Transport Investment and Economic Growth Elasticities

Technical Annex I: Meta-analysis

Department for Transport

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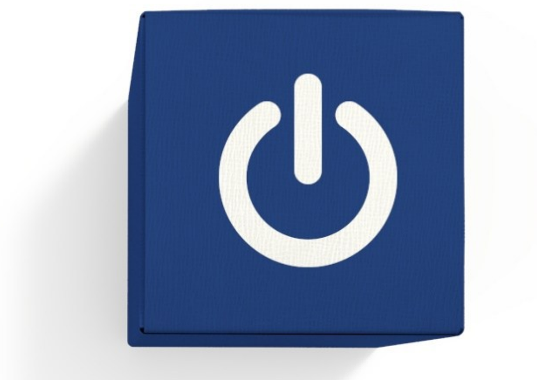

FINAL REPORT

Important notice

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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 programme comprises two separate strands of analysis:

- **Meta-analysis:** a meta-analysis of elasticity estimates, pooling evidence across studies to generate generalisable benchmarks and identify patterns that are not random or study-specific but rather consistent and recurring across studies.
- **UK region 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.

The main report triangulates results from both these pieces of analysis, delivering a rich and credible basis for policy design and evaluation. This technical annex sets out the details of the meta-analysis. It is designed as a standalone document: while the main report presents headline findings for policy audiences, this annex provides the methodological detail and academic framing needed to assure readers of the rigour of our approach.

1.1. WHY META-ANALYSIS?

Transport infrastructure investment is often justified by claims of productivity growth, agglomeration benefits, and wider economic impacts. Yet despite decades of empirical research, the estimated elasticity of output with respect to transport investment varies considerably – across countries, time periods, transport modes, and methodological approaches. For decision-makers, this heterogeneity creates uncertainty about which estimates are reliable and transferable to the UK context.

Meta-analysis provides a transparent and statistically rigorous way of reconciling this variation. By pooling results across studies and modelling systematic differences in estimation, we can derive central benchmarks while also understanding how elasticities vary by context. This is increasingly recognised as best practice in economics and policy evaluation – whereas a traditional literature review can only report that estimates differ, meta-analysis uses quantitative methods to explain why they differ by relating elasticities to model, data and contextual choices.

1.2. EXISTING EVIDENCE BASE

Our work builds on a growing body of meta-analyses, systematic reviews, and evidence syntheses in transport economics. The foundational transport-specific meta-analysis by Melo, Graham and Brage-Ardao (2013)[1] quantified elasticities from 33 studies and highlighted substantial variation across transport modes, industries and geographies. However, this study placed a lot of weight on US evidence, which estimated a systematically higher elasticity than European studies. More recently, Holmgren & Merkel (2017)[2] broaden this to consider infrastructure more generally, with 776 estimates, showing the elasticities are sensitive to infrastructure type, industry, and country. Other works, such as the recent review by Välilä (2025)[3], confirm that while transport infrastructure is generally positively associated with economic outcomes, effect sizes differ substantially depending on study features.

[1] Melo, P. C., Graham, D. J., & 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.

[2] Holmgren, J., & Merkel, A. (2017). Much ado about nothing? A meta-analysis of the relationship between infrastructure and economic growth. *Research in Transportation Economics, 63*, 13–26.

[3] Välilä, T. (2025). The economic impact of transport infrastructure: A review. *Transport Reviews, 45*(2), 247–268.

In the UK policy context, the What Works Centre for Local Economic Growth (2015; 2021 update)[4] [5] undertook systematic reviews of transport investment impacts, highlighting consistent evidence of employment and productivity effects but also raising concerns about methodological robustness and displacement. More recently, DfT has commissioned targeted evidence reviews on economic outcomes including unemployment, productivity, and gentrification.[6] These confirm the association between transport and productivity but stress that the scale, direction, and persistence of impacts remain highly context dependent.

Taken together, the existing literature highlights the need for an updated transport-specific meta-analysis that:

- **Incorporates recent studies** beyond those covered by Melo et al. (2013);
- **Applies stringent quality filters,** retaining only studies with robust identification strategies;
- **Emphasises UK and European evidence** (subject to the above quality filters), while situating it within an international benchmark; and
- **Quantifies heterogeneity** across transport modes, regions, and econometric methods.

1.3. Objectives of this annex

This technical annex has four main objectives:

1. To **summarise the international literature** on transport investment elasticities.
2. To **present pooled estimates** of the elasticity of output with respect to transport investment, using random-effect meta-regression models.
3. To **explore sources of heterogeneity** in reported elasticities, including transport mode, geography, and estimation strategy.
4. To **assess robustness and bias**, including tests for publication bias and sensitivity to study quality.

The remainder of this annex is organised as follows. Section 2 describes the methodology, including literature search, inclusion criteria, and the statistical framework for meta-analysis. Section 3 summarises the data collected and provides descriptive evidence on the distribution of elasticity estimates. Section 4 presents the results, covering pooled estimates, heterogeneity analyses, and publication bias. Section 5 discusses the findings in light of previous meta-analyses and policy debates. Section 6 concludes.

[4] What Works Centre for Local Economic Growth. (2015). *Evidence review 7: Transport*. London: WWCLEG.

[5] What Works Centre for Local Economic Growth. (2021). *Transport update: Evidence review*. London: WWCLEG.

[6] Department for Transport (DfT). (2024). *The economic impacts of transport interventions: Evidence review*. London: Department for Transport.

CEPA economics matters

2. METHODOLOGY

The methodological objective of this workstream is to provide a transparent and replicable quantitative synthesis of the international evidence on the elasticity of economic output with respect to transport capital investment. To achieve this, we combine:

1. **A systematic literature review** to identify and curate relevant studies;
2. **Data extraction and coding** of study characteristics;
3. **Random-effects meta-analysis** to obtain pooled central elasticity estimates;
4. **Meta-regression** to model systematic heterogeneity across studies; and
5. **Robustness checks and diagnostics**, including publication bias and sensitivity analyses.

This section sets out each of these stages.[7]

2.1. Literature search and inclusion criteria

We undertook a systematic literature search designed to identify all relevant empirical studies estimating the elasticity of economic output with respect to transport capital investment. The process was structured around three main stages: initial search, long list construction, and short list screening.

Search protocol

We developed Boolean search strings combing transport investment terms, economic outcome terms, and econometric terms, as detailed in Box 1. These were implemented in Scopus and Web of Science and were supplemented with targeted searches in Google Scholar. To ensure coverage of grey literature, we reviewed key references in prior reviews[8] [9] and followed citation chains. Most relevant grey literature was captured through the systematic search.

Box 1: Boolean search used in Scopus

TITLE-ABS-KEY (("transport infrastructure" OR "transport capital" OR "transport investment" OR "rail investment" OR "road investment" OR "public transport investment")

AND

("GDP" OR "economic output" OR "economic growth" OR productivity)

AND

(elasticity OR "marginal effect" OR regression OR "panel data"))

Inclusion criteria

We applied a set of shortlist criteria aligned with the Statement of Requirements and agreed by DfT:

- **Geography:** Only UK and European studies were retained for the meta-analysis.
- **Mode of transport:** Road, rail, public transport, and active transport were included. Studies of *general public capital* without a transport-specific breakdown were excluded.
- **Outcome variables:** Gross Domestic Product (GDP), Gross Value Added (GVA), or productivity.

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[7] All coding and estimation was undertaken in R, using packages such a *metafor* and *clubSandwich*.

[8] What Works Centre for Local Economic Growth. (2015). *Evidence review 7: Transport*. London: WWCLEG.

[9] Department for Transport (DfT). (2024). *The economic impacts of transport interventions: Evidence review*. London: Department for Transport.

- **Methodological quality:** Only studies rated using the Maryland Scientific Method Scale (SMS) at a level 3 or above were included. [10]

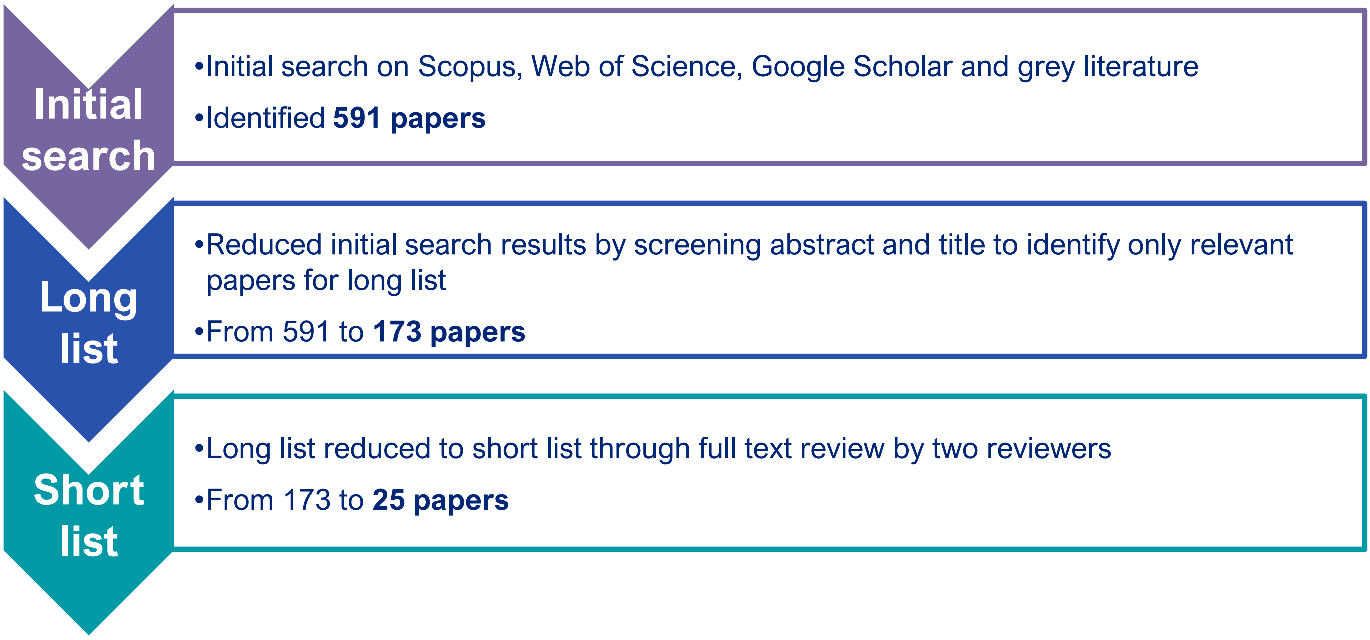
- **Publication date:** 2010 onwards, to ensure policy relevance and contemporary applicability.

Screening results

The results of our screening procedure are summarised in Figure 2.1 below. The same criteria were used when reducing from the initial search to the long list, and from the long list to the short list. The first stage of screening was done by reading the title and abstract only, identifying where papers clearly didn't meet geographic, time period, or outcome variable criteria. This also screened out literature reviews and other meta-analyses. The most common reasons for excluding papers at this stage were because they focused on broader public capital or were set outside the UK/EU.

In the second stage, two reviewers independently read the full text of each paper. The most common reason for exclusion at this stage was papers not meeting the methodological quality standards. There was a high level of agreement between the two reviewers of which papers should be included in the short list. Zhang and Cheng (2023)[11] was the only paper in our long list that focussed on the UK but did not pass the quality screen as their time series analysis did not discuss causality or identification. UK data was used in numerous OECD / EU / Global studies included in the sample.

Figure 2.1: Summary of screening results.



2.2. Data extraction and coding

For each shortlisted study, we extracted elasticity estimates and coded study-level characteristics to serve as potential moderators – study characteristics that may be correlated with the size of the estimate – in the meta-

[10] SMS level 3 compares treated groups pre- and post-treatment, and against a control group, while discussing comparability (and controlling for differences) between treated and control groups. This includes well developed panel data methods, difference-in-difference (DiD) designs, instrumental variables (IV), regression discontinuity designs, models with clear identification arguments, exploitation of natural experiments, and randomised control trails (RCTs). For more details on the scale please visit https://whatworksgrowth.org/resource-library/the-maryland-scientific-methods-scale-sms/.

[11] Zhang, Y. and Cheng, L. (2023). *The role of transport infrastructure in economic growth: Empirical evidence in the UK*. Transport Policy

regression. Table 2.1 summarises the variables coded from each study and used as moderators in the meta-regression.

Table 2.1: Coding framework for extracted study characteristics

Variable	Description	Coding / categories
Elasticity estimate	Reported elasticity of output with respect to transport capital/infrastructure.	Continuous value
Standard error	Standard error of the reported elasticity estimate.	Continuous value
Transport mode	Mode of transport to which the estimate refers.	Road, Rail, Airport, Port, All
Measure of infrastructure	Type of measure used for transport infrastructure.	Physical (e.g. km of roads); Monetary (e.g. investment value)
Geographic coverage	Country or region included in the study.	Country name, OECD, EU, Global
Industry / sector	Industry or sector to which the estimate applies.	Manufacturing, Services, Whole economy
Time horizon	Whether the elasticity is short-run or long-run.[12]	Short-run, Long-run
Level of aggregation	Geographic or statistical level of data aggregation.	Regional, National, Cross-national
Econometric method	General econometric approach used to obtain the elasticity.	Dynamic panel (GMM), Static panel (TWFE), Time series (VAR)
Use of instrumental variables	Indicator for the use of an instrumental variable approach to address endogeneity.	0 = no IV, 1 = IV
Specification of fixed/random effects	Inclusion of time, region, or random effects in the model.	0 = none; 1 = time FE; 2 = region FE; 3 = time & region FE; 4 = random effects
Additional controls	Indicators for inclusion of supplementary model controls.	Spillovers (0/1/2); Corruption (0/1); Innovation (0/1); Non-linear terms (0/1)
Study quality (SMS score)	Maryland Scientific Methods Scale score used as study quality filter.	3 = panel with controls, 4 = quasi-experimental / IV, 5 = RCT
Year of publication	Year the study was published.	YYYY
Study identifier	Unique identifier linking estimates to a specific study.	100xxx
Model label	Name or abbreviation of the model used in the original study.	As reported in study

[12] Several studies explicitly estimated long-run or short-run effects. Otherwise, we defined any estimated effect of over one year as a long-run effect.

2.3. STATISTICAL FRAMEWORK

Random-effects meta-analysis

The central challenge in meta-analysis is that estimates differ not only because of sampling error but also because of genuine variation across studies (e.g., in geography, time period, or transport mode). A random-effects model accounts for this by assuming that the true elasticity varies across studies around an overall mean.

Recognising that the same paper may report multiple elasticity estimates under different model specifications (i.e., with different controls or fixed effects), let $\hat{\beta}_{ij}$ denote the reported estimate of the elasticity from study j under specification i. In the meta-analysis, $\hat{\beta}_{ij}$ is the observed dependent variable and is treated as the true underlying elasticity, β, plus a study specific error term, and specification specific error term. [13]

Formally, the model we estimate is

$$\hat{\beta}_{ij} = \beta + u_j + \varepsilon_{ij}$$

Where $\hat{\beta}_{ij}$ is the estimated elasticity from study j, $u_j \sim N(0, \tau^2)$ captures the between-study heterogeneity and $\varepsilon_{ij} \sim N(0, \sigma^2)$ reflects within-study sampling error.

We estimate the model using restricted maximum likelihood (REML), which provides unbiased estimates of the between-study variance τ^2. The pooled estimate of β can be interpreted as the average elasticity across all included studies, weighted by their precision but allowing for heterogeneity.

This approach follows established practice in the transport literature. For example, Melo et al. (2013) employ a random-effects specification to pool over 560 elasticity estimates, while Holmgren and Merkel (2017) adopt a similar framework in their infrastructure meta-analysis. We diverge slightly by adopting REML rather than the DerSimonian-Laird (DSL) estimator (used in earlier studies). Langan et al. (2019)[14] find that the DSL estimator, while often used, can be (negatively) biased and other methods are available. They recommend REML, as it balances unbiasedness and efficiency, and is more reliable especially in small samples.

Meta-regression

While the random-effects model summarises the central tendency, it does not explain why estimates vary. To address this, we employ meta-regression, which relates reported elasticities to coded study characteristics:

$$\hat{\beta}_{ij} = \alpha + \boldsymbol{\delta}'\boldsymbol{Z_{ij}} + u_j + \varepsilon_{ij}$$

Where $\boldsymbol{Z_{ij}}$ is a vector of moderators, such as transport mode, econometric method, time horizon. The vector of coefficients δ captures how each characteristic systematically shifts the reported elasticity. A positive (negative) δ_k means studies with feature k report higher (lower) elasticities on average, holding other factors constant. Again, this is consistent with prior transport meta-analyses including Melo et al. (2013) and Holmgren and Merkel (2017).

Hypotheses tested

In addition to producing a central estimate and uncertainty range, this approach allows us to formally test hypotheses such as:

- **Mode effects:** Are road elasticities larger than rail or other modes?
- **Geographic effects:** Do cross-country studies yield different elasticities compared to specific countries?
- **Time horizon effects:** Are long-run elasticities larger than short-run ones?

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[13] The hat in $\hat{\beta}_{ij}$ indicates that this is an estimate reported by the literature, not a parameter of the meta-regression. This notation is standard in the meta-analysis literature (see, for example, Melo et al., 2013).

[14] Langan, D., Higgins, J. P. T., Jackson, D., Bowden, J., Veroniki, A. A., Kontopantelis, E., Viechtbauer, W., & Simmonds, M. (2019). A comparison of heterogeneity variance estimators in simulated meta-analyses. *Research Synthesis Methods, 10*(1), 83–98.

- **Methodological effects:** Do dynamic estimates differ systematically from OLS estimates?

Interpretation and limitations

The pooled estimate should be interpreted as the average elasticity conditional on the included studies and coding framework. It is not a single "true" elasticity, but rather a benchmark around which real-world estimates vary. The meta-regression identifies systematic sources of this variation but cannot eliminate all unobserved heterogeneity.

2.4. Robustness and publication bias

To ensure our results are robust to data limitations and modelling assumptions, we undertook a range of sensitivity checks and diagnostic tests.

Imputation of missing standard errors

In a minority of cases, studies reported coefficient estimates and statistical significance levels but did not provide standard errors. To retain these observations, we imputed the corresponding standard errors as the largest value consistent with the reported significance threshold. This is a conservative approach: it likely overestimates the true uncertainty and therefore gives these studies less weight in the pooled analysis. As a robustness check, we re-estimated the pooled regression excluding imputed observations and results were substantively unchanged.

Treatment of extreme variances

Meta-analysis weights observations by the inverse of their sampling variance. As a result, estimates with very small, reported variances can disproportionately influence the pooled result. To mitigate this, we winsorised sampling variances to the 5th and 95th percentiles of the distribution and re-estimated the models.[15] This prevents undue leverage from extreme values while retaining the information contained in all studies.

Publication bias

Publication bias arises when statistically significant results are more likely to be published, creating a bias in reported elasticities away from zero. We assessed this in three ways: visual inspection of funnel plots, formal regression tests, and, where appropriate, trim-and-fill corrections.

Our primary formal test was the Egger regression test (Egger et al., 1997)[16]. The test is run by regressing the standardised effect size (the reported elasticity divided by its standard error, i.e., a t-statistic) on the inverse of the standard error:

$$\frac{\hat{\beta}_{ij}}{SE_{ij}} = \alpha + \lambda \cdot \frac{1}{SE_{ij}} + \varepsilon_{ij}$$

Under the null hypothesis of no publication bias, the intercept α should be zero, meaning that effect sizes are symmetrically distributed around the pooled mean, regardless of study precision. A statistically significant non-zero intercept indicates asymmetry in the funnel plot, consistent with selective reporting (e.g., small studies showing only large positive elasticities being published).

The Egger test works because, in the absence of bias, there should be no systematic relationship between effect size and study precision. Any such relationship is evidence that results are being "pushed" in one direction, especially in small sample studies.

[15] Winsorising refers to capping extreme values above a chosen upper percentile (e.g., 95th percentile) and flooring those below a lower percentile (e.g., 5th percentile).

[16] Egger, M., Smith, G. D., Schneider, M., & Minder, C. (1997). Bias in meta-analysis detected by a simple, graphical test. *BMJ, 315*(7109), 629–634.

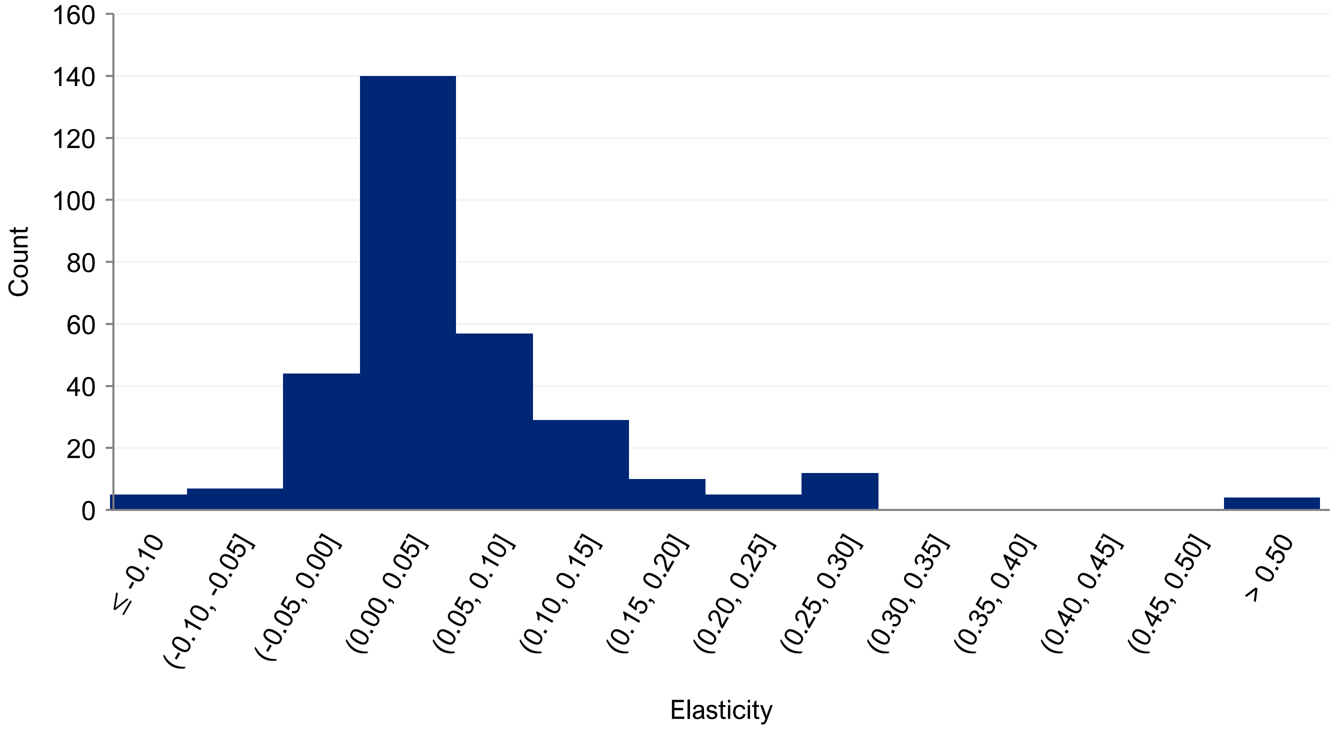
3. DATA SUMMARY

The final database contains 22 studies and 317 elasticity estimates, reflecting variation across transport modes, geographies, time horizons, and econometric approaches. While smaller than earlier transport and general infrastructure meta-analyses (e.g. Melo et al., 2013), this reflects more stringent inclusion criteria rather than data limitations, and the sample is sufficient for robust meta-analytic inference. This section summarises the distribution of estimates and highlights key patterns in the evidence base.Distribution of elasticities

Reported elasticities vary substantially. The mean estimate is 0.064, while the median is considerably smaller at 0.026. The distribution is right-skewed. Most estimates lie close to zero, with a long tail of larger positive elasticities. The largest reported elasticity is 1.77, while the most negative estimate is -0.92. The dispersion of uncertainty is also notable: standard errors have a median value of 0.02, but in some cases exceed 0.80.

Figure 3.1 illustrates the distribution of reported elasticities. Most estimates are clustered just above zero, with a small number of studies reporting markedly larger effects. This skew mirrors the findings of earlier meta-analyses, where the bulk of estimates suggest modest but positive effects of transport investment, with occasional high outliers.

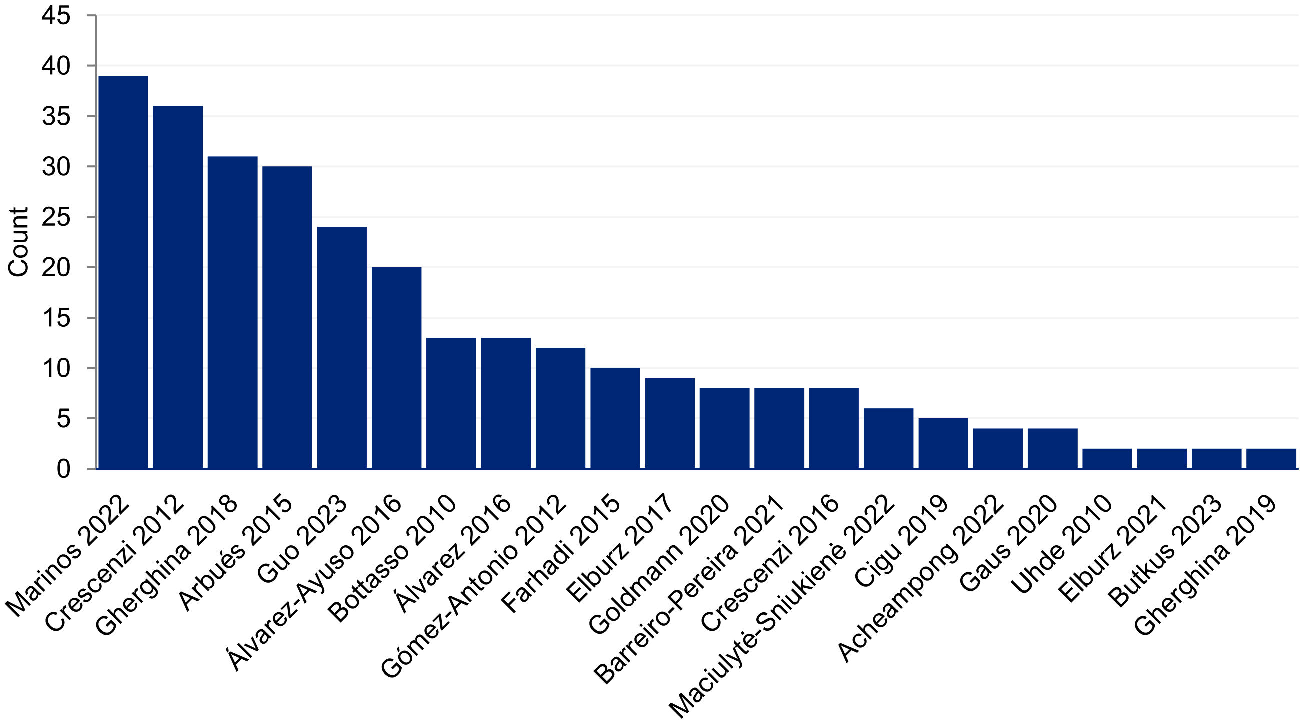
Figure 3.1: Distribution of reported elasticities



Source: CEPA analysis

There is also evidence of variation across studies in the number of reported estimates. Some studies provide a large set of specifications (e.g., varying controls, modes, or horizons), while other report only one or two. Figure 3.2 shows the distribution of the number of estimates per study. This highlights the need to cluster standard errors at the study level in meta-regressions to avoid overweighting the studies that report many estimates.

Figure 3.2: Number of estimates per study



Source: CEPA analysis

3.1. Breakdown by study characteristics

The evidence base spans multiple modes, geographies, timeframes, and econometric approaches. The key features are:

- **Transport modes:** Around one-fifth of estimates relate to general transport investment and half focus specifically on roads. Rail, airport, and port studies each contribute roughly 10% of estimates. Average elasticity values are similar for road (0.085) and general transport investment (0.096), and lower for all other modes.
- **Geographic coverage:** Most estimates come from European or cross-country analyses, with an average elasticity of 0.094. Spain accounts for around 30% of all estimates (primarily on road investment) with a notably lower average elasticity (0.029), while Greece also features prominently (0.022). No UK-specific studies exist in our shortlist, underscoring the need for bespoke regression analysis.
- **Time horizon:** Roughly 60% of estimates capture short-run effects, or how GDP changes with a contemporaneous change in transport investment. The average short-run elasticity is 0.070, compared with 0.053 for estimates that consider effects over a time-horizon longer than one year.
- **Measurement of infrastructure:** Just over half of the studies use monetary indicators of transport infrastructure (e.g., investment values), yielding an average elasticity of 0.054. The remainder rely on physical measures, which produce a higher average elasticity (0.077).

- **Econometric approaches:** The literature is nearly evenly split between static panel fixed-effects models (average elasticity 0.046) and dynamic GMM models (0.084). A small share of the literature uses time-series VAR frameworks (0.047).[17]
- **Instrumental variables:** About 30% of studies employ instrumental variables for causal identification, mostly relying on Arellano-Bond instruments[18] in dynamic models. These studies report higher average elasticities (0.091) than those without IVs (0.053).
- **Model specifications:** Most analyses include both time and fixed effects, reflecting broad recognition of unobserved heterogeneity.
- **Controls:** Spillovers, corruption, and innovation are the most common jointly modelled factors. Estimated elasticities tend to be smaller when corruption and innovation controls are included.
- **Methodological quality:** Around 70% of estimates come from studies with an SMS score of 3 (average elasticity 0.074). Studies with a score of 4 report lower average elasticities (0.039), and there are no studies with a score of 5.

Together, these patterns demonstrate that while the literature is diverse, it is also skewed towards certain contexts – especially European road investments estimated using panel models. This heterogeneity, and in particular the dominance of some modes and geographies, reinforces the importance of random-effects estimation and moderator analysis. Appendix A.1 includes a table detailing the breakdown of study characteristics.

[17] GMM is a dynamic panel estimation method that uses internal instruments (typically lagged variables) to address endogeneity and omitted variable bias in models with autoregressive structures. VAR is a time-series framework in which multiple variables are modelled jointly, each depending on its own past values and the past values of the others, allowing for analysis of dynamic interdependencies.

[18] Arellano-Bond instruments are constructed from lagged levels and differences of the endogenous variables in dynamic panel GMM estimators. They help address endogeneity but can be weak or overabundant, leading to biased estimates and weakened validity tests.

4. RESULTS

4.1. POOLED META-ANALYSIS

The random-effects meta-analysis provides a pooled estimate of the elasticity of output with respect to transport capital investment. As shown in Table 4.1, across the 317 elasticity estimates, the pooled elasticity is estimated at 0.038 (SE = 0.006, $p < 0.01$) using cluster-robust standard errors. This implies that, on average, a 10% increase in transport capital is associated with a 0.38% increase in output, holding other factors constant.

Table 4.1: Pooled elasticity of output with respect to transport capital

Variable	(1) Baseline
Pooled elasticity β	0.038*** (0.006)
Model diagnostics	
Between-study variance (τ^2)	0.0023
Observations (k)	317
Clusters (studies)	22

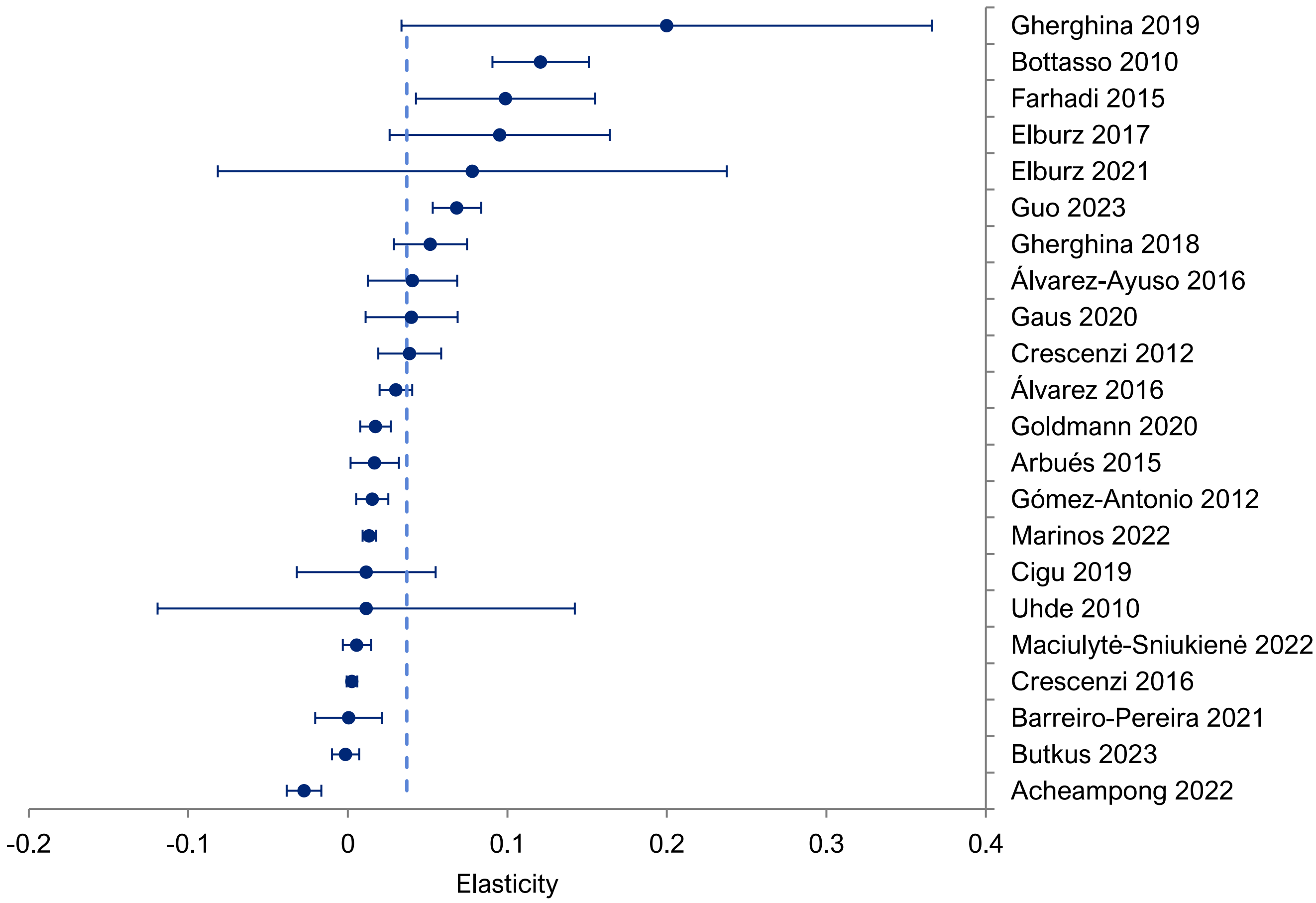
Notes: Random-effects pooled estimate computed via REML. Standard errors shown are cluster-robust (CR2) with Satterthwaite small-sample degrees of freedom, clustered at the study level.
*Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

The standard errors are small and the estimate is highly statistically significant, reflecting the relatively large sample of studies and the weighting scheme applied in the meta-analysis. The between-study variance parameter is estimated at 0.0023, indicating modest, but non-trivial heterogeneity across studies. This is consistent with expectations: the literature varies by geography, transport mode, econometric specification, and time horizon, and the random-effects model explicitly accommodates this diversity rather than assuming a single true effect size.

The pooled estimate is also illustrated in Figure 4.1, which presents a study-level forest plot. While the central tendency is clearly positive, there is considerable dispersion across studies. Some report elasticities close to zero or even negative, while others report much larger positive effects. This variation underscores the importance of interpreting the pooled elasticity as a benchmark or "average tendency" rather than a single universal figure applicable in all contexts. The forest plot also highlights that certain studies contribute a relatively large number of estimates, reinforcing the need to cluster at the study level to avoid overweighting.

Taken together, these results suggest that transport investment has a positive but moderate effect on output, with an elasticity comfortably above zero but smaller than some individual estimates might suggest. This aligns with the findings of previous meta-analyses such as Melo et al. (2013), who reported central elasticities in the range of 0.02–0.04 once methodological quality was accounted for.

Figure 4.1: Study-level forest plot, illustrating the study-level variation around the central estimate



Source: CEPA analysis

Robustness

To assess the sensitivity of the pooled elasticity estimate to modelling assumptions and data treatment, we conducted a set of robustness checks. The first test excludes estimates for which standard errors had to be imputed. As reported in Table 4.2, the pooled elasticity remains positive and statistically significant at 0.034 (SE = 0.006, $p < 0.01$). This value is around 10% lower than the baseline, indicating that the inclusion of imputed standard errors does not materially affect the results.

Table 4.2: Robustness checks: pooled elasticity estimates under alternative specifications

Variable	(2) Excluding imputed SEs	(3) Winsorised variances
Pooled elasticity β	0.034*** (0.006)	0.044*** (0.008)
Model diagnostics		
Between-study variance (τ^2)	0.0023	0.0040
Observations (k)	276	317
Clusters (studies)	22	22

Notes: Random-effects pooled estimate computed via REML. Standard errors shown are cluster-robust (CR2) with Satterthwaite small-sample degrees of freedom, clustered at the study level.
*Significance: * p < 0.10, ** p < 0.05, *** p < 0.01.*

The second robustness check winsorises the sampling variances at the 5th and 95th percentiles. After winsorisation, the pooled elasticity rises modestly to 0.044 (SE = 0.008, $p < 0.01$). This result again confirms the stability of our findings: while the point estimate shifts upward, the elasticity remains well within the same order of magnitude and continues to be estimated with high statistical significance.

Together, these sensitivity analyses provide a range of estimates for the pooled elasticity and reinforce the robustness of the central conclusion that transport investment has a small but positive and statistically significant impact on economic output. The pooled estimates remain in the range of 0.034 – 0.044, regardless of whether potentially noisy or extreme-variance estimates are included, providing confidence that our main findings are not driven by a narrow subset of studies.

4.2. Meta-regression

We now examine **sources of heterogeneity** using meta-regressions (Table 4.3 and Table 4.4). These models relate reported elasticities to observable study features, such as transport mode, geography, time horizon, measurement choices, and econometric specification. **Coefficients are interpreted as deviations from the relevant baseline category, holding other moderators constant.** For clarity, a boxed note below sets out how to recover implied elasticities for any given combination of characteristics, with a worked example.

Box 2: Interpreting coefficients

The meta-regressions relate elasticity estimates to study characteristics. Categorical features are coded with indicator (dummy) variables; one category is omitted and defines the baseline. The constant (intercept) is the predicted elasticity for the baseline category. Each reported coefficient is the difference from that baseline, holding other features constant: positive values indicate higher elasticities than the baseline; negative values indicate lower elasticities. To recover an implied elasticity for any category (or combination of categories in models with multiple features), add the relevant coefficient(s) to the constant.

Worked example

In the mode regression, the constant is 0.053, which relates to studies which look at all transport investment. The coefficient for air is -0.044 and the coefficient for road is -0.004.

The implied elasticity for air is: 0.053 – 0.044 = 0.009

The implied elasticity for road is: 0.053 – 0.004 = 0.049.

Our analysis provides a structured account of how estimates vary systematically with study design and context, highlighting when and why elasticities tend to be larger or smaller. We consider different model specifications.

Table 4.3: Mode, region and time frame

In model 2, we explore the impact of transport investment by mode. With “all transport” as the baseline, the coefficient is 0.053 (SE = 0.015; $p < 0.05$). This implies that, on average, a 10% increase in aggregate transport capital is associated with a 0.53% increase in output, holding other factors constant. This estimate is larger than the pooled baseline elasticity of 0.038 because it represents a conditional mean for studies analysing aggregate transport investment, and therefore excludes estimates for lower-elasticity modes, most notably ports and airports.

Relative to this baseline, all mode-specific coefficients are negative, and two are statistically significant. Investments in air transport exhibit a coefficient of -0.044 (SE = 0.016; $p < 0.05$), while ports show an effect of -0.059 (SE = 0.019; $p < 0.05$). Given the constant, the overall effect of port and airport spending is not statistically different to zero. The coefficients for rail (-0.029; SE = 0.027) and road (-0.004; SE = 0.018) are not statistically significant, implying the elasticities for rail and road investment are not significantly different to aggregate transport investment. This means, for road and rail, we cannot reject the null hypothesis that their elasticity is equal to the elasticity for general transport investment.

In model 3, we estimate region-specific elasticities using multi-country studies (EU/OECD/global) as the reference category. The constant term is 0.052 (SE = 0.009; $p < 0.01$), implying that a 10% increase in transport capital is associated with an average 0.52% increase in output in cross-country analyses. Relative to this baseline, the coefficients for Greece (–0.031; SE = 0.009; $p < 0.05$) and Spain (–0.027; SE = 0.010; $p < 0.05$) are significantly lower, indicating that estimated elasticities in these country-specific studies are smaller than those reported in multi-country analyses. The coefficient for the Other[19] category (–0.004; SE = 0.016) is not statistically different from the multi-country analyses. These estimates indicate that country-specific elasticities may be systematically lower in Greece and Spain relative to the broader group of multi-country studies.

In model 4, we introduce a short-run vs. long-run distinction. The constant term, representing the short-run elasticity, is 0.038 (SE = 0.007; $p < 0.01$), indicating that a 10% increase in transport investment is associated with a 0.38% increase in output in the short run. The coefficient on the long-run indicator is –0.000 (SE = 0.013), showing effectively no difference between short- and long-run elasticities. This finding suggests that the distinction between short- and long-run estimates does not drive systematic variation in reported elasticities across studies.

Across the three expanded models, the adjusted R-squared values range from 0.12 to 0.40, indicating that mode and region explain a moderate share of cross-study variation, while time frame explains relatively little. The estimated between-study variance (τ^2) remains low and stable across all specifications, reinforcing the conclusion that the pooled elasticity estimates are robust to different forms of heterogeneity present in the literature.

Table 4.3: Mode, region and time frame

Variable	(1) Baseline	(2) Mode	(3) Region	(4) Time frame
Constant	0.038*** (0.006)	0.053** (0.015)	0.052*** (0.009)	0.038*** (0.007)
Air		-0.044** (0.016)		
Port		-0.059** (0.019)		
Rail		-0.029 (0.027)		
Road		-0.004 (0.018)		
Greece			-0.031** (0.009)	
Spain			-0.027** (0.010)	
Other			-0.004 (0.016)	
Long run				-0.000 (0.013)
Model diagnostics				
Adjusted R-squared		0.40	0.30	0.12
Between-study variance (τ^2)	0.0023	0.0020	0.0022	0.0023
Observations (k)	317	317	317	317
Clusters (studies)	22	22	22	22

Notes: Random-effects pooled estimate computed via REML. Standard errors shown are cluster-robust (CR2) with Satterthwaite small-sample degrees of freedom, clustered at the study level.
*Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*
Baseline categories: Mode = All; Region = EU/OECD/Global.

Table 4.4: Econometric specification

Elasticities appear remarkably consistent across different ways of measuring transport investment. When contrasting monetary indicators, the baseline, with physical measures such as kilometres of road or network length,

[19] Other includes Germany, Italy and Turkey.

the average elasticity reported in monetary-based studies is 0.037 (SE = 0.009; p < 0.01). Physical measures add only 0.003 (SE = 0.012), a negligible and statistically insignificant difference. This suggests that the choice between monetary and physical metrics does not materially influence the estimated output response, supporting the pooling of studies that use different types of infrastructure indicators.

Turning to the econometric framework, results remain stable across static panel, dynamic panel, and time-series approaches. Static fixed-effects models yield an average elasticity of 0.033 (SE = 0.009; p < 0.01). Dynamic panel (GMM) estimates are only slightly higher than the static fixed-effects (0.010; SE = 0.014), and VAR/time-series models differ by an even smaller margin (0.002; SE = 0.022). None of these differences are statistically meaningful, implying that the underlying modelling strategy has limited influence on the magnitude of the elasticity, and offering little evidence of meaningful dynamic persistence once heterogeneity is accounted for.

Differences in study quality also do not appear to drive systematic variation. Studies below the "high quality" threshold report an average elasticity of 0.042 (SE = 0.007; p < 0.01). High-quality studies, by contrast, produce estimates that are slightly lower (–0.014; SE = 0.012), but the difference lacks statistical significance. This suggests that elasticity estimates are broadly robust to the methodological standards captured by the SMS metric.

The use of instrumental variables leads to a similarly muted pattern. Non-IV studies return an average elasticity of 0.035 (SE = 0.007; p < 0.01), and IV-based studies report a 0.012 (SE = 0.017) higher estimated elasticity. Again, this difference is not significant, implying that IV approaches, though designed to address endogeneity, do not systematically raise or lower the elasticity after controlling for study characteristics.[20]

Specification choices, however, do make a more noticeable difference. The baseline elasticity in this model is 0.034 (SE = 0.009; p < 0.05) for specifications with time and region fixed effects (TWFE) but no controls. Estimates fall significantly when region and time fixed effects are omitted (–0.027; SE = 0.011; p < 0.10), indicating that such specifications yield lower elasticities relative to the baseline TWFE specification andpointing to downward bias when unobserved heterogeneity is not accounted for. Specifications with only region fixed effects also reduce the elasticity (–0.033; SE = 0.013; p < 0.10), while specifications with only time fixed effects increase it (0.028; SE = 0.015), though not significantly. Controls for corruption (–0.017) and innovation (–0.017) similarly lower the elasticity, indicating that these factors capture part of the mechanism linking infrastructure investment and growth. These results highlight that modelling decisions, rather than measurement choices or estimation frameworks, are key sources of variation across studies.

Because the earlier meta-regressions revealed systematic differences by mode and region, a natural concern is that some of the patterns we attribute to measurement, model choice, or study quality might actually reflect underlying compositional differences in where and how investment takes place. To address this, we re-estimate the measure, general model, study quality, IV and specification models with a common set of mode and region controls (Table B.1 in the appendix).

The augmented models confirm that the main conclusions are not driven by mode or regional composition. Baseline elasticities in these specifications are somewhat higher, around 0.06–0.07 across columns, reflecting the fact that we now hold constant the lower-elasticity modes and regions identified earlier. Once mode and region are controlled for, physical measures of infrastructure are associated with a somewhat smaller elasticity than monetary measures (–0.035; SE = 0.018), but the difference remains only weakly significant. Dynamic panel (GMM) and time-series (VAR) estimates continue to be statistically indistinguishable from the static baseline (coefficients close to zero with relatively large standard errors), and neither study quality nor the use of instrumental variables has a discernible impact on the magnitude of the elasticity (0.005 and 0.018, respectively, both far from significant). By contrast, specification choices and additional controls retain a clearer signal: omitting fixed effects still tends to lower the elasticity (–0.020; SE = 0.016), while models that include spillovers report slightly larger effects (0.015; SE = 0.005; p < 0.10), and those that control for corruption (–0.039; SE = 0.013) or innovation (–0.033; SE = 0.015)

[20] While the difference is not significant, the group mean for IV studies is 0.0506 (SE = 0.017) and is statistically significant at 5% level.

yield smaller elasticities. The adjusted R-squared values around 0.40–0.50 and the modest between-study variances ($\tau^2 \approx 0.002$) suggest that, even conditional on mode and region, these extended models explain a substantial share of cross-study variation without overturning the core message: transport investment is associated with a small but robustly positive effect on economic output.

Table 4.4: Econometric specification

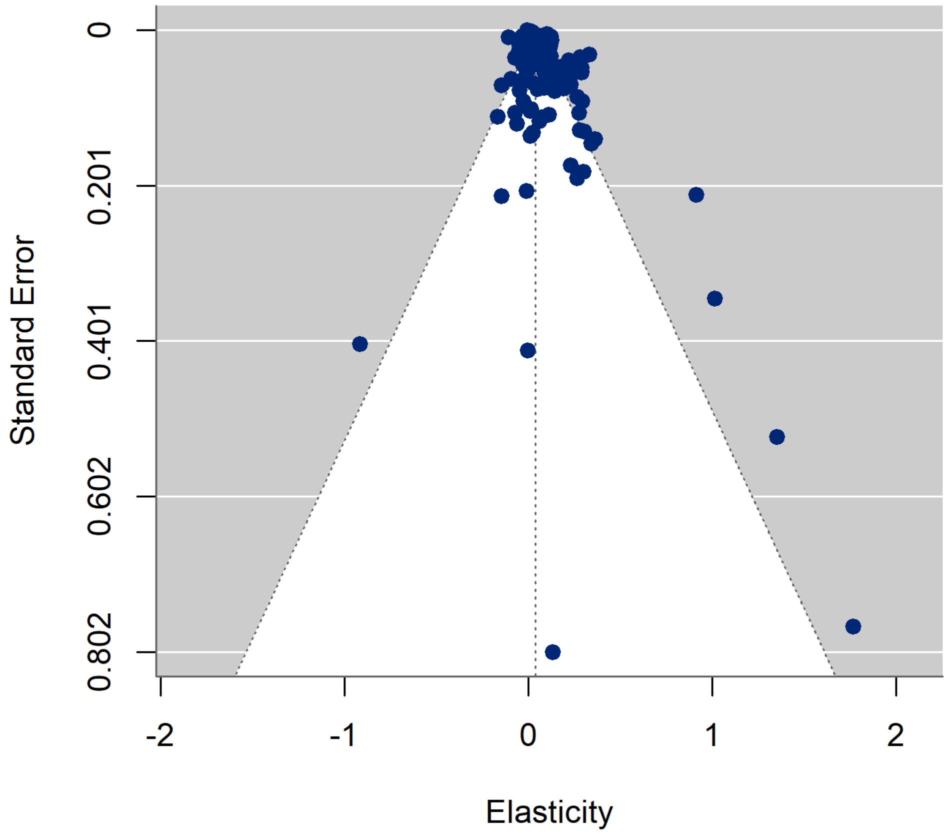
Variable	(5) Measure	(6) General model	(7) Study quality	(8) Instrumental variables	(9) Econometric specification
Constant	0.037*** (0.009)	0.033*** (0.009)	0.042*** (0.007)	0.035*** (0.007)	0.034** (0.009)
Physical	0.003 (0.012)				
Dynamic panel (GMM)		0.010 (0.014)			0.018 (0.013)
Time series (VAR)		0.002 (0.022)			0.011 (0.023)
High quality			-0.014 (0.012)		
With IV				0.012 (0.017)	
No FE					-0.027 (0.011)
RE					0.011 (0.040)
Region FE only					-0.033* (0.013)
Time FE only					0.028 (0.015)
Spillovers					0.006 (0.008)
Corruption controls					-0.017 (0.008)
Innovation controls					-0.017 (0.012)
Model diagnostics					
Adjusted R-squared	0.19	0.13	0.35	0.36	0.41
Between-study variance (τ^2)	0.0023	0.0023	0.0023	0.0023	0.0023
Observations (k)	317	317	317	317	317
Clusters (studies)	22	22	22	22	22

Notes: Random-effects pooled estimate computed via REML. Standard errors shown are cluster-robust (CR2) with Satterthwaite small-sample degrees of freedom, clustered at the study level.
*Significance: * p < 0.10, ** p < 0.05, *** p < 0.01.*
Baseline categories: Time frame = Short run; Measure = Monetary; General model = Static panel; Specification = OLS with TWFE and no controls.

4.3. Publication bias

Publication bias is a concern in any meta-analysis, as studies with statistically significant results are more likely to be published and cited. This can inflate pooled estimates if smaller, non-significant studies are systematically under-represented in the literature.

Figure 4.2: Baseline meta-analysis funnel plot



Source: CEPA analysis

Figure 4.2 shows the funnel plot of reported elasticities against their standard errors. In the absence of publication bias, studies should be symmetrically distributed around the pooled mean, with greater dispersion at higher standard errors. Here, the plot shows a degree of asymmetry: several small-sample studies report disproportionately large positive elasticities, while corresponding negative estimates are sparse. This pattern is suggestive of potential publication bias or selective reporting.

To formally test for asymmetry, we applied the Egger regression. For the full sample, the test strongly rejects the null of symmetry ($z = 12.9$, $p < 0.001$), consistent with the visual impression from the funnel plot. The result is robust across alternative specifications: when excluding imputed standard errors ($z = 11.5$, $p < 0.001$) and when winsorising variances ($z = 9.0$, $p < 0.001$), the Egger test continues to indicate significant asymmetry. This provides compelling evidence that the reported literature is affected by publication bias.

Trim-and-fill adjustment

To explore the implications of potential bias, we applied the trim-and-fill method, which imputes "missing"[21] studies to restore funnel symmetry. For the baseline sample of 317 estimates, trim-and-fill imputes 47 additional estimates. The adjusted pooled elasticity falls from 0.038 (95% CI: 0.031–0.044) to 0.025 (95% CI: 0.017–0.033). This suggests that publication bias may lead to a modest upward bias in the pooled estimate, although the central effect remains statistically significant and within the same order of magnitude.

[21] The "missing" estimates are not observed in the dataset but are statistically inferred to correct for potential publication bias, under the assumption that small, non-significant studies are less likely to be published.

Table 4.5: Trim-and-fill adjustment for publication bias

Variable	(1) Observed estimates	(2) Trim-and-fill adjusted
Pooled elasticity β	0.038 [0.031 0.044]	0.025 [0.017, 0.033]
Model diagnostics		
Observations (k)	317	364
Clusters (studies)	22	22

Notes: Trim-and-fill imputed 47 additional estimates to restore funnel plot symmetry. Estimates are based on a random-effects model with REML estimation. Cluster-robust (CR2) corrections ***were not*** *applied.*

Taken together, the evidence indicates that while publication bias is present, it does not overturn the main finding that transport investment has a positive impact on output. Instead, the results suggest that the "true" average elasticity may lie toward the lower end of the reported distribution, closer to 0.025 rather than the unadjusted 0.038. This is consistent with broader findings in the literature (e.g., Melo et al., 2013), which also emphasise that higher quality studies tend to report smaller, but more reliable, elasticities.

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5. DISCUSSION

The meta-analysis points to a positive, moderate association between transport capital and economic output, with meaningful heterogeneity across studies. However, robustness checks indicate publication bias: smaller or less precise studies tend to report larger effects. After applying standard bias adjustments, the central estimate shifts from 0.038 to about 0.025, leaving the sign and interpretation unchanged but moving the average toward the lower end of the distribution. This pooled elasticity is consistent with the wider literature, including Melo et al. (2013), in which European studies were found to estimate, on average, significantly lower elasticities.

Mode is the most policy-relevant dimension, as DfT appraises and allocates funding on a mode-by-mode basis. Relative to studies that aggregate all transport, point estimates are significantly lower for airports and ports, while estimates for road and rail are not significantly different. Estimates are imprecisely estimated once study level characteristics and clustering takes place. This pattern is consistent with Melo et al. (2013), who likewise report larger effects for roads and a thinner evidence base for non-road modes.

We also find that regional differences are important: multi-country studies yield significantly larger elasticities (0.052) than analyses taking place within Greece and Spain. The difference between multi-country estimates and other within country estimates are not statistically significant. We also find no systematic difference between monetary and physical investment measures, nor a stable short- versus long-run gap in panel settings.

Methodological choices continue to explain more variation in elasticities than either measurement approach or geographical context. Across estimation frameworks, however, the differences are small. Dynamic panel estimators produce coefficients (0.010; SE = 0.014) that are only marginally larger than those from static models, and VAR or time-series approaches yield similarly small and imprecise differences (0.002; SE = 0.022). Study quality also has little effect: high-quality studies report slightly lower elasticities (–0.014; SE = 0.012), but the gap is not statistically meaningful. Instrumental-variable approaches produce estimates that are modestly higher (0.012; SE = 0.017), though again imprecisely estimated. This aligns with findings in Melo et al. (2013), which finds evidence that IV estimates produce statistically significant higher elasticities than OLS.

Specification choices, by contrast, generate more pronounced shifts. Omitting fixed effects substantially lowers the estimated elasticity (–0.027; SE = 0.011), while including region or time effects pulls estimates in opposite directions (–0.033 and +0.028, respectively). This emphasises the importance of controlling for unobserved region controls or national shocks, and it is likely these estimates suffer from omitted variable bias.

Additional controls, particularly for corruption (–0.017; SE = 0.008) and innovation (–0.017; SE = 0.012), also reduce elasticities, suggesting that part of the association between transport investment and output is mediated through institutional or productivity channels. These patterns underscore that identification strategy and model design have stronger implications for the magnitude of the elasticity than the choice of measurement or general econometric framework.

Several limitations should be noted. First, the evidence base is not UK-specific: no UK studies passed our quality screen, and estimates are concentrated in a small number of countries with better data. This raises external-validity concerns for the UK and underscores the need for a credibly identified UK-specific estimate (e.g., using IV or quasi-experimental designs). Second, identification quality varies across studies, and cross-study comparability is constrained by differences in outcome definitions, investment measures, time horizons, and the number of estimates contributed per study. Accordingly, policy use should rely on ranges rather than single point estimates and interpret results conditional on identification strategy, while prioritising UK-focused evidence with credible identification.

6. CONCLUSION

This study provides a current, policy-relevant synthesis of the GDP elasticity of transport capital, focused on European / OECD evidence and accompanied by mode-specific results. Building on Melo et al. (2013), we incorporated more recent studies, applied a stricter quality filter that retains only papers with credible identification, and fully documented search, coding and inclusion decisions for reproducibility. To accommodate both between-study heterogeneity and systematic differences across study characteristics, we employed two complementary estimators: a random-effects model with study-clustered inference to obtain the pooled elasticity, and meta-regressions with study-level moderators to assess how estimation strategy, transport mode, geography and measurement choices influence reported elasticities. The resulting outputs are ranges, not a single number, intended to support DfT in refining business-case assumptions, strategic models and mode-specific policy decisions.

Our results are broadly consistent with earlier meta-analyses. The central elasticity is close to, though slightly below, Melo et al. (2013), and, as in their study, road investments sit above other transport modes. We differ on the time-horizon dimension: once study-level clustering and heterogeneity are accounted for, we do not find a robust average gap between short- and long-run panel estimates.

Three conclusions emerge from the meta-analysis. First, the elasticity of output with respect to transport capital is positive but moderate, offering a realistic benchmark for aggregate appraisal while recognising the uncertainty that surrounds individual estimates. Secondly, much of the variation across studies stems from modelling choices rather than from differences in measurement or geographical setting. In particular, estimates are sensitive to specification decisions, such as the inclusion of fixed effects or additional controls, highlighting the importance of research designs that adequately account for unobserved heterogeneity. Thirdly, while mode-specific analysis is policy-relevant, the evidence base remains uneven across modes, especially outside road transport. This underscores the need for careful interpretation and complementary sensitivity analysis when applying these elasticities in practice.

Future research should prioritise UK-specific, mode-disaggregated estimates with credible identification, parallel reporting of short- and long-run responses, and improved coverage of sectoral elasticities (e.g. industry versus services) relevant to appraisal. Consistent diagnostics (weak-IV checks, over-identification tests, and tests for spatial dependence) and harmonised outcome and investment measures would further enhance comparability. Addressing these gaps will tighten the evidence base for UK policy and clarify the conditions under which transport investment yields the largest economic returns.

Appendix A ADDITIONAL DESCRIPTIVE STATISTICS

A.1. SUMMARY OF STUDIES INCLUDED

Table A.1: Summary of studies used in the meta-analysis

Study ID	Title	Authors	Year
100029	Output effects of infrastructures in East and West German states	Nicole Uhde	2010
100042	The productive effect of transport infrastructures: Does road transport liberalization matter?	Alessandro Bottasso; Michele Conti	2010
100061	Spatial effects of transport infrastructure on regional growth: the case of Turkey	Z. Elburz; K. M. Cubukcu	2021
100131	The spatial spillover effect of transport infrastructures in the Greek economy (2000–2013): A panel data analysis	T. Marinos; A. Belegri-Roboli; P. G. Michaelides; K. N. Konstantakis	2022
100141	Unveiling the effect of transport infrastructure and technological innovation on economic growth, energy consumption and CO_2 emissions	A. O. Acheampong; J. Dzator; M. Dzator; R. Salim	2022
100148	Transport infrastructure and long-run economic growth in OECD countries	M. Farhadi	2015
100149	The spatial productivity of transportation infrastructure	Pelayo Arbués; José F. Baños; M. Mayor	2015
100150	A spatial autoregressive panel model to analyze road network spillovers on production	I. C. Álvarez; J. Barbero; J. L. Zofío	2016
100154	TEN-T Corridors – Stairway to Heaven or Highway to Hell?	Kathrin Goldmann; Jan Wessel	2020
100226	Integrating Network Analysis with the Production Function Approach to Study the Spillover Effects of Transport Infrastructure	I. C. Álvarez-Ayuso; A. M. Condeço-Melhorado; J. Gutiérrez; J. L. Zofío	2016
100276	Spillover effects of transport infrastructure and regional conflicts in Spain	F. Barreiro-Pereira	2021
100295	Infrastructure and regional growth in the European Union	Riccardo Crescenzi; Andrés Rodríguez-Pose	2012
100337	Evaluating the effect of public investment on productivity growth using an urban economics approach for the Spanish provinces	M. Gómez-Antonio; A. A. Garijo	2012
100364	Public infrastructure and regional growth: Evidence from Turkey	Z. Elburz; P. Nijkamp; E. Pels	2017
100391	Transport Infrastructure Investments as a Factor of Economic Growth of European Union Countries	Mindaugas Butkus; Alma Maciulytė-Sniukienė; Kęstutis Matuzevičiūtė	2023
100417	The spatial spillover effect and functional routes of transport infrastructure investment on economic growth: Evidence from panel data of OECD members and partners	P. Guo; J. Fang; K. Zhu	2023
100418	Empirical evidence from EU-28 countries on resilient transport infrastructure systems and sustainable economic growth	Ș. C. Gherghina; M. Onofrei; G. Vintilă; D. S. Armeanu	2018
100419	Transport infrastructure development, public performance and long-run economic growth: A case study for the EU-28 countries	E. Cigu; D. T. Agheorghiesei; A. F. Gavriluță; E. Toader	2019

Study ID	Title	Authors	Year
100422	Exploring foreign direct investment–economic growth nexus – Empirical evidence from Central and Eastern European countries	S. C. Gherghina; L. N. Simionescu; O. S. Hudea	2019
100531	Government quality and the economic returns of transport infrastructure investment in European regions	Riccardo Crescenzi; Marco Di Cataldo; Andrés Rodríguez-Pose	2016
100563	Economic effects of transportation infrastructure quantity and quality: A study of German counties	Dennis Gaus; Heike Link	2020
100578	Does infrastructure development contribute to EU countries' economic growth?	Alma Maciulytė-Sniukienė; Mindaugas Butkus	2022

A.2. Breakdown of study characteristics

Table A.2: Distribution of estimates by study characteristics

Dimension	Category	Number of estimates	Share	Mean	SD
Mode	Road	162	0.51	0.085	0.186
	Rail	30	0.09	-0.009	0.176
	Airport	31	0.10	0.015	0.049
	Port	25	0.08	-0.014	0.048
	All modes (aggregate)	69	0.22	0.096	0.178
Geographic coverage	EU/OECD	155	0.49	0.094	0.207
	Spain	98	0.31	0.029	0.053
	Greece	40	0.13	0.022	0.055
	Other	24	0.08	0.079	0.292
Aggregation	Regional	170	0.54	0.035	0.120
	Cross-national	147	0.46	0.097	0.212
Time horizon	Short-run	197	0.62	0.070	0.207
	Long-run	120	0.38	0.053	0.086
Measure of infrastructure	Monetary	183	0.58	0.054	0.122
	Physical	134	0.42	0.077	0.222
Instrumental variable	No	227	0.72	0.053	0.115
	Yes	90	0.28	0.091	0.263
Study quality (SMS)	3	226	0.71	0.074	0.198
	4	91	0.29	0.039	0.061
Econometric approach	Static panel (TWFE)	159	0.50	0.046	0.127
	Dynamic panel (GMM)	149	0.47	0.084	0.210
	Time-series (VAR)	9	0.03	0.047	0.108
Specification (FE/RE)	None	11	0.03	0.008	0.043
	Time FE only	29	0.09	0.099	0.272

Dimension	Category	Number of estimates	Share	Mean	SD
	Region FE only	45	0.14	0.026	0.066
	Time + region FE	229	0.72	0.069	0.173
	Random effects	3	0.01	0.056	0.062
Spillover controls	None	260	0.82	0.063	0.168
	Direct	57	0.18	0.069	0.187
Innovation controls	No	285	0.90	0.068	0.178
	Yes	32	0.10	0.027	0.076
Corruption controls	No	300	0.95	0.067	0.175
	Yes	17	0.05	0.012	0.046

Appendix B ADDITIONAL REGRESSION TABLES

Table B.1: Econometric specification with region and mode controlled for

Variable	(5) Measure	(6) General model	(7) Study quality	(8) Instrumental variables	(9) Econometric specification
Constant	0.067*** (0.010)	0.058** (0.016)	0.061** (0.013)	0.059*** (0.013)	0.067** (0.018)
Physical	-0.035 (0.018)				
Dynamic panel (GMM)		-0.001 (0.014)			-0.003 (0.012)
Time series (VAR)		-0.009 (0.030)			-0.001 (0.025)
High quality			0.005 (0.018)		
With IV				0.018 (0.018)	
No FE					-0.020 (0.016)
RE					0.007 (0.031)
Region FE					-0.024 (0.034)
Time FE					0.016 (0.018)
Spillovers					0.015* (0.005)
Corruption controls					-0.039** (0.013)
Innovation controls					-0.033* (0.015)
Model diagnostics					
Region and mode controls	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.50	0.43	0.41	0.40	0.50
Between-study variance (τ^2)	0.0018	0.0020	0.0020	0.0020	0.0018
Observations (k)	317	317	317	317	317
Clusters (studies)	22	22	22	22	22

Notes: Random-effects pooled estimate computed via REML. Standard errors shown are cluster-robust (CR2) with Satterthwaite small-sample degrees of freedom, clustered at the study level.
*Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*
Baseline categories: Time frame = Short run; Measure = Monetary; General model = Static panel; Specification; OLS, TWFE, no controls

CEPA
economics matters

UK

Queens House

55-56 Lincoln's Inn Fields

London WC2A 3LJ

T. **+44 (0)20 7269 0210**

E. **info@cepa.co.uk**

www.cepa.co.uk

Australia

Level 20, Tower 2 Darling Park

201 Sussex Street

Sydney NSW 2000

T. **+61 2 9006 1308**

E. **info@cepa.net.au**

www.cepa.net.au