

Interactive comment on “Global climate response to idealized deforestation in CMIP6 models” by Lena Boysen et al.

Lena Boysen et al.

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Dear David Lapola,

thank you for your acknowledgement of our work and your valuable feedback to our manuscript. In the following I will address your points and how we want to improve the manuscript.

Referee comment: This is a relevant study that shows the modeled climatic impacts of a concerted removal of forests across the globe. The authors should be applauded for the effort in first conducting this multi-model exercise and also for the well-organized and systematized analysis. The article presents a proper level of self-critique regarding the lack of surface-atmosphere feedbacks such as the missing effects of CO₂ emitted

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with such a deforestation and its physiological (e.g. CO₂ fertilization) and radiative feedbacks to the land vegetation (this much should be highlighted in the abstract!).

Reply: Thank you for you for this positive comment and acknowledgement.

Referee comment: My major concern is regarding the influence of the ocean on the resulting climatic patterns. In the introduction (L70) the authors state that atmosphere-only simulations of this kind tend to present a stronger (-than-real?) signal-to-noise ratio for deforestation. Using ESMs is indeed a huge advantage over an atmosphere-only approach. However the authors lose an ideal opportunity to demonstrate this by showing the incurred changes in sea surface temperature and circulation patterns. There is historically a number of atmosphere-only modeling studies showing indeed a strong temperature sensitivity to deforestation, especially in the tropics. Not much has been said in these previous studies on the Time of Emergence, a nice tool the authors use in this current manuscript. Is the ToE presented here related to the climatic inertia of the ocean? The exploration of the ocean-related variables could clarify this, as well as other issues presented in the article (e.g. cooling of western Europe in some models and reduction of precipitation in the tropics).

Reply concerning the influence of the ocean:

We will discuss the role of the ocean a bit more. However, we tend to not include new variables or foci to the study and would leave such detailed analysis to follow-up studies (e.g. as currently done for the changes in atmospheric circulation). A thorough analysis of ocean dynamics would also require longer simulations that are currently not available to assess changes to the ocean stratification, mixing etc. As shown in Figure 2 (dT_{as}) oceans show hardly any response except for models with strong sea-ice-albedo feedback (CanESM, UKESM) or those simulating the ‘warming blob’ (IPSL, CESM2, UKESM). Attached, you can find the plot for surface temperature which resembles the sea surface temperature over ice-free oceans. Over oceans, there are only little differences to detect compared to Figure 2. Consequently, changes in latent or sensible

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heat fluxes (also attached) are not very pronounced. We will however include the below ocean-related explanations and discussions in the text. Thanks for pointing them out!

Reply concerning the warming blob caused by accelerated THC? (Line 323):

Yes, the acceleration of the THC could cause the observed “warming blob”. This result would be in line with the opposed finding of Rahmstorf et al. (2015) who found a cooling blob due to freshwater input caused by global warming. We will add this to the manuscript.

Reply regarding why atmosphere-only simulations overestimate the signal-to-noise ratio (Line 70):

Davin and de Noblet-Decoudré (2010) separated the effects of global deforestation on global near-surface temperature with and without coupled ocean. The main component involved is the decreased land surface albedo. Less radiation is absorbed by the surface, thus the whole troposphere is cooled and contains less moisture. This effect is then transferred to the ocean surface which receives less longwave radiation from the atmosphere. Prescribing the ocean SST would lead to an overestimated tropical warming and underestimated boreal cooling. We will explain this more clearly in the manuscript.

Reply concerning the inertia of ToE signals (Line 501):

In order to show a detectable signal, dT_{as} has to be twice as large as the climate variability. The ocean certainly plays a role in that it influences the variability and stabilizes climate through its inertia in heat uptake and feedback to the observed climate on land. Thus, the deforestation extent has to reach a certain magnitude. We will clarify this.

Reply concerning mentioning probable results from tropical or boreal deforestation alone:

The deforest-globe experiment was set up to cover both regions, the boreal and the tropical. Speculating on the effects of deforestation of only one region is very difficult,

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especially, as models simulate varying responses. We would therefore like to avoid to do this.

Thank you for your further comments to the text which we will take into account when revising the manuscript and you can find them in the supplement.

With kind regards, Lena Boysen & co-authors

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2020-229/bg-2020-229-AC3-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-229>, 2020.