

Reply to the Referee Report on the Discussion paper n 2008-27: *The New Growth Theories and their Empirics after Twenty Years*, by Rosa Capolupo

I thank the referee for his valuable comments that contribute to improve the paper. Below are my answers to major and minor issues raised. Before doing it, I would express my thanks also to commentators of the paper. Their suggestions have been really appreciated. In this revised version of the paper I have taken into account many of Professor Seater's suggestions. Therefore, the Semi-endogenous growth theories as well as the Full- endogenous growth theories and their empirics- have been now added. The paper has an overview of contents.

Major Issues

1. *The definition of NGT is somewhat flimsy.*

Your impression was right. I refer to the Endogenous Growth Theories, but I shall understand that it would be misunderstanding since I discuss also theories based on fundamental causes of economic growth. However, I think that these deep factors should be included in the endogenous growth paradigm that has as a main objective to eliminate the standard dichotomy between growth and development theories. This is now stated clearly in the introduction (p.5)

2. *The author states on page 2 that "we will search for salient growth determinants..." What is the meaning of salient in this context ?*

I explain on page 4 (Introduction) what, in my view, is a salient growth determinant. I clarify that it is one that has been modelled in growth theories (the major determinants are known from neo-Keynesian and neo-classical theories of growth) as well as those that have been found significant in a sufficient number of empirical studies.

3. *In section 2.1, the paper goes through a large number of methods that are used by growth researchers. The author states on page 10 that "...when the number of regressors exceeds the number of countries in the dataset the analysis becomes flawed". The different methods described in section 2 are analyzed by Jensen and Würtz (2006) in this situation. They show that Bayesian averaging methods and general-to-specific methods give correct inference, when the true model is sufficiently small, but as this assumption need not be satisfied, the methods seem to have some fundamental flaws. A brief discussion of these results would be in place. Regarding the idea of robustness, the author would also benefit from reading Aldrich (2006).*

I have read with interest the papers suggested by the referee and the main results are now included in the review (pp. 14-15)

4. *Section 2.2 describes dynamic panel models. The main motivation for using GMM methods is not mentioned. The fixed effects estimator is inconsistent for the estimation of the coefficient to lagged income, when the number of time periods is fixed and the cross-section goes to infinity. This is the famous Nickell bias (see Nickel 1981)*

As suggested (see minor issues n. 2) I discuss the Nickell bias and eliminate some technicalities by referring to original papers.

5. *In section 3.1, I miss a reference to Cook's 2002 article on convergence which uses WWI as a natural experiment to solve the endogeneity problem.*

The Cook paper is now discussed on page 21.

6. *In section 3.3, I miss a discussion of Glaeser et al.'s (2004) finding that human capital has a positive effect on growth. They use log years of schooling which is clearly a stock measure. Barro (2001) also used a stock measure and found a positive association.*

I discuss both papers now. The Barro paper in section 3.3 dedicated to regression estimations from education and the Glaeser *et al.* paper is just cited in this section but discussed in section 4.2 (institutions view) as stated by the referee in the subsequent point (point 7).

7. *Regarding the discussion of the institutions hypothesis, it should be mentioned that the instrument used "log settler mortality" is argued to be correlated with other potential growth determinants. Glaeser et al. (2004) mention that is correlated both with human capital and measures of malaria risk.*

See answer point 7.

8. *I am also missing a discussion of the geography view (see e.g. the chapter by Acemolgu et al. (2005) in the Handbook of Economic growth).*

You completely right. However ,now I briefly discuss these factors, without going into depth but giving some important references, as suggested, in section 4.2 page 82.

9. *David Albouy has published an NBER working paper which argues that the settler mortality instrument is miscoded and shows that this has an impact on the quality of the instrument. This finding merits mentioning – at least in a footnote.*

As suggested the Albouy arguments have been considered on page. 76.

Minor issues

1. *The paper would benefit from a language check.*

2. *The discussion of dynamic panel models could be substantially shortened. Simply mention the Nickell bias and refer to other articles for details on the GMM procedures .*

List of articles not already cited by the author:

Albouy, D. (2008). *The Colonial Origins of Comparative Development: An Investigation of the Settler Mortality Data.*, NBER working paper 14130.

Aldrich, J. (2006). *When are inferences to fragile to be believed ?* *Journal of Economic Methodology* 13(2), 161 - 177

Barro, R. (2001). *Human capital and growth.* *American Economic Review* 91(2), *Papers and Proceedings*, 12-17.

Cook, D. (2002). *World War II and convergence.* *Review of Economics and Statistics* 84(1), pp. 131-138

Jensen, P. S. and Wurtz, A. (2006). *On determining the importance of a regressor with small and undersized samples.* *Economics Working paper series 2006-8, Department of Economics, University of Aarhus.*

Nickell, S. (1981). *Biases in Dynamic Models with Fixed Effects.* *Econometrica* 49(6), 1417-1426.

All the minor issues has been accepted and accomplished, as requested.