We would like first to thank the reviewer for the useful comments. Several important points have been raised that will be considered for the revision of the manuscript.

*I recommend publication of the study with minor corrections, but I strongly suggest skipping the term "unavoidable" in the title and in the text. This term is not explained in the text. It just sounds alarmistic, as if the model would have a choice to "avoid" any negative consequences of model tuning. Quite the contrary, model tuning is done to improve the model performance.*

The term was indeed chosen to call attention to these feedbacks that are difficult to anticipate and understand in a fully coupled system. However, the two reviewers have a different interpretation of the use of this term compared to our intention. We therefore propose replacing it with 'first order', as this study is indeed discussing these well-known but poorly understood first-order coupled feedbacks. We propose to change the title to :

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

*When reading the paper, I see that the new IPSLCM6 yields a much greener mid-Holocene Sahara than the former IPSLCM5 did. That is an exciting result. The authors highlight this achievement in one sentence (line 278/9) and a half-sentence (line 284/285). It would deserve more appreciation in the conclusions. Perhaps, a figure with a zoom on African biomes, using the biomization tool by Dallmeyer et al (2019), for example, which has already been applied to ORCIDEE PFTs, would be useful for a better comparison with other ESM simulations. But I leave this to the authors to decide.*

We haven't analysed the results in Africa sufficiently to be able to provide a well-grounded explanation of the different aspects that lead to the representation of the green Sahara in this version of the IPSL model. This should be the focus of another publication that we are planning on this topic. We are also very interested to understand better why the new version of the model represents the African humid period. We will thus only note that this was not anticipated from our mid-Holocene PMIP simulations (Braconnot et al. 2021), and we will suppress the end of the paragraph (lines 283 to 285).

*Perhaps a more formal analysis using the Alpert-Stein factor separation would yield a better understanding of feedbacks and synergies between feedback. But, again, I do not insist on doing a new analysis which would require $2^4$ simulations. It would be sufficient to mention that the present study does not differentiate between the pure contributions triggered by a new parameterization and the possible synergies emerging from combining new parameterizations.*

We agree with the reviewer that this would be a good idea. However, the traditional method of performing such a decomposition using coupled and standalone simulations is not entirely appropriate for high latitudes. This is because sea ice is modelled differently in atmosphere-alone simulations and in the fully coupled system, which affects the snow-ice albedo feedback and introduces errors in the estimates. In this manuscript, we focus on the total effect of the changes made to the model in the different versions. We emphasised the separation of

atmospheric feedbacks. This is why the choice was to use the simplified partial radiative perturbation approach to disentangle the role of atmospheric and surface albedo feedback on the atmospheric radiative budget. In the revised version of the manuscript, we will ensure that the fact that we do not estimate vegetation feedback itself is clearly stated.

*Finally, I suggest skipping trivial common places like the very last paragraph (lines 630 – 634). It is completely true that dynamical vegetation is an important factor which should be considered in ESMs. But this study is not the first one to point to the importance of dynamic vegetation. We (including the authors) have convincingly addressed this topic by numerous studies over the last roughly 30 years.*

This point is well noted. We fully agree that we are not the first to show that dynamical vegetation is an important factor. We will suppress or reformulate the text where needed to clarify the specific points we address in this manuscript. There are few studies connecting feedbacks to global energetics and energy conservation in a fully coupled model, which is what we do here. The two reviewers have made comments on the sentence in which we refer to global conservation, which suggests that this aspect is not entirely clear. We will expand this sentence.

*Minor comments:*

*Line 116 and following: It would be useful to learn something about the interaction with the C-cycle. Into which carbon pools of the plants and the soil is the carbon gain by photosynthesis fed? Or does this issue do not play any role here?*

This certainly plays a role, but there is no change between the model versions. We will provide a brief description here and add a comment later in the text explaining that an increase in GPP first leads to an increase in NPP, and then to an increase in biomass. This affects the carbon in the soil. In cold boreal forests, where decomposition is slow, a small increase in GPP induces a large relative increase in soil carbon.

*Line 242, Fig. 3: The abbreviations in the title lines (dtas, dpr) are not defined in the caption. Why not put a \Delta T_s or \Delta P_r in the title lines and in the caption?*

To avoid too long titles, we use tas and pr because these are standard CMIP names. We will reconsider it.

*Line 257, Fig. 4: The new parameterizations increase the simulated annual mean precipitation in WA, but still, the simulated precip amounts to only a factor of 0.4 of the reconstructed precip. How is the aggregation of data points and comparison with grid box results done? Using any area-mean? (Would be sensible to only consider grid boxes for which reconstructions are available.)*

Thank you for this remark. A short paragraph explaining how we did it disappeared between the different versions of the manuscript and we didn't realise. The model sampling is indeed done where there are reconstructions. This follows what was done in Braconnot et al. 2021. We will complete the explanation in the text and add a sentence to the figure caption.

*Line 283 ff: "It results from vegetation feedbacks amplified by synergy with ocean feedbacks …" surely, it does. But without differentiation between feedback and synergy, it remains a*

*trivial statement and could be skipped – in contrast to the second half of the sentence which likely is the real reason and would deserve more attention.*

Well noted

*Line 287, Fig. 5: The labels on the colorbars are partly hidden behind the colorbars. Please shift. The global maps, specifically for the differences in lai, are too small to see any details outside the tropics. Please enlarge the figures to the size of the other global maps in the other figures.*

We will fix this, there is a bug somewhere…

*Line 304: It would be helpful to note that alpha_p is the surface albedo. Commonly, one would symbolize the planetary albedo with the subscript 'p'.*

Alpha _p is the planetary abedo and alpha_s the surface albedo, this is provided line 310

We will add alpha_s in parenthesis on line 304 to avoid any confusion.

Eq.(3) and other places in the text: Sometimes the subscripts appear as subscripts, sometimes as an extension of the variable, for example as in SWsi  vs. SW_{si}  or LWsup vs. LW_{sup}. Please harmonize.

Well noted. It will be done

Line 318: gases instead of gazes

Well noted, thank you

*Line 348: What are pft 7 and 8? It would help reading, if the names of the PFTs are mentioned here or in a table.*

Line 454 and other places: Sahara Sahel or Sahel Sahara sounds a bit cumbersome, because the Sahara and the Sahel (region) are pretty different regions.

We will clarify this

*Line 457: …, so that the magnitude …*

Noted

*Line 509: "The suite of mid-Holocene  … allow us to dig into the complexity of the Earth's climate system." That is a rather generic and bold statement as this study just touches a small subset and very specific aspects of the global climate system.*

We will rephrase to be more precise here.

*Line 510: "We insist on the fact …" I do not understand, why you have to 'insist' on the fact, instead of highlighting the fact.*

This is a word that does not have exactly the same meaning in French in this context. We will revisit the sentence.

*Line 519 ff: I do not quite understand the meaning of this sentence. Perhaps it is just the wording 'associate to' ... The word 'fulfil' should be 'fulfill'.*

We will clarify

*Line 527: "We show that dynamical vegetation reveals how ...." I am not sure how dynamical vegetation can reveal anything. The analysis of the climate-vegetation interaction can certainly do, but, again, only with respect to the processes considered, not with respect of the entire complexity of all biospheric processes.*

We agree. We will rephrase

*Line 537: Which "step changes between the model version ... is (shouldn't is 'are') different from ...?*

We will add precision

*Line 542: Which "model content" ... lead(s) to different vegetation cover ...?*

The physics of all components except the land surface is the same between model versions and constraints model feedbacks. This is where we will provide more detail on aspects of the studies that depend on the model and those that are more generic, in order to address the point raised at the beginning of the review.

*Line 562 ff. Indeed, the statement that "simulated vegetation is an integrator ..." is "trivial". Perhaps a more modest statement would be sensible. This study is not the very first one to highlight the importance of vegetation dynamics.*

We will consider it.

*Line 565/567. I agree that one cannot infer vegetation feedbacks from studies in which vegetation patterns are kept fixed. In this sense, the titles of early studies (e.g. Kutzbach et al. Nature 1996) are misleading. These studies analyzed impacts rather than feedbacks.*

We will consider the remark and complete the text

*Line 596: This would require (instead of requires)*

Noted

*Line 600: ... because land use (not land used) is not*

For sure, thank you.