Comments on Rosa Capolupo "The New Growth Theories and Their Empirics after Twenty Years", Discussion Paper 2008-27

In great parts, this survey paper is a useful review of the empirical research undertaken on the area of new growth theories. It is well-organised and generally well-written, besides some typos[1]. Nonetheless, I have some reason for criticism.

In section *2. Methodological critiques on growth empirics*, I enjoyed the discussion on model uncertainty and the issues of identification. But after this extensive discussion, the paper is, at least in this section, almost silent about regression model (mis-)specification and causality issues.
A minor thing is that, rather than 'other non-parametric approaches (p. 12), only one work is briefly discussed.
Then a rather long introduction, first, to cross section regressions, and then to (dynamic) panel regressions follows. Some issues of how to estimate these models are address, but the selection seems random to me. On the one hand, the author discusses weakly exogenous explanatory variables, but one the other hand spares out serial correlation in the error term. In any case this methodological review could be omitted from the survey on empirics of new growth theories, despite these techniques are applied in many studies reviewed afterwards, since it only comprises textbook material and well-established context, e.g. the Arellano-Bond approach.

The actual survey continues with section *3. Models and their empirical validation.* When presenting regression result (which only is once in the entire paper, p. 22), I would clearly prefer not only to learn the estimated coefficients, but also their standard errors or t-statistics, and their measurement units. Moreover, I would appreciate to read about the sample characteristics, i.e. what countries and what time frame, and estimator that was applied, even if it is OLS. Without this information, the coefficients are useless, since the interest reader needs to refer to the original paper anyways. Hence dropping this part would be fine, too.

Regarding the subsection on research-based models, a comparison between transitional dynamics in different models is missing. Since in standard R&D models the economy is always on the steady state, the empirics should be substantially different from classical growth models.
Without knowing the empirical literature, I was wondering that no paper was mentioned that tried to test the implications of variety models or the quality ladder (creative destruc-

[1] Obvious typos are in the numbering of section *4*, in the very last paragraph, and in the bibliography, which is especially unfortunate for a survey.

tion) models directly. Instead the survey lists a couple of studies using different proxies for R&D spending in growth regressions. However, since both variety and quality ladder models predict that R&D spending is triggering economic growth, these studies would fail to distinguish between the two conceptual different new growth theories, i.e. *product versus process innovation*[2]. A discussion of a test discriminating between these models would be useful.
In this context, the survey also misses out on models of international technological diffusion.

In section *4. Evidence on public policy and institutions* the author reviews the literature in this area thoroughly. I only would like to add that contrary to the Acemoglu-Johnson-Robinson finding that economic institutions (for which they use settler mortality as an instrument) determine growth, very recent results by Kapstein and Converse suggest a the reversed causality, i.e. that good economic performance is needed to establish good institutions.

Section *5* concludes the survey. The author could strengthen her conclusions by outlining a more detailed agenda for future research.

References

Ethan B. Kapstein and Nathan Converse. The Fate of Young Democracies. mimeo.

Alwyn Young. Growth without Scale Effects. *Journal of Political Economy*, 105:41–63, 1998.

[2]Young (1998) combines both product and process innovation.