Response to the comments made by reviewer 2

We would like to thank the reviewer for the careful reading of the document and the useful comments, which will help us to improve the manuscript.

1) *1 The main finding is that vegetation feedbacks are incredibly important and that they vary seasonally and by location. These have been quantified for 4 different model configurations. From this I'm left wondering whether this is a way to discern more clearly between the model versions. Even if this is not done here, it could be discussed what observations from either present-day or from the mid-Holocene would be required.*

This is an important comment. Yes, the dynamic vegetation is a way to distinguish more clearly between different model versions. Indeed, it provides indications of critical aspects to look at in a fully coupled system, such as the soil evaporation in spring in mid-latitudes or how the photosynthesis parametrisation triggers the plant seasonal development. While this is not a new concept, this study introduces new elements that can inform Earth system thinking. This way of thinking is still difficult to incorporate into model development, as most approaches still rely on impact-based reasoning rather than feedback-based reasoning. The mechanisms we discuss and the way they trigger atmospheric feedbacks are model-independent. What is model-dependent is the mean climate state, which depends on these factors and, critically, on the atmospheric or ocean-ice physics (for the timescales considered here). We will reinforce the conclusion on these aspects. As requested, we will also expand the discussion about observations, without adding model-data diagnoses to this manuscript. However, the key point we raise in the conclusion is that it is almost impossible to find the right way to evaluate the model. Present-day observations are affected by land use, and paleoclimate data are indirect. We can address this by examining different types of paleoclimate indicators. More importantly, however, the available reconstructions still have incomplete data coverage in several key regions. This is why, in the conclusion, we propose that looking at different past periods for which changes in seasonality are the dominant factor. Together with the preindustrial and the present-day climates, these past periods, such as the mid-Holocene or the Eemian, allow us to evaluate the ability of a climate model to reproduce seasonality and the seasonal feedbacks, considering both the seasonal processes and the factors arising from differences in the climate mean state.

2) *The Discussion and Conclusion section lacks focus and covers perhaps too many topics. I think this paper would have much greater impact if these two aspects could be separated and a more concise and clear Conclusions section were to be developed.*

We agree that the discussion and conclusion sections are too long and should be reorganised and refocused. We have two possible solutions for it. The first one is to keep the discussion and conclusion section and add subtitles. The second solution is the one proposed by the reviewer, which consists in adding a Section 5 before the conclusion. We propose to do this and add a section 5 "Synthesis and implication for the carbon fluxes," where we will discuss fig 13 and 14. The conclusion, section 6, will then summarise the key findings and provide a perspective for model evaluation and model development.

3) *Editing for grammar, typos and figure presentation is needed.*

Thank you for highlighting these typos and errors. Some of these typos could have been avoided. Others are more complex to detect for non-native English, and have not been detected by our English corrector. We will improve this. Regarding the figures, we will consider the different remarks and adjust the figures accordingly. Concerning the cropped edges of figures, the small piece missing for one of them results from the inclusion of the figures in Word tables for the production of the complete

manuscript. The original figures are correct. The issue with the numbers in the legend colour bars it is more complex, and requires fixing a bug to improve this. As suggested, we will first try to reduce the number of colour bars so as to enlarge the size of the maps..

Minor corrections:

Title: I find the word "unavoidable" slightly misleading here. It has connotations of committed climate change etc. I recommend rewording throughout with something like robust or parameterisation-independent.

We understand this comment. It is consistent with Reviewer 1's comment. The term was chosen to draw attention to the fact that these feedbacks are difficult to anticipate and understand in a fully coupled system. Since our discussion focuses on first-order feedback, we will replace the term with 'first-order', which is consistent with the way we discuss these feedbacks in the manuscript.

We propose therefore changing the title to :

"Dynamic vegetation highlights first-order climate feedbacks and their dependence on the climate mean state "

Line 132: This is very similar to the changes to soil moistures stress in transient Holocene simulations by Hopcroft & Valdes, 2021 PNAS.

Our changes consist of adding biomass-dependent resistance to bare soil evaporation. As far as we understand, it is not exactly the same as in Hopcroft and Valdes (2021, PNAS). The changes they made in their study affect all PFTs and thus have a major impact everywhere. There is already soil moisture stress for the different PFTs in ORCHIDEE, and we have kept this as it is. Here, we only consider bare soil moisture stress, and thus the proportion of total evaporation between plants and soil. This explains why the effect is significant in mid-latitudes and in spring, subsequently affecting tree growth, whereas it is small in the Sahel or has almost no effect on the green Sahara. Although we are specialists in the African monsoon, we have chosen not to discuss Africa in too much detail in this manuscript, focusing instead on the differences between the simulations.

*Figure 4: The cyan (data) points are not easy to see. Could you redraw using thicker lines for the data points?**

Yes, we will improve this, and also add the large error bars for the reconstructions (they are considered in Braconnot et al. 2021).

Figure 7: for clarity could you consider creating a single colour bar for all panels and labelling it with PFT groupings instead of numbers.

Yes ,we will try to do it if we find the way to adjust the relative size of the panels.

Figure 8: similar comment as above - label the y-axis with the PFT names not numbers.

This is more difficult to do. The reason is that the PFT names are too long and would appear too small. We will certainly use PFT acronyms instead.

Line 260: "We synthesize the mid-Holocene differences with preindustrial by showing the mean root mean square difference between the two climates in Fig. 5 for leaf area index (lai), snow, and atmospheric water content."

It's not clear why this choice is made at this point. It will compress everything to be a positive anomaly which is reducing the information. Is this intended?

Yes, it is. The annual mean is the residual of large seasonal variations. This is a way of showing on a map where the largest variations occurred between the mid-Holocene and the pre-industrial period, taking into account shifts in the annual mean and changes in seasonality (magnitude and seasonal phase).

Lune 278: "all of these model versions produce a green Sahara"

I'm not sure I agree with this. The precipitation anomaly shown in figure 4 is too small and the LAI anomaly is only covering half of the Sahara?

Yes, they do, and grass is the dominant PFT. Note that we use a threshold of 0.2 for the LAI map; thus, there is a visual artefact. We will revisit the figure, trying to use a different colour map and, as already suggested, suppressing the redundant colour legend.

Line 284: "and from atmospheric physics and land surface improvement between the IPSLCM5 and IPSLCM6 versions of the IPSL model (Boucher et al., 2020; Hourdin et al., 2020)."

I'm not sure this is very well supported. can you either explain in more detail ?

You are right; it is not well supported, so we will suppress this sentence. It is based on the authors' knowledge of the model and still needs to be fully analysed. This is beyond the scope of this manuscript.

Lines 333-336: "The snow albedo effect is amplified

334 when grass is replaced by forest in the mid-Holocene simulation, which occurs over a large area in Eurasia for V2

335 and V3 compared to V1 where grass is dominant or V4 where a larger fraction of forest is still present in the

336 preindustrial simulation (Fig. 7)."

Should this be the other way around or could you clarify? Grass being replaced with trees would result in lower albedo overall because trees are lower albedo than grasses and trees cannot be covered as efficiently by snow as can grass?

The concept of work amplification applies to both positive and negative effects. This sentence seems to cause some confusion. We will rephrase it for clarity.

Line 362: "It appears to be a critical model aspect contributing to a better representation of boreal forest."

Again I'm not sure I agree as the difference in the boreal forest pft 7 seems very small between V1 and V2.

PFT 7 is just one of the boreal forest PFTs. The boreal forest encompasses PFTs 7 to 9. PFT 9 covers a large area in V2, but the cold climate still prevents PFT 7 and 8 from expanding. So yes, it is a critical aspect, and all the tests we have conducted confirm that it is particularly critical for mid latitudes. We have already adjusted bare soil evaporation by a factor in the previous simulations with dynamical vegetation (a had-hoc solution). This was already done to limit evaporation in spring and allow vegetation to grow in the mid-latitudes.

Technical corrections

Overall there are a lot of minor typos, grammatical errors and cropped edges of figures. Some of these are included below.

Thank you for highlighting the typos and remaining errors. regarding the figures, the issue with the numbers in the legend is a bug that needs to fixed. We will revisit the layout of the concerned figures by reducing the number of colour legends.

Line 9: "with the IPSL climate models for which dynamic vegetation is switch on." This should be: switched on

Thank you

Lines 16-17: "which are needed to fulfill the global energy conservation constraint of the climate system."

I don't really understand what this means in this context.

This is an important point. These are coupled equilibrium experiments, for which energy conservation is a strong model constraint. We will rephrase this and provide the missing explanations in the text.

Lines 18:control -> controls

Line 18:nb"Photosynthesis parameterization .." should be "The photosynthesis parameterization …"

Line 25:"The Green Sahara"

*Line 41: "The increase *in the* number of "*

Line 42: "has emphasize" -> "has emphasized"

Thank you for highlighting these errors. We will correct them and improve the way English is checked throughout the document.

Line 129-131: "This adjustment in the bare soil evaporation parameterization was not incorporated into IPSLCM6A-LR due to the fact that it induces a surface warming that was

not fully understood to be used in the whole suite of CMIP6 simulations (Cheruy et al., 2020)."

This is a grammatical error in this sentence.

We propose to supress the end of the sentence after understood.

Line 896: "the vcmax curves are plotted toe a mean temperature"typo

Line 152-155: "Another important difference is that in PhotoCM6, the response to temperature is adapted to the local long term (i.e. 10 years) temperature of each pixel whereas in PhotoCM6, the temperature dependence is fixed for the whole pft."

This does not make sense to me.

The second one should definitely be PhotoCM5. We will revisit this sentence. It's a correction that should have been made before the submission.

Line 198: "It guaranties the entire consistency between the simulated climate and the simulated vegetation."

This doesn't really make sense to me.

This is the only way to ensure that all parameters used in the land surface model are reinitialised and consistent across the model's dynamics, hydrology, and carbon components. We can suppress this sentence as it refers to minor inconsistencies that have been corrected depending on whether the model is used offline, online, with or without dynamical vegetation. Therefore, the reference to Braconnot et al. (2019) in the previous sentence is sufficient.

Line 217: "A conclusion from Fig. 1 is that 300 years of" this should be figure 2.

Yes, you are right. We added figure 1 late in the writing process and forgot to update this number

Line 244: |standard IPSL model without dynamical vegetation"which model configuration is that - state here please.

We will adjust this sentence. It refers to the PMIP4 mid-Holocene simulations that were run using the IPSLCM6 model (Braconnot et al., 2021).

Line 302: "atmospheric diffusion do you mean scattering and absorption?

Yes, this is an error. We will correct

Line 296: "Positive values (negative) indicate that the feedback brings more (less) energy to the climate system in V4"

These double meaning sentences in brackets are in my opinion extremely hard to read and should be avoided. e.g. https://doi.org/10.1029/2010EO450004

We agree and will suppress the text in brackets here.

Line 510-511: "We insist on the fact that climate-vegetation interactions induce seasonal feed backs that trigger unavoidable first order albedo and water vapor radiative feedbacks"

This use of "insist" and "unavoidable" comes across a little odd. Could you clarify e.g. "We find" instead of we insist, and instead of unavoidable use a word like robust or parameterisation-independent?

This comment is similar to a comment by reviewer 1. We will revisit the sentence

Lines 517-521: "The LW radiative feedback is less discussed when the role of vegetation is inferred from vegetation alone simulations or simulations where the sea surface temperature and sea-ice cover are prescribed. It is a first order effect associate to the change in temperature and fulfil the convective radiative equilibrium which serves as a basis for the reasoning on climate sensitivity (Dufresne and Bony, 2008; Manabe and Wetherald, 1975; Sherwood et al., 2020)"

This isn't clear at all.

We will detail a little bit here coming back to the constraint on the global energy conservation in the ESM model, and the long wave and short-wave balance needed at equilibrium. Intermediate explanations are indeed needed, including the fact that the radiative balance can be broken in atmosphere alone simulations.

Figure 4: it's really not clear which circle is what in this figure. Please improve the legend.

We will do it.

Line 934" Not that" should be "Note that"

Thank you

FIgure 9: make panel titles in English not in model variable codenames please. e.g. total soil moisture instead of mrso.

We agree. We'll have to find a way to keep this long name readable.

Figure 13: consider connecting the same-coloured dots with lines for clarity?

We will not do it because we already tested it and we know the figure becomes a mess.

Figure 14: this and other figures have edges of the figure cropped.

For this figure, it comes from the way the figure was included in a table in the Word file. The original figure is correct.