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The Impact of Infrastructure on Development Outcomes

A Meta-Analysis

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Abstract

This paper presents a meta-analysis of the infrastructure research done over more than three decades, using a database of over a thousand estimates from 221 papers reporting outcome elasticities. The analysis casts a wide net to include the transport, energy, and digital or information and communication technology (ICT) sectors, and the whole set of outcomes covered in the literature, including output, employment and wages, inequality and poverty, trade, education and health, population, and environmental aspects. The results allow for an update of the underlying parameters of interest, the "true" underlying infrastructure elasticities, accounting for publication bias, as well as for heterogeneity stemming from both study design and context, with a particular focus on developing countries.

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The Impact of Infrastructure on Development Outcomes: A Meta-Analysis*

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1 Introduction

The academic debate on the impact of infrastructure on development outcomes has been ongoing for several decades, at least since Aschauer published his seminal paper in 1989 (Aschauer, 1989). It has fueled considerable interest in policy circles because of its relevance to address some of the main development challenges of the time: by 2022, one billion people live more than two kilometers away from an all-season road, 685 million people around the world lack access to electricity and 2.1 billion to clean cooking facilities, and 2.6 billion people do not use the internet. Even when services are available, they are often not of sufficient quality and reliability for households and businesses. Of course these gaps mostly concentrate in the developing world. For example, 16 of the top 20 electricity access deficit countries are located in Sub-Saharan Africa (IEA et al., 2023), and there are 42 and 52 times more kilometers of paved roads per square kilometers of land in Europe than in Latin America and Africa respectively (Fay et al., 2017). Lack of access to these critical infrastructures in turn often means that people are being curtailed from opportunities to learn, receive good quality healthcare, access good job openings, or develop a business.

While these facts may lead to the simple conclusion that more investment in infrastructure is needed, they also raise complex questions. What is the most efficient way to extend access in each sector? For electricity, should grid extension be prioritized, or is it more pressing to provide a reliable electricity supply to those who already have access? For transport, how do returns from investment in rural roads compare with those directed towards upgrading critical interurban arteries or alleviating congestion in cities? At the root of these issues lies a fundamental question: given the limited resources available, do investments in some sectors have higher social and economic returns than in others? And, are these trade-offs different depending on the context, the initial level of development for example?

To start providing answers to this question, this paper performs a meta-analysis of the infrastructure research done over the last three decades regarding the transport, energy, and digital or information and communication technology (ICT) sectors, casting a wide net to include the whole set of outcomes covered in the literature: output micro and macro, labor market inequality and poverty, trade, human capital, population, environment, and land Value. Hundreds of papers and several literature reviews have been published and their focus has evolved over time. The first generation of research was mostly analyzing growth and productivity effects, based on a production function framework and using public capital data in a panel cross-section framework.[1] More recently, with the increasing availability of more granular information, including geospatial data, and the computing power to process very large databases, the focus has shifted to studies analyzing a wide array of outcomes, specific sectors, and implementing robust micro-econometric identification strategies.[2]

We built a database of over a thousand estimates from 221 papers reporting infrastructure elasticities or semi-elasticities that can readily be converted into comparable elasticities. The papers included are the result of a systematic search through literature reviews and publication databases and were produced between 1983 and 2023.

We then analyze this data, starting with a visual examination through funnel plots, and distribution plots of test statistics following Brodeur et al. (2023). We then implement several econometric methods. We start by providing basic FAT-PET-PEESE type estimates (Stanley and Doucouliagos, 2012). FAT estimates and Caliper tests allow us to characterize potential publication bias and p-hacking. To provide estimates of specific "true" underlying effects of infrastructure, we report PEESE estimates. We then analyze in more details the heterogeneity in the sample, by introducing a large list of moderators related to both study design, and infrastructure sector and context characteristics. We report results based on several selection methods of moderators, including rigorous penalization for the LASSO, LASSO with 10-fold cross-validation for tuning parameter selection, and several Bayesian Model Averaging methods. We report results by sectors, as well as sector-outcomes.[3]

[1] See Straub (2011) for a critical review, and Bom and Ligthart (2014) for a meta-analysis of that specific strand of applications.

[2] See Foster et al. (2023) for a recent qualitative review of this evolution.

[3] We only include sector-outcomes categories with enough observations, but note that all outcomes still

Overall, the analysis leads to five main conclusions. First, the literature on infrastructure has diversified hugely since the 1980s. From an almost exclusive initial focus on the output-elasticity and the use of public capital data, it has extended to specific sectors, many different outcomes, and a large variety of data sources and types. This has led to increasing heterogeneity in the literature, and has implications for the way we conduct this meta-analysis, leading us to focus mostly on specific sub-samples.

Second, as is generally the case for many other economic topics, it is subject to significant publication bias in favor of positive and significant results. Third, as the literature recognized the limitations and moved away from its initial focus on public capital, the estimates it has produced have become relatively smaller. We report average estimated elasticities that range between zero and 0.06 for most of the sector-outcome categories we consider. We are however careful in interpreting these magnitudes. As we discuss in the concluding section, given the differences in the nature of infrastructure indicators used, it is unclear whether lower elasticities necessarily entail smaller marginal product of capital.

Fourth, we look at the average elasticities specifically for developing countries samples, finding that they are generally larger than those for developed economies for the digital and transport sector, but not for energy. A few large estimates are found in very specific sector-outcome categories, such as digital-micro-output, and transport-labor. Finally, we discuss how these estimates could be leveraged to produce marginal rates of return for specific sector-outcomes, highlighting the lack of relevant data in this area, which appears to be the next priority in terms of research.

This study contributes to the literature on infrastructure by providing a systematic assessment of the body of research reporting elasticities of infrastructure for a large set of outcomes and across three sectors. In that sense, it aims at providing updated reference points regarding the quantitative effect of infrastructure, useful for both researchers and practitioners engaged in infrastructure projects. A number of review papers have been written on infrastructure, including the early essay by Gramlich (1994), the 1994 World Development Report (WorldBank, 1994), more recently Straub (2008) and Straub (2011),

contribute to overall sector meta-elasticity estimates.

as well as a number of sector-specific reviews such as Redding and Turner (2015), Redding and Rossi-Hansberg (2017), Berg et al. (2017), Lee et al. (2020), Bertschek et al. (2015), and Greenstein (2020). Previous meta-analysis include Bom and Ligthart (2014) and García et al. (2017), which focus specifically on production function studies. This paper's contribution is to extend the review to a more recent period and to provide a meta-analysis of the literature beyond the macroeconomic production function-based papers, in particular covering more recent microeconometric research. A companion paper, Foster et al. (2023), provides a qualitative review and discussion of the evolution of research in this field over the last decades.

The paper is structured as follows. Section 2 describes the selection criteria used, the data construction process, and the variables included in the analysis, and provides general descriptive statistics. Section 3 then presents the basic methodology used to analyze the data. Section 4 presents the main findings from plots and estimation results. Section 5 discusses the results and their implications and concludes. Additional information on the sample of papers covered, the moderators variables, and robustness checks is provided in the Appendix.

2 Data

2.1 Studies included in the review: search and selection criteria

This meta-analysis analyzes the impact of infrastructure on a large array of economic and social outcomes. Infrastructure is understood here as covering three main sectors:

- Transport, comprising roads, railroads, ports and airports.
- Energy, covering production, transmission, and distribution of electricity to households and firms (referred to as access from a demand point of view).
- Digital, also often referred to as information and communication technology, or ICT, and including fixed and mobile phones, Internet access and use, and backbone In-

ternet development.

Broadly speaking, we include any study with a specification broadly of the form:

$$Y_{it} \quad = \quad f(X_{it}, \theta_i, \theta_t, \varepsilon_{it}) \tag{1}$$

where i is the aggregation level of the study, which we discuss below, t is the time unit, on the right-hand side the independent variable X_{it} is an indicator of infrastructure in one of the sectors defined above, and θ_i and θ_t are observation-level, and time fixed effects respectively. Note that not all studies always include the full set of fixed effects. Some are cross-sectional analyses and do not display within-unit time variation, others cover several time periods but do not report between-unit variation.

On the left-hand side the dependent variable Y_{it} is an economic or social outcome pertaining to one of the following categories:

- Output, which includes indicators of production, income, expenditures, or productivity, in levels or growth rates. We categorize it as output micro when observations are at the level of firms or households, and output macro when it is at a higher level of aggregation (district, regions, or country).
- Labor market, including indicators of employment and wages.
- Inequality and poverty, such as Gini or poverty share indicators.
- Trade, including exports and imports, as well as extensive margin indicators of access to external markets for example.
- Human capital, covering all indicators related to educational and health outcomes.
- Population, including levels as well as migration flows.
- Environmental variables, such as pollution, greenhouse gases (GHG) emissions, etc.

In order to be able to perform a quantitative meta-analysis on a comparable set of estimates, we restrict the sample to studies that report elasticities (where both the dependent and the independent variables are in log form) or semi-elasticities (where the dependent variable is in log form, and the independent one of interest is a dummy variable) that can be readily converted to elasticities.[4]

The starting point of the sample collection is Straub (2008), which covered 140 specifications from 64 empirical papers. We then collected additional references from other meta-analysis papers and literature reviews on transport, energy, and digital issues written since 2008.

These papers include Bom and Ligthart (2014) and García et al. (2017), which focus specifically on production function studies. For transport-related infrastructure improvements, we identified additional references from the surveys of Redding and Turner (2015), Redding and Rossi-Hansberg (2017), and Berg et al. (2017). For energy, we collected papers from Burgess et al. (2020) and Lee et al. (2020). Finally, for digital and ICT topics, we relied on Bertschek et al. (2015), Greenstein (2020), and Vergara Cobos and Malasquez (2023). We also added references through searches for infrastructure-related keywords in Google, the World Bank Working Paper Series, and the George Washington Universities' digital libraries, as well as relevant recent working papers submitted to the World Bank conferences.

We include all the papers and reports that comply with the following basic requirements: they are written in correct, understandable language, mostly in English with a few exceptions of papers written either in Spanish or French, they state the research question clearly, contain basic information on the econometric specification allowing an assessment of its soundness, and report estimates together with either standard errors or t-values, as well as number of observations used in the estimations.

We report several estimates for each study. Although we aim to cover all relevant specifications, we apply the following criteria to limit their number. When covering a given econometric specification that reports several results successively including additional controls, we pick only the last one including all the relevant controls. So for example, for

[4] Elasticities could be computed even when the independent variable is not a dummy variable, but the information needed to do so, such as the sample average of the independent variable, is generally lacking.

a paper reporting five OLS and five 2SLS estimations involving the same dependent and independent variables, in which more controls or fixed effects are successively included, we report only the last OLS and the last 2SLS ones (usually the ones in the right-most columns on the respective tables). In the case of a table reporting different ways of addressing the endogeneity of the explanatory variable, say standard 2SLS and GMM, we report both estimates. We repeat this process for all relevant combinations of right-hand and left-hand side variables but exclude robustness checks on sub-periods or sub-samples of the main analysis. While this may seem quite parsimonious, we still end up with cases where we include several dozen estimates from the same study.

Finally, we take care to harmonize the reported estimates across studies. In particular, papers addressing the same issue may report positive or negative effects depending on the way they code the explanatory variables. For example, some papers use distance, as resulting from the building of new roads, as an explanatory variable for GDP growth, in which case a negative coefficient means an improvement of the outcome under study, while others directly use the reduction or the inverse of the distance as a regressor, finding positive effects in case of improvements. We adjust the signs accordingly.

In addition to the papers' reference and main estimates, we systematically code moderator variables, grouped in two categories: study design characteristics, and infrastructure sector and context characteristics.

Study design characteristics include the aggregation level (country-, region, district, or unit specific-level data; firm or household data) and type of data used (monetary measures of infrastructure such as public capital vs. physical measures), the theoretical framework used as a reference, whether the data has a spatial dimension, and the type of econometric technique used, with a specific focus on whether endogeneity is explicitly addressed and how. Some studies in this literature use instrumental variable techniques, such as two-stage least square (2SLS) or Generalized Method of Moments (GMM) to address endogeneity. Other claim exogeneity based on "natural experiment" type arguments, implement a Randomized Control Trial (RCT) or use identification strategies such as difference-in-differences or regression discontinuity design. In all cases, we attempt a fair assessment of whether the study handles endogeneity concerns in a credible way.

Regarding the infrastructure sector and the context, we record the type of publication (peer-reviewed or working paper) and its timing, the affiliation of the authors (academia, government, international organizations, private sector), the sample covered (single or multi-country and whether these are developing countries or not), the time frame of the data, the category of the dependent variables (see above) and of the independent variables (sectors and sub-sectors, and specific types of variables used). A full list of the variables included in the dataset is in Appendix A.

2.2 Descriptive statistics

The final database includes 221 papers and 1074 estimates. The full list of papers with the number of estimates included in the analysis is in Table A.1 in Appendix B, while Table A.2 in Appendix D provides summary statistics at the paper level: the sample contains on average 5 estimates per paper, three-quarters of the papers are published, more than half since 2010, and a third of them in top field or top general interest journals. About one paper in five uses micro-data, around 70 percent address endogeneity, mostly using instrumental variables. Figure 1 shows the time coverage of the studies in our sample. Cross-sectoral studies cover mostly the period 1960 to 1990.[5] Papers on transport and energy also cover earlier decades, sometimes even providing historical insights from previous centuries. Finally, the coverage of digital topics reflects two periods, with fixed telephony being the object of samples from the 1960s to the 2000s, and studies on internet and mobile phones populating the subsequent period.

Summary statistics in Table 1 presents the distribution of estimates, by sectors and types of outcomes. In terms of sector, the largest category is transport with 393 estimates, followed by studies using a cross-sectoral measure of infrastructure, energy, and digital, all with slightly over 200 estimates. It is also noteworthy that despite a trend towards the diversification of outcomes, overall 958 out of 1074 estimates correspond to only three

[5] Cross-sectoral here refers to studies covering data from multiple sectors as well as public capital.

categories: micro and macro output, and labor market. Transport studies is where this inclusion of new topics is more obvious, with 90 outcomes out of 393 not belonging to the three main categories.

Table 2 shows the distribution of the sign of estimates by sectors.[6] Finally, approximately half of all estimates (540 out of 1074) correspond to developing countries sample. This proportion is highest for energy (82 percent of estimates) and transport (60 percent), and much smaller for digital (38 percent) and cross-sectoral estimates (16 percent).

3 Methodology

This section presents the steps taken in the paper to quantitatively analyze the large number of estimates gathered in our sample of studies. Specifically, we aim at deriving our own summary estimates of the underlying "true" effect that can be inferred from existing studies, explain the observed heterogeneity in the existing evidence, and assess to which extent it may suffer from publication bias.

When running estimations, we also look at specific sub-samples combining one sector and one outcome, whenever enough observations are available, because combining homogeneous dependent and independent variables categories is likely to reduce sample heterogeneity. There are eight of these: two for the digital sector (micro- and macro-output), three for energy (micro- and macro-output, and labor), and three for transport (micro- and macro-output, and labor). Finally, we also break down results between developed and developing countries.

Assessing publication bias is critical to a correct appraisal of the literature. It has been shown to be pervasive across many fields of economic research.[7] When publication bias

[6]Table A.3 shows signs by sector and types of outcomes. Positive estimates represent again a consistently high share of 80 percent or more, with two notable exceptions: only 59 percent of estimates in inequality / poverty related studies and of population effects are reported as positive. This must be considered in a context where the number of estimates included in our data is however relatively small (17 and 34 respectively).

[7]Publication bias stems both from the tendency of editors and referees to favor statistically significant results for journal publication, and from the related "file-drawer" problem, as this generates incentives for

is large, ignoring it may lead to summary estimates of policy-relevant parameters that are way off their potential true value, sometimes by a factor of two or three.

We start by presenting distribution plots of p-values and z-statistics, following Brodeur et al. (2023), in order to visually inspect whether there is evidence of bunching at statistical thresholds in our sample, and provide related tests of bunching and discontinuity at significance thresholds.

Next, we move to the FAT-PET-PEESE approach proposed by Stanley and Doucouliagos (2012).[8] The FAT-PET-PEESE approach consists of three steps. First, the Funnel asymmetry test (FAT) provides an assessment of publication bias by looking at the relationship between the effect size of the studies and their precision as measured by the standard error of the estimate. Under random sampling theory, the two should be independent.

Visually, this translates into the expected symmetry of the funnel plot, where estimates are on the horizontal axis, and the inverse of the standard error on the vertical axis. Under the hypothesis of no publication bias, we expect the mass of points to be symmetrically distributed around a vertical line proxying the "true" underlying effect.

Econometrically, the corresponding test relies on the following equation:

$$\widehat{\theta}_{is} = \beta_0 + \beta_1 SE_{is} + \varepsilon_{is} \tag{2}$$

where $\widehat{\theta}_{is}$ is the individual effect i from study s, and SE_{is} is its standard error. $H_O : \beta_1 = 0$ is a test of publication bias, and rejecting H_O means that we cannot rule out the existence of such bias.

The variance of the effect, and therefore ε_{is}, is likely to vary across studies, generating obvious heteroskedasticity. Stanley and Doucouliagos (2017) show that the equation is best estimated using Unrestricted Weighted Least Squares. For simplicity, we follow Stanley and Doucouliagos (2019) and estimate the unrestricted WLS by running the simple

authors to write-up and submit in priority these significant results. See Stanley and Doucouliagos (2012) and Andrews and Kasy (2019) for a recent account.

[8] It has been shown to perform best in reducing the bias, in the sense that it comes closer to the results of pre-registered replications (Kvarven et al., 2020).

OLS regression derived from the one above divided by the standard error, which becomes:

$$t_{is} = \beta_1 + \beta_0 \frac{1}{SE_{is}} + \epsilon_{is} \tag{3}$$

where $t_{is} = \dfrac{\widehat{\theta}_{is}}{SE_{is}}$ is the t-statistic of each effect, and we assume that the error term $\epsilon_{is} = \dfrac{\varepsilon_{is}}{SE_{is}}$ is now of constant variance.

Next, based on the same equation, the Precision-effect test (PET) indicates whether there is an effect beyond potential publication bias. It consists of testing the hypothesis $H_O : \beta_0 = 0$. Rejecting H_O means that there is a non-zero effect in the literature under consideration.

Stanley and Doucouliagos (2012) show however that the coefficient β_0 is also likely to be biased in the presence of publication bias. This leads to the third step, called precision-effect estimate with standard error (PEESE), which uses the variance (SE_{is}^2) instead of the standard error in (2), based on the fact that a quadratic relationship appears to provide a better fit between effects and their standard errors:

$$\widehat{\theta}_{is} = \beta_0 + \beta_1 SE_{is}^2 + \varepsilon_{is} \tag{4}$$

which, after operating the same transformation as above, leads to the following PEESE estimating equation:

$$t_{is} = \beta_0 \frac{1}{SE_{is}} + \beta_1 SE_{is} + \epsilon_{is} \tag{5}$$

Here, rejecting $H_O : \beta_0 = 0$ again means that we accept the existence of a significant effect in the literature of reference.

Note that several estimates from the same studies are sometimes included in our sample. To account for the potential correlation of error, when estimating (3) and (5), we cluster standard errors at the study level.

Finally, the last step is to explicitly account for the heterogeneity across studies, by including in the estimations relevant moderator variables.[9] Formally, the error term in (2) can be expanded and written as $\nu_{is} + \epsilon_{is}$, where ϵ_{is} is sampling noise, and ν_{is} is heterogeneity in treatment effect across studies due to differences in countries, sector, time period, type of data and econometric methodologies, etc. Given a set of moderator variables M_k capturing these differences, we assume that there is no single true effect, but rather that it may vary depending on a number of characteristics of the underlying sample in any given study or on the technical characteristics of this study.[10] We can write:

$$\nu = \sum \beta_k M_{kis} + \upsilon_{is}$$

Equation 2 thus becomes:

$$\widehat{\theta}_{is} = \beta_0 + \sum \beta_k M_{kis} + \beta_1 SE_{is} + \upsilon_{is} + \epsilon_{is} \tag{6}$$

Simply adding the whole set of moderator variables produces very noisy estimates and is likely plagued by multicollinearity issues, as some of these variables are highly correlated. To address this issue, we apply several methods to select an appropriate model with a smaller subset of control variables. First, we implement two alternative LASSO procedures, the rigorous penalization approach in Belloni et al. (2012, 2014), and LASSO in which the tuning parameter is chosen by 10-fold cross-validation.[11] Second, we use Bayesian Model Averaging, either including covariates with posterior inclusion probabil-

[9]Note that in some cases, moderator variables also vary within studies.

[10]Some of this variation may also be assigned to the publication bias, which may now vary across (set of) studies. Think, for example, of specific econometric techniques leading to less statistical significance overall and hence to lower publication or circulation probability.

[11]LASSO is a popular model selection tool. It minimizes mean squared error subject to a penalty on the absolute size of coefficient estimates. Based on the nature of the penalty the lasso sets some of the coefficient estimates exactly to zero thereby removing some predictors from the model. The degree and type of penalization are controlled by tuning parameter(s). Under the rigorous approach, the tuning parameters are theoretically grounded, guaranteeing optimal rate of convergence for prediction and parameter estimation. There is a high priority on controlling overfitting which results in parsimonious models. Alternatively, the tuning parameter can be selected using cross-validation (CV) to optimize out-of-sample prediction performance. CV methods are universally applicable but are computationally expensive. We implement these using *rlasso* and *cvlasso* commands in Stata.

ity above 0.1, or those from the model with the highest posterior model probability.[12] [13] We implement the procedure in two steps: (i) model selection using LASSO or BMA; and (ii) using post-estimation OLS, we run the PESSE regression.

Our sample of estimates is likely to be highly non-random in terms of coverage and methodology. For example, the literature contains many studies on the US or other developed countries, and in the case of developing and emerging countries, on some specific ones such as China or Brazil. The choice of estimation techniques is equally unbalanced, with a large preponderance of instrumental variables and to a lesser extent diff-in-diffs techniques. For this reason, rather than focusing on the interpretation of the moderator coefficients, we see their inclusion as a way to control for the biases due to sample selection. We come back to this point when discussing the results below.

4 Results

4.1 Main Plots and Estimations

In this section, we start by presenting funnel plots by sectors and by types of outcomes. The plots include a vertical line at zero, as well as two solid curves, corresponding to the 5 percent significance level on both sides.[14] For visualization purposes, we exclude a few extreme values, namely elasticities below -3 or above 3, and 1/standard errors above 200.[15] Funnel plots are a good way to visualize the distribution of estimates in the literature and

[12] Bayesian Model Averaging estimates 2^K regressions (where K is the number of covariates) representing possible combinations of true data generating process (DGP). Each model is assigned a probability called posterior model probability which reflects how likely it is that a given combination of covariates represents the true DGP. Each covariate is also assigned a probability called posterior inclusion probability which indicates the likelihood of that covariate belonging to the true DGP. We implement the procedure using the *bmaregress* command in Stata.

[13] In practice, when adding moderators, we do not divide by the standard errors and stick to equation 6, as it has been shown that the main effect is more precisely estimated in that case, and in any case the Stata commands to implement the LASSO and BMA routines do not allow for models without a constant term.

[14] For each value on the x-axis, this is given by y=1.96/x if x is positive, and y=-1.96/x if x is negative.

[15] This leads us to exclude 84 observations in the whole sample: 28 for the digital sector, 15 for energy, and 35 for transport. We formally address the robustness of the estimation results to outliers in Section 4.4 below.

the likelihood that it suffers from publication bias.

Figure 2 presents the funnel plot for the whole sample. As expected, less precise estimates, i.e., those with larger standard errors at the bottom of the graph, are more dispersed. In addition, it is immediately obvious that the graph is asymmetrical and there are more positive estimates, especially among less precise ones. This is a first indication of publication bias. In addition, there is evidence of bunching above the statistical significance lines, especially on the positive side, indicating a high prevalence of studies with results just above the 5 percent significance level. While the sheer number of studies makes this less visible in the whole sample, this is especially remarkable in the plots by sub-samples, reported in Figure A.1 in Appendix C.

Next, Figure 3 plots the distribution of p-values and z-statistics for the full sample of estimates, following Brodeur et al. (2023). The upper panel displays clear humps around the statistical significance thresholds, in particular at 5 percent. The lower panel shows evidence of jumps in p-values below the conventional thresholds of 0.01, 0.05, and 0.10. Plots with similar patterns disaggregated for the three main sectors are shown in Figure A.2 in Appendix C. Tables A.5 to A.7 in Appendix D present various related p-hacking tests. Tables A.5 reports Elliott et al. (2022) binomial and discontinuity tests on the p-curve. The results are inconclusive and do not indicate manipulation at the thresholds, but that may be due to the small sample size. Table A.6 follows in performing binomial manipulation tests for different window sizes. For the 10 and 5 percent significance thresholds, there are significantly more estimates to the left of the cutoffs than to the right. Finally, Table A.7 tests whether specific covariates are systematically related to the reporting of significant results. Notably, the use of micro-data, IVs, and difference-in-differences leads to more significant results at the 10 and 5 percent level, consistent with the findings in Brodeur et al. (2020). Published papers, on the other hand, contain more results significant at the 1 percent level.

Next, we move to the FAT-PET-PEESE estimations of equations 3 and 5 to quantify both the potential publication bias and the underlying effects of interest. We start with the sector-level results, in Tables 3 and 4.

The significant coefficient of the constant in column 1 of Table 3 indicates that overall there is significant publication bias in the infrastructure literature, with estimates skewed towards positive values, consistent with the visual information and the tests reported above. When breaking down the sample of estimates, publication bias also shows up in all the sub-samples focusing on the single sectors (digital, energy, and transport), and is only rejected for the sample of cross-sectoral estimates. Next, the PEESE coefficients for 1/se in Table 4 provide estimates of the "true" underlying effect. It is estimated at 0.16 for the studies using cross-sectoral measures, consistent with some of the earlier meta-analysis. Strikingly, the estimated elasticities appear to be much smaller once we move to single-sector studies: the reference values are significant and equal to 0.007 for energy, and 0.03 for transport, and at 0.015 and not significantly different from zero for the digital sector.

4.2 Moderators

To understand in more details the heterogeneity due to variation across studies in terms of study design and context, we next present results based on equation (6). The list of variables related to study design and underlying context of the study and data used is in Appendix A.

Given the large number of moderators and the fact that despite the sample size, their relevance is highly unbalanced, we consider several ways to select the appropriate controls to be included in each estimation. The first one applies the rigorous penalization LASSO. Alternatively, the moderators are selected using LASSO with 10-fold cross-validation for tuning parameter selection. In addition, we used two Bayesian Model Averaging methods to select covariates, by focusing on those with posterior inclusion probability > 0.1, or those from the model with the highest posterior model probability.

The results showing the list of selected covariates by each of the four methods are included in Appendix D, Tables A.8 to A.11.[16] As can be seen, few covariates are consistently selected or significant across the different methods, with two exceptions. In the digital

[16]Note that the tables' lines report all variables even if not selected to make the visual comparison of tables easier.

sample, three of the models select only the firm-level data characteristic as control, with a positive and significant coefficient (+0.04). In the energy sample, the CV-LASSO and the BMA models select very similar controls, and report significantly higher effects when relying on spatial data (+0.05) and district-level data (+.04). We draw two conclusions from this. First, the literature is characterized by a very large heterogeneity in the sense that the samples of studies analyzing energy outcomes vs. transport ones for example have very distinct characteristics in terms of their design, the data, or the methodology they use. This reinforces the case for analyzing sub-samples separately. Second, the inclusion of moderators should be seen primarily as a way to control for the specific biases induced by the sample composition.

In Table 5, we summarize the main result from that exercise, reproducing the first line of the Tables in the Appendix and leaving out the moderators. Panels B to E report the results from the four different moderator selection procedures. The four sets of results are generally consistent. The elasticity in the full sample is positive and significant in the range 0.02-0.04, the digital elasticity is in the 0.018-0.21 range, energy between 0.028 and 0.040, and transport between 0.15 and 0.026. In order to unpack these results further, next we move to finer sector-outcomes results.

4.3 Disaggregated Results

Sector-level samples bundle together a number of different outcomes. For example that the transport literature in our data includes 34 estimates of micro output, 208 of macro output, 61 labor, 25 trade, and 33 population outcomes among others. To address this, we first look at the sector-outcome sub-samples for which enough observations are available: digital sector-micro and -macro output, energy-micro, -macro output, and -labor, and transport-micro, -macro output, -labor, -trade, and -population. The results for these sub-samples are in Table 6. Panel A reports simple PEESE estimations without moderators, while panel B includes controls selected through rigorous LASSO procedure. We concentrate mostly on panel B results in the interpretation below.

The results display large variation within sectors. The estimated elasticity in the digital sector are large and significant for micro-output (estimated on the basis of firm- or household-level observations), between 0.05 and 0.06, but about three times smaller for macro output. Note that this is consistent with the positive and significant coefficient found for firm-level data in Table A.9 to A.11. For energy, micro- and macro-elasticities are comparable, between 0.04 and 0.05 and significant, but indistinguishable from zero when looking at labor market outcomes. Finally, for transport, elasticities are systematically significant, being the largest for trade (0.9), followed by labor outcomes (0.16), and micro-output (0.08), and smaller for macro-output (0.03), and null for population. It thus appears that across sectors, elasticities are larger when based on studies covering micro-, and labor market data than more aggregated data.

Next, results by level of country development are of specific interest for drawing policy implications. Theoretically, it could be that in contexts where there is greater scarcity of capital, the marginal product of infrastructure investment is higher, and that decreasing returns kick in and the marginal product decreases as countries accumulate larger stocks of capital. However, there could be threshold or network effects, so that returns are very different across countries at different levels of development or of coverage. Finally, marginal returns may be high in case of last-mile connections only.

In Figure 4, we represent sector-level (panel A) and sector-outcome results (panel B) including rigorous LASSO-selected controls, splitting the sample between developed and developing countries.[17] The results are somewhat surprising, as elasticities are higher for developed countries in 2 out of 4 sub-samples, namely digital and energy studies, and only larger for developing countries in the case of transport and cross-sectoral studies. This may argue for the existence of network effects, whereby high returns require a minimum level of quality coverage. They can also be related to studies showing low returns to investment in some developing countries contexts, for example in the case of rural electrification (Lee et al., 2020). On the other hand, the results for transport are consistent with some qualitative evidence for large deficits and high returns in this sector (Foster et al.,

[17] The corresponding Tables are in Appendix D, Table A.12 and A.13

2023). Additional evidence looking at specific outcomes helps qualifying these results. As shown in the lower panel, in developing countries we find higher elasticities for micro-level digital studies (0.17), consistent with for example the evidence in Hjort and Poulsen (2019). The outcome-level analysis also reinforces the evidence for the strong impact of transport investments, especially for labor market outcomes (0.19).

4.4 Robustness

Table 7 presents a number of robustness checks. In meta-analysis estimations, more precise estimates carry a larger weight. One issue is that observations with extremely small standard errors may then have a disproportionate impact on the outcome. A visual examination of the data shows a few observations that have very small standard errors (i.e., values of 1/standard error of more than 200, going up to 7,000 in some cases). Of course, very small standard errors may simply correspond to equally small coefficients. However, an examination of the data shows in a majority of cases these do correspond to estimates that report extreme t-statistics of up to 245. In light of this, in panel A we run robustness checks excluding observations with 1/standard error larger than 200 and a t-statistics larger than 4. Overall, this tags 52 observations, 6 cross-sectoral, 30 in digital, 4 in energy and 12 in transport. In panel B, we restrict the sample to the set of estimates that were reported as elasticities in the original studies, excluding semi-elasticities based on dummy independent variables. In panel C, we include only estimates that explicitly address endogeneity concerns, thus eliminating about half of the sample. Finally, panel D considers only single-country estimates, excluding cross-country estimates.

While the different robustness exercises introduce some variation in the resulting meta-elasticities, these remain broadly consistent overall. In the full sample, the estimated coefficients are very stable, only slightly smaller in panel A, while in the cross-sectoral sample, they remain comparable to previous results, between 0.09 and 0.16. Focusing on the three main sectors, the overall elasticity for digital is now slightly higher, between 0.03 and 0.06, which is perhaps not surprising given that studies focusing on this sector concentrate most of the outliers in the full sample. For energy, estimates are very stable, only about half smaller when including only estimates addressing endogeneity. Finally, estimates for transport remain in the 0.02 to 0.03 range and are significant, with the exception of panel A, where it is reduced to 0.01.

5 Discussion and conclusion

A few key conclusions emerge from the results above. First, there is evidence of systematic publication bias in this literature. Studies reporting positive results tend to be over-represented. This comes out clearly from the different visual representations and the analytical results. Identifying this bias is important, as it allows us to isolate the residual "true" effect in different sub-samples. Of course, this is not unusual in the economic literature, as many areas suffer from a similar bias.[18]

Second, since the topic became of relevant policy concern in the 1980s, the literature has diversified in many directions. It has moved from an initial focus on public capital measures of infrastructure to other types of data, including physical measures and more granular micro- and spatial data. As a result, the share of sector-specific studies has increased over time. On the outcome side, a similar diversification has occurred, moving from an initial dominance of studies looking at output or productivity effects to more recent ones analyzing labor market effects, inequality and poverty, trade, education and health, or environmental aspects among others. While these studies remain a minority, there is clearly a trend toward the multiplication of issues being scrutinized under the infrastructure label.

This has important implications for any attempt to draw lessons from the literature, including of course this meta-analysis. The diversification trend affecting both the dependent variables (outcomes) and the independent ones (the sectoral aspects and the way they are measured) means that this is an increasingly heterogeneous literature. This heterogeneity may translate across sub-fields into differences in terms of publication bias, in terms of underlying elasticities, and in terms of the effect of key moderators. A meta-

[18] see for example Ioannidis et al. (2017).

analysis requires a certain degree of homogeneity of the studies it includes to yield interpretable results. Because of this, we chose to make sub-fields the main focus of our analysis, looking at specific sectors and when possible at combinations of sectors and specific outcomes. When doing this, we indeed find evidence of the heterogeneity mentioned above.

Third, moving to the results, we show that studies based on public capital measures yield larger estimates. Cross-sectoral studies, which rely in more than 80 percent of cases on public capital measures, have an estimated average elasticity of 0.16, even after controlling for publication bias. On the other hand, sector studies relying almost exclusively on physical, access, or usage measures yield elasticities that range between zero and 0.06 at most. This is in line with several sector-level literature reviews, which find relatively small effects of the different types of infrastructure. We also note that the reduction in the size of estimates does not seem to be due primarily to a change in methods or more sophisticated identification strategies. The reliance on instrumental variables to address endogeneity does not show up as significant when used as a moderator. Similarly, restricting the sample to estimates addressing endogeneity explicitly does not always lead to smaller estimates, and there is no clear time trend in term of the size of estimates. We remain cautious about interpreting this reduction in the magnitude of estimates, given the change in the type of infrastructure indicators used in the latter studies, from public capital to physical units or access and usage rates. Ultimately, we care about the actual social rates of return of infrastructure, which as discussed below may not necessarily be ordered in the same way as the elasticities reported here.

Which are the combination of sectors and outcomes that yield significant elasticities? When taking sectors as a whole, the average elasticities come out at around 0.02 to 0.03 for energy, transport, and digital. When looking at sector sub-samples, digital-micro output has an elasticity of 0.05, energy macro and micro output of 0.04 and 0.05 respectively, while transport yields larger values for micro output, at 0.08, and 0.16 for labor. Robustness checks excluding outliers based on extreme standard errors and t-statistics values or restricting the samples in various ways do not alter dramatically the general results.

Four, of specific interest to us is how these numbers would differ when splitting the sample of studies according to the level of development of countries. Again here, we find important heterogeneity. While effects appear larger in developed countries for digital and energy studies, cross-sectoral and transport studies based on developing countries produce larger elasticities. Notably, larger estimates are found when looking at specific outcomes in developing countries, such as the impact of digital investments on micro-level output, at 0.16, or of transport investments on labor market outcomes, at 0.18.

Finally, there is an important issue regarding how to interpret these elasticities for policy recommendations. Deciding on which policies or sectors to prioritize, and possibly on potential financing strategies, would require that we translate elasticities into specific rates of returns (see Gardner and Henry (2023)). This in turn implies the need for infrastructure capital stock figures. Using a basic Cobb-Douglas production function approach for illustration, the marginal rate of return for a specific type of infrastructure can be approximated by the following formula: $mrr = \gamma_{Inf} \cdot \frac{Y}{Inf}$, where γ_{Inf} is the elasticity, Y is GDP, and Inf is the stock of infrastructure.

There are however several difficulties involved in finding suitable proxies for Inf. Regarding public capital, different methods combine national account data, public budget data, and information from the Private Participation in Infrastructure (PPI) database to estimate infrastructure investments, which can then be used through inventory methods to compute estimates of stocks. As discussed in Fay et al. (2019), each method generates both exclusion and inclusion errors. In addition, public budget data is the only source allowing for sectoral breakdown. This practically means that the only recent source available, namely IMF data on Gross Fixed Capital Formation (GFCF) would be of little use here, not even considering the fact that it misses most of the private investment that dominates for example the digital sector.

Alternatively, one could generate bottom-up estimates of the value of sectoral infrastructure capital stocks by taking physical data on infrastructure stocks at the country level and applying unit replacement cost values to convert these into replacement value capital stocks in monetary units, as was done in Canning and Bennathan (2000) for the 1980s and

90s. Such country-level estimates would be needed to make meaningful conversions, as there are likely huge differences in GDP to infrastructure ratio, with countries with very large infrastructure stocks such as for example China boasting low values, possibly around two-thirds, while other very poor countries with very small infrastructure endowments may have a ratio of up to five. We intend to explore these important issues in subsequent work.

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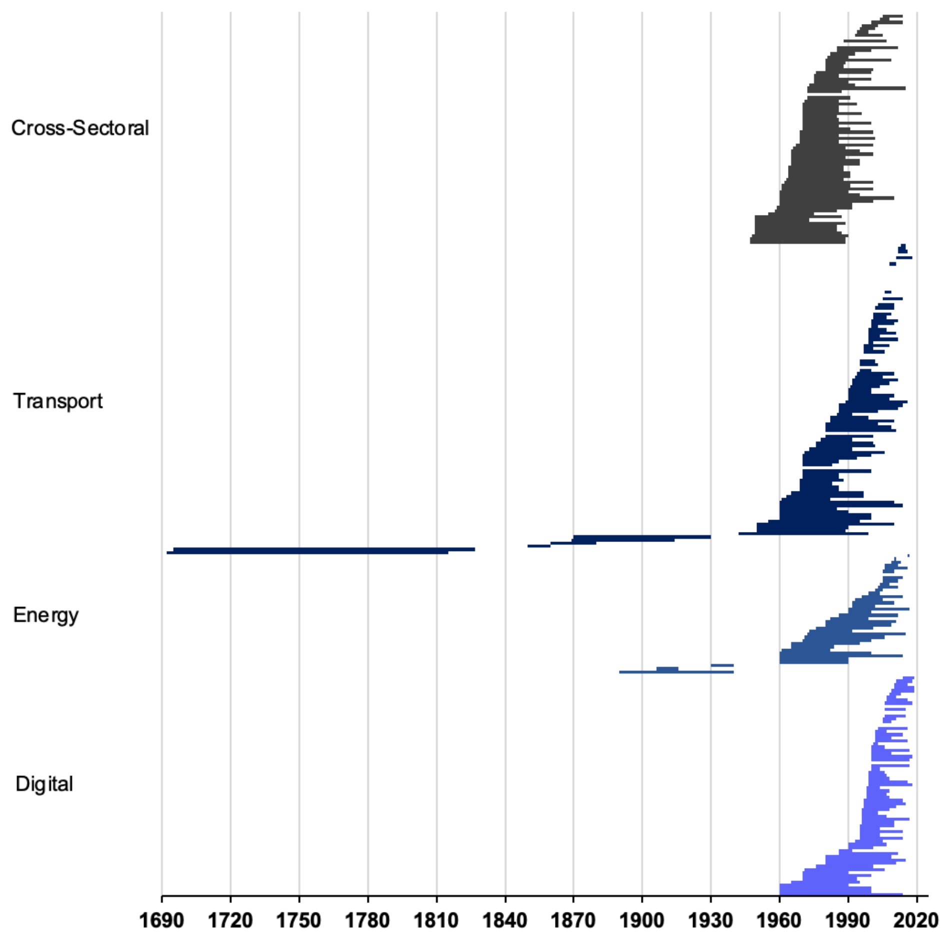
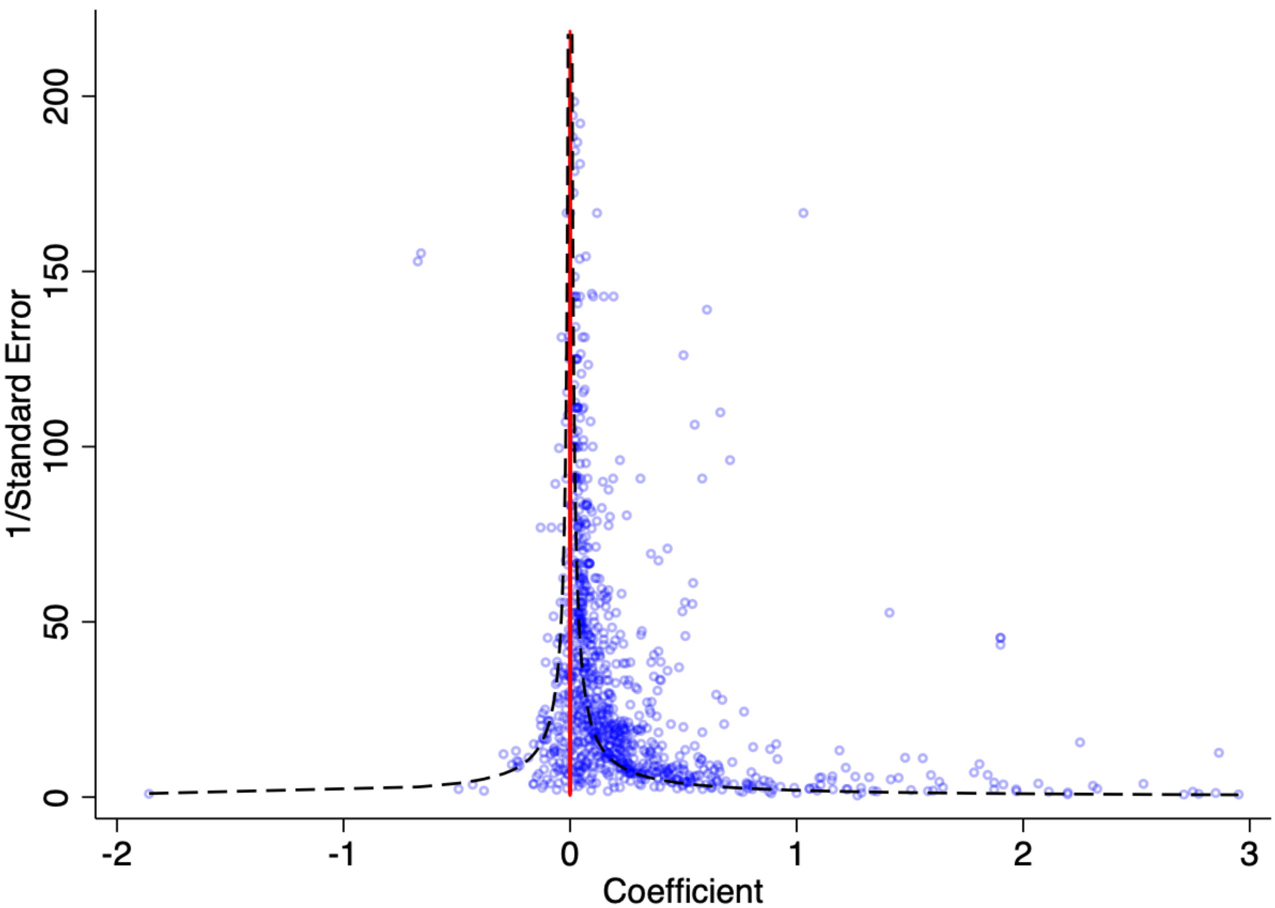
Figures

Figure 1: Time coverage of studies in the sample



Note: The infrastructure sectors studied in each paper (y axis) are plotted against the time-period covered in the paper (x axis).

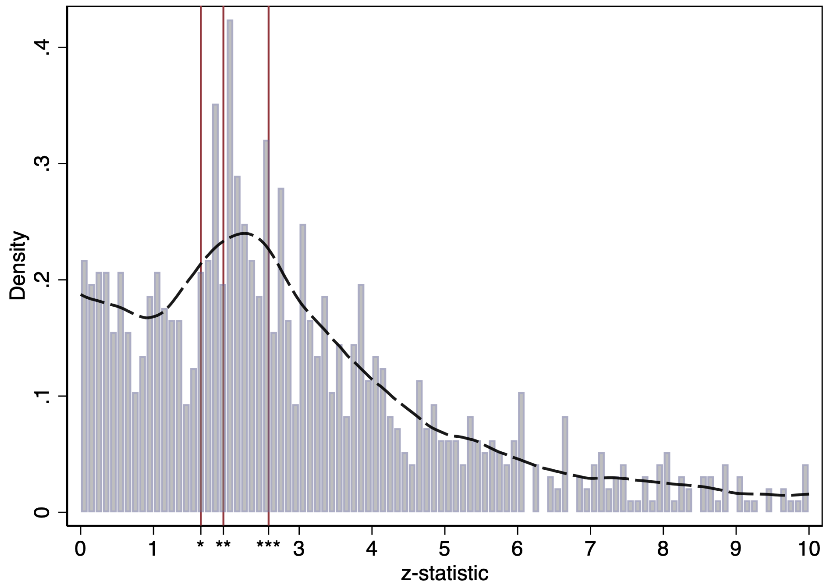
Figure 2: Funnel Plot - Full Sample



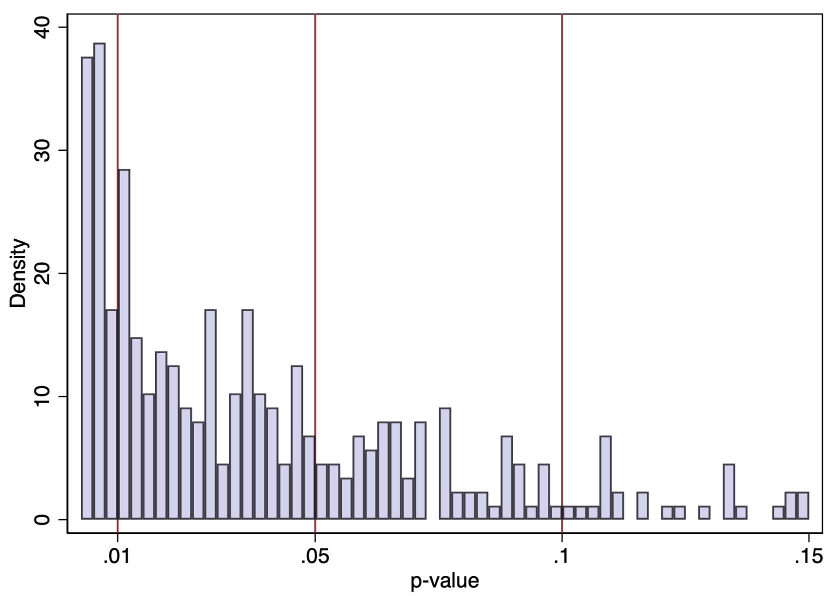
Note: This figure plots, for each estimate in the dataset the coefficient (horizontal axis) against the inverse of the standard error (vertical axis). The solid curves correspond to the 5 percent significance relationship, i.e., for each θ on the x-axis, the hypothetical value of $1/\text{se}(\theta)$ such that $1/\text{se}(\theta) = 1.96/\theta$ if $\theta > 0$ and $1/\text{se}(\theta) = -1.96 / \theta$ if $\theta < 0$. Elasticity coefficients larger than 3 or smaller than -3, and standard errors smaller than 1/200 are excluded for readability.

Figure 3: Distribution of z-statistics and p-values of Estimates

(a) Full Sample: z-statistics



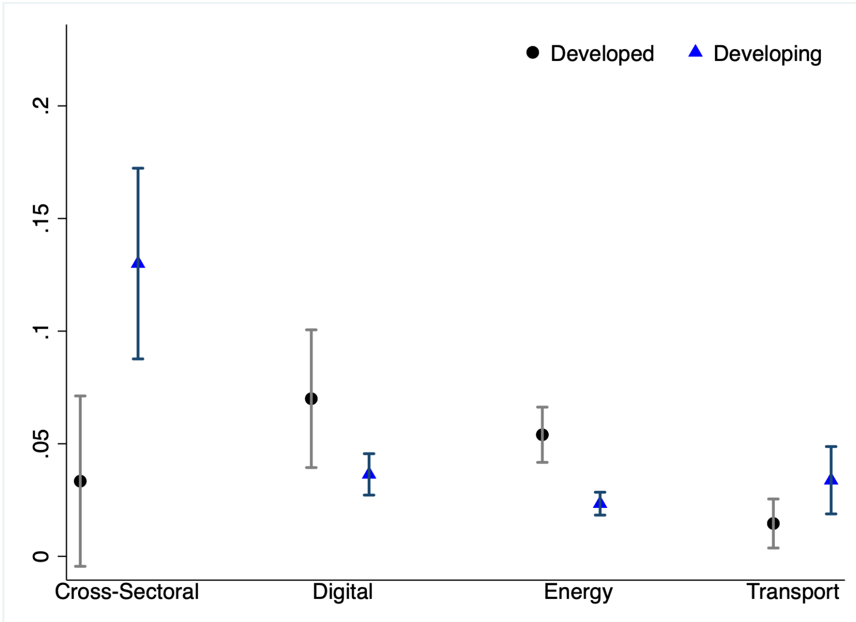
(b) Full Sample: p-values



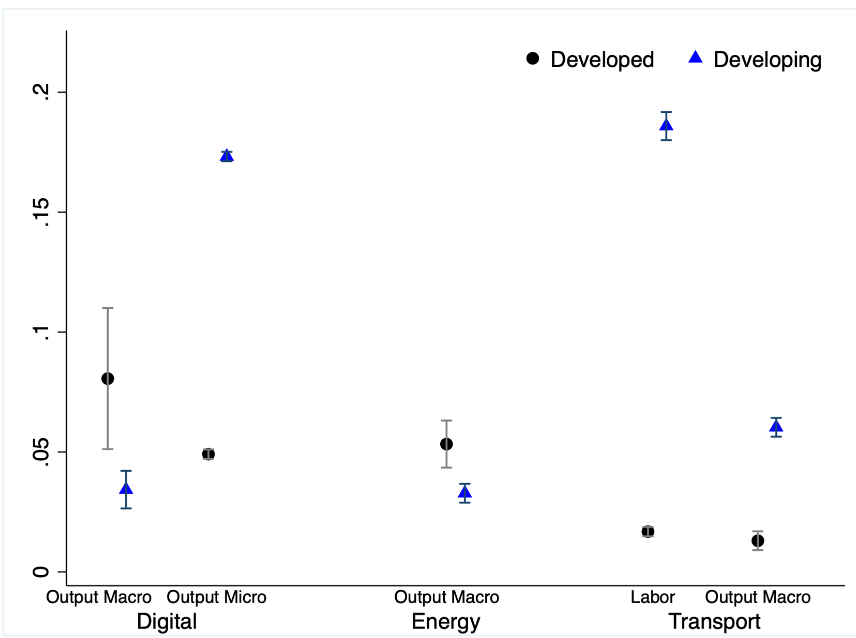
Notes: We follow Brodeur et al. (2023) and plot the distribution of z-statistics and p-values of our estimates for various cuts of the data. The figure in the top panel displays a histogram of test statistics for z ∈ [0,10], with bins of width 0.1. The figure in the bottom panel displays a histogram of p-values ∈ [0.0025,0.1500], with bins of width 0.0025. Vertical reference lines are displayed at conventional two-tailed significance levels. For the histograms in the left panel, we superimpose an Epanechnikov kernel density curve. We do not weight observations.

Figure 4: PEESE Estimates by level of development

(a) Sector-level samples



(b) Sector-outcome-level Samples



Notes: Figure plots PEESE estimates of the respective sub-samples. In the lower Panel, we do not show the estimates of Energy: Output Micro and Labor and Transport: Output Micro and trade categories as we do not have sufficient observations to run regressions with the developed country sample. 95% confidence intervals indicated by the error lines.

Tables

Table 1: Number of Estimates by Sector and Outcomes

	Digital	Energy	Transport	Cross-Sectoral	Total
Output Macro	146	60	208	212	626
Output Micro	50	88	34	14	186
Labor Market	28	55	61	2	146
Inequality / Poverty	4	9	4	0	17
Trade	1	0	25	4	30
Human Capital	0	6	8	0	14
Population	1	0	33	0	34
Environment	0	1	9	0	10
Land Value	0	0	11	0	11
Total	230	219	393	232	1074

Table 2: Sign of Estimates by Sector

	Digital	Energy	Transport	cross-sectoral	Total
Negative	23	20	59	39	141
Percentage	10%	9.13%	15.01%	16.81%	13.13%
Positive	207	199	334	193	933
Percentage	90%	90.87%	84.99%	83.19%	86.87%
Total	230	219	393	232	1,074

Table 3: FAT-PET Estimates

	(1) Full Sample	(2) Digital	(3) Energy	(4) Transport	(5) cross-sectoral
1/SE	0.0163	0.0135	-0.0004	0.0230	0.1624***
	(0.012)	(0.013)	(0.001)	(0.014)	(0.021)
Constant	3.9578***	3.3843***	3.1852***	3.9941**	-0.1170
	(0.934)	(1.163)	(0.365)	(1.633)	(1.042)
Observations	1,074	230	219	393	232
R-squared	0.087	0.130	0.000	0.061	0.566

Notes: FAT-PET estimates at the sector level. Robust standard errors, clustered at the article level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 4: PEESE Estimates

	(1) Full Sample	(2) Digital	(3) Energy	(4) Transport	(5) cross-sectoral
1/SE	0.0199	0.0153	0.0065**	0.0311**	0.1613***
	(0.012)	(0.013)	(0.003)	(0.013)	(0.021)
SE	5.2852**	0.9091	8.0849**	7.9020***	4.4989*
	(2.208)	(0.905)	(3.794)	(2.381)	(2.262)
Observations	1,074	230	219	393	232
R-squared	0.132	0.171	0.182	0.126	0.618

Notes: PEESE estimates at the sector level. Robust standard errors, clustered at the article level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 5: PEESE Estimates with Moderators selected by Various Methods

	(1) Full Sample	(2) Digital	(3) Energy	(4) Transport	(5) cross-sectoral
Panel A : No Moderators					
Main Effect	0.0199	0.0153	0.00651**	0.0311**	0.161***
	(0.0123)	(0.0129)	(0.00258)	(0.0135)	(0.0215)
Panel B : Rigorous LASSO					
Main Effect	0.0301***	0.0184***	0.0399***	0.0180*	0.0904***
	(0.00521)	(0.00348)	(0.00500)	(0.0100)	(0.00682)
Panel C : Cross Validation LASSO					
Main Effect	0.0388***	0.0205*	0.0339***	0.0145	0.0941
	(0.0101)	(0.0115)	(0.00483)	(0.0136)	(0.0757)
Panel D : BMA (covariates with PIP>0.1)					
Main Effect	0.0291***	0.0205*	0.0338***	0.0189*	0.108*
	(0.00907)	(0.0115)	(0.00465)	(0.0100)	(0.0616)
Panel E : BMA (model with largest PMP)					
Main Effect	0.0173*	0.0205*	0.0277***	0.0257**	0.185**
	(0.00988)	(0.0115)	(0.00751)	(0.0106)	(0.0852)
Observations	1,074	230	219	393	232

Note: The table presents PEESE estimates based on OLS regressions with controls for study characteristics (i.e. moderators). We refer the coefficient of 1/SE in the PEESE regression as *Main Effect* in the table. Panel A reproduces the PEESE estimates without any moderators from Table 4 for comparison. The moderators used in the OLS model in Panel B are selected based on rigorous LASSO developed in Belloni et al. (2014). For OLS estimates in Panel C, the moderators are selected using LASSO with 10-fold cross-validation for tuning parameter selection. To select moderators for Panels D and E, we used Bayesian Model Averaging. In Panel D, we present OLS with covariates with posterior inclusion probability > 0.1. In Panel E, we present OLS with covariates from the model with the highest posterior model probability. We do not report the controls and the squared-standard error terms for brevity. Robust standard errors, clustered at the study level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: PEESE Estimates by Sector-Outcome categories

	(1) Digital Output Micro	(2) Digital Output Macro	(3) Energy Output Micro	(4) Energy Output Macro	(5) Energy Labor	(6) Transport Output Micro	(7) Transport Output Macro	(8) Transport Labor	(9) Transport Trade	(10) Transport Population
Panel A : No Moderators										
Main Effect	0.0583***	0.0152	0.0062	0.0412***	0.0021***	0.0104	0.0474***	0.0446	0.5397	0.0107
	(0.016)	(0.013)	(0.005)	(0.003)	(0.000)	(0.012)	(0.010)	(0.048)	(0.447)	(0.012)
Panel B : Moderators selected using RLASSO										
Main Effect	0.0543***	0.0181***	0.0490***	0.0412***	0.0050	0.0756***	0.0322***	0.1636***	0.9009***	0.0107
	(0.004)	(0.004)	(0.002)	(0.003)	(0.004)	(0.014)	(0.005)	(0.014)	(0.009)	(0.012)
Observations	50	146	88	60	55	34	208	61	25	33

Notes: PEESE estimates at the sector-outcome level. Panel A does not include any moderators and Panel B uses rigorous penalization LASSO for moderator selection and presents the post-estimation OLS results. Data is grouped by sector-outcome categories, whenever the number of observations is large enough. Robust standard errors, clustered at the article level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 7: PEESE Estimates: Robustness Checks and Other Sub-Samples

	(1) Full Sample	(2) Digital	(3) Energy	(4) Transport	(5) cross-sectoral
Panel A : Exclude outliers					
Main Effect	0.0127**	0.0274***	0.0361***	0.0106	0.158**
	(0.00562)	(0.00275)	(0.00505)	(0.0106)	(0.0650)
Observations	1,022	200	215	381	226
Panel B : Estimate reported as elasticity in the article					
Main Effect	0.0364***	0.0590***	0.0463***	0.0186*	0.0905***
	(0.00440)	(0.0165)	(0.00242)	(0.0110)	(0.00684)
Observations	770	158	102	279	231
Panel C : Endogeneity addressed estimates					
Main Effect	0.0345***	0.0276***	0.0165*	0.0242**	0.157***
	(0.00415)	(0.00555)	(0.00917)	(0.0101)	(0.0229)
Observations	550	128	136	212	74
Panel D : Estimates based on a single-country					
Main Effect	0.0314***	0.0453***	0.0616***	0.0302***	0.0902***
	(0.00618)	(0.00697)	(0.00550)	(0.0112)	(0.00725)
Observations	753	94	159	332	168

Notes: This table reports PEESE estimates at the sector level. *Main Effect* corresponds to the coefficient on 1/SE in the PEESE regression. All regressions include moderators selected using rigorous penalization LASSO. Panel A excludes observations with a value of 1/SE larger than 200 and t-stat (absolute value) larger than 4. Panel B restricts the sample to estimates originally reported as elasticities in the study. Panel C restricts the sample to causal estimates (i.e. estimates that account for endogeneity). Panel D restricts the sample to estimates obtained from a single country (i.e. excludes estimates of elasticities from cross-country regressions). Robust standard errors, clustered at the study level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

The Impact of Infrastructure on Development Outcomes: A Meta-Analysis

Not for publication

A Variables in the Dataset

We have compiled a rich dataset containing the following information:

- **Identifiers**: Title of the Paper, Author(s), Source, Year, Type of Publication (Peer-reviewed or other), Author(s) affiliation (Academia, Government, International Institution, Private)
- **Sample and Data**: Names of countries covered in the study; Number of countries covered in the study, Indicator for cross-country study, Indicator for developing country only sample, Indicator for developed country only sample, Region, Income Group of the countries covered in the study, Time period covered, Type of data used (Country-level, Region-level, district/municipality-level, household-level, firm-level), Indicator for the use of Spatial data in the analysis,
- **Framework**: Indicator for the framework used in the analysis (General Equilibrium Trade model; Production Function; Cost Function; Other Structural Model)
- **Empirical Strategy**: Indicator for whether the paper treats endogeneity; Technique used to address potential endogeneity (IV, GMM, RCT, RD, DID, Fixed-effects), Indicator for cointegration, Indicator for using long-difference models
- **Outcome and Treatment**: Dependent variable, Broad category of the dependent variable (Output-Micro, Output-Macro, Labor), Independent variable (treatment), Broad category of the independent variable (Digital, Energy, Transport, Cross-Sectoral), Coefficient reported in the paper, Coefficient used in the analysis (semi-elasticities converted to elasticities and sign of treatment effect corrected depending on the definition of the independent variable), Standard Error, Level of Statistical Significance, Number of Observations, t-statistic, Link between Infrastructure and Development (Positive or Negative)

B List of Papers

Table A.1: Description of the papers used in the meta-analysis

No.	Article	Publication	Sectors	# Est.
1	Abeberese & Chen (2022)	Journal of Development Economics	Transport	3
2	Acheampong et al. (2021)	Energy Economics	Energy	14
3	Aevarsdottir et al. (2017)	IGC Working Paper	Energy	4
4	Aguirre (2012)	Cámara Chilena de la Construcción	cross-sectoral	4
5	Ai & Cassou (1995)	Applied Economics	cross-sectoral	2
6	Aker & Fafchamps (2015)	World Bank Economic Review	Digital	1
7	Akpandjar & Kitchens (2017)	Economic Development and Cultural Change	Energy	6
8	Ali et al. (2015)	World Bank Policy Research Working Paper	Transport	10
9	Ali et al. (2015)	World Bank Policy Research Working Paper	Transport	6
10	Ali et al. (2018)	Journal of Development Studies	Transport	2
11	Allcott et al. (2016)	American Economic Review	Energy	6
12	Andersson et al. (1990)	Regional Science and Urban Economics	Transport	10
13	Andrews & Swanson (1995)	Growth and Change	cross-sectoral	4
14	Arvanitis & Loukis (2009)	Information Economics and Policy	Digital	8
15	Aschauer (1989)	Journal of Monetary Economics	cross-sectoral	9
16	Atasoy (2013)	ILR Review	Digital	7
17	Badran (2012)	Topics in MENA Economies	Digital	2
18	Bahia et al. (2019)	30th European Conference of ITS	Digital	3
19	Bahia et al. (2020)	World Bank Policy Research Paper	Digital	6
20	Bahrini & Qaffas (2019)	Economies	Digital	4
21	Bajo-Rubio & Sosvilla-Rivero (1993)	Economic Modelling	cross-sectoral	1
22	Baltagi & Pinnoi (1995)	Empirical Economics	Transport, cross-sectoral	4
23	Banerjee et al. (2020)	Journal of Development Economics	Transport	6
24	Barron & Torero (2017)	Jr. of Environmental Economics and Management	Energy	1
25	Baum-Snow (2007)	Quarterly Journal of Economics	Transport	5
26	Baum-Snow et al. (2017)	The Review of Economics and Statistics	Transport	7
27	Baum-Snow et al. (2020)	Journal of Urban Economics	Transport	8
28	Berechman et al. (2006)	Transportation	Transport	3
29	Berndt & Hansson (1992)	Scandinavian Journal of Economics	cross-sectoral	1
30	Bertschek & Niebel (2016)	Telecommunications Policy	Digital	4
31	Bertschek et al. (2013)	Information Economics and Policy	Digital	2
32	Binswanger et al. (1993)	Journal of Development Economics	Energy, Transport	6
33	Bird & Straub (2019)	World Development	Transport	9
34	Blankespoor et al. (2017)	World Bank Policy Research Working Paper	Transport	15
35	Bloom et al. (2010)	Report	Digital	5
36	Boarnet (1998)	Journal of regional science	Transport	5
37	Bogart (2005)	Explorations in Economic History	Transport	1
38	Bogart (2009)	Economic History Review	Transport	3
39	Bonaglia et al. (2000)	Giornale degli Economisti e Annali di Economia	Digital, Transport, cross-sectoral	7
40	Bronzini & Piselli (2009)	Regional Science and Urban Economics	Transport, cross-sectoral	8

Table A.1 – continued from previous page

No	Article	Publication	Sectors	# Est.
41	Brooks et al. (2021)	IMF Economic Review	Transport	1
42	Bustillos & Flores (2012)	Estudios fronterizos	Transport, Digital, Energy, cross-sectoral	8
43	Buys et al. (2010)	Journal of African Economies	Transport	4
44	Cadot et al. (2006)	Journal of Public Economics	Transport	2
45	Calderon & Serven (2004)	World Bank Policy Research Working Paper	Energy, Digital, Transport	3
46	Calderón et al. (2014)	Journal of Applied Econometrics	Transport, Digital	2
47	Canning (1999)	World Bank Policy Research Working Paper	Energy, Digital	2
48	Canning & Bennathan (2000)	World Bank Policy Research Working Paper	Energy, Transport	2
49	Canning & Fay (1993)	Columbia University	Transport	6
50	Cantos et al. (2005)	Transport Reviews	Transport	10
51	Casaburi et al. (2013)	Mimeo	Transport	2
52	Castaldo et al. (2018)	Applied Economics	Digital	1
53	Chakamera & Alagidede (2018)	International Review of Applied Economics	cross-sectoral	4
54	Chakravorty et al. (2016)	Working Paper	Energy	8
55	Charlot & Schmitt (2000)	Working Paper, UMT INRA ENESAD	cross-sectoral	3
56	Chavula (2013)	Information Technology for Development	Digital	3
57	Chen & Whalley (2012)	American Economic Journal: Economic Policy	Transport	3
58	Chen et al. (2018)	Cambridge Jr. of Regions, Economy and Society	Transport	4
59	Colombo et al. (2013)	Information Economics and Policy	Digital	2
60	Creel & Poilon (2008)	International Review of Applied Economics	Transport, cross-sectoral	3
61	Cropper & Suri (2022)	Working Paper	Transport	4
62	Cropper & Suri (2022)	Working Paper	Transport	5
63	Crowder & Himarios (1997)	Applied Economics	cross-sectoral	4
64	Czernich et al. (2011)	The Economic Journal	Digital	2
65	Da Silva Costa et al. (1987)	Journal of Regional Science	cross-sectoral	3
66	Damania & Wheeler (2015)	World Bank Policy Research Working Paper	Transport	2
67	Dasso & Fernandez (2015)	IZA Journal of Labor and Development	Energy	8
68	De la Fuentes & Vives (1995)	Economic Policy	cross-sectoral	1
69	De Stefano et al. (2014)	Working Paper	Digital	2
70	Del Bo & Florio (2012)	European Planning Studies	Transport, Digital, cross-sectoral	25
71	Demetriades & Mamunea (2000)	The Economic Journal	cross-sectoral	13
72	DeStefano et al. (2018)	Journal of Economic Behavior & Organization	Digital	3
73	Donaldson (2018)	American Economic Review	Transport	4
74	Dorosh et al. (2012)	Agricultural Economics	Transport	9
75	Duranton & Turner (2012)	Review of Economic Studies	Transport	4
76	Duranton et al. (2014)	Review of Economic Studies	Transport	8
77	Edquist (2022)	Telecommunications Policy	Digital	2
78	Edquist et al. (2018)	Information Economics and Policy	Digital	7
79	Eisner (1991)	New England Economic Review	Transport, cross-sectoral	5
80	Eisner (1994)	Journal of Economic Behavior and Organization	cross-sectoral	1
81	Emran & Hou (2013)	The Review of Economics and Statistics	Transport	1
82	Erenburg (1998)	Applied Economics Letters	cross-sectoral	4

Table A.1 – continued from previous page

No	Article	Publication	Sectors	# Est.
83	Esfahani & Ramirez (2003)	Journal of Development Economics	Digital, Energy	2
84	Estache et al. (2005)	Working Paper	Energy, Digital, Transport	3
85	Evans & Karras (1994)	The Review of economics and statistics	Transport, cross-sectoral	5
86	Evans & Karras (1994)	Journal of Macroeconomics	cross-sectoral	5
87	Eynde & Wren-Lewis (2021)	CEPR Discussion Paper	Energy, Transport, cross-sectoral	3
88	Faber (2014)	Review of Economic Studies	Transport	6
89	Fajgelbaum & Redding (2022)	Journal of Political Economy	Transport	7
90	Fan & Chan-Kang (2005)	IFPRI Research Report	Transport	1
91	Farhadi et al. (2012)	PLOS one	Digital	4
92	Fedderke & Bogetić (2009)	World Development	Transport, Digital, Energy, cross-sectoral	9
93	Ferreira (1994)	Epge ensaios economicos	cross-sectoral	4
94	Finn (1993)	Fed Reserve Bank Richmond Econ. Quarterly	Transport	1
95	Fisher-Vanden et al. (2015)	Journal of Development Economics	Energy	1
96	Fiszbein et al. (2022)	NBER Working Paper	Energy	4
97	Ford & Poret (1991)	OECD Working Paper	cross-sectoral	1
98	Gachassin (2013)	Journal of African Economies	Transport	2
99	Gachassin et al. (2015)	Development Policy Review	Transport	2
100	Garces et al. (2002)	WP at Centro de Estudios Andaluces	cross-sectoral	1
101	Garcia-Mila & McGuire (1992)	Regional Science and Urban Economics	Transport	1
102	Garcia-Mila et al. (1996)	Review of Economics and Statistics	Transport	2
103	Ghani et al. (2016)	Economic Journal	Transport	18
104	Gibbons & Machin (2005)	Journal of Urban Economics	Transport	3
105	Gibbons & Wu (2020)	Journal of Economic Geography	Transport	6
106	Gibbons et al. (2019)	Journal of Urban Economics	Transport	11
107	Gonzales-Navarro & Turner (2018)	Journal of Urban Economics	Transport	4
108	Grainger & Zhang (2019)	Energy Policy	Energy	6
109	Greenstein & Spiller (1995)	Industrial and Corporate Change	Digital	2
110	Grimes et al. (2009)	Working Paper	Digital	1
111	Gruber et al. (2014)	Telecommunications Policy	Digital	1
112	Haftu (2018)	Telecommunications Policy	Digital	2
113	Haines & Margo (2006)	NBER Working Paper	Transport	5
114	Haller & Lyons (2015)	Telecommunications Policy	Digital	2
115	Harb (2017)	Economic Analysis and Policy	Digital	2
116	Hasbi (2017)	Conference Paper	Digital	6
117	Haughwout (2000)	Working Paper	Transport	2
118	Holtz-Eakin (1994)	The Review of Economics and Statistics	cross-sectoral	12
119	Holtz-Eakin & Lovely (1996)	Regional Science and Urban Economics	cross-sectoral	4
120	Holtz-Eakin & Schwartz (1995)	International Tax and Public Finance	Transport	3
121	Holtz-Eakin & Schwartz (1995)	Regional Science and Urban Economics	cross-sectoral	4
122	Hornbeck & Rotemberg (2021)	Mimeo	Transport	9
123	Hovhannisyan & Stamm (2021)	World Bank Policy Research Working Paper	Energy	6
124	Hulten et al. (2006)	The World Bank Economic Review	Energy, Transport	2

Table A.1 – continued from previous page

No	Article	Publication	Sectors	# Est.
125	Iacovone & Pereira-López (2018)	The World Bank	Digital	12
126	Ivus & Boland (2015)	Canadian Journal of Economics	Digital	4
127	Jacoby (2000)	The Economic Journal	Transport	4
128	Javid (2019)	Sustainability	Energy, cross-sectoral	8
129	Jedwab & Storeygard (2022)	Journal of the European Economic Association	Transport	6
130	Ji & Zhang (2017)	Economic Modelling	Energy, Transport, Digital	4
131	Jung & Lopez-Bazo (2020)	Telecommunications Policy	Digital	2
132	Kallal et al. (2021)	Technological Forecasting & Social Change	Digital	1
133	Kamps (2006)	IMF Staff Papers	cross-sectoral	26
134	Kandilov & Renkow (2010)	Growth and Change	Digital	3
135	Kara et al. (2016)	Regional Studies	Energy, cross-sectoral	4
136	Kassem (2021)	Working Paper	Energy	18
137	Kataoka (2005)	Review of Urban and Regional Dev. Studies	cross-sectoral	4
138	Katz et al. (2020)	Telecommunications Policy	Digital	2
139	Kawaguchi et al. (2009)	Jr. of Japanese and International Economies	cross-sectoral	3
140	Kebede (2021)	Mimeo - JMP	Transport	4
141	Kelejian & Robinson (1997)	Papers in Regional Science	Transport, cross-sectoral	4
142	Kemmerling & Stephan (2002)	Logistics and Transportation Review	cross-sectoral	2
143	Khan & Majeed (2019)	South Asian Studies	Digital	12
144	Khandker et al. (2009)	Economic Development and Cultural Change	Transport	12
145	Khandker et al. (2012)	World Bank Policy Research Working Paper	Energy	18
146	Khandker et al. (2013)	Economic Development and Cultural Change	Energy	10
147	Khanna & Sharma (2020)	The Quarterly Review of Economics and Finance	Transport, Energy, Digital	16
148	Khanna & Sharma (2021)	Applied Economics Letters	cross-sectoral	12
149	Kim et al. (2021)	East Asian Economic Review	Transport, Digital, Energy	12
150	Kitchens & Fishback (2015)	Journal of Economic History	Energy	6
151	Kolko (2012)	Journal of Urban Economics	Digital	4
152	Koolwal et al. (2017)	The World Bank Economic Review	Energy	8
153	Koutroumpis (2009)	Telecommunications Policy	Digital	2
154	Koutroumpis (2019)	Technological Forecasting & Social Change	Digital	2
155	Kurniawati (2021)	Journal of Asian Business and Economic Studies	Digital	5
156	Lall et al. (2009)	World Bank Policy Research Working Paper	Transport	1
157	Lam & Shiu (2010)	Telecommunications Policy	Digital	2
158	Li et al. (2019)	ADBI Working Paper	Transport	8
159	Ligthart (2002)	Quarterly Review of Economics and Finance	cross-sectoral	3
160	Limao & Venables (2001)	World Bank Economic Review	cross-sectoral	4
161	Lv et al. (2023)	Int. Jr. of Environmental Research and Public Health	Digital	2
162	Majeed & Ayub (2018)	Pakistan Journal of Commerce and Social Sciences	Digital	8
163	Marrocu & Paci (2010)	Applied Economics	cross-sectoral	1
164	Martincus & Blyde (2013)	Journal of International Economics	Transport	1
165	Martincus et al. (2017)	Journal of Development Economics	Transport	5
166	Mas et al. (1996)	Regional Studies	cross-sectoral	2
167	Masaki et al. (2020)	World Bank Policy Research Paper	Digital	12

Table A.1 – continued from previous page

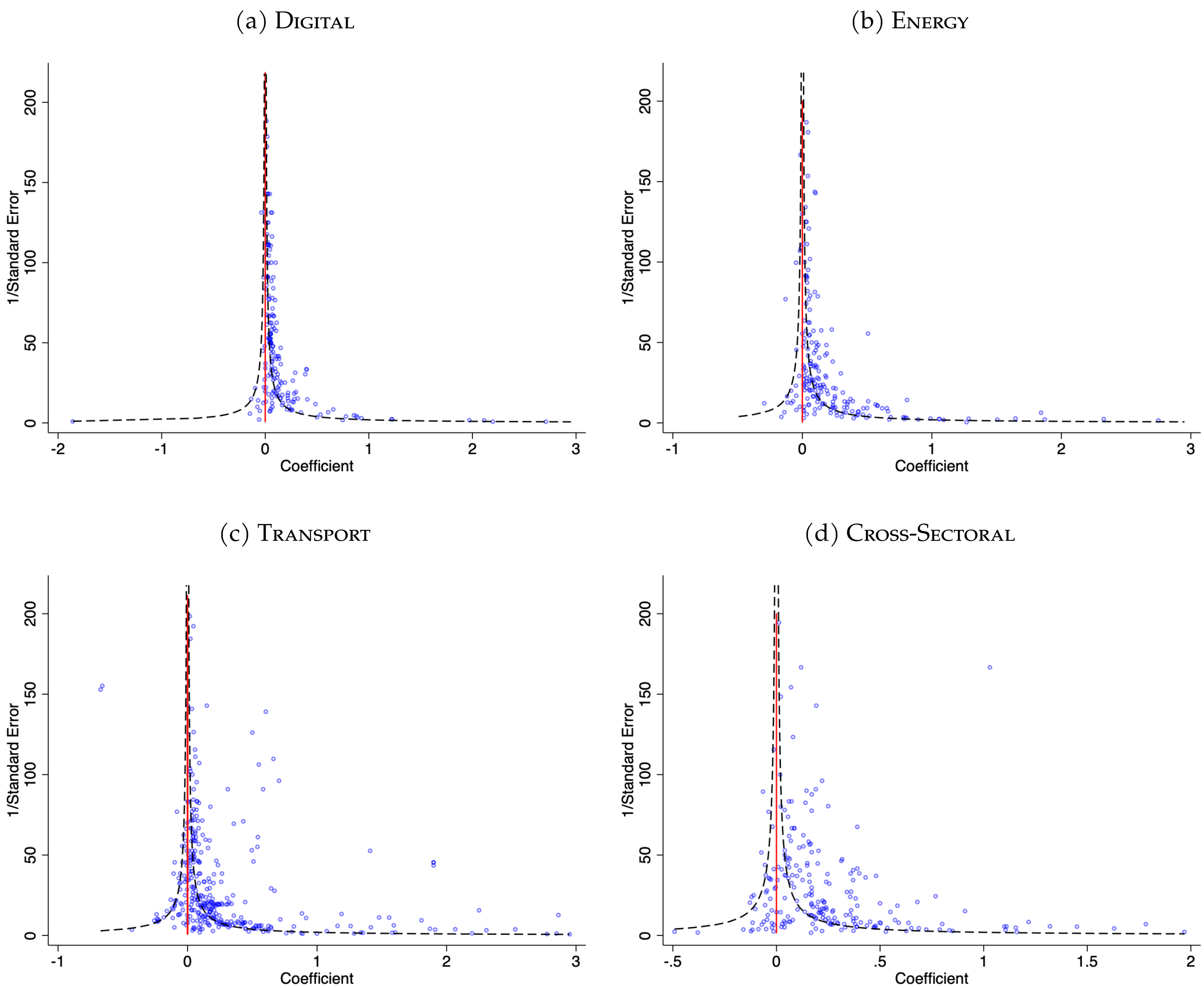
No	Article	Publication	Sectors	# Est.
168	Matilde et al. (1993)	Papeles de Economica Espanola	cross-sectoral	2
169	Matilde et al. (1994)	Moneda y Credito	cross-sectoral	2
170	Mensah (2018)	World Bank Policy Research Working Paper	Energy	32
171	Michaels (2008)	Review of Economics and Statistics	Transport	3
172	Mizutani & Tanaka (2010)	The annals of regional science	cross-sectoral	4
173	Molinder et al. (2021)	The Journal of Economic History	Energy	1
174	Moreno & Lopez-Bazo (2007)	Regional Science Review	Transport	2
175	Moreno et al. (1997)	International Jr. of Development Planning Literature	cross-sectoral	3
176	Moreno-Monroy & Ramos (2021)	Research in Transportation Econonomics	Transport	2
177	Morten & Oliveira (2018)	NBER Working Paper	Transport	4
178	Munnell (1990)	New England Economic Review	cross-sectoral	2
179	Naaraayanan & Wolfenzon (2019)	Working Paper	Transport	2
180	Nakamura et al. (2019)	World Bank Policy Research Working Paper	Transport	2
181	Nannan & Jianing (2012)	Proceedings of ICPM 2012	cross-sectoral	1
182	Navarro & Domeque (2016)	Review of Economics and Statistics	Transport	3
183	Ndubuisi et al. (2021)	Telecommunications Policy	Digital	6
184	Niebel (2018)	World Development	Digital	2
185	Nombela (2005)	Presupuesto y gasto público	Transport	5
186	Noumba Um et al. (2009)	World Bank Policy Research Paper	Transport, Digital, Energy	3
187	Oreggia & Pose (2004)	World Development	cross-sectoral	4
188	Otto & Voss (1994)	Economic Record	cross-sectoral	2
189	Otto & Voss (1996)	Southern Economic Journal	cross-sectoral	1
190	Otto & Voss (1998)	Journal of Monetary Economics	cross-sectoral	2
191	Otto & Voss (2003)	Applied Economics	cross-sectoral	2
192	Ouattara & Zhang (2019)	Empirical Economics	cross-sectoral	2
193	Owyong & Thangavelu (2001)	Applied Economics	cross-sectoral	2
194	Ozbay et al. (2007)	Transport Policy	Transport	3
195	Picci (1999)	Giornale degli Economisti e Annali di Economia	cross-sectoral	3
196	Ratner (1983)	Economics Letters	cross-sectoral	2
197	Rivera & Toledo (2004)	Estudios de economía	cross-sectoral	1
198	Roberty et al. (2012)	Regional Science and Urban Economics	Transport	6
199	Roller & Waverman (2001)	American Economic Review	Digital	2
200	Rud (2012)	Journal of Development Economics	Energy	5
201	Samad & Zhang (2016)	World Bank Policy Research Paper	Energy	18
202	Samad & Zhang (2017)	World Bank Policy Research Paper	Energy	14
203	Sawng et al. (2021)	Telecommunications Policy	Digital	1
204	Sen & Saray (2019)	Jr. of Economic Cooperation and Development	Digital	9
205	Sheard (2014)	Journal of Urban Economics	Transport	8
206	Shioji (2001)	Journal of economic growth	cross-sectoral	12
207	Sridhar & Sridhar (2007)	Applied Econometrics and Int. Development	Digital	4
208	Stephan (2003)	International Review of Applied Economics	cross-sectoral	2
209	Storeygard (2016)	Review of Economic Studies	Transport	3
210	Sturm & de Haan (1995)	Economic Modelling	cross-sectoral	2

Table A.1 – continued from previous page

No	Article	Publication	Sectors	# Est.
211	Syverson (2017)	Journal of Economic Perspectives	Digital	1
212	Tatom (1991)	Federal Reserve Bank of St Louis Review	cross-sectoral	1
213	Thompson & Garbacz (2011)	Telecommunications Policy	Digital	2
214	Toader et al. (2018)	Sustainability	Digital	8
215	Urrunaga & Aparicio (2012)	Revista de la CEPAL	Digital, Energy, Transport	3
216	Vijverberg et al. (1997)	Review of Economics and Statistics	cross-sectoral	2
217	Wang et al. (2022)	Sustainability	Digital	2
218	Waverman et al. (2005)	Working Paper	Digital	1
219	Yousefi (2011)	Economics of Innovation and New Technology	Digital	3
220	Zhang et al. (2022)	Finance Research Letters	Digital	2
221	Zhou (2022)	Journal of Infrastructure, Policy and Development	Digital	2

C Additional Figures

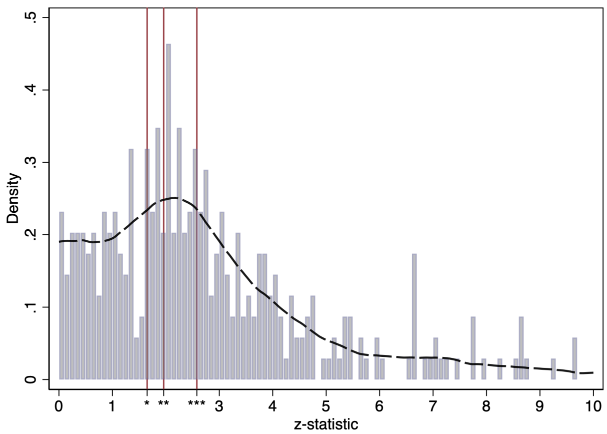
Figure A.1: Funnel Plots by Sector



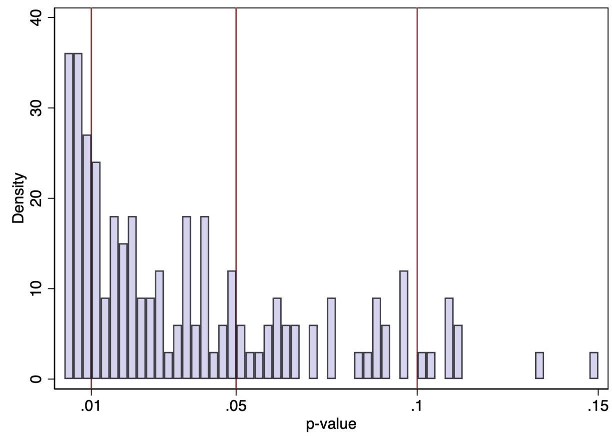
Source: Authors' elaboration

Figure A.2: Distribution of *z*-statistics and *p*-values of Estimates by Sector

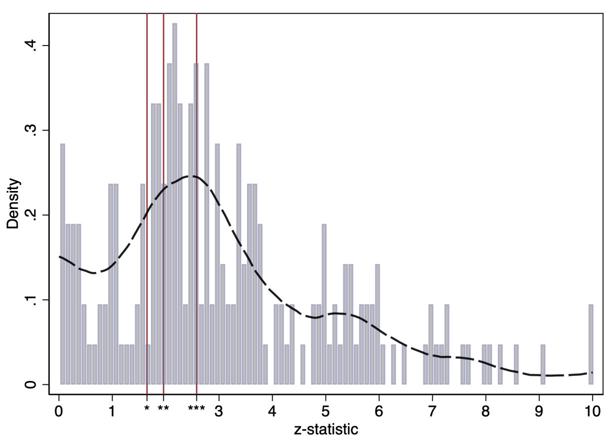
(a) Transport: *z*-statistics



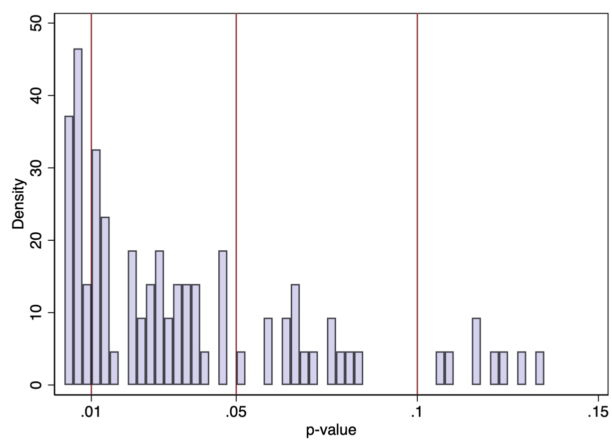
(b) Transport: *p*-values



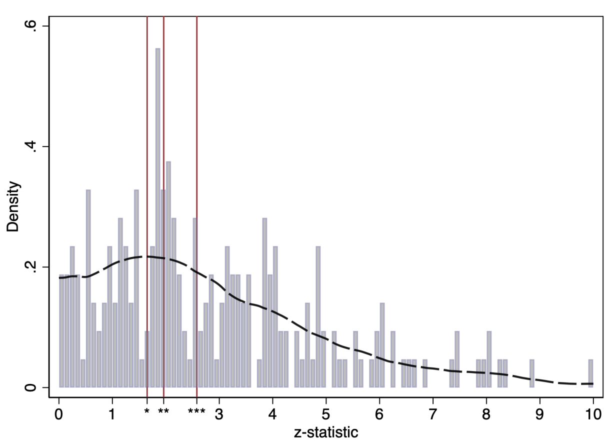
(c) Digital: *z*-statistics



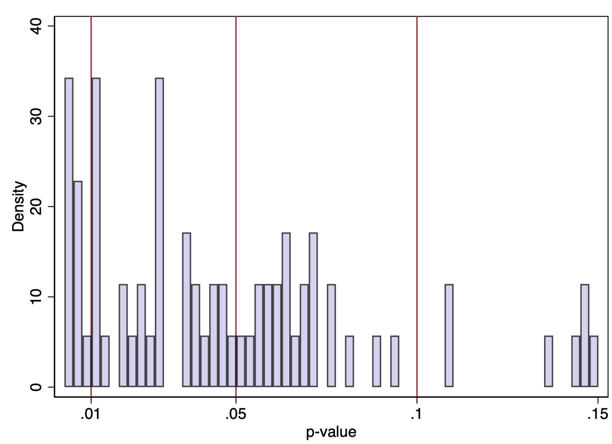
(d) Digital: *p*-values



(e) Energy: *z*-statistics



(f) Energy: *p*-values



Notes: We follow Brodeur et al. (2023) and plot the distribution of z-statistics and p-values of our estimates for various cuts of the data. The figure in the left panel displays a histogram of test statistics for z ∈ [0,10], with bins of width 0.1. The figure in the right panel displays a histogram of p-values ∈ [0.0025,0.1500], with bins of width 0.0025. Vertical reference lines are displayed at conventional two-tailed significance levels. For the histograms in the left panel, we superimpose an Epanechnikov kernel density curve. We do not weight observations.

D Additional Tables

Table A.2: Summary Statistics at the Paper Level

	Full Sample	Digital	Energy	Transport	cross-sectoral
Number of Estimates	4.860	3.673	9.238	4.838	3.843
	(4.559)	(2.906)	(7.509)	(3.480)	(4.351)
Published	0.760	0.818	0.524	0.721	0.882
in top 5 journal	0.041	0.018	0.048	0.103	0
in top field journal	0.190	0.055	0.143	0.382	0.118
Author in academia	0.751	0.782	0.714	0.721	0.725
Solo-authored paper	0.231	0.273	0.143	0.221	0.294
Publication Year					
published in 1990s	0.195	0.018	0	0.103	0.569
published in 2000s	0.217	0.109	0	0.250	0.294
published in 2010s	0.434	0.636	0.762	0.471	0.118
published in 2020s	0.154	0.236	0.238	0.176	0.020
Study uses micro-data	0.204	0.236	0.667	0.235	0.020
Endogeneity addressed	0.697	0.818	0.905	0.765	0.431
Empirical Strategy					
Instrumental variables	0.466	0.545	0.524	0.529	0.314
Difference-in-difference	0.100	0.091	0.190	0.176	0
Regression discontinuity	0.014	0.018	0	0.029	0
Randomized control trial	0.009	0	0.048	0.015	0
Cross-country study	0.253	0.564	0.143	0.088	0.137
Number of countries	15.2	33.4	15.9	4.4	6.1
Developing country included	0.439	0.382	0.762	0.603	0.157
Region					
North America & Europe	0.376	0.291	0.143	0.353	0.608
South Asia	0.100	0.018	0.333	0.132	0
Africa	0.122	0.127	0.143	0.206	0.020
East Asia and Pacific	0.136	0.109	0.190	0.162	0.157
Latin America	0.077	0.036	0.095	0.118	0.059
Includes multiple regions	0.190	0.418	0.095	0.029	0.157
Observations	221	55	21	68	51

Note: This table presents mean outcomes for our sample at the paper level, split by the four categories of infrastructure treatment: digital, energy, transport, and cross-sectoral (including public capital). Standard deviations are reported in parentheses. Top 5 journals are: American Economic Review, Econometrica, Quarterly Journal of Economics, Journal of Political Economy and Review of Economic Studies. Top field journals include top 5 journals, and AEJ: Economic Policy, Journal of Monetary Economics, Journal of Development Economics, Journal of Public Economics, Journal of Urban Economics, Journal of International Economics, Review of Economics and Statistics, Economic Journal, Journal of Applied Econometrics, Journal of the European Economic Association, and Journal of Economic Growth. 26 papers study more than one infrastructure treatment and are not included in computing the sector-level summary statistics. Results are qualitatively similar and are available on request.

Table A.3: Summary Statistics of Elasticity Estimates across cuts of Data

	Unweighted			Weighted			
	mean	σ	median	mean	σ	median	N
Full Sample	0.270	0.822	0.092	0.215	0.539	0.087	1074
Digital	0.101	0.591	0.053	0.119	0.412	0.050	230
Energy	0.404	1.451	0.099	0.269	0.982	0.097	219
Transport	0.305	0.615	0.099	0.258	0.548	0.096	393
cross-sectoral	0.250	0.338	0.170	0.230	0.273	0.170	232
Digital $\times$ Output-micro	0.099	1.206	0.085	0.177	0.798	0.081	50
Digital $\times$ Output-macro	0.082	0.130	0.050	0.077	0.118	0.048	146
Energy $\times$ Output-micro	0.352	0.674	0.126	0.269	0.443	0.116	88
Energy $\times$ Output-macro	0.737	2.614	0.088	0.301	1.499	0.064	60
Transport $\times$ Output-micro	0.335	0.524	0.189	0.283	0.383	0.175	34
Transport $\times$ Output-macro	0.281	0.625	0.077	0.215	0.486	0.074	208

Note: This table presents the mean, standard deviation, and median of the estimates for various cuts of the data. In the right panel, we use the inverse of the number of estimates presented in the same article to weigh observations following Brodeur et al. (2023). We suppress outcome-type estimates for cross-sectoral studies for brevity. We only show two outcome types (viz. output-micro and output-macro) and suppress others such as labor etc for brevity.

Table A.4: Sign of estimates by types of outcome

	Output Macro	Output Micro	Labor	Ineq. / Poverty	Trade	Human Capital	Population	Environment	Land Value	Total
Negative	76	23	16	7	1	2	14	2	0	141
Percentage	12.14	12.37	10.96	41.18	3.33	14.29	41.18	20.00	0.00	13.13
Positive	550	163	130	10	29	12	20	8	11	933
Percentage	87.86	87.63	89.04	58.82	96.67	85.71	58.82	80.00	100.00	86.87
Total	626	186	146	17	30	14	34	10	11	1074

Table A.5: Elliot et al. (2022) Tests at Thresholds

	10 percent Sig.		5 percent Sig.		1 percent Sig.	
Panel A. Binomial Test						
	p-value	Obs. [0.09, 0.1]	p-value	Obs. [0.04, 0.05]	p-value	Obs. [0.005, 0.015]
Full Sample	0.623	10	0.229	29	0.901	87
Panel B. Discontinuity Test						
	p-value	Obs. [0.061]	p-value	Obs. [0.013]	p-value	Obs. [0.003]
Full Sample	0.566	135	0.308	63	0.317	51

Notes: Table reports Elliott et al. (2022) binomial and discontinuity tests on the p-curve for the full sample. Panel A performs the binomial test which examines the null hypothesis that the p-curve is non-increasing just below a significance cutoff. For the 5 percent significance threshold, we follow Elliot et al. and split [0.04, 0.05] into two sub-intervals [0.040, 0.045] and (0.045, 0.050]. Under the null of no p-hacking, the fraction of p-values in (0.045, 0.050] should be smaller than or equal to one-half; i.e., the fraction of p-values in the bin closer to the cutoff should be weakly smaller than the fraction in the bin farther away. For the 10 percent significance level, we split [0.09, 0.1] into two sub-intervals [0.090, 0.095] and (0.095, 0.010] and for the 1 percent significance level, we use the sub-intervals [0.005,0.01] and (0.01, 0.015] following Brodeur et al. (2023). Panel B performs the density discontinuity test of Cattaneo et al. (2020). Optimal bandwidth is reported in square brackets along with the effective number of observations.

Table A.6: Binomial Tests at Significance Thresholds

Panel A.	10 percent signifiance			
	window length /2	Observations		p-value
		< 0.1	>=0.1	
	0.034	44	20	0.0037
	0.037	52	21	0.0004
	0.040	60	21	0.0000
	0.043	67	21	0.0000
	0.046	69	23	0.0000
Panel B.	5 percent significance			
	window length /2	Observations		p-value
		< 0.05	>=0.05	
	0.011	35	20	0.0581
	0.012	35	20	0.0581
	0.012	35	21	0.0814
	0.012	37	21	0.0479
	0.013	40	23	0.0430
Panel C.	1 percent significance			
	window length /2	Observations		p-value
		< 0.01	>=0.01	
	0.003	20	27	0.3817
	0.003	21	27	0.4709
	0.003	21	27	0.4709
	0.003	22	27	0.5682
	0.003	24	27	0.7798

Notes: We perform manipulation tests on the p-curve based on finite sample exact binomial testing following the results in Cattaneo et al. (2020). p-values of binomial tests around significance thresholds are reported in the table along with the number of observations around the cutoff (statistical significance levels) and 5 different half lengths of the window around the cutoff. Tests performed using the rddensity command in Stata.

Table A.7: Caliper Test, Bunching near Statistical Significance Thresholds

	10 percent significant		5 percent significant		1 percent significant	
	(1)	(2)	(3)	(4)	(5)	(6)
Solo-authored	-0.144	-0.133	-0.159	-0.141	0.0729	0.0280
	(0.136)	(0.137)	(0.124)	(0.121)	(0.125)	(0.123)
Published	0.0827	0.0942	0.0877	0.0497	0.332***	0.282***
	(0.107)	(0.107)	(0.0979)	(0.0973)	(0.0951)	(0.100)
Micro-data used	0.249**	0.284***	-0.188*	-0.218**	-0.0806	-0.0750
	(0.103)	(0.102)	(0.101)	(0.0926)	(0.126)	(0.122)
Endogeneity addressed	-0.247**		0.214**		0.0847	
	(0.0973)		(0.0868)		(0.105)	
Identification strategy						
IV		-0.289**		0.180*		0.193
		(0.121)		(0.105)		(0.170)
DID		-0.724***		0.670***		-0.0938
		(0.138)		(0.249)		(0.134)
others		-0.110		0.142		0.0431
		(0.125)		(0.116)		(0.113)
Observations	113	113	166	166	126	126
z sample bounds	[1.35,1.95]	[1.35,1.95]	[1.66,2.26]	[1.66,2.26]	[2.28,2.88]	[2.28 2.88]

Notes: Table reports marginal effects from logit regressions. An observation is a test statistic. The caliper test compares test statistics within a narrowly chosen bandwidth. We use a bandwidth of ±0.3. The dependent variables are dummies for whether the test statistics are significant at the 10, 5, and 1 percent levels. *others* include identification strategies such as RCT, regression discontinuity, propensity-score matching, generalized method of moments etc. Robust standard errors are in parentheses, clustered at the study level. We follow Brodeur et al. (2023) and use the inverse of the number of test statistics presented in the same study to weight observations.

Table A.8: PEESE Estimates by Sector: Model Selection by Rigorous LASSO

	(1) Full Sample	(2) Digital	(3) Energy	(4) Transport	(5) cross-sectoral
Main Effect	0.0301***	0.0184***	0.0399***	0.0180*	0.0904***
	(0.00521)	(0.00348)	(0.00500)	(0.0100)	(0.00682)
𝟙(Author Affl.- Academia)			-0.0170		
			(0.0162)		
𝟙(Author Affl.- Govt.)	0.0619***	0.0670***			
	(0.0166)	(0.0149)			
𝟙(Author Affl.- Institution)					
Public Capital	0.0530**		-0.0164		
	(0.0205)		(0.0212)		
Endogeneity	0.00893				
	(0.00551)				
Developing country		0.0145	-0.0291***		
		(0.0107)	(0.00871)		
Production Function			0.0400***		
			(0.0121)		
DID			0.0526**		
			(0.0228)		
Spatial Data					-0.156***
					(0.0175)
IV					-0.123***
					(0.0189)
Cointegration	0.0476***			0.0483***	
	(0.00830)			(0.0108)	
Peer Reviewed					
Publication Year					
Cross-country					
Regional data					
District-level data					
Fixed Effects					
Firm-level data					
Median sample time					
Household data					
Observations	1,074	230	219	393	232
R-squared	0.367	0.643	0.291	0.077	0.142

Notes: Table reports PEESE regression estimates with moderators selected using rigorous penalization LASSO. *Main Effect* corresponds to the coefficient on 1/SE in the PEESE regression. The coefficient on standard error (squared) is omitted for brevity. Robust standard errors, clustered at the study level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: PEESE Estimates by Sector: Model Selection by CV-LASSO

	(1) Full Sample	(2) Digital	(3) Energy	(4) Transport	(5) cross-sectoral
Main Effect	0.0388***	0.0205*	0.0339***	0.0145	0.0941
	(0.0101)	(0.0115)	(0.00483)	(0.0136)	(0.0757)
𝟙(Author Affl.- Academia)					0.0705
					(0.0726)
𝟙(Author Affl.- Govt.)					
𝟙(Author Affl.- Institution)				0.0547	
				(0.0418)	
Public Capital				-0.0537	0.139
				(0.0497)	(0.0880)
Endogeneity					0.193**
					(0.0916)
Developing country	0.0286***			-0.0329	
	(0.00973)			(0.0284)	
Production Function					-0.160
					(0.129)
DID				0.0171	
				(0.0316)	
Spatial Data	-0.0112		0.0456**	-0.0552	
	(0.0176)		(0.0174)	(0.0387)	
IV	-0.00679		-0.00443		-0.302***
	(0.0124)		(0.00733)		(0.0793)
Cointegration				0.00120	
				(0.0672)	
Peer Reviewed	0.0251			-0.0123	0.112**
	(0.0177)			(0.0398)	(0.0464)
Publication Year	-0.00493***				-0.00332
	(0.00111)				(0.00705)
Cross-country	-0.00553			-0.135	0.397***
	(0.0154)			(0.0955)	(0.133)
Regional data	0.00584				0.0406
	(0.0401)				(0.0540)
District-level data	0.0145		0.0438***	0.0385	-0.0808
	(0.0152)		(0.0111)	(0.0713)	(0.0737)
Fixed Effects	0.0236			0.0223	-0.113*
	(0.0159)			(0.0470)	(0.0596)
Firm-level data		0.0434**			0.232
		(0.0207)			(0.140)
Median sample time				-0.000929	-0.000881
				(0.00111)	(0.00537)
Household data				-0.000470	
				(0.0404)	
Observations	1,074	230	219	393	232
R-squared	0.282	0.006	0.182	0.154	0.650

Notes: Table reports PEESE regression estimates with moderators selected using cross-validation lasso. *Main Effect* corresponds to the coefficient on 1/SE in the PEESE regression. The coefficient on standard error (squared) is omitted for brevity. Robust standard errors, clustered at the study level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: PEESE: Model Selection by Bayesian Averaging (PIP > 0.1)

	(1) Full Sample	(2) Digital	(3) Energy	(4) Transport	(5) cross-sectoral
Main Effect	0.0291***	0.0205*	0.0338***	0.0189*	0.108*
	(0.00907)	(0.0115)	(0.00465)	(0.0100)	(0.0616)
𝟙(Author Affl.- Academia)					
𝟙(Author Affl.- Govt.)					
𝟙(Author Affl.- Institution)				0.0619	
				(0.0579)	
Public Capital					0.165
					(0.128)
Endogeneity					0.120
					(0.0788)
Developing country				-0.0112	
				(0.0188)	
Production Function					-0.136
					(0.112)
DID					
Spatial Data	-0.0272		0.0455**	-0.0490*	
	(0.0224)		(0.0170)	(0.0253)	
IV	0.0281		-0.00536		-0.215***
	(0.0245)		(0.00696)		(0.0689)
Cointegration					
Peer Reviewed					0.0623
					(0.0445)
Publication Year					-0.00671
					(0.00498)
Cross-country	-0.0258			-0.103	0.403**
	(0.0216)			(0.0739)	(0.178)
Regional data					-0.0243
					(0.0322)
District-level data			0.0395***	0.0361	-0.0522
			(0.0125)	(0.0368)	(0.0515)
Fixed Effects				0.0173	
				(0.0336)	
Firm-level data		0.0434**			0.215
		(0.0207)			(0.157)
Median sample time			-8.26e-05	-0.000366	
			(0.000136)	(0.000966)	
Household data					
Observations	1,074	230	219	393	232
R-squared	0.108	0.006	0.183	0.140	0.597

Notes: Table reports PEESE regression estimates with moderators selected using Bayesian Model Averaging with posterior inclusion probability > 0.1 . *Main Effect* corresponds to the coefficient on 1/SE in the PEESE regression. The coefficient on standard error (squared) is omitted for brevity. Robust standard errors, clustered at the study level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A.11: PEESE: Model Selection by Bayesian Averaging (*highest* PMP)

	(1) Full Sample	(2) Digital	(3) Energy	(4) Transport	(5) cross-sectoral
Main Effect	0.0173*	0.0205*	0.0277***	0.0257**	0.185**
	(0.00988)	(0.0115)	(0.00751)	(0.0106)	(0.0852)
$\mathbb{1}$(Author Affl.- Academia)					
$\mathbb{1}$(Author Affl.- Govt.)					
$\mathbb{1}$(Author Affl.- Institution)				0.0359	
				(0.0415)	
Public Capital					
Endogeneity					
Developing country					
Production Function					-0.0239
					(0.0461)
DID					
Spatial Data	-0.0103		0.0680***	-0.0450	
	(0.0180)		(0.0227)	(0.0284)	
IV					
Cointegration					
Peer Reviewed					0.0543
					(0.0369)
Publication Year					
Cross-country				-0.0905	0.287
				(0.0654)	(0.302)
Regional data					0.0416
					(0.0437)
District-level data					-0.0422
					(0.0428)
Fixed Effects				0.00776	
				(0.0285)	
Firm-level data		0.0434**			-0.117***
		(0.0207)			(0.0434)
Median sample time					
Household data					
Observations	1,074	230	219	393	232
R-squared	0.008	0.006	0.168	0.122	0.248

Notes: Table reports PEESE regression estimates with moderators of the model with the highest posterior model probability obtained after performing Bayesian Model Averaging on all combinations of models. *Main Effect* corresponds to the coefficient on 1/SE in the PEESE regression. The coefficient on standard error (squared) is omitted for brevity. Robust standard errors, clustered at the study level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: PEESE by Sector: Developed and Developing Country Samples

	(1) Digital	(2) Energy	(3) Transport	(4) cross-sectoral
Panel A : Developed Country				
Main Effect	0.0700***	0.0540***	0.0146**	0.0334*
	(0.0156)	(0.00626)	(0.00555)	(0.0193)
Observations	142	39	158	195
R-squared	0.642	0.101	0.062	0.150
Panel B : Developing Country				
Main Effect	0.0364***	0.0234***	0.0338***	0.130***
	(0.00469)	(0.00259)	(0.00763)	(0.0216)
Observations	88	180	235	37
R-squared	0.620	0.334	0.093	0.796

Notes: Table reports PEESE regression estimates with moderators selected using rigorous penalization LASSO. Panel A and B restrict the sample to estimates from developed and developing countries respectively. *Main Effect* corresponds to the coefficient on 1/SE in the PEESE regression. Robust standard errors, clustered at the study level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table A.13: PEESE by Sector-Outcomes: Developed and Developing Country

	(1) Digital Output Micro	(2) Digital Output Macro	(3) Energy Output Micro	(4) Energy Output Macro	(5) Energy Labor	(6) Transport Output Micro	(7) Transport Output Macro	(8) Transport Labor	(9) Transport Trade	(10) Transport Population
Panel A : Developed Country										
Main Effect	0.0491***	0.0806***		0.0533***			0.0130***	0.0168***		0.2226***
	(0.001)	(0.015)		(0.005)			(0.002)	(0.001)		(0.019)
Observations	26	98		26			116	16		9
R-squared	0.479	0.825		0.367			0.650	0.582		0.958
Panel B : Developing Country										
Main Effect	0.1732***	0.0343***	0.0490***	0.0328***	0.0467***	0.0755***	0.0603***	0.1859***	0.2629**	0.0062
	(0.001)	(0.004)	(0.002)	(0.002)	(0.004)	(0.014)	(0.002)	(0.003)	(0.112)	(0.010)
Observations	24	48	88	34	53	32	92	45	17	24
R-squared	0.371	0.627	0.683	0.701	0.355	0.739	0.050	0.880	0.859	0.869

Notes: Table reports PEESE regression estimates with moderators selected using rigorous penalization LASSO. Panel A and B restrict the sample to estimates from developed and developing countries respectively. *Main Effect* corresponds to the coefficient on 1/SE in the PEESE regression. Robust standard errors, clustered at the study level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1. RLASSO command resulted in internal error for Transport × Trade developed country sub-sample with 8 observations.