Response to reviewer #1

We would like to thank the reviewer for the constructive comments on our paper. In blue below is our response to the reviewer comments and suggestions (in black).

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.

We would like to thank the reviewer for the positive comments on our paper.
Tuning is certainly a fundamental part of any model development and we will follow the suggestion by the reviewer (and reviewer #3) and add a subsection where we give more details about the model tuning strategy.

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.

We will add a sentence about model limitations to the abstract.

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).

In the revised manuscript, we will explicitly state that the 2 dimensions are latitude and longitude and that the vertical variations are *diagnostic* only. We will insist on this point also when discussing the figures on the 3D atmospheric structure.

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

We will modify this. Where sulfates are mentioned for the first time in the paper, we will specify that the spatial distribution of sulfate aerosols has to be prescribed in the model.

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

We did not find any advantages in a separate treatment of stratus and cumulus clouds in the framework of our modelling approach. We will add this sentence to the revised manuscript.

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?

We will rephrase this to: "The frictional-geostrophic dynamics strongly damp momentum and are characterised by weak gyre circulation and associated momentum-driven effects including generally weak ACC and Gulf Stream as a result."

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

Yes, the open ocean fluxes are computed the same way as in the ice-free fraction of the partly ice-covered grid cells. We will make this clearer in the revised manuscript.

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.

The latent and sensible heat fluxes are computed the same way over the ocean, but since the air-sea fluxes are computed in the sea ice model (even in the absence of sea ice), they are described as part of the sea ice model. In this respect, SISIM is not only sea ice model but also atmosphere-ocean interface. We agree that this might be confusing to the reader and will clarify this point in the revised paper.

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).

We will add a few sentences here describing the main features of PALADYN, to give the reader a short overview of the model without having to read the separate PALADYN description paper.

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

Deep water around Antarctica is formed on the continental shelf and not through open ocean convection. We will add this information to the revised manuscript.

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

We will change the corresponding legend entry in the figure from GLOMAP to Paul 2021.