Reply to the First Referee Report
on
"Openness to International Trade and Economic Growth: A Cross-Country Empirical Investigation"
by Bülent Ulaşan

Firstly, I would like to express my thanks to the anonymous referee for valuable comments and criticisms. My detailed responses to his/her comments are as follows:

Comments 1, 2, 3 and 6. In light of these comments, I will make required modifications and corrections in revised version of the paper.

Comment 4. Following this comment, I am planning to review both theoretical and empirical literature briefly in a separate section. Indeed, I intended to do this while writing the paper, but I decided to drop that section because the paper was already lengthy. My only concern on this comment is whether reviewing the case studies such as Chandran and Munusamy (2009) is appropriate. There is no doubt that country-level case studies provide valuable information on openness-growth connection. It is, however, obvious that country-level case studies are beyond the scope of the present paper because it is based on cross-country growth analysis.

Comment 5. I follow the standard approach in the literature and randomly select countries according to the criterion of data availability. This approach implicitly assumes that parameters are invariant across countries. Although this is very common assumption in most applied work in economics, it is surely very difficult to accept this assumption in

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growth context since countries are different in many aspects. Therefore, parameter heterogeneity is an important problem and thus researchers should pay sufficient attention to it.

It is true that a substantial part of GDP in major oil-exporting countries depends on the usage of their oil resources rather than value added and hence some studies in the literature exclude these countries from the sample. However, in a different study by me, (Ulaşan, 2011) based on the same data set, I take into account this concern and I run the MRW specification dropping five oil-producing countries.[1] The regression results show that exclusion of oil-producing countries does not alter basic findings. Therefore, I prefer to keep them in the regression analysis. Moreover, in the same study, I also divide the full sample into low-income and high-income countries according to initial income level and carry out a parameter stability test and conclude that parameter are stable across high income and low income countries. Furthermore, inclusion of geographical dummies does not change the main conclusions.[2]

Of course, these findings do not mean that parameter are constant across countries. However, these findings clearly indicate that there is a systematic pattern and tendency in the data. As pointed out by Durlauf et al. (2005) the main goal of the empirical cross-country growth work is to investigate whether a particular hypothesis, (such as "does openness boost economic growth in the long run") has been supported by data, not to provide precisely defined causal relationship between a particular variable and growth for each country in the sample. This implies that coefficient estimates of cross-country growth regressions should be considered as average patterns of correlations across countries and hence should not be applied to a particular country. This point is particulary important for policy advices drawn from cross-country growth work.

Finally, omitting oil-exporters from the sample may not be good solution to account for parameter heterogeneity. The reason is that, by the same token, we should also drop the large countries (in terms of population or area) or small countries or land locked countries, or sub-Saharan

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[1]These countries are Algeria, Indonesia, Nigeria, Iran and Venezuela The other oil-producers, such as Saudi Arabia, United Arab Emirates, Iraq are already missing due to the data availability.

[2]While submitting the current paper to *Economics*, I also provided the data set as a STATA file (Version 11). An interested reader can easily download the data set and then check these results.

African countries and so on.

My attitude to sample selection problem is quite pragmatic because there is no easy and reliable solution, as argued by Levine and Renelt (1991). These authors rightly point out that the researches should explain their criteria of sample selection in a reasonable way and also provide information on changes in their findings when they use different sample selection criteria. I follow this advice in this paper.

Comment 7. As pointed out by the referee, various measures of openness are evaluated in the paper. Addressing proxy uncertainty for openness, my aim is to provide a wider picture for assessment of existing openness variables as well as carrying out the empirical analysis over the longer sample period of 1960-2000. The main result of the paper is that there is no direct and robust relationship between openness and economic growth in the long run. This result is robust to alternative measures of openness and none of them is robustly correlated with long-run economic growth. (Notice that the cross-country growth specification in the paper includes proxy variables for both physical and human capital accumulation. This means that the coefficient estimate of openness variable reveals the impact of this variable on growth through productivity channel, i.e., direct effect of openness on growth.) Therefore, on the contrary to the referee report, empirical findings of the paper are clear and conclusive.

To be honest, I have no adequate knowledge on Principal Component Analysis (PCA) as a methodology suggested by the referee to deal with the proxy uncertainty. As far as I know, this methodology constructs a composite index consisting of weighted averages of a group of collinear variables. Thus, PCA is particularly useful for dealing with the problems arising from multicollinearity. However, given the scope and aim of the present paper, I have some doubts about its suitability. There are two reasons for this: first, the composite index should be economically meaningful; and second, the variables that are combined via PCA should be measured in common units. However, justifying these two conditions for each category of openness variables is very difficult. For instance, consider the first category, measures of trade volumes: Seven different measures for this variable are used in the paper. These are imports ratio of World Bank (WB), exports ratio of WB, trade ratio of WB, current openness of Penn World Tables (PWT), real openness of PWT, trade ratio with OECD and trade ratio with NONOECD. As noted in the paper (Table 1 on p.8), pairwise correlations are very high

and thus it is possible to say that these variables are collinear. All variables are expressed in common units, namely as a ratio of trade volume to GDP (in percentage terms). However, it is obvious that there are some important conceptual differences among these trade shares, for instance current openness vs. real openness or trade ratio with OECD vs. trade ratio with NONOECD, exports ratio vs. imports ratio. Therefore, it is very difficult to justify a composite index including all these trade ratios and hence a cross-country regression analysis based on such a composite index will not be economically meaningful. Similar concern is valid for other categories. For example, the second category includes direct trade policy measures, namely tariffs, non-tariff barriers (NTBs) and black market premium (BMP). At first sight it may appear that combination of these policy barriers via PCA is very reasonable. As argued in the paper, the right measure of openness entails some aggregation of the different policy barriers with reasonable weights. (Thus, the present paper attempts to construct such a composite trade policy index and this could be considered as a novelty of the paper.) However, the key words here are *reasonable weights*. Whether the weights come from PCA, (generally from the first principal components of variables) are reasonable remains questionable because weights are determined according to purely statistical basis. Moreover, trade policy barriers are measured in different units and this makes the weights obtained from PCA more questionable.

That is why it seems to me that PCA is not an appropriate tool for tackling proxy uncertainty over openness. In my view, simply regressing growth on openness plus the control variables with the support of some robustness tests is a more appealing way than PCA to decide whether to include the openness variable as a long-rung growth determinant. Regarding this point, I want to emphasise two things: First, the inclusion of openness variables as a fixed regressor does not mean that we are certain that openness has an impact on long-run economic growth and hence it should be considered as a true growth determinant. This only means that openness variable should be retained in all growth models under the consideration of the paper if we want to reach more reliable inferences on openness-growth connection (see Durlauf et al. (2005), for a discussion). Second, the possible colinearities among openness variables do not lead to multicollinearity problem because they are entered into the growth regression one by one.

Comment 8. I am not sure that re-testing findings of previous studies over the 1970-1990 period is really necessary. First of all, increasing the

number of regressions and tables, this may deteriorate the structure of the paper in terms of fluency. As noted by the referee, the paper is already lengthy and includes a large number of regressions and tables. More to the point, since most of the previous studies are focused on 1970-1990 period and their findings are available everywhere, it seems to me that carrying out the regression analysis once again over the same period is not very meaningful. In other words, given the aim of the paper, we do not need to replicate the previous studies for the 1970-1990 period in order to compare our findings with those obtained from the previous studies.

On the contrary to the referee report, the present paper compares its results with the findings of previous ones. As noted above (and also in the paper), differently from the previous work, this study finds no evidence that openness is directly and robustly correlated with economic growth in the long run. And this finding is robust to alternative openness measures, except own-weighted tariff rates on capital goods and intermediate inputs. Finally, the current paper compares its findings with those of Yanikkaya (2003) in some aspects. For instance, this sentence belongs to the paper (page 9): "[T]hese results confirm the findings of Yanikkaya (2003) and imply that technology spillover effects of international trade on economic growth are not very important compared to the effects of comparative advantage and scale economies." Nevertheless, I will try to more elaborate and systematic comparison with Yanikkaya (2003) and also others in revised version of the paper.

Comments 9 and 10. Indeed, the paper has some important policy implications. According to data evidence in the paper, economic institutions, ethonlingusitic fragmentation, government consumption and geography are key variables of long-run growth while trade openness is not robustly and directly correlated with the long-run economic growth. As argued in the conclusion section of the paper, these findings suggest that without building better institutions, maintaining conflict management along ethnolinguistic dimension, and following sound and stable fiscal policies, openness to international trade will not guarantee economic growth and thus economic reforms in these areas should take priority over the policies enhancing trade openness. Nevertheless, I agree with the referee that in its current form, the reader may not grasp the findings and implications of the paper from the conclusion section. In other words, the conclusion section should be revised to reflect findings and policy implications of the paper more boldly, as pointed out by the referee.

Comment 11. I will examine the text carefully to correct writing errors and to improve English usage in the paper.

To summarize, I agree with the referee's comments except a few ones. In particular, I concur with the referee's assessment on the current structure of the paper. The referee rightly argues that the paper should be shortened and its structure could be improved in terms of fluency and precision. Therefore, I will revise the paper in accordance with the referee's comments. There is no doubt that these comments will substantially improve the paper. Thus, I would like to express my gratitude to the referee once again.

References

V. G. R. Chandran and Munusamy. Trade openness and manufacturing growth in Malaysia. *Journal of Policy Modeling*, 31(5):637–647, September-October 2009.

S. N. Durlauf, P. A. Johnson, and J. R. Temple. Growth econometrics. In P. Aghion and S. N. Durlauf, editors, *Handbook of Economic Growth*, volume 1A, chapter 8, pages 555–677. North-Holland, Amsterdam, 2005.

R. Levine and D. Renelt. Cross-country studies of growth and policy: methodological, conceptual, and statistical problems. Policy Research Working Paper Series 608, The World Bank, Washington, DC, March 1991.

B. Ulaṣan. Augmented neoclassical growth model: A replication over the 1960-2000 period. Working Paper 11/01, Research and Monetary Policy Department, Central Bank of the Republic of Turkey, Ankara, Turkey, January 2011. Downloadable at http://www.tcmb.gov.tr/research/discus/2011/WP1101.pdf.

H. Yanikkaya. Trade openness and economic growth: A cross-country empirical investigation. *Journal of Development Economics*, 72(1):57–89, October 2003.