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Geoscientific Model Development Discussions

Interactive comment on "The NUIST Earth System Model (NESM) version 3: Description and preliminary evaluation" *by* Jian Cao et al.

Anonymous Referee #1

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Review of 'The NUIST Earth System Model (NESM) version 3: Description and preliminary evaluation' by Cao et al. submitted to Geosci. Model Dev. Discuss.

This paper introduces a new version of the NUIST coupled climate model and evaluates its basic performance in terms of its mean states and climate sensitivity. Based on these preliminary results, it is generally convincing that this model has the 'fidelity and suitability to address the global climate variability and change issues', targeting for participation of the coming CMIP6 inter-model comparison project. First of all, I think this effort should be applauded particularly for a university with relatively limited personnel and computer resources. I have three major but many minor comments while I think these can be fully addressed.

Major comments: 1) It is valuable to show that this model has reached a quasi-

equilibrium state. However, I think this is somewhat overemphasized in the current form. The authors mentioned that their strategy is to tune parameters in a coupled model framework, while it should be cautious that the coupled model will eventually obtain a near-zero TOA radiation balance after a certain period of integration no matter how big the initial imbalance is. Therefore, the results (Fig. 2-5) are not quite surprising/informative.

Further, the TOA radiation balance (including OLR in Fig. 6) is strongly coupled to the boundary layer SST. For a fair comparison with observation and other models, the AMIP results with prescribed observed SST should be presented as well. The observed precipitation and OLR are also under current climate not from PI.

2) Following comment #1, I think some AMIP simulation results are desired, which can be directly compared to observation and also very helpful to understand the coupled model behaviors.

3) Besides the time evolution of the variables shown in this paper, many other fields are even more important to show so as to have a more complete assessment of the model's performance, such as atmospheric and oceanic circulation, land surface temperature, annual cycle, diurnal cycle and so on.

Minor comments: 4) Line 73-83: Several statements are not well supported from this paper. The authors should show some figures and cite the results in previous publications. Line 77: 2W/m2 is also from PI coupled run? 5) Line 195: 'longitudinal' to 'horizontal'? 6) Line 233-235: Most of current climate models have the initial drift issue that is also relevant to the ocean models. Whether this version of NESM3 has the initial drift problem during the spin up period? 7) Line 254-257: I do not understand how these parameters are tuned in a coupled model to obtain a 'better' (near-zero) net global mean heat flux budget. The TOA imbalance will decrease with time and eventually will be close to zero. See my major comment #1. 8) Line 262-264: statement without figure support. 9) Line 272: what are the default configurations? 10) Line

290: how does the increased deep convective entrainment and convective mass flux induce the reduced zonal wind stress and cold tongue biases? Through convective momentum transport? It should be cautious that entrainment rate is one parameter that affects nearly every aspect of the parameterized convection. Again, the tuning of these parameters should be tested in AMIP simulations before applying to the fully coupled model simulations. 11) L306: What is 'modern'? 12) Line 345-346: I assume that the net fluxes at TOA and surface are downward positive. The difference suggests that the atmosphere loses energy rather than gain energy, right? I think this is likely due to the dynamic core in the atmosphere model. 13) L379: I think this model tends to underestimate the AMOC strength (14.8 Sv). 14) Figure 10, which 10 CMIP5 models? Whether these 10 models are representative of the CMIP5 models (more than 40)? 15) Line 407-417: Comparison of the coupled model to observation is misleading, as the OLR depends on SST (Fig. 7), and the OLR is likely to be very different from its corresponding AMIP simulation. How about the net TOA radiation bias pattern, and the shortwave absorption pattern that largely represents the cloud simulation in this model? Line 431-434: the formation of the double ITCZ is very complicated and the convective parameterization is only one of them. 16) Line 452: fresh water bias? 17) Line 508: is the positive shortwave clear sky feedback due to ice-albedo feedback? 18) Line 512-514: I do not understand how this is consistent with the conclusion derived from the CMIP5 models? 19) Line 539-549: It is better to discuss the TCR in observations that is about 1.3-1.7K (et al., Otto et al. 2013; Richardson et al. 2016). The increased stratiform clouds through the tuning efforts tend to contribute to the overestimated TCR in this model. This deserves some discussion. 20) Line 581: The acronym 'SWT' represents 'Sea Water Temperature'? 21) Line 595: what do you mean 'slightly'? Better to quantify and compare to other CMIP5 models. 22) For OLR, SST, precipitation, please show their global mean value and also the RMSE. 23) Some places require references: Line 397, Line 453, Line 514, Line 522. 24) The version 2 of NESM needs some discussion. Whether there is substantial code difference from v1? If not, I think the current version should be called v2 rather than v3.

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