

## ***Interactive comment on “Coupled Climate–Economy–Biosphere (CoCEB) model – Part 2: Deforestation control and investment in carbon capture and storage technologies” by K. B. Z. Ongutu et al.***

**Anonymous Referee #1**

Received and published: 14 July 2015

This is an interesting ms that tackles an important and difficult question. The ms shows promise, but requires a careful and responsive revision to address several issues in order to \*potentially\* rise to the level of papers one expects in ESD. Key issues are enumerated below.

1. Presentation: Overall, the ms would benefit drastically from a careful round of edits. For example:

a. The ms is part 2 of a sequence and expands on the first paper. However, the first paper has also several problems that need to be addressed. This second paper seems

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to be in better shape than the first one. One approach may be to combine the papers and put much of the technical detail in an appendix.

b. The use of mathematical symbols in the sentences makes for a slightly confusing reading (e.g., l. 5 p. 882). Maybe use the full name in words instead?

c. The exposition of the model in form of equations is too terse. Maybe describe this in general words and have the equations in an appendix?

d. The paper suffers from a few slightly odd word choices such as “incertitude” or “resumed”. Please do a careful edits to improve (at least review) the wording.

e. The paper has grammar mistakes.

2. The ms would benefit from a more careful discussion of the prior relevant studies. For example, what about Keith, D. W., M. Ha-Duong, and J. K. Stolaroff (2006), Climate strategy with CO<sub>2</sub> capture from the air, *Climatic Change*, 74(1-3), 17-45, doi:10.1007/s10584-005-9026-x. Other papers also analyze the effects of leakage. This paper assumes this to be zero (p. 872).

3. Overall, the main results as discussed in the current ms seem not very surprising. Given that both papers focus a lot on model development, the question is whether ESD is the appropriate venue.

4. Does the model represent the potential reduction in CCS costs in the future? If not, how would this change the conclusions?

5. Are there not simpler ways to represent CCS due to afforestation than shown in equations 11 and 12? What are the dynamics of this system?

6. What does the capital stock (p. 877) refer to?

7. The analysis (attribution) on page 880 (page 6 onwards) is interesting, but too terse to follow easily and also not backed up by evidence. Can this be explained and analyzed better (e.g., through sensitivity studies)?

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8. The ms discusses how “current sequestration potential primarily reflects depletion due to past land-use”. How does the model at hand handle this?
9. The reference to “IPCC, 2013” is not precise enough. Which chapter? Should the reader look at all “1535” (p. 893) pages?
10. The conclusions need to be rephrased to make clear that these statements are about the model (i.e., a highly stylized and simplified approximation), not the real world.
11. What is the evidence behind the claim of: “Delaying action may mean that high temperatures and low growth are approached on a path that becomes irreversible” (p. 888).
12. Do the IPCC reports really “propose” and/or “recommend”(p. 888)?
13. How is “best” defined (p. 889)?
14. Please add parameter names and units to the tables as needed.
15. The figures are of poor quality (e.g. rasterized).
16. The effects on GDP growth (figure 2) are quite large. Please discuss and also show the integral.

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Interactive comment on Earth Syst. Dynam. Discuss., 6, 865, 2015.

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