We thank the anonymous reviewer for the time and effort they invested in critically reviewing our manuscript. Please find answers to your very helpful comments and suggestions below.

Answers to reviewer 2

Major points

1) Sect. 2.3.2: Do I understand it correctly that JSBACH is run in offline mode at one (grid) point, using an atmospheric forcing from observation or reanalyses? Then, why is a spin-up of ten years needed? For instance, Chen et al. (1997) have shown that in PILPS Phase 2a the vertical profiles of soil temperature and water content converged to an eqilibrium after two to three years. This was in Cabauw (The Netherlands), not in the rainforest. Anyway, since there is an extensive rain period, you could start the simulation there, initialize the soil water content at saturation, and run the model into equlibrium. You should show that this is not already reached after two or three years, but that you really need ten years. This would make more sense in order to understand the model behaviour of JSBACH. As you say, the soil is not even particularly deep. Maybe, with a shorter spin-up, you find periods of the ATTO measurements with less data gaps?

We did not mean to suggest that all future studies of this type should use a 10-year spin-up period. In fact, our results in Sect. 3.2.1 indicate that for many of the considered variables, a spin-up period of less than 1 year would be sufficient. After longer time periods, we would only expect additional changes for soil temperatures in the deeper layers and for carbon pools (see new results in point 2 of reviewer 1) at this specific site.
While it would have been possible to choose a time period with two or three years of nearly complete ATTO measurements for the spin-up forcing, we made the decision to incorporate reanalysis data as well. This approach allows for greater versatility, as it can be utilized in other studies where only data from shorter campaigns, spanning a few weeks or months, are accessible. By incorporating reanalysis data into the spin-up, our setup becomes applicable in a wider range of scenarios. We now added a discussion of these points in the first paragraph of Sect. 3.2.3:
"Based on the results of the previous sections we construct an optimized version of the spin-up run. The findings from Sect. 3.2.1 indicate that the duration during which the choice of spin-up forcing data set has a significant impact on most variables is less than one year. As a result, a spin-up period of two or three years would be sufficient to reach an equilibrium state for soil water content and soil temperatures in the upper layers at this specific site. However, variables like temperatures of the deeper soil layers or the green carbon pool require a longer spin-up duration. Therefore, when employing a standalone land surface model, the selection of the spin-up period should be determined by the specific processes of interest. In our case, we adopt a cautious approach and use a 10-year spin-up period for the model, which has the following characteristics: ... "

2) Sect. 3.3.1: The wind looks difficult. Likely, there is a problem with the representativity, since the reanalyses can not describe the high forest canopy, but in reality it exists. It is not clear to me how a bias correction should work here. Maybe, it is mainly a matter of finding the correct heights above ground (or canopy top) to make the quantities comparable? Could you discuss this a bit more?

We assume that you are referring to Sect. 3.2.2 "Sensitivity to wind speed and precipitation biases". While it is evident that reanalyses lack a distinct canopy layer and therefore cannot fully capture all relevant processes, the forest canopy's influence on the wind profile is partially considered by increasing the roughness length. However, considering the biases identified in other studies in regions with different terrains and land uses (as listed in Sect. 3.1.1), it is

more plausible that the bias is related to a general issue with the wind representation in the reanalyses, rather than simply a discrepancy in height alignment of the wind profiles.
Nonetheless, we acknowledge that the approach we used in Sect. 3.2.2, where we applied an offset to correct the biases in wind speed, is an oversimplification. The main objective of this section was to demonstrate the potential impact that biases of this magnitude could have on the model results. We added a short description of this limitation to the first paragraph of Sect. 3.2.2:
"The results presented in Sect. 3.1.1 indicate that the underestimation of the wind speed by the two reanalyses is a complex issue. For simplicity, we apply a very simple bias correction in this sensitivity study by adding an offset of the annual mean wind speed bias of -1.2 ms^{-1} between 2014 and 2018 to the MERRA-2 data. The results are then compared to those using the original MERRA-2 forcing."

3) L. 472-474: Instead of "temperature damping effect" I would call it rather "shading effect". Without a vegetation canopy the model has no chance to get any of the following temperatures right: Soil temperature (different depths), surface temperature, and 2-m temperature. Instead of a complex canopy scheme, a simpler way to represent this mechanism is e.g. a conceptional "skin temperature" scheme, see e.g. Viterbo and Beljaars (1995), Heidkamp et al. (2018), or Schulz and Vogel (2020). The work of Heidkamp et al. (2018) is available in JSBACH. It may be advisable to apply it in your study, in order to represent the shading effect due to the vegetation. This would reduce the amplitudes of the diurnal cycles of the soil temperatures, and increase the amplitude of the surface and 2-m temperature. It would be good if you could demonstrate this in your manuscript, because the observations you have available.

We selected the term "temperature damping effect" to provide a straightforward description of the fact that the diurnal cycle of the surface temperature actually looks like soil temperature from a deeper layer, where the amplitude is dampened. While the term "shading effect" you proposed is indeed applicable in this context, it may primarily invoke associations with radiation-related processes and might not encompass the crucial aspect of canopy heat storage, which is also important for this effect. Consequently, we believe it is more appropriate to adhere to the original term of "damping".
In general, we agree with your remark concerning the "skin temperature" scheme. A simple approach, which includes only the canopy heat storage instead of an explicit representation of the canopy layer, would likely be able to capture at least a part of the dampening/shading effect. Unfortunately, the approach by Heidkamp et al. (2018) was originally implemented in JSBACH 3 (version 3.11) and has not yet been transferred to version 4 of JSBACH, which we used for the model runs in this study. Nevertheless, we have expanded discussion of the dampening effect and write now:
"However, JSBACH (version 4) does not include an explicit canopy layer or a parametrization of the canopy heat storage effect. Consequently, the model is not able to capture this dampening effect." (original l. 438)
"Secondly, it would be beneficial to include a representation of the canopy heat storage effect into the model. This could be accomplished by modeling the processes in a separate canopy layer explicitly or by adopting a simpler approach that parametrizes the heat storage by the canopy. Heidkamp et al. (2018) and Schulz and Vogel (2020) demonstrated that a simple approach, which is based on a skin temperature formulation, reduces the underestimation of the amplitude of the diurnal cycle of surface and soil temperatures and the corresponding incorrect phase shifts. Moreover, the skin temperature formulation improves biases in latent and sensible heat fluxes (Schulz and Vogel, 2020; Renner et al., 2021)." (original ll. 472)

4) L. 475-476: The evaporation of water from the interception reservoir is usually less relevant for the simulated soil temperature. It may play a role after dew fall in the morning (or after rain fall) for the 2-m temperature. Please rephrase.

The sentences were rephrased to:
"To reduce the soil temperature bias of the model, it might also be beneficial to re-evaluate the representation of additional cooling terms within the canopy layer. For example, evaporation of dew or of rainfall intercepted by the vegetation impacts near-surface air temperatures, which then in turn influence soil temperatures."

Minor points

5) L. 49: ... errors in the ...

Done.

6) L. 54: ... based on two ...

Done.

7) L. 58: ... (Yang et al. 1995), ...

Done.

8) L. 66: ... of the forcing data ...

Done.

9) L. 71: ... turbulent heat fluxes ...

Done.

10) L. 72: shortcomings

Done.

11) L. 74: Section numbers are missing

Done.

12) L. 90: less or equal, or larger or equal 36 m?

It was corrected to $\geq$36m.

13) Fig. 9: (a) and (b) are mixed

The panels were re-aranged to match the order of appearance in the text and the figure caption was corrected.

14) L. 398: "field" capacity. Anyway, field capacity is not saturation, this would be pore volume. Please rephrase.

The sentences were rephrased to:
"In the wet season, all soil layers approach a constant value of about 35 %, which is close to the field capacity of the soil. This indicates that the soil water content approaches saturation levels during rainfall events."

15) L. 404: soil types → soil textures

Done.

16) L. 407: an exponential root profile would be typical

We added the following sentence:
"Other studies indicated that it is beneficial to adopt an exponential root profile assumption (e.g. Jackson et al., 1996; Zeng, 2001)"

17) L. 468 and around: soil types → soil textures

Done.

References

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