Chair of Forestry Economics and Forest Planning
• Tennenbacher Str. 4 • D-79106 Freiburg

Editorial Board of
Biogeosciences

**Faculty of Environment and Natural Resources**

**Dr. Rasoul Yousefpour**
Telefon 0761 / 203-36 88
Telefax 0761 / 203-36 90
E-mail: rasoul.yousefpour@ife.uni-freiburg.de
http://www.ife.uni-freiburg.de

Datum: 16.11.2018

**Dear Editor,**
**Dear Prof. Dr. Anja Rammig,**

We thank you for your effort with our submitted paper in discussion, bg-2017-531 "Simulating sustained yield harvesting adaptive to future climate change".

We appreciate the reviews contributing to improvement of our manuscript. However, we would like to raise attention to the report from review round #3 forwarded to us on 2018/11/06.

Mainly, we perceive the main argument of the reviewer as unfounded. Report-1 of review round #3 states that our concept design *"implies that any forest worldwide is regarded as a potential wood harvest, no biodiversity hotspot, conservation areas or last wilderness areas are excluded from the potential area, thus carbon potential for wood harvest.* ***The only restriction applied refers to accessibility."***

This is wrong. We describe clearly our approach on MF (managed forests) in section 2.5, e.g. lines 168-170: "we conduct a post-processing step overlaying a map that masks out forest areas subject to conservation, infrastructural limits, or not being influenced by human activities so far due to other reasons". We continue by explaining that we used for this purpose the published map of managed forests produced by Kraxner et al., 2017.

This change in our methodology, which we introduced following the second round of revisions, satisfied the former reviewer of the manuscript (report 2) fully as she/he clearly states "I particularly liked the inclusion of the "managed forest" mask, which restricts the growth-based harvest method to forest locations that are considered managed - which gives a more realistic bound on the potential of growth-based harvest methods."

Beyond the main criticism of review round #3 being unfounded we have substantial problems with understanding the comments raised by report-1:

- Sentences are not complete e.g. *"The Life-cycle analysis is limited to the decay rates of the 3 reason why wood is harvested, bioenergy, paper and wood products."*
- Statements are scientifically incorrect or subjective, e.g**.,** *„Thus, by design this study has a very global view on forest*

*conditions, neglecting that natural forests, i.e. woods, are not managed and should not be managed, i.e. harvested. It seems to be triggered by the image of European forestry of what forests are worldwide that I find highly disputable, it ignores biome- or ecoregion- specific conditions*."

- The review asks for information that exists in the manuscript, e.g.,
    - *"An overview on the methodological approach of computing wood harvest in IAMs has to be provided in order to allow the reader to compare and follow on the carbon estimates of wood products quantified by ESM (your study) and IAMs (the approach you suggest to improve). There are several IAMs being used in the scenario-production work, but you do not cite any of them to substantiate your argument that the climate-dependent simulation of wood harvest is considered."* and again *„Lines 308-310: statement not substantiated by methods and clear explanation of the criticized IAM approach, no citation to IAM publication provided."* → We explain and reference three IAM approaches in l. 356-367.
    - *„The introduction section should end with a clear explanation of the modelling concept of this study (the abstraction of representing respective carbon fluxes and pools) and its objectives to provide the reader with an overview of what to expect and to cross-check later on what to conclude from this study."* → l. 95-103 explains the goals of this study, l. 103-112 gives a general summary of the approach to assessing mitigation potentials, l. 66-94 gives a general summary of the growth simulation in our model, ESMs and IAMs in general. Carbon fluxes and pools specifically in our model are detailed in section 2.1
    - *„Lines 405-407: experiments and setting not explained in methods",* while there is the reference in l. 407 to the supplemental text S1, which explains the experimental setup.
- The following comment (and similar comment later) reveals a lack of understanding of some fundamentals of carbon cycle science: *„Line 108-111: This assumption [using an impulse response function] is flawed because it ignores carbon emissions from industry, fossil fuels and land-use change. […] Such an assumption implies that only GPP and respiration fluxes are exchanged between the ocean and terrestrial vegetation, and that carbon release from*

*wood products is the only additional source of carbon. […] This assumption needs to be revised as it is central to the entire study."* An impulse response function approach approximates the uptake of emissions by natural sinks in land/ocean and is a common tool to estimate the fraction of emissions held by the atmosphere at year x after the emission occurred (e.g., Caldeira and Kasting, 1993; Pongratz et al., 2011; O'Halloran et al., 2012; Millar et al., 2017). It makes no statements about the source of emissions (wood harvest, fossil fuel, …) as the CO2 uptake in the sinks is independent of the source of carbon. It is only important that the response curves are derived under comparable climate/CO2 conditions. Even if the reviewer's criticism of our method were valid -- the analysis of the mitigation potential would only be a side aspect of our study that does not affect our main conclusion and is *not* *"central to the entire study".*

This list deals with the major comments by the reviewer, but even more comments exist in the review that we perceive as inappropriate for the standards of a scientific journal. The overall picture to us is that the reviewer is not capable or not willing of assessing the quality of our study. There is no reason to expect he/she would be capable or willing to perform an adequate review in a further round of revisions. We therefore ask you, as the editor, to evaluate our manuscript ignoring the unfounded comments by review round #3. Considering the editor's expertise in the field of climate-vegetation-interactions and the fact that our manuscript has been seen by two reviewers before (with the one reviewer, who re-evaluated our response (report 2), recommending that our revised version should be published), we believe the review process has been covering a large amount of external expertise already in the prior rounds of review. Therefore, we are willing to take into account any final comments the editor finds crucial and with that finalize the paper for publication.

Best regards,

Rasoul Yousefpour on behalf of all co-authors

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