expectation that OLS estimates are biased by endogeneity – particularly if higher public investment is directed toward slower-growing regions, leading to a downward-biased elasticity estimate.

The IV results, reported in columns (2) to (6) of Table 5.1, suggest a markedly different pattern once endogeneity is addressed. Using the rail-based Bartik instrument, the estimated elasticity increases to 0.071 (s.e. 0.017), statistically significant at the 1% level. This implies that a 10% increase in transport investment per capita is associated with an average 0.7% increase in GVA per capita. The instrument exhibits reasonable strength, with a Kleibergen-Paap F-statistic of 24.4, above conventional weak-instrument thresholds.

A similar magnitude is obtained when using the lagged rail exposure instrument, which produces an elasticity of 0.078 (s.e. 0.022), significant at the 1% level, with a similar first stage (KP F = 24.9). The consistency across rail-based specifications supports the validity of the identification strategy and indicates that exogenous variation in rail exposure provides a credible source of identifying variation.

The road-based instrument yields a positive but less precise elasticity of 0.066 (s.e. 0.039), which is not statistically significant. The first-stage relationship is weaker (KP F = 6.7), indicating some risk of weak-instrument bias. Nonetheless, the direction and magnitude of the road elasticity remains comparable to the rail estimates, suggesting that the absence of significance reflects imprecision rather than a substantively different effect.

To further test robustness, an over-identified model including two lagged rail instruments was estimated. This specification yields an elasticity of 0.084 (s.e. 0.017), significant at the 0.1% level. Both instruments enter significantly in the first stage (L1: 0.019, s.e. 0.003; L2: 0.014, s.e. 0.003), although the first stage is noticeably weaker than previous rail-instrument based models (Cragg-Donald F = 4.00). The Sargan over-identification test ( $p = 0.91$ ) does not reject the null of joint exogeneity, supporting the validity of the instrument set.

A combined rail-and-road instrument was also estimated as an additional robustness check. This model produces a smaller, imprecisely estimated elasticity of 0.067 (s.e. 0.032), which is statistically significant only at the 10% level. This model also has weak joint first-stage diagnostics (CD F = 1.99). The over-identification test again fails to reject ( $p = 0.91$ ), but the limited first-stage strength suggests that inference in this specification should be interpreted with caution.

Taken together, these results point to a consistent pattern: OLS estimates detect no significant relationship between transport investment and regional GVA, while IV estimates indicate a positive and economically meaningful elasticity in the range of 0.07-0.08. The preferred specification, the just-identified rail-based IV model, offers a stable, statistically significant estimate that is robust across alternative lags of the instrument.

These findings imply that once endogeneity is addressed, transport investment has a causal and quantitatively important effect on regional economic growth. The estimated elasticities are economically plausible and align with the view that infrastructure investment enhances productive capacity and regional competitiveness. In line with best-practice econometric guidance (e.g., Cunningham 2021);<sup>25</sup> the just-identified rail model is therefore taken as the benchmark estimate for subsequent discussion and policy interpretation.

## 5.2. MODE-SPECIFIC RESULTS

Table 5.2 disaggregates the analysis by mode of investment, reporting separate specifications for rail and road expenditure per capita. This allows us to assess whether the aggregate elasticities reported in Table 5.1 masks important heterogeneity across transport modes. The sample is restricted to 2009-2023, reflecting the period of which the Country and Regional Analysis (CRA) data provide consistent subfunction-level reporting.

<sup>25</sup> Cunningham, S. (2021) *Causal Inference: The Mixtape*. New Haven, CT: Yale University Press. ISBN: 978-0-300-25168-5.

Table 5.2: Elasticity of regional GVA per capita with respect to rail and road transport investment per capita

| In(GVA pc)                        | (1) OLS Rail      | (2) OLS Road      | (3) IV: Rail      | (4) IV: Road      |
|-----------------------------------|-------------------|-------------------|-------------------|-------------------|
| <b>Second stage</b>               |                   |                   |                   |                   |
| ln(Rail TI pc)                    | 0.015+<br>(0.008) |                   | 0.106*<br>(0.047) |                   |
| ln(Road TI pc)                    |                   | -0.016<br>(0.012) |                   | -0.308<br>(0.431) |
| <b>First stage:</b>               |                   |                   |                   |                   |
| Rail Density<br>× Nat Budget (L1) |                   |                   | 0.028*<br>(0.010) |                   |
| Road Density<br>× Nat Budget (L1) |                   |                   |                   | -0.007<br>(0.013) |
| <b>Model diagnostics</b>          |                   |                   |                   |                   |
| Observations                      | 179               | 179               | 165               | 165               |
| R Squared (second stage)          | 0.995             | 0.995             | 0.996             | 0.996             |
| Weak-IV F                         | N/A               | N/A               | KP F = 6.91       | KP F = 0.25       |

Notes: Two-way fixed effects (region and year) in all columns; standard errors clustered at the region level in parentheses. KP = Kleibergen-Paap Wald F; CD = Cragg-Donald F. Significance: +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Sample is 2009-2023 due to data limitations.

The OLS estimates indicate significant variation in the strength and direction of association across modes. For rail investment, the elasticity of GVA per capita with respect to investment per capita is 0.015 (s.e. 0.008), statistically significant at the 10% level. This suggests that a 10% increase in rail investment per person is associated with an approximately 0.16% increase in regional GVA per person. By contrast, road investment shows a small and statistically insignificant coefficient (-0.016, s.e. 0.012), implying no clear relationship between road spending and local output growth. Although these estimates are not causal, they indicate that the association between investment and economic performance differs by mode, with rail showing a stronger contemporaneous link to regional output.

The contrast becomes more pronounced once endogeneity is addressed using IV estimation. Instrumenting rail investment with the rail-based shift-share instrument increases the estimated elasticity to 0.106 (s.e. 0.047), statistically significant at the 5% level. This elasticity implies that a 10% rise in rail capital per person is associated with a 1.1% increase in GVA per person, a larger effect than our aggregate IV results in Table 5.1. However, instrument strength is relatively weak (Kleibergen-Paap F = 6.9), suggesting that the true effect may be imprecisely estimated and should be interpreted cautiously.

For road investment, the IV specification yields a large negative but highly imprecise elasticity of -0.308 (s.e. 0.431), with extremely weak first-stage diagnostics (KP F = 0.5). The wide confidence interval and weak identification indicate that this estimate provides no informative evidence about the sign or magnitude of any productivity effect from road investment.

Overall, the mode-specific IV results suggest investment in rail produces slightly larger effects on GVA compared to general transport investment, which will mix rail, road and other transport investment. In contrast, there is no robust evidence of a positive effect for road investment given current data and instrument limitations. These findings underscore the importance of distinguishing between modes when evaluating the productivity effects of transport capital and highlight the need for stronger identification strategies in future work on road investment impacts.

### 5.3. SPILLOVERS

Table 5.3 reports estimates from the spatial IV specification, which extends the baseline model by including a spatial lag of transport investment to capture potential spillover effects from neighbouring regions' spending. The spatially lagged Bartik instruments allow separate identification of local and neighbour (spillover) elasticities.

Table 5.3: Elasticity of regional GVA per capita to transport investment per capita: spatial IV estimates

| ln(GVA pc)                          | (1) OLS          | (2) IV: Rail        | (3) IV: Road      | (4) IV: Rail + Road |
|-------------------------------------|------------------|---------------------|-------------------|---------------------|
| <b>Second stage</b>                 |                  |                     |                   |                     |
| ln(TI pc)                           | 0.025<br>(0.018) | 0.080**<br>(0.019)  | 0.066+<br>(0.032) | 0.071*<br>(0.028)   |
| W ln(TI pc)                         | 0.049<br>(0.033) | 0.083<br>(0.153)    | -0.001<br>(0.135) | 0.002<br>(0.133)    |
| <b>First stage: ln(TI pc)</b>       |                  |                     |                   |                     |
| Rail Density<br>× Nat Budget (L1)   |                  | 0.023***<br>(0.004) |                   | 0.007<br>(0.009)    |
| Road Density<br>× Nat Budget (L1)   |                  |                     | 0.030*<br>(0.011) | 0.023<br>(0.020)    |
| <b>First stage: W ln(TI pc)</b>     |                  |                     |                   |                     |
| W Rail Density<br>× Nat Budget (L1) |                  | 0.026***<br>(0.005) |                   | 0.009<br>(0.016)    |
| W Road Density<br>× Nat Budget (L1) |                  |                     | 0.026<br>(0.019)  | 0.020<br>(0.027)    |
| <b>Model diagnostics</b>            |                  |                     |                   |                     |
| Observations                        | 253              | 231                 | 210               | 210                 |
| R Squared (second stage)            | 0.993            | 0.995               | 0.995             | 0.995               |
| Weak-IV F                           | N/A              | CD F = 2.40         | CD F = 3.12       | CD F = 1.77         |
| Sargan J                            | N/A              | N/A                 | N/A               | p = 0.97            |

Notes: Two-way fixed effects (region and year) in all columns; standard errors clustered at the region level in parentheses. CD = Cragg-Donald F statistic. Sargan J is an overidentification test; N/A indicates not applicable for exactly-identified models.

Significance: +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

The spatial TWFE-OLS benchmark provides little evidence of systematic spatial dependence. The coefficient on local transport investment per capita is small and statistically insignificant (0.025, s.e. 0.018), while the spatial lag term is also insignificant (0.049, s.e. 0.033). These results indicate no discernible contemporaneous spillovers once regional and year fixed effects are included, and as with earlier OLS models, should not be interpreted causally.

Instrumenting both local and neighbouring investment with the rail-based Bartik instrument increases the estimated magnitude of the local effect. The elasticity on local investment per capita rises to 0.080 (s.e. 0.019), significant at the 1% level, while the estimated spillover effect also grows in magnitude (0.083, s.e. 0.153), but is very imprecisely estimated and not statistically significantly different to zero. This result suggests that the benefits of transport investment is concentrated locally, with no evidence of spillover or displacement effects.

However, the Cragg-Donald F statistic of 2.85 indicates relatively weak instrument strength, meaning that these coefficients should be treated cautiously. The spatial instruments do not provide sufficient independent variation to reliably distinguish true spillover effects from weak-IV distortions.

Using the road-based Bartik instrument produces similar estimates for local elasticities and no elasticity for neighbouring transport investment. The coefficient on local investment is 0.066 (s.e. 0.032), significant at the 10% level, while the spillover term is almost zero and imprecisely estimated (-0.001, s.e. 0.135). First-stage strength remains relatively weak (CD F = 3.12), implying that the data offer limited information about spatial dependence in road investment. Despite the positive point estimate for local investment, the imprecision of the spillover term suggests that spatial interdependencies in road spending are not reliably identified.

The combined rail-and-road instrument yields an estimated local elasticity of 0.071 (s.e. 0.028) significant at the 5% level, and a spillover elasticity of 0.002 (s.e. 0.133). First-stage strength remains limited (CD F = 1.77), though the

Sargan over-identification test ( $p = 0.97$ ) does not reject the null of joint instrument exogeneity. Nonetheless, weak first-stage diagnostics suggest that these results should not be interpreted as robust.

Overall, the spatial IV models do not provide clear evidence about the diffusion of transport investment effects across regions. Across all IV specifications, the elasticity of local transport investment remains positive and statistically significant (at various levels), while the null hypothesis of no spillover effects from neighbouring regions' transport investment cannot be rejected. However, the relatively weak first stage F statistics and large standard errors for the spillover effect does not imply a precisely estimated null result. That is, we cannot conclude with certainty that there are no positive or negative spillover effects from transport investment.

The null result is likely a result of using extremely aggregated regional data and does not mean that displacement as applied in appraisal does not exist. It is likely that analysis using smaller regions would find different results and reach different conclusions.

## 6. DISCUSSION

### 6.1. MAIN FINDINGS AND INTERPRETATION

The UK-specific regressions indicate a positive, moderate relationship between transport investment and economic output once endogeneity is addressed. We find the Bartik instruments to be plausibly valid on theoretical grounds: as set out in Section 3.3, they satisfy the criteria of relevance, independence, and the exclusion restriction. Empirically, first-stage diagnostics indicate adequate strength – particularly for the rail shift-share instrument – and over-identification tests do not reject the exclusion restrictions in rail-only specifications. Taken together, these checks suggest that our strategy isolates a genuinely causal effect rather than a reduced-form correlation.

In our preferred rail-based specification, a 1% increase in transport investment per capita is associated with a 0.07% increase in GVA per capita. This lies in the middle of the 0.06–0.08 range implied by alternative IV models. While this elasticity exceeds headline pooled averages from international studies, including Melo et al. (2013) and our own meta-analysis, we do not interpret it as overstated. Instead, several factors explain why a UK-specific causal IV estimate would naturally exceed the average of a diverse international literature.

First, our estimate is a flow elasticity derived using an IV design that isolates the marginal effect of new investment. Much of the international evidence reports *stock* elasticities using methods that do not fully address endogeneity. Under standard perpetual-inventory models, flow elasticities are mechanically larger than stock elasticities, and IV estimates are systematically larger than OLS or fixed-effects estimates where investment is targeted toward slower-growing regions. The large gap between our OLS and IV estimates is consistent with this expected downward bias in non-IV studies.

Second, the UK study period (2001–2023) is characterised by large, renewal-intensive rail programmes – electrification, signalling upgrades, and major corridor renewals – that target binding capacity constraints and network reliability. These renewal-heavy, rail-dominant investment cycles may be associated with higher marginal productivity in the UK context. Many international studies pool more heterogeneous investment bundles – including many smaller road schemes – resulting in lower average elasticities. Our UK estimate therefore reflects a context with plausibly higher marginal returns.

This is partially supported by our mode-specific regressions, which indicate that the aggregate elasticity is driven by rail investment. However, the insignificance in road elasticity reflects limitations in the road specification, particularly weak first-stage strength and missing historical exposure for Scotland, rather than evidence that road investment has no effect, and we therefore treat the road estimates as inconclusive.

Third, extensive diagnostic analysis provides no evidence of mechanical upward distortion. In particular:

- **Instrument strength:** The first-stage F-statistic ( $>10$  in preferred models) exceeds accepted thresholds.
- **Outlier sensitivity:** Excluding influential regions (e.g., London) reduces precision but does not reverse sign or implied magnitude.
- **Alternative normalisations:** Estimates in levels vs per capita yield coherent patterns.
- **Multiple instruments:** Sargan/Hansen tests do not reject over-identifying restrictions in rail-only specifications.
- **Regional trends:** Including regional time trends slightly decreases the estimated elasticity (0.065 vs. 0.071), consistent with expectations that trends absorb some slow-moving variation but do not materially alter the underlying result

None of these diagnostics indicate systematic upward bias. The rail-based instrument is sufficiently strong, and second-stage coefficients are stable across lag structure.

The IV estimate should be interpreted as a Local Average Treatment Effect (LATE).<sup>26</sup> In a shift-share design such as ours, the instrument shifts regional transport investment in proportion to historical rail network density interacted with national budget shocks. As a result, the causal effect we identify pertains to the set of regions whose investment responds systematically to these shocks – the “compliers” with the instrument. First-stage diagnostics show that several regions, including London, the South West, East of England, North West, North East and West Midlands, exhibit strong instrument responsiveness while regions with sparse historical networks contribute less to identification. The preferred IV elasticity therefore represents the causal impact of additional transport investment for these high-exposure regions, rather than a uniform effect across all ITL1 regions. This is a standard feature of IV designs and does not imply that other regions experience no effect, only that they contribute less to the identification of the causal parameter.

Taken together, these considerations indicate that the UK elasticity reflects UK-specific structural factors and appropriate correction for endogeneity, rather than methodological inflation. Its magnitude remains within the plausible range given the heterogeneity observed in well-identified IV studies internationally.

## 6.2. SPILLOVER ESTIMATES

The spatial IV extensions do not provide robust evidence of economically meaningful spillovers. Across all specifications, the estimated spillover elasticities are small in magnitude, fluctuate in sign, and are imprecisely estimated. In two specifications the spatial spillover term is close to zero, in others it is weakly positive, and in no case is it statistically distinguishable from zero. These patterns, combined with weak first-stage statistics for the spatially lagged investment term, mean that the spatial results cannot be given a substantive interpretation.

Importantly, the instability of the spillover coefficient across instruments indicates that the spatial model is not well identified. The Bartik-style instruments used to isolate variation in neighbouring regions’ investment flows do not generate sufficient independent variation, leading to low instrument strength in the spatial equation. Under such conditions, IV estimates become highly sensitive to minor specification changes and cannot reliably separate genuine spillovers from noise. As a result, the spatial IV results should be viewed as diagnostic rather than evidential.

The absence of precise spillover estimates does not imply the absence of spillovers in reality. Instead, it reflects data limitations and the difficulty of credibly identifying cross-regional effects with annual investment flows at the ITL1 level. In particular, the spatial relationships embedded in the model are sensitive to the choice of weighting matrix. Our analysis uses a simple contiguity-based matrix, which is transparent and widely used but does not fully reflect the economic connectivity of UK regions. Weighting matrices based on distance, travel times, commuting flows, or interregional trade might provide a more realistic representation of functional linkages and could produce different results.

More flexible spatial econometric frameworks such as the Spatial Durbin Model (SDM) could also help to disentangle multiple channels of spatial dependence by simultaneously modelling spatial lags of the dependent variable and the regressors. However, these approaches typically require strong instruments and larger samples than are available at the ITL1 level and were therefore outside the scope of this study.

Overall, the spatial analysis reinforces the conclusion that the **only robust and precisely estimated effect is the local elasticity**, which is broadly consistent with the non-spatial IV result. The spillover coefficients cannot be interpreted quantitatively, and the current evidence is too weak to support policy use. Future work using richer data, alternative weighting structures, or SDM-type models may provide a stronger basis for identifying and quantifying spatial spillovers. This does not imply displacement as applied in appraisal doesn’t occur; it may be the

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<sup>26</sup> The Local Average Treatment Effect (LATE) is the average causal effect of transport investment on output for those regions whose investment levels are shifted by the instrument (i.e., the “compliers”), rather than the average effect for all regions. In this setting, the estimate is primarily informed by high-exposure, high-density regions – such as London, the South West, the East of England, and the North West – that exhibit stronger first-stage responsiveness to the instrument.

case that displacement takes place at a finer spatial resolution or over a longer time period than our model can identify.

### **6.3. LIMITATIONS**

Several limitations should be noted. First, the analysis is conducted at the ITL1 level due to data availability. This constrains the cross-sectional sample and, in turn, instrument strength and precision, particularly when influential regions are excluded. Road specifications additionally omit Scotland due to data gaps, which further weakens identification. The use of ITL1 regions, while necessary given current transport investment data, also limits our ability to capture the localised economic effects of investment. Many productivity and accessibility gains from transport schemes operate at subregional or local scales, which are averaged out at this level of aggregation. Developing consistent, long-run datasets on transport investment at finer spatial scales (i.e., local authority level) would substantially strengthen future causal analysis. Moreover, at this level of aggregation, spillovers are difficult to isolate credibly with the current instruments.

Second, although we consider the risk limited in our setting, historical infrastructure density may correlate with persistent agglomeration forces or long-run growth trajectories not fully absorbed by fixed effects. For example, historically rail-dense areas (e.g., parts of the North West) may also host enduring industrial bases that influence contemporary growth through channels unrelated to current transport spending, creating scope for residual confounding. It is likely that this would upwardly bias our coefficient. In the appendix, we test for this by including regional time trends, and do not find significantly different results.

Third, the reported parameters represent average effects over 2001-2023 (2009-2023 for mode-specific regressions) and may vary with institutional changes, investment composition, and macroeconomic conditions. Policy use should therefore interpret results as period- and context-specific.

## 7. CONCLUSION

This annex addresses a clear gap in the UK evidence base by estimating a UK-specific, causally identified elasticity of economic output with respect to transport capital. We assemble a balanced ITL1 panel (11 regions, 2001-2023) and implement an IV strategy based on Bartik shift-share instruments that interact historical infrastructure density with lagged national budget shocks. The identification logic is set out in Section 3.3: historical exposures are predetermined, national shocks are aggregate and common across regions, and the interaction provides plausibly exogenous variation in investment. TWFE absorb time-invariant regional heterogeneity and UK-wide shocks. Empirically, first-stage diagnostics and over-identification tests support instrument validity in the preferred specifications (especially rail). Together, these features support a causal interpretation of the IV estimates.

**The headline result is a per-capita elasticity of about 0.07 from the just-identified rail IV model, with a broader IV range of 0.06-0.08 across specifications.** Robustness checks using totals vs per-capita scaling, alternative instrument sets, and exclusion of influential observations generally support the core finding, while also underscoring the importance of strong instruments and adequate sample variation; in particular, excluding London leaves point estimates positive but materially weakens first-stage strength and statistical precision.

For policy, these UK-calibrated elasticities provide a clear, evidence-based parameter for DfT to use in strategic decision-making. They provide a high-level benchmark on the overall relationship between transport investment and short-run economic output, primarily reflecting the demand-side stimulus associated with investment spending. The elasticities are intended to inform transport policy-making, but are not directly comparable to the supply-side impacts typically estimated through transport appraisals, which focus on the productivity and connectivity gains from completed infrastructure. They offer a UK-specific benchmark against which to compare the international evidence synthesised in the meta-analysis. The results from this study and the meta-analysis are synthesised in the main report.

There is scope for further research. Work at a finer spatial resolution would increase identifying variation and allow a richer examination of heterogeneity across places. Improving mode-specific exposure measures, particularly for roads, would strengthen instrument performance and precision. Developing fully dynamic specifications would help distinguish short- from medium-run responses more transparently. Developing a full Spatial Durbin Model would help refine and identify spillover effects. Constructing a valid, long duration, and spatially disaggregated series of transport capital stock would assist with identify the long-run structural elasticity of output with respect to marginal improvements in transport stock. Progress along these lines would sharpen UK policy inference and clarify the conditions under which transport investment yields the largest economic returns.

## Appendix A ROBUSTNESS CHECKS

### A.1. TOTAL GVA

Table A.1 reports estimates with variables in levels (logs of totals). In this specification, the coefficient is the elasticity of total GVA with respect to total transport investment, so it reflects both scale (larger regions have more people and larger economies) and intensity effects. Unlike the per-capita models, it does not normalise by population, so differences in regional size can drive co-movement in both output and transport investment.

Table A.1: Elasticity of regional GVA with respect to transport investment

| In(GVA)                           | (1) OLS          | (2) IV: Rail        | (3) IV: Road      | (5) IV: Rail + Road |
|-----------------------------------|------------------|---------------------|-------------------|---------------------|
| <b>Second stage</b>               |                  |                     |                   |                     |
| ln(TI)                            | 0.024<br>(0.024) | 0.064**<br>(0.020)  | 0.067<br>(0.038)  | 0.064+<br>(0.032)   |
| <b>First stage:</b>               |                  |                     |                   |                     |
| Rail Density<br>× Nat Budget (L1) |                  | 0.022***<br>(0.005) |                   | 0.006<br>(0.009)    |
| Road Density<br>× Nat Budget (L1) |                  |                     | 0.030*<br>(0.011) | 0.024<br>(0.020)    |
| <b>Model diagnostics</b>          |                  |                     |                   |                     |
| Observations                      | 276              | 231                 | 210               | 210                 |
| R Squared (second stage)          | 0.998            | 0.998               | 0.998             | 0.998               |
| Weak-IV F                         | N/A              | KP F = 21.95        | KP F = 6.63       | CD F = 2.44         |
| Sargan J                          | N/A              | N/A                 | N/A               | p = 0.842           |

Notes: Two-way fixed effects (region and year) in all columns; standard errors clustered at the region level in parentheses. KP = Kleibergen-Paap Wald F; CD = Cragg-Donald F. Sargan J is an overidentification test; N/A indicates not applicable for exactly-identified models.

Significance: +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

The baseline OLS regression shows no statistically significant association between total transport investment and regional GVA. The coefficient on ln(TI) is small and imprecisely estimated (0.024, s.e. 0.026), consistent with the hypothesis that endogeneity biases the OLS estimate downward – for instance, if higher public investment is systematically directed toward slower-growing regions.

By contrast, the IV estimates reveal a stronger and statistically significant elasticity once endogeneity is addressed. Using the rail Bartik instrument, the estimated elasticity of regional GVA with respect to transport investment rises to 0.064 (s.e. 0.020), statistically significant at the 1% level. The first-stage Kleibergen-Paap statistic ( $F = 21.95$ ) lies comfortably above conventional weak-instrument thresholds, providing evidence of instrument relevance. This estimate implies that a 10% increase in total transport investment is associated with a 0.6% increase in GVA, holding other factors constant.

The road-based IV specification yields a similarly positive elasticity of 0.067 (s.e. 0.038), though not statistically significant at conventional levels. The first stage is somewhat weaker (KP  $F = 6.63$ ), implying potential weak-instrument concerns. Nonetheless, the direction and size of the coefficient are consistent with the rail-based result, suggesting that OLS likely understates the true effect due to endogenous investment targeting.

When both rail and road instruments are included in an over-identified specification, the estimated elasticity is 0.064 (s.e. 0.032). Instrument strength remains modest (Cragg-Donald  $F = 2.44$ ), although the Sargan J test ( $p = 0.84$ ) does not reject the null of joint instrument exogeneity, supporting validity of the instrument set. The coefficient closely matches those from the single-instrument specifications, indicating that the over-identified model captures the same underlying causal relationship but with somewhat lower precision.

Taken together, the total investment specifications imply a causal elasticity of regional GVA with respect to transport investment in the range of 0.06-0.07, while the OLS estimate remains indistinguishable from zero. These findings support the conclusion that transport investment exerts a positive and economically meaningful effect on regional economic output once endogeneity is addressed.

Finally, the elasticities using total investment are smaller than those in the per-capita models (around 0.07-0.08 in Table 5.1). This difference likely reflects that the level specifications mix scale and intensity effects: larger regions receive greater absolute investment but also have proportionally larger economies. In contrast, the per-capita approach isolates the productivity-relevant component of investment intensity, providing a clearer measure of responsiveness per person rather than per pound of total spending.

## A.2. INCLUDING REGIONAL TIME TRENDS

Including region-specific linear time trends provides a useful robustness check on our preferred IV estimates. Regional trends allow each ITL1 region to follow its own underlying long-run growth path, above and beyond the fixed differences absorbed by region fixed effects and the national macroeconomic shocks captured by year fixed effects. This specification helps address concerns that the estimated relationship between transport investment and regional GVA might be driven by pre-existing regional growth trajectories – for example, if regions with higher historic rail density were already on steeper long-run growth paths for reasons unrelated to transport infrastructure. By controlling for these region-specific trends, the model relies only on deviations from each region’s long-run trajectory, tightening the exclusion restriction and reducing the risk of bias from correlated long-run structural forces.

Table A.2: Elasticity of regional GVA per capita to transport investment per capita, accounting for regional trends

| ln(GVA pc)                        | (1) OLS           | (2) IV: Rail        | (3) IV: Road      | (5) IV: Rail + Road |
|-----------------------------------|-------------------|---------------------|-------------------|---------------------|
| <b>Second stage</b>               |                   |                     |                   |                     |
| ln(TI pc)                         | -0.001<br>(0.017) | 0.065**<br>(0.018)  | 0.058<br>(0.036)  | 0.059+<br>(0.029)   |
| <b>First stage:</b>               |                   |                     |                   |                     |
| Rail Density<br>× Nat Budget (L1) |                   | 0.022***<br>(0.004) |                   | 0.007<br>(0.010)    |
| Road Density<br>× Nat Budget (L1) |                   |                     | 0.030*<br>(0.011) | 0.022<br>(0.020)    |
| <b>Model diagnostics</b>          |                   |                     |                   |                     |
| Observations                      | 276               | 231                 | 210               | 210                 |
| R Squared (second stage)          | 0.995             | 0.997               | 0.997             | 0.997               |
| Weak-IV F                         | N/A               | KP F = 22.73        | KP F = 6.30       | CD F = 3.80         |
| Sargan J                          | N/A               | N/A                 | N/A               | p = 0.92            |

Notes: Two-way fixed effects (region and year) in all columns; standard errors clustered at the region level in parentheses. KP = Kleibergen-Paap Wald F; CD = Cragg-Donald F. Sargan J is an overidentification test; N/A indicates not applicable for exactly-identified models.

Significance: +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

Table A.3 indicates that including regional trends does **not overturn the core findings**. The OLS estimate remains close to zero. The preferred IV estimate based on the rail-density instrument remains positive, statistically significant, and similar in magnitude to the baseline model without trends (0.065 vs. around 0.07 in Table 5.1). This suggests that the main rail-based IV estimate is not driven by long-run differential growth paths across regions.

For road investment, the instrument continues to operate, with positive and marginally significant first-stage coefficient, though the second-stage elasticity remains more imprecise. When both instruments are included together, the local elasticity remains positive (around 0.06) but statistical precision falls, reflecting weaker

instrument relevant once region-specific trends and multiple instruments are jointly included. Importantly, the Kleibergen-Paap F statistic remains above conventional weak-IV thresholds for the rail-only specification, indicating the first stage is still adequately powered when the rail density instrument is used on its own.

### A.3. PER CAPITA, EXCLUDING LONDON

Table A.3 examines the sensitivity of the estimated transport investment elasticities to the exclusion of London, which represents both the UK's largest regional economy and a potential source of leverage for the instruments. Excluding London tests whether the estimated causal relationships are driven by this highly influential observation or remain stable in a more homogeneous regional sample.

Table A.3: Elasticity of regional GVA per capita to transport investment per capita, excluding London

| In(GVA pc)                        | (1) OLS           | (2) IV: Rail     | (3) IV: Road     | (5) IV: Rail + Road |
|-----------------------------------|-------------------|------------------|------------------|---------------------|
| <b>Second stage</b>               |                   |                  |                  |                     |
| ln(TI pc)                         | -0.002<br>(0.017) | 0.096<br>(0.066) | 0.060<br>(0.144) | 0.080<br>(0.158)    |
| <b>First stage:</b>               |                   |                  |                  |                     |
| Rail Density<br>× Nat Budget (L1) |                   | 0.038<br>(0.021) |                  | 0.023<br>(0.053)    |
| Road Density<br>× Nat Budget (L1) |                   |                  | 0.023<br>(0.027) | 0.028<br>(0.036)    |
| <b>Model diagnostics</b>          |                   |                  |                  |                     |
| Observations                      | 253               | 210              | 189              | 189                 |
| R Squared (second stage)          | 0.976             | 0.982            | 0.983            | 0.983               |
| Weak-IV F                         | N/A               | KP F = 2.77      | KP F = 0.64      | CD F = 0.47         |
| Sargan J                          | N/A               | N/A              | N/A              | p = 0.769           |

Notes: Two-way fixed effects (region and year) in all columns; standard errors clustered at the region level in parentheses. KP = Kleibergen-Paap Wald F; CD = Cragg-Donald F. Sargan J is an overidentification test; N/A indicates not applicable for exactly-identified models.

Significance: +  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

The OLS estimate without London shows no statistically significant relationship between transport investment per capita and regional GVA per capita. The coefficient on ln(TI pc) is small and statistically insignificant (−0.002, s.e. 0.017), offering no evidence of a systematic association once the capital region is excluded. The loss of precision relative to the full-sample results reflects the reduced cross-sectional variance in investment intensity and economic output after removing the largest region.

The rail-based IV estimate is positive (0.096, s.e. 0.066) but statistically insignificant, with a weak first stage (Kleibergen–Paap F = 2.8). The road-based IV estimate is also small and insignificant (0.060, s.e. 0.144), accompanied by an even weaker first stage (KP F = 0.64). Both estimates are within sampling noise of zero, consistent with limited identifying power once London is excluded.

When both instruments are combined in an over-identified specification, the point estimate is 0.080 (s.e. 0.158) but remains statistically indistinguishable from zero. First-stage strength remains weak (Cragg–Donald F = 0.47), and although the Sargan test ( $p = 0.77$ ) does not reject joint exogeneity, the overall diagnostics indicate that the instruments are too weak to deliver meaningful inference.

Across all specifications, the London-excluded results display weak first stages, and a loss of statistical precision. However, point estimates do not change significantly or flip sign, characteristic of weak-IV bias rather than genuine changes in underlying elasticities. The absence of significant coefficients does not imply the absence of a causal effect, but rather that removing London substantially reduces the identifying variation in the Bartik-style instruments.

Taken together, these results demonstrate that London contributes materially to the variation used for identification, especially in the rail exposure component. Excluding the region leaves too little independent variation in investment shocks to support reliable inference. Consequently, the London-excluded estimates should be interpreted as a robustness stress test, not as an alternative set of effect sizes.

For policy interpretation and inference, the full ITL1 sample, where instrument strength and diagnostic statistics are satisfactory, remains the most credible empirical basis for estimating the elasticity of regional output with respect to transport investment.

## Appendix B DATA SUMMARY

Table B.1: Variable definitions, transformations and sources

| Variable                  | Definition                                                                | Transformations                                                                        | Source(s)                                                                                                                                                                | Notes                                                                   |
|---------------------------|---------------------------------------------------------------------------|----------------------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------|
| GVA                       | Regional gross value added at current basic prices.                       | £ million (log-transformed for estimation); deflated to 2024 prices using GDP deflator | ONS, <i>Regional Gross Value Added (Balanced) at current basic prices</i> , ITL1, 1998-2023                                                                              | Used as a proxy for regional economic output.                           |
| Population                | Mid-year resident population, ITL1                                        | Persons                                                                                | ONS, <i>Population Estimates</i>                                                                                                                                         | Used to calculate per-capita variables                                  |
| Transport investment      | Total public capital expenditure on transport (all modes)                 | £ million (log); deflated to 2024 prices using GDP deflator                            | HM Treasury, <i>County and Regional Analysis (CRA)</i> and HM Treasury, <i>Public Expenditure Statistical Analysis (PESA)</i> ; 2024, 2019, 2014, 2009 and 2007 vintages | Series for sub-function only available in 2014, 2019 and 2024 vintages. |
| National transport budget | Aggregate UK transport capital spending (all regions)                     | £ million; first-difference, lagged 1 year; deflated to 2024 prices using GDP deflator | HM Treasury, <i>CRA</i>                                                                                                                                                  | Used to construct time-varying “shock” component of Bartik instrument.  |
| Rail investment           | Capital expenditure on rail transport (CRA sub-function)                  | £ million (log); deflated to 2024 prices using GDP deflator                            | HM Treasury, <i>CRA</i>                                                                                                                                                  | Available 2009-2023                                                     |
| Road investment           | Capital expenditure on local and national roads (CRA sub-functions)       | £ million (log); deflated to 2024 prices using GDP deflator                            | HM Treasury, <i>CRA</i>                                                                                                                                                  | Available 2009-2023                                                     |
| Rail density              | Route-kilometres of rail per km <sup>2</sup> of region in 1950            | Ratio (standardised z-score for IV)                                                    | Cambridge Group for the History of Population and Social Structure                                                                                                       | Not available for Northern Ireland                                      |
| Road density              | Route-kilometres of turnpike roads per km <sup>2</sup> of region in 1892. | Ratio (standardised z-score for IV)                                                    | Cambridge Group for the History of Population and Social Structure                                                                                                       | Not available for Northern Ireland and Scotland                         |
| Rail instrument           | Interaction: rail density x lagged change in national transport budget    | Normalised                                                                             | Constructed by CEPA from above sources                                                                                                                                   | Used as IV                                                              |

| Variable                                        | Definition                                                             | Transformations                          | Source(s)                                    | Notes                                                    |
|-------------------------------------------------|------------------------------------------------------------------------|------------------------------------------|----------------------------------------------|----------------------------------------------------------|
| Road instrument                                 | Interaction: road density x lagged change in national transport budget | Normalised                               | Constructed by CEPA from above sources       | Used as IV                                               |
| Spatial lag of transport investment (W x TI pc) | Weighted average of neighbours' transport investment per capita        | Log £ per person; weights row-normalised | CEPA computation using ILT1 adjacency matrix | Captures potential spillovers from neighbouring regions. |
| Spatial lag of instruments                      | Weighted average of neighbours' Bartik instrument                      | Normalised                               | CEPA computation                             | Used as IV for spatial spillover term.                   |
| Year fixed effects                              | Dummy for each calendar year                                           | 0/1 indicator                            | Derived                                      | Controls for national shocks common to all regions.      |
| Region fixed effects                            | Dummy for each ITL1 region                                             | 0/1 indicator                            | Derived                                      | Controls for time-invariant regional characteristics     |



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