

Interactive comment on “No way out? The double-bind in seeking global prosperity along with mitigated climate change” by T. J. Garrett

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The following review was sent to me and I post it in the name of the anonymous reviewer.

Axel Kleidon, Editor

===== begin of review =====

The ms addresses and interesting question and makes very strong statements. Alas, the ms suffers from several severe problems. Below, I enumerate a subset of these problems.

1) The ms makes many statements that are factually wrong. For example, some IAMs

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do represent physical flows.

2) Several statements (e.g., about the “capacity of hurricanes to cause damage”) are not supported by citations or evidence.

3) The exposition is wordy and imprecise: units are missing, it is unclear who are the subjects in statements such as “what we normally term”, what precisely is meant by an “economic signal that can be meaningfully distinguished from noise”, etc.

4) The ms discusses a hypothesis, but does not provide a careful test of this hypothesis.

5) The ms bases very long-term projections on a much shorter calibration period. What are the effects of this out-of-range forecast?

6) The ms claims that the model can “make accurate multi-decadal forecasts for growth of the global economy and atmospheric composition”. How is this evaluated formally using standard methods of forecast evaluation?

7) The ms makes very strong conclusion about the real world (e.g., the last sentence of the paragraph) based on a very simple (and some would argue very poor) model. What would be the effects of considering an alternative model? How could one test which model provides more skillful forecasts?

8) Why take only two estimates of ocean and land carbon uptake?

9) How are the uncertainties calculated and displayed in Fig. 5?

10) Why show only a subset of the SRES scenarios in Fig 6?

Interactive comment on Earth Syst. Dynam. Discuss., 2, 315, 2011.

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