

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.

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 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!).

Thank you for you for this positive comment and acknowledgement.

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).

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 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!

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.

Clarifying 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.

Inertia of ToE signals (Line 501):

In order to show a detectable signal, dTas 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.

As a minor suggestion, the authors could at least discuss the probable results from a separate experiment of deforestation in the boreal zone alone and another one of deforestation in the tropics alone.

Reply: 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, 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 (see detailed list below).

With kind regards,
Lena Boysen & co-authors

Line 51: future losses in what time horizon?

Reply: We will add 'until the end of the century'.

Line 52: not only to free land for food and bioenergy but to speculate on land ownership itself (i.e. to justify land grabbing [see Rajão et al. 2020 Science]).

Reply: Yes, that is true. However, to our knowledge this is not included in Hurtt et al. (2020) and therefore we would like to leave it as it is.

Line 70: "An experimental setup with atmosphere-only models in which sea surface temperatures are prescribed allows to increase the signal-to-noise ratio of models' response to deforestation. This setup assumes that the effect of large-scale circulation changes is small and can be ignored." Please explain this better. You mean the sensitivity of models to deforestation increases (artificially?) in atmosphere-only model setups? "(. . .) the effect of large-scale (atmospheric?) circulation changes is small (compared to what?) and can be ignored."

Reply: We will add: 'The cooling of the of the land surface via enhanced albedo cools and dries the whole troposphere which in turn transfers this signal via reduced longwave radiation further to the ocean. With prescribed SSTs the mediating effect of the ocean on the land temperatures is missing resulting in overestimated tropical warming and underestimated boreal cooling (Davin and de Noblet-Ducoudré, 2010).'

Line 125: if tree and forest are different things why using them interchangeably then?

Reply: We will change all 'tree fractions' or 'tree area' to 'forest fractions' or 'forest area'. We deleted the line 'Note, that we here use the terms tree and forest fraction interchangeable, although they are defined distinctively in reality.'

Table 1: are you sure the units of delta Pr is mm/yr?

Reply: Yes.

Line 323: is this somewhat related to an acceleration of the Atlantic THC? By the way that is the only region the IPCC shows as cooling in the SRES scenarios. Please explore the implications of this result deeper.

Reply: We will add: 'This result is in line with the reversed finding by Rahmstorf et al. (2015) who found a 'cooling blob' due to the freshwater input from the Greenland ice shield caused by global warming slowing down the meridional overturning circulation.'

Line 353: the increase in leaf area is such that LAI surpasses that of a typical tropical forest?

Reply: Yes, since the regrowth of forest is suppressed in this setup.

Line 359: But this also decreases incoming shortwave radiation, doesn't it? Fig 3f doesn't show a considerable increase in longwave radiation.

Reply: This observed phenomena is very local and averaged out in Fig. 3. You would see it in a map of shortwave radiation which is not shown and you can actually see in in Fig. S6 and S7. We will add this to the text.

Line 378: a "global" cooling effect due to CO2 removal. . .

Reply: Will be added.

Line 379: uncertain

Reply: Will be corrected.

Lines 389-390: Is this based on in-field experimental studies or modeling? How trustful is it?

Reply: Winckler et al. (2019b) performed model simulations with the MPI-ESM but also compared the results with various observational data sets which capture by nature only local effects.

Line 414: Tas or Tsurf?

Reply: 'Tas'

Fig. 6a: ToE in the tropics seems restricted to 30 years or more (i.e. values smaller than 30yr do not occur). Is that due to the use of a 30-yr moving average?

Reply: If you zoom into IPSL or CESM2 you can recognize single pixels with brighter colors / years below 30. The data was processed with the same script (no moving average applied!) so we can conclude that no robust effects appear in MPI before 30 years.

Line 501: "...robustly detectable after a few decades". What is that time lag attributable to?

Influence of slower thermal sensitivity of the adjacent oceans? That's one strong reason to show sea surface temperature and circulation changes in this article.

Reply: We will add '... decades as climate variability and mediating effects from the ocean have to be overcome (Davin and de Noblet-Ducoudré, 2010).'

Lines 508-509: but notice that this study did not test that (i.e. the climatic effects of re/afforestation). Would be a nice follow-up.

Reply: We will change 'are' to 'could be'.

Line 523: What is an unfavourable climatic condition? I suggest a different term also on L546.

Reply: We will add '(too dry or hot)'.

Fig. 9: after how many years?

Reply: Like all maps as described in the methods section 2.3 first sentence: Climatic variables cover the last 30 years, carbon variables the last 10 years. We will add 'averaged over year 50 to year 79' or 'averaged over year 70 to year 79' to every spatial plot.

Line 591: I think the authors should present arguments why the C4 grasses are “overly” productive. C4 grasses like sugarcane or others can indeed have a NPP higher than that of tropical forests.

Reply: C4 grasses are parametrized in a way that they survive in arid regions. When entering the tropics, they are therefore too productive. This will be laid out in line 523 (see above).

Line 621: It is unclear why figure 9f shows a steep decline of GPP in northern Amazon, considering that considerable increase in precipitation for that specific region was previously presented in the paper for BCC.

Reply: We will add 'Only in northern Amazon region, GPP is reduced under high temperatures and despite the observed precipitation increase (Fig. 7).'

Line 765: Why wasn't this analysis of large-scale circulation patterns done in this study?

Reply: Such a thorough study deserves its own publication and would exceed the scope for this manuscript (BGP and BGC effects).

Line 767: “. . . temperature gradient may alter the large-scale circulation. . .” Atmospheric or oceanic?

Reply: We will add 'atmospheric'.

Line 774: Is it possible to be a bit more specific on the sort of observational data needed?

Reply: We have listed Duveiller et al. (2018b) who created a spatially and temporally explicit data set based on various observational data sets (ESA CCI and MODIS) to investigate the non-local effects to land cover change.

Fig. S13: the brownish color in the tropics in panel f and in the boreal zone in panel d doesn't seem to correspond to any color in the legend.

Reply: This will be fixed.