Dear Dr. Koster:

Thank you very much for your decision regarding our paper "Infrastructure and Trade: A Meta-Analysis" and for conveying the constructive feedback provided by the referees. We hereby submit the revised version of the paper. Below we describe the changes we have made in response to the comments from you and the referees (which are for convenience reproduced in this letter in italics). We also made a few other (minor) improvements and updated some references.

Your comments:

1) *The study needs a better rationale. This should include the question why a meta-study is the best method to achieve the goal.*

We now elaborate on the benefits of the meta-analysis in Sections 1 and 4.

2) *The meta-study includes studies that regard different types of infrastructure and I agree with Reviewer A that this complicates the direct comparison between the studies. This could be better addressed in the results section and preferably in the analysis.*

We already distinguish between four different types of infrastructure. The available studies do not permit an alternative distinction without specific types becoming unique to one particular study; thereby diminishing the value of meta-analysis. We modified the text to indicate how we provide quantitative conclusions from a more general/macro perspective rather than from a specific/micro perspective. We emphasize that the impact of composite measures appears to be larger, which is plausible given the interdependence between different types of infrastructure (as discussed in the paper). We introduce a new sub-section (7h) in which we show how closely aligned some individual studies are to the generalizations we derive from the meta-regression model.

It may be possible to include additional variables addressing the differences between the studies included.

Given the available information, we introduced and discussed a large range of study characteristics that varied across the studies and conducted a wide range of robustness checks (many of which could not be included in the paper due to space constraints). A common issue in meta-analysis is that alternative study characteristics may be envisaged but, again, these often only have relevance with respect to one or a few studies. However, to permit the reader to check additional specifications, we now offer downloadable online resources that include: (1) the coded data; (2) the annotated Stata do files; (3) the results previously reported in the Appendix.

Reviewer A:

The purpose of this exercise is not exactly clear to me. Meta-analysis is typically applied when there is dissent whether or not there is an effect of x on y. For infrastructure, however, there seems to be a broad consensus that infrastructure investment has a positive and significant effect on trade. Given this general agreement in empirical findings, meta-analysis may then be used to identify the average magnitude of the effect (by isolating factors that help explaining differences in estimates across studies).

Precisely. We agree with the referee that our paper is not about statistical significance, but about economic significance. We make this point clearer in the text.

However, since the empirical studies that are analyzed in this study employ a wide variety of measures of infrastructure, a direct comparison of the individual estimates is not particularly useful. As it is stated in the paper (p. 17): Because effect sizes come from studies with different geographical coverage, methodology, and model specifications, it is questionable that there would be an underlying universal effect size.(While I would not necessarily worry about the problems that are mentioned in the first half of this sentence, the different definitions of infrastructure in the underlying studies are an issue.)

Many meta-analyses, including some of the most cited ones, are only concerned with statistical significance because the primary regressions are too different to compare coefficient values. However, as is discussed in many critiques of conventional econometric modelling; what is the points of ensuring a policymaker that an effect is statistically significant at the 5 percent level (which is with todays very large datasets invariably the case) unless some information on the magnitude (economic significance) is provided? Our meta-analysis has the major advantage that we have been able to retrieve directly comparable dimensionless elasticities for each study and therefore we can conclude on economic significance: we provide bounds (admittedly fairly broad) for the kinds of effects that may be expected from new investments in infrastructure. We also now comment on the importance of the findings for developing countries.

The interpretation of the results seems dubious. In contrast to the author(s)s claim above, it is later argued (p. 28) that the "key result of our research is that the own infrastructure elasticity of the exports of a country is about 0.6." What does that mean? Does this finding imply that an increase in a countrys road length by one percent increases its exports by 0.6 percent? If yes, what are the effects of a one percent increase in road density, or the number of phone lines, or the number of flight connections?

As we now point out in the paper, we generate a stylized fact that is useful for macro-level policy simulations. It is an "average result" (although it is not "universal"). There are not enough data, in a meta-analytic sense, to be able to distinguish between elasticities for road density, the number of phone lines, etc. However, we do test for differences between broad types of infrastructure.

I also do not understand the thought experiment on p. 28 that builds on the difference in the estimated elasticities for exports and imports. In the paper, it is argued that "an expansion of trade infrastructure may have an attractive return through its impact on the external trade balance." However, suppose that all countries in the world expand their trade infrastructure in similar proportions. Does that imply that there is an improvement in the trade balances worldwide?

The referee is correct that if authors were to estimate and simulate a global general equilibrium model in which infrastructure in all countries was raised by 1 percent, the average effect on exports should be the same as the average effect on imports, given that every export is also an import. Hence, if there are some countries that found the trade balance to improve, it must logically have deteriorated in others. Global general equilibrium gravity models that have this property are actually very rare, but see e.g. Bikker (1987). The studies in our meta-sample are all partial analyses concerned with a limited number of origin and destination countries and a rectangular rather than square trade matrix. In that case, the empirical evidence shows that, ceteris paribus, an increase in infrastructure improves the trade balance in the countries concerned.

A key issue in meta-analysis is replicability. However, the process of how the studies have been selected is poorly described. At a later stage, some studies have been discretionarily dropped.

The dropping of studies was not done discretionarily but to improve the comparability of the remaining studies. So there is always a trade-off: gathering more effect sizes allows better statistical analyses, but introduces greater (and partially uncontrollable) heterogeneity. See e.g. Poot (2014). Some of the studies generated estimates that could not have possibly come from the same data

generating process. We removed extreme outliers that were more than three times away from the bounds of the interquartile range. We have noted this in a footnote. As noted above, we now also make all data and statistical code available for replication and re-analysis.

It is puzzling to note that previous meta-analyses on the effects of variables which are typically used in gravity regressions, such as Disdier and Head (Review of Economics and Statistics, 2008) on distance and Egger and Lassmann (Economics Letters, 2012) on language are missing.

By definition, all gravity models include a distance variable and we have compared gravity models with other specifications. So we have accounted for distance. We have also made this clear in the text. Similarly, there is a dummy for colonial, cultural or linguistic distances. These variables turned out to be statistically insignificant in the meta-regression analysis. Finally, we have included references to the articles mentioned by the referee.

Additional study characteristics could be included in the analysis, such as the time period and the number of countries (or sample size) that are analyzed.

The sample size, in the form of number of observations in the primary study, was used as weights in our WLS robustness estimations. Therefore, implicitly sample size has been taken into account in our analysis. Effectively we also account for differences in sample sizes by means of our publication bias tests. The time periods regarding the data used in the primary studies, are almost always largely overlapping even though the initial and final years used may differ. Assigning dummy variables to different time periods therefore cannot be straightforwardly done.

Reviewer C:

Overall I think that this is a very good paper although it is a bit too long. For example, section (3) should begin with equation (4) thus skipping the derivation of the gravity model that any reader interested in the paper would be familiar with.

Not much space would have been gained from dropping these equations. They make the theory section in any case self-contained. Instead, we have shortened the paper by making the Appendix downloadable online.

Best regards,

Mehmet Guney Celbis
also in behalf of authors Peter Nijkamp and Jacques Poot

UNU-Merit
Maastricht University
Keizer Karelplein 19
6211 TC Maastricht
06 81 74 11 72
celbis@merit.unu.edu

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