

Response to reviewer's two comments.

We would like first to thank the reviewer for the careful reading of the manuscript and the useful and constructive comments. Indeed, we found these comments useful to understand why and where some of the difficult aspects of the paper would benefit from clarification. In the following, we include the reviewer's comments in italic and blue, and our response below each major comment in black.

General comments

With the interdisciplinary readership of ESD in mind, I recommend expanding more on the study's motivation and context, especially in the abstract and introduction. Currently, the manuscript lacks context/explanation at the beginning of both the abstract and the introduction. As a result, the entry barrier to the manuscript could be very high for readers who are not already familiar with the thinking of land modelers. Beyond that, the manuscript takes it somewhat as a given that the chosen time frames (PI, MH) and parametrizations (bare soil, photosynthesis, tcrit) are of interest. The same goes for introducing the relevance of vegetation feedbacks and the reasoning behind DGVMs as a whole. I reckon re-ordering thoughts popping up here and there in the introduction could reduce these entry barriers and improve its readability at the same time. For example, I would recommend touching on the PI and MH time frames much earlier - right now, this is done in greater detail only at the end of the introduction, but it would be helpful a lot earlier. Another example would be to touch on vegetation feedback with concrete examples early on, e.g., after the first sentence of the introduction, rather than jumping right to the standard literature review.

Following the reviewer's comments and suggestions, we reordered the introduction and added some context upfront. We also revisited the abstract accordingly.

In addition to that, I suggest the authors expand on how they employ the term "feedback" throughout the manuscript. On the one hand, they modify the strength and character of the vegetation's response to climate, which alters the vegetation-climate feedback loops, which is clearly formulated. However, on the other hand, they compare the radiative effects between the different model configurations/climate states and term the result of this analysis "feedback" as well (e.g. Figures 6, 11). Although one can "sense" the proximity of these radiative effects to the concept of a feedback factor (think ECS as an example), their understanding is not precisely described. As a result, I am asking myself the question to what degree the difference in radiative effect of say the "surface albedo feedback" diagnosed between the two climate states (Fig 6) is directly comparable to the radiative effect diagnosed between different model versions (Fig 11). Thus, I am missing a brief explanation of what is considered the forcing in the different analyses. Maybe adding a conceptual figure to the introduction or to Section 3.3 would have avoided my confusion.

During the first round of revisions, we made the effort to call radiative feedbacks the feedbacks connected to the estimation of climate sensitivity, and use the generic word "feedback" for vegetation-climate interactions. There are indeed some places where this was not fully done. We also agree that explaining it quite early in the manuscript would be useful, and we added it in the introduction. In some cases, the word "feedback" was replaced by "interactions" when dealing with vegetation-climate interactions rather than directly addressing vegetation-climate feedbacks. It should help to avoid confusion in the terminology. We also clarified why we use the same framework to diagnose radiative feedbacks between two climatic periods or for the same climate between two model versions. This is done both in the introduction and in section 4.1.

Finally, I am convinced that providing the final manuscript to a native-language proofreader and utilizing one of the many spell- and grammar-checkers would greatly help improve its readability and accessibility. Currently, it contains a lot of typos and convoluted sentences. I would not expect an extremely polished text, but as a reader, I found it very hard to get the author's point on a number of occasions, and it should be in the author's interest to make the paper easily accessible to the reader.

The paper has gone through spell and grammar checkers. We hope that having clarified some of the places where there was a lack of context or where the sentences were too complex provides the needed improvement in the language. It is difficult to do better with the tools and resources we have.

I am listing a number of technical comments at the end of my statement, but this list is not exhaustive.

Specific comments

- General remark on figures: Many figures use a color scheme that is not colorblind-safe. I am not expecting the authors to necessarily change this aspect, but I would recommend keeping this in mind for upcoming work. Also, with the current projection of map plots, I found it challenging to recognize details in the high latitudes – This is a bit unfortunate, since a substantial part of the results centers around exactly those areas.

We understand these comments. This whole set of figures has been time-consuming to draw, and we are doing it using versions of software available at the computing center to be close to the archive. Unfortunately, everything is not possible, and there is a bug in pyferret that prevents us from having a correct figure when rotating the reference latitude and longitude for the projection. We have worked on the layout as much as we could.

- The authors have updated the title. However, I find the newly added word “highlight” inconclusive. To my understanding, the authors utilize it in the sense of “modulating” or “amplifying” in the text as well (e.g., l 498), and in my opinion, both of these terms would describe their intention more precisely.

Well, the first version of the title, “Dynamic vegetation reveals unavoidable climate feedbacks and their dependence on climate mean state,” was closer to the exact focus of the study and the reason why we decided to write this paper. In the first round of reviews, we had strong comments on the title and decided to change it. The confusion comes from the fact that it was interpreted by the reviewers as if we pretended to be the first to advocate the need to consider dynamic vegetation in climate change experiments. This is not our aim. Our aim is the analysis we do of the close imbrications of first-order feedbacks, and the fact that these strong linkages trigger unavoidable feedbacks we do not really know how to deal with when developing climate models. These feedbacks are highlighted when dynamic vegetation is considered. This is why we chose the word “highlight” rather than “reveal” (although in French, “reveal” in the context we are using it is better) and revisited the title.

The proposition of the reviewer to adjust the title once more is interesting. We will not adopt it, because it puts too much emphasis on the dynamic vegetation, when the emphasis is on the imbrication of feedbacks and the fact that the change in vegetation is part of this first-order feedback. In our opinion, the title that best reflects a “pitch” of the paper content is

“Dynamic vegetation highlights unavoidable first-order climate feedbacks and their dependence on climate mean state.”

We thus propose this new version of the title and would appreciate feedback on it, or a discussion with the editor about it.

- l 26: *mid-Holocene and pre-industrial climate* → *mid-Holocene and pre-industrial vegetation state? (I am not convinced the green Sahara and boreal forest are part of the climate state in the physical sense.)*

In this case, this is indeed the case. The Green Sahara and boreal forest are part of the climate in a physical sense and characterise climate change. We rewrote the introduction, and this sentence has been revisited. It now only mentions vegetation.

- l65: *I find this statement a bit too much black and white, since there is more granularity to discuss on how much vegetation dynamics are (not) considered. Maybe the authors can expand a bit on this statement/add a reference.*

We reorganised the introduction so that the comment is now better connected with the other references. We also slightly modified the sentence so that it doesn't appear too black and white to readers who have been considering these couplings for a long time.

- l194/195: *What kind of inconsistencies and why are they not relevant to this study?*

It has mainly to do with the way the different types of vegetation, carbon, and water were reallocated to very small fractions of vegetation. These inconsistencies do not alter the results, and when corrected, the results remain similar, with a poor representation of boreal forest in V1. The corrections were also needed to guarantee that the model is the same when switching off or on the dynamical vegetation.

- Section 2.3 title: *"Vegetation-climate equilibration" ?*

We prefer the word adjustment here. We completed the title to explicitly say "Vegetation-climate adjustments to incoming solar radiation and atmospheric composition".

- Figure 3/Section 5.3: *Is Bartlein et al. 2011 really the most up-to-date/comprehensive reconstruction that is suitable? Also, it is only pollen-based and does not use multiple proxies. Would newer reconstructions like Erb et al. 2022 (Clim Past, multi-proxy but no pollen cores) be an alternative, or are there other reasons that are not discussed here?*

Bartlein et al. 2011 is indeed not the most up-to-date reconstruction, but it provides a consistent reconstruction of biophysical variables (coldest month, warmest month, and not a particular month) that are better suited to relate to the life cycle of vegetation than summer or winter temperature or a particular month. In addition, it provides uncertainties on the reconstructions, and the lead author knows very well the limitations of this product. There are now interesting new products, such as the one by Erb 2022, with data assimilation using climate models. The authors lack expertise on this product and on the way to properly use it for model-data comparisons. The Erb 2022 manuscript also mentioned several caveats in the reconstructions, part of which come from the multiproxy approach and that these proxies may reflect different aspects of the climate, some of which might be different from the original interpretation. There are lots of caveats in model-data comparison for the Holocene. The choice for this paper was to use a well-known product we also used in our 2021 publication discussing the PMIP Holocene simulations with the IPSLCM6 model. Doing something different from what we did before would require a specific study and in-depth intercomparison of the different products and the quantification of uncertainties. This will certainly happen in the new phase of the Paleoclimate Modelling Intercomparison Project.

- Section 3.2: *I wonder if it has been tested whether this approach is sensitive to the length of the different simulations. Since they have different lengths, they result in a different number of 100-year-long slices, and I could imagine this to have consequences for the statistics.*

The approach is sensitive to the length of the simulations, as is any statistical method with little sampling. We checked with the 1000-year-long simulations that it doesn't affect the results when we consider only 200 or 400 years of the simulation to compute the cross differences. The limitation in the number of degrees of freedom is the reason why we directly provide the results of each 100 years and do not try to estimate the mean 100-year average and error bars using a Student's t-test or another non-parametric method.

- *l306-308: I actually do not agree with how the figure is interpreted here. From Fig.5 it appears that the LAI in V2 is similar to V4 as well. And it seems to me that the snow cover changes are definitely the largest for V2 and V3.*

The comment is for Eurasia and eastern North America, and V2 and V3 have indeed the largest changes we wanted to highlight. Thank you, we didn't realise the error in the first parenthesis. It is now corrected to V2 and V3, the same for LAI and snow.

- *l313-315: From Fig.5 it also appears to me that V2 and V3 produce the lowest changes in precipitable water in the tropics, not the largest. In the high latitudes, the opposite applies.*

In this paragraph, the comment is only for high latitudes. We adjusted the text of the paragraph and added a comment on the fact that changes in precipitable water are larger in the tropics for the other two simulations.

- *l400: Figure 10 is not referenced, but I think it is discussed here.*
We added the reference to figure 10.

- *l410: I am pretty sure this should be Fig 11, not 12, referenced here.*
This is right. We changed the order of the figures, and it seems that there were still some inconsistencies in the revised version.

- *Figure 12: As far as I can see, it is neither referenced nor explained anywhere.*
Figure 12 is reference at line 432, 444 and should have been also referenced at line 487. We added the reference there.

- *The third-to-last paragraph in the Results is hard to read and could be simplified.*
We adjusted the paragraph, suppressed the parentheses and adopted a more direct style.

- *l524: I do not agree with how the finding is described here. The seasonality (difference between the highest and lowest amplitude) is stronger for V2, but it is not for V3, which in turn is very similar to V4. V1 resembles V2 more than it resembles V4.*

We adjusted the text by referring now to the peak LAI during boreal summer. However, V1 and V4 are parallel, and V2, even if it resembles V4, has a slightly larger seasonality. But we agree that because of the offset of V1 with the other simulations, it was not easy to follow.

- *l532-535: The authors argue that a seasonally lower GPP drives a higher soil moisture content. However, I cannot infer a seasonally higher moisture content in V4 from Figure 9, although I do see an overall offset. I would rather interpret this in a way that seasonally lower GPP reduces (evapo-)transpiration, leading to higher soil moisture.*

The reviewer's interpretation is the same as our interpretation. We didn't realise that "implies" would be interpreted as a direct link. Therefore, we added the link with the evapotranspiration, which is better.

- *l535-536: I am not convinced whether one can actually call this aspect counterintuitive. To me, it rather appears to be a straightforward consequence of the different photosynthesis schemes.*

This is true; "counterintuitive" comes from the fact that, with a given photosynthesis scheme, a simulation with a larger vegetation cover would certainly have a larger LAI and GPP, and this reasoning is often applied when comparing model results. When the connection is made with the photosynthesis parameterisation, as we do here, it becomes obvious that this is not what happens between our simulations and that it arises as the result of the different formulation of the photosynthesis parameterisations. We slightly expanded the reasoning in the text.

- Section 5.3: *When discussing land use effects, it should be noted that they also induce non-local effects. Therefore, the "NOLU" reference values could (and likely will) be affected by land use as far as I can see. Non-local effects could be briefly mentioned here.*

We expanded the text in this section and added two references, Smith et al. 2016 and Marquer et al. 2023, to better introduce the effect of land use and recent publications from LUMIP or the REVEAL pollen data set combined with modelling.

- Section 6: *As already pointed out by other referees, the case for DGVMs has been made before – which does not imply that the relevance for including them in ESM simulations should not be mentioned here. However, to perhaps suggest an additional aspect for the conclusions here: To me, this study is a great example for "no model (configuration) suits all needs" – one configuration/model might be better suited to simulate a Sahara greening, while another one might result in lower climate biases in another region of interest (and in addition climate is not just mean climate). And this diversity stems from the fact that the Earth system is highly complex and dynamic. Maybe the authors are interested in taking up this aspect.*

The point raised by the reviewer is interesting. Even though we share it, we decided not to go as far as proposed. We simply added a sentence in the last paragraph highlighting that models cannot be perfect and compromises need to be made.

Technical corrections

Multiple occasions:

- "Northern Hemisphere" (capital letters)
done with a systematic search

- "pre-industrial", "mid-Holocene" (mind the dash)
done with a systematic search

- "fully coupled/fully-coupled" (adopt one convention)
We are using fully coupled throughout the text

- "model content" – "model configuration" is probably more precise
In our mind, model configuration is the assemblage of model components we use. Here, we indeed refer to model formulation, i.e., the parameterisation itself with its assumptions. It means these are the same configurations, but not the same "physical package". We agree that in terms of model, the choice of different parameterisations can be seen as a configuration, but we would like to go one step further by pointing to how a phenomenon is represented.

- At several occasions "It"/"This" is used for a couple of sentences in a row. At some point, you lose track of what the term actually refers to, which is not helpful.
We have revisited the text with this comment in mind.

- l 28: *Major aims have been to either ... and to ...*
Corrected

- l34: *for the last glacial inception*
Corrected, but the sentence has also changed

- 137: What is the “initial effect of vegetation” – are you referring to the model spin-up? I wouldn’t consider this a very physical motivation for vegetation feedbacks.

We are not referring to model spin-up but to the direct effect of vegetation on climate. We thus change to direct effect of vegetation on climate.

- 137: “*They*” – *Who*?

The results of these simulations. This part of the introduction has been revisited.

- 148: I guess the authors are referring to Holocene simulations? Isn’t interactive vegetation common in a number of models, meanwhile, for present-day and future?

No, we are referring to model configurations used for climate projections, which is why we provided the Arias et al. 2021 reference, which is the reference of the IPCC WG1 2021 technical summary in which there is a synthesis figure about. We added the number of the Arias et al.’s figure in the reference (Figure TS2.2. In paleoclimate, most groups are convinced dynamic vegetation needs to be included, since vegetation is the first manifestation of climate change as seen in paleo pollen records...., but do not necessarily have it in the version of the models they have access to.

- 149: “still have”

Corrected

- 150: “model biases ... as those discussed by” – As the whole paper is about improving a vegetation model, it would add to the motivation of the study to name a few of the biases specifically

We added the underestimation of the green Sahara as an example

- 154: “Climate-vegetation feedbacks on climate sensitivity... in estimates of climate sensitivity”

Thank you with also added “radiative” → “albedo and atmospheric moisture radiative feedbacks”, to distinguish between the different types of feedback we are discussing.

- 156-58: The sentence is confusing. It seems to mention the same aspect twice.

The first part is climate mean state, as the one we would consider between different climatic periods. The second one is the simulated climate mean state, i.e., it can be different between two models for the same climatic period. We have tried to clarify this point.

- 161: interconnections

Corrected

- 163: may be

Corrected

-164: “fully-coupled” and then the sentence does not make any sense to me afterwards

The sentence has been revisited, and we mention model performance instead of climate system, which is more accurate in this context.

- 167-69: The sentence is very long and convoluted

We have simplified the sentence

- 171: “the vegetation-climate feedback” – which feedback? This would be an opportunity to be more precise/expand

We specified biophysical vegetation-climate feedback

- 177: “on the model content”

Corrected

- 183: *“We focus on estimating the atmospheric..”*

Corrected

- 190: *remainder*

Corrected

- 1105: *“run using ...”* → *“operates on the atmospheric”*

Changed following the proposition

- 1119: *“two parametrizations”?*

We decided to keep formulation here

- 1182: *similar orbital configuration*

Corrected

- 1186/187: *“It somehow provides..”* Sounds a bit spongy – could you be more precise?

Changed to “It also provides”

- 1211: *imposes a cold start for the land surface*

Corrected

- 1214: *“recovery” is not so much the right word here, I would suggest “adjustment”?*

We changed to “adjustment”

- Figure 2: *There are some data gaps in panel (I), which I would not expect. Also, there is a type in “bare soil” (panel a)*

Thank you for raising this point. We corrected the typo. In panel I, this was not a data gap; it was due to the lower limit on the y-axis and the fact that lines were not drawn across the x-axis. This has been corrected.

- 1252: *interpolated to*

Corrected

- 1254 and following: *“centennial” would be an alternative to “100-year”*

We decided to keep 100-year because it corresponds to the exact size of the window we consider.

- 1265: *“when accounting for uncertainties (Fig 5)”* – *Figure 5 does not show any uncertainties*

Corrected to Fig. 4. The sentence has been adjusted and now starts with “The regional averages plotted in Fig. 4” .

- 1266: *“as it is expected with vegetation feedback.”* *Why is this expected?*

Part of the sentence was missing “larger precipitation than the IPSL standard IPSL PMIP4 simulations”. We also added the reference to Braconnot et al. 1999.

- 1280: *“we computed”*

No change, the text is correct here.

- 1301: *omit “with”*

Corrected

- 1329: *by replacing ... one by one by those obtained ...*

Corrected

- l338: *LW_{sup}*

Corrected

- l362: *The first-order feedbacks between ... highlighted in the previous section*

Corrected

- l376: *“include the change”/“benefit from the change”*

Corrected

- l 384: *“With the bareold scheme, ... “*

Corrected

- *Figure 9: “Evapnu”?*; *“Transpiration”*

We change evapnu to bare soil evaporation

- l414: *“are the differences in seasonal..”*

Corrected

- l419: *“mainly originates from the relative..”*

Corrected

- l431: *“does not prevent”*

The sentence has changed and this part has been suppressed.

- l438: *“outgoing”*

Corrected

- l439: *“increasing temperature ... the higher atmospheric..”*

Corrected and sentence revisited

- l444-445: *This sentence does not have any meaning*

For sure, the verb “are found” was missing. Corrected

- l450: *“The feedback differences between model versions”*

Corrected

- l460: *“To first order, the distribution ...”*

Corrected

- l498: *“highlights” → “modulates” or “amplifies”?*

We prefer keeping highlights here. The reason is that these feedbacks are also active when dynamic vegetation is not considered, but are not necessarily seen. Of course, this is due to the fact that vegetation changes amplify the feedback because they have an effective effect on the vegetation cover, which is not the case when vegetation is prescribed.

- *Figure 13: “Atmosphere”, “pre-industrial”*

Corrected.

- l527-528: *This sentence appears superfluous to me.*

Well, we keep it because it was not obvious at first glance to realise that the shape of these curves is driven by the photosynthesis parameterisation and not the distribution of vegetation.

- *Figure 14: wrong panel labels for (c) and (d). Also, I was wondering about the sign convention for the NEE here – do positive values correspond to more or less uptake?*

Corrected. The legend has been completed with the indication of the sign convention. Positive values indicate a reduction in the vegetation carbon sink.

- *1552: “photosynthesis parametrization”*

Yes, parameterization is better here. Corrected

- *1558-559: I was curious if the authors could speculate about the nature of these different characteristics.*

No we will not at this stage. We didn't dig deeply enough into the details of the changes in land carbon between the simulations.

- *1561: What does “direct development” imply?*

“directly on the mid-Holocene climate and not on the modern climate, as it is usually the case”

- *1575: I think the reference should be to Fig.8*

Corrected