

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-56

Anonymous Referee #1

Referee comment on "The Earth system model CLIMBER-X v1.0 – Part 1: Climate model description and validation" by Matteo Willeit et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-56-RC1>, 2022

The paper presents a comprehensive description of the EMIC CLIMBER-X. This description is well balanced with a main text presenting the important principles and choices at the basis of model development as well as the model performance while the equations and parameters are given in the appendices. The model is designed as the successor of a very successful model, CLIMBER-2, which has been used extensively over the past 20 years. CLIMBER-X will very likely be as successful and this model description will thus be very useful for the scientists running the model and analyzing its results.

The paper is very clear and well written. It includes all the key elements needed for the description of a new model. I thus have mainly some minor suggestions of clarification. The only point that would deserve more discussion is the model tuning or calibration. Tuning is mentioned in the appendices and once in the main text for specific components as well as in the author's contribution but without much details or a global view of the way tuning was achieved. I think it would be nice to have a specific section, for example as a section 2.5, to explain the tuning strategy. I guess some parameters have defaults, fixed values that are not supposed to be modified. Some parameters in the various modules were tuned specifically 'offline' for those modules (e.g., lines 281, 820, 852) while some others may have been tuned so that the full model fits with observations. It would be interesting to describe which are the main parameters that were tuned, what were the calibration targets for this general tuning (for example global mean temperature, total amount of precipitation, sea ice extent, temperature trend over the past century, etc.) and the method followed for the tuning.

Minor points

Lines 7-8. I would be more specific in the abstract on the performance of the model, giving some examples where the agreement with observations is good and mention the limitations, such as the poor performance in the tropics.

Line 101. I would suggest to state at this stage that the two dimensions are latitude and longitude. If I follow well, all the vertical variations are then prognostic only. If it is the case, that would be good to mention it explicitly here. It is also probably worth insisting on this point for comparisons with observations including vertical variations (like figures 3, 6 and 8).

Sulphate aerosols are mentioned first line 159 but the spatial distribution is discussed line 166. This may give the feeling that this distribution is only valid for longwave.

Line 171. It would be useful to justify in one or two sentences why an approach even simpler than the one in CLIMBER-2 has been retained.

Line 200. What is exactly meant by 'lack of a Gulf Stream extension propagating to high latitudes'? Does it have an impact on model biases, on the location of deep-water formation or a link with the AMOC simulated by the model?

Line 250. Are the fluxes in the open ocean computed in the same way as in the ice-free fraction of the grid cells?

Line 254. Are the latent and sensible heat fluxes computed in the same way over the ocean? More generally, the computation of the surface balance is well explained here for SISIM in section 2.3 but not explicitly for the oceanic part in section 2.2.

Line 276. This would be useful to add here a few sentences describing the main characteristics of the model without the need to read Willeit and Ganopolski (2016).

Line 378-380. Is the deep water formed in Antarctica above the continental shelf or because of open ocean convection?

Caption of Figure 28. The reference to Paul et al. 2021 is not the same as on the figure (GLOMAP).