

# Reply to Anonymous Reader's Comments on “Cross-Country Growth Empirics and Model Uncertainty: An Overview” by Bülent Ulaşan

Firstly, I would like to thank the anonymous reader (AR) for helpful comments and criticism.

Before replying to the AR's comments, I want to emphasise a few points about the objective and scope of the paper. As stated in the introduction, following two path-breaking studies by Barro (1991) and Mankiw, Romer and Weil (MRW, 1992), the empirical cross-country growth literature blossomed. The prominent aspect of these papers is that they derive a basic cross-country growth specification for the subsequent empirical research. Almost all studies in this era used either the Barro approach or the MRW specification and concluded many different results on causes of economic growth and cross-country growth differentials. However, the MRW specification differs from the Barro approach in some aspects. The most important difference is that while the Barro approach is based partly on *ad hoc* specification, MRW derive the basic linear cross-country growth regression in a purely theoretical basis. In other words, theoretical foundation of basic linear growth specification was firstly introduced in MRW. Since this point is sometimes neglected in the literature, my first aim in this survey is to emphasize the MRW approach as the analytical basis of the recent cross-country studies and to discuss its main shortcomings. My objective is neither to review theories of economic growth nor to summarise the findings of empirical cross-country growth literature. Due to the obvious reasons, discussing this voluminous literature is not appropriate and possible in this overview. It is fair to say that this overview is selective and naturally many important issues are missing. Therefore, as referring the readers to related studies, I have only touched some important

topics, if they are very close to the content of the paper, such as convergence. Similarly, I revisited the model uncertainty problem, my second objective, even though other econometric problems, for instance simultaneity, outliers, parameter heterogeneity, measurement errors, are equally important.

Frankly, I quite agree with the AR's criticism and comments except a few ones. In light of the AR's report, I have made two major changes in the paper: a detailed discussion on panel data approach to growth and an overview on Bayesian model averaging approach in the empirical growth literature. I have organized my reply to the AR according to structure of the AR's report, for convenience. My detailed responses are as follows:

### **Introduction**

**Comment 1.** I have cited some of pioneering papers on the theory of endogenous growth as the AR comments. There is no doubt that Oded Galor is one of the leading economist in the field of economic growth and the unified growth theory has been largely developed by his contribution, as indicated by the AR. However, given the the emphasize and scope of the paper, I think that mentioning of this theory is not suitable in the introduction.

**Comment 2.** I have rephrased the paragraph indicated by the AR following his/her suggestions.

**Comment 3.** The term of proximate determinants of growth refers to production factors and technological progress (TP) and commonly used in the literature (Temple (1999), Rodrik (2003), Durlauf et al. (2008), Acemoglu (2009), are few examples amongst others). Defining this term is useful not only conceptually but also for the aim of the paper: First, this concept is useful to emphasise the importance of fundamental (or deeper) causes of grow differences across countries. Saying that country A grows faster than Country B because country A invests more than Country B and/or Country A experiences a higher rate of TP compared to country B is not very illuminating on the causes of growth differences between these two countries. But, the explanations such that the quality of institutions in Country A is better than that in County B or Country A has some geographical advantages while the Country B is landlocked are surely more informative and satisfactory to clarify growth differentials. Second, distinguishing the fundamental determinants from proximate causes is important because while the almost all studies in the literature include the same proximate variables, the fundamental determinants change from study to study. In

other words, there is no complete consensus on the fundamental growth variables and selection of these variables is partly arbitrary. This, of course leads to an important econometric problem; model uncertainty. Reviewing this problem is obviously the second aim of this paper.

**Comments 4 and 5.** Following the comments of the AR, I have made suggested modifications and corrections in the revised version of the paper.

**Comment 6.** I have corrected the typos indicated by the AR.

### **Empirical Framework**

**Comment 1.** In light of the AR's comment, I have changed the title of this section as "Theoretical Foundation of Growth Regressions".

**Comments 2 and 5.** As noted in the AR's report, convergence is an important theme in the cross-country literature both theoretically and empirically. However, surveying the literature on convergence is beyond the scope of this paper. As mentioned above, I just simply touched the concept of convergence and referred to readers the relevant studies for details. On the contrary to the report, different notions of convergence are briefly, but clearly defined in the text. It is true that both absolute and conditional convergence are emphasised more boldly than the sigma and club convergence in the survey. The reasons are; first, the concepts of both absolute and conditional convergence are the classic well-known predictions of neo-classical growth model; second and more importantly these two concepts are directly related to content of the second part of the paper and mathematical derivations therein. I would also like to thank the AR for suggesting Galor's (1996) paper on the topic. I think it is better to refer the reader to the special *Economic Journal* controversy on the convergence, edited by Steven Durlauf (1996) since that controversy also includes Galor's paper as well as the other good references on the topic.

**Comments 3 and 7.** I quite agree with the AR about these comments. The AR rightly argue that panel data methods can solve some of drawbacks of empirical approach developed by MRW and hence the paper should have focused much more on this issue. That's why I have discussed the use of panel growth models in greater detail in the revised paper. For this purpose, i) I formally rewrite the augmented neoclassical growth model in a dynamic panel data model with fixed effects; ii) I briefly review three panel data methods, namely the fixed-effects

estimator, the first-differenced GMM estimator and the system GMM estimator, in the cross-country growth context; iii) Finally, some important problems associated to panel growth models are also examined since panel data methods solve some problems (say omitted variable bias) on the one hand, raise another problem (like attenuation bias induced by measurement error) on the other hand. Moreover, it is very likely that existing panel studies with 5- or 10-year averages contain business cycle factors and hence miss the long-run growth information. Therefore, in my opinion, a panel growth model is not alternative to a single cross-country growth regression and these two are in fact complementary. In this context, this part of the revised paper concludes that carrying out empirical investigation by using these two methods and comparing the findings are a better and more consistent strategy for cross-country growth analysis. Please see pages 11-19 of the revised version of the paper for the aforementioned revision.

**Comment 4.** The AR stresses the time series literature, obviously another important line of empirical growth literature. However, time-series studies on growth is hardly specific to my selective review on cross-sectional growth studies.

**Comment 6.** The AR raises a concern about the aim and scope of the second section of the paper. Perhaps this concern raised because the aim of this section was not introduced very clearly in the first draft. As mentioned above, the objective of this section is presenting the MRW approach as a theoretical basis of the most cross-country growth studies and discussing its main drawbacks. Therefore, I have tried to reflect the aim of this section more boldly in the revised version. For this, I have changed the title and the introductory sentences of this section. Please see page 5 in the revised version of the paper for these changes.

**Comments 8, 9 and 10.** Following the AR's comments, I have made suggested revisions and/or corrections. In the revised version of the paper, please see the second paragraph at page 20 for comment 8, the top at page 22 for comment 9, and the second paragraph at page 22 for comment 10.

**Comment 11.** In my opinion, the suggested sentence by the AR is not appropriate since the main point I want to emphasise is whether the additional growth variables,  $Z_i$  allow for the differences in the initial level of technology or in the rate of technological progress is not clearly defined in the extended version of MRW approach. Nevertheless, I

have revised the relevant part of the section in the revised version by reflecting this point more boldly. Please see, pages 23-24 in the revised paper.

**Comment 12** I have corrected the omissions/typos highlighted by the AR.

### **Model Uncertainty and Cross-Country Growth Regressions**

The AR is rightly concerned about the inconsistency on structure of this section. As pointed out by the AR, although Bayesian model averaging (BMA) is the most promising method for tackling model uncertainty, very small part of this section is dedicated to this technique. I completely agree with the AR in this point and extend this section with the inclusion of an overview of BMA and of its applications to cross-country growth data. In light of the AR's suggestions;

**Comment 1.** I have shortened the part related to extreme bounds analysis (EBA).

**Comments 2 and 3.** I have provided a detailed overview of BMA in growth context. In doing so, i) I introduce BMA in a formal way and clearly define the related concepts, such as prior model probability, Bayes factor, posterior inclusion probability and so on; ii) I discuss the implementation issues. Two important difficulties, namely assigning priors and computation of posteriors are emphasised; iii) I summarise the findings of Sala-i-Martin et al. (2004) and Fernández et al. (2001), the most prominent BMA examples in the literature; iv) I also emphasise two important, but generally neglected points about BMA applications in empirical growth literature: First, Bayesian hypothesis testing as a formal criterion to determine robust growth determinants, and second, the sensitivity of BMA results to chose of data source for income per capita; v) I briefly mention about the recent BMA applications which adapt this approach to panel data models for simultaneously dealing with model uncertainty and endogeneity problems. Please see, pages 31-36 of the revised version of the paper.

**Comment 3.** Concerning the last point about Comment 3, I do not think that the AR's suggestion is appropriate since the selection process described at pages 23-24 of the first draft refers to multiple-search algorithm employed in the studies by Hoover and Perez (2004) and Hendry and Krolzig (2004). In other words, both studies implement the GETS approach by employing the automated search algorithm which is developed by Hoover and Perez (1999) and Krolzig and Hendry (2001)

against the criticism of that there may be many simplification paths from general model and a particular path might not lead to the true model. Therefore, the selection progress explained at pages 23-24 refer to the general-to-specific (GETS) approach based on multiple-search program, not to the standard GETS methodology.

**Comment 4.** I have made the suggested revision and explanation against the misinterpretation of the sentence (at the bottom of page 24 of the first draft) in the revised version of the paper (see the bottom of page 32).

### **Model Uncertainty and Policy Evaluation in Cross-Country Growth Regressions**

The issue of policy evaluation in the presence of model uncertainty is an important and actual topic in the cross-country growth literature, and hence keeping work on it is a worthwhile task, as the AR points out. However, given the length and other emphasises of the current paper, I think that the topic has been well explained and discussed sufficiently. On the contrary to the AR's report, a more elaborate discussion will not improve the reading of the paper in terms of fluency and coherence. More clearly, it will be more appropriate to extent this section in a different paper focusing only on the subject of policy analysis under the model uncertainty. Especially, exploring policy implications of cross-country growth analysis by employing some alternative decision formulations arising under different assumptions for the policy maker's utility function (such as risk neutrality, risk aversion, minimax regret, ambiguity aversion and so on) with the BMA application to cross-country growth data is obviously a fruitful avenue for future research.

**Comment 1.** I have corrected typos indicated by the AR in the revised version of the paper.

Once again I thank the anonymous reader for reviewing my paper and making such valuable comments. It is obvious that the comments of the anonymous reader have substantially improved the paper.

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