Geosci. Model Dev. Discuss., 5, C737–C741, 2012
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Geoscientific
Model Development
Discussions

Interactive comment on "The Norwegian Earth System Model, NorESM1-M – Part 1: Description and basic evaluation" *by* M. Bentsen et al.

Anonymous Referee #1

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This paper consists in the overall description and basic evaluation of the new Earth System Model NorESM1-M (medium resolution). This model is based on the CCSM4 model, and has an isopycnic coordinate ocean model and chemistry-aerosol-cloud-radiation interaction schemes that provides further complexity to the atmospheric processes. The paper is well written and is easy to read. I only have some comments and suggestions that would require minor revisions. For this, I recommend this paper for publication in GMD.

Minor comments and suggestions

Page 3, line 30: I do not understand the meaning of "a vehicle for advanced training of Earth System researchers". Trying to keep the same ideas in the sentence, I think that it would be more appropriate to write "an advanced tool for Earth system researchers".

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Page 7, line 14: change "determmined" for "determined".

Page 9, line 20: I suggest to change "for layers to exist that are unstable" for "for layers that are unstable to exist".

Page 9-10, section 2.4: the new TKE model that you've chosen for NorESM seems to simulate a mixed layer depth in high latitudes that is more in agreement with the observations than two previous TKE options available in MICOM. This is well explained in the text but it would be highly interesting to illustrate this improvement on a figure.

Page 10, last paragraph: I suggest putting this last paragraph describing the grid in second position (after the first paragraph) in section 2.4.

Page 12, line 1: change "CAM-Oslo" for "CAM4-Oslo"

Page 13, line 17: change "illustration for the complete" for "illustration of a complete"

Page 18, line 17-18: this sentence is not clear. Revise it in such a way that we clearly understand why you average the same period in the model and in the observations (length of the observation period, type of variable). Give an example to illustrate ("For some analyses (i.e. variable1, variable2 and variable3) [. . .]").

Page 19, second paragraph, with Figures 5 and 6 : for convenience, I suggest to make only one figure with Figures 5 and 6, since the vertical scale is the same and both are surface fluxes. For example, on the left column would be the sensible heat fluxes, and on the right column the latent heat fluxes. As well, the maps of the differences between NorESM and FLUXNET-MTE would be of great help. You say on line 17 that the latent heat fluxes are better represented than the sensible heat fluxes from the distribution point of view. However, you do not show any distribution. Your point could be easily supported with a spatial RMSE score, centered (you've already calculated the mean biases) and normalized by the spatial standard deviation of the observations (score written in the upper corner of the differences maps), or a spatial correlation coefficient. This will give an objective measure of the model-observations agreement.

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Page 19, line 25: there is no dataset labeled "IPCC" at the Climatic Research Unit. Thus remove IPCC in this line, and in the legend of Figure 7. Furthermore, you cite two different papers for the CRU dataset you use, and you use only one, so which one is it? Cite only one paper, and precise the name of the dataset (apparently CRU TS 2.1).

Page 20, line 9-10: although the meaning of this sentence is understandable, say what are the dynamical factors and geographically determined feedbacks. Be more precise.

Page 21, line 12-13: please cite a paper or give objective arguments to support the fact that CCSM4 is according to observations. Moreover, "in better agreement with the observations" would be more appropriate.

Page 24, second and third paragraphs: it would be much more convenient for the reader to have the thickness values cited directly in the text rather than having to find them in the cited papers. For each area that you comment, give the thickness value in NorESM and in the observations.

Page 30, second paragraph: I'm not comfortable with the way you describe your EOF analysis (which is probably relevant at the end). The EOFs (spatial patterns) are obtained by the decomposition in eigenvectors of the covariance matrix; then, the principal components (the time series) are calculated as the projection of the total anomaly field (spatio-temporal) onto the eigenvectors. Here, you describe the contrary. To compare the amplitude of the EOF of two different datasets, I suggest following this protocol: first calculate the EOF/PCs of both datasets then normalize the first principal components PC1 so they have unit variance (divide by their respective standard deviations, refered to as σ_PC1) and multiply the EOF1 by their associated σ_PC1 this way, the EOFs of both datasets show the patterns associated with a unit deviation from the mean in their associated PCs, and can thus be reliably compared. This may be the protocol that you have followed, but I did not understand it in the text. Eventually, although this protocol should be cleaner, I don't think it will dramatically change your

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findings. I thus suggest revising your methodology or just explaining it more clearly.

Page 30, same paragraph, line 12: it is not obvious for me that the amplification of the centers of action in NorESM than in NCEP-2 can be the cause of more variance explained by the first EOF in NorESM than in NCEP-2. This is very likely the case, but saying that implies that the SLP variance fields are the same in NorESM and NCEP-2. And because you do not show this, I thus suggest replacing "As a consequence" with "This likely explains why" (or a similar expression removing the call of a “cause-consequence” phenomenon).

Figure 20: please add the label of the X axis.

Figure 21: the figure is too small, the arrows can barely be seen. Please provide a figure at least twice as large. Add the X and Y axis labels.

Legend of Figure 22: add "in" between "(20-90°N)" and "Historical1".

Page 31, discussion on the results on the AMO: a recent paper (Booth et al., 2012) claims that the AMO is actually a forced variability due to the indirect effect of anthropogenic aerosols during the industrial period. This reference can put some additional value to discuss your results on the AMO.

Page 31, line 17-31: as an element of discussion, Escudier et al. (2012) have thoroughly described a 20-yr cycle at play in the IPSLCM5A coupled model in a recently accepted paper. This could help supporting your discussion.

Page 33, line 23: replace "A" with "a".

Legend of Figure 26: I do not understand the difference between the two panels. They are labeled a and b, but nothing identify them in the legend.

References

Booth, B. B. B., Dunstone, N. J., Halloran, P. R., Andrews, T., & Bellouin, N. (2012). Aerosols implicated as a prime driver of twentieth-century North Atlantic climate vari-

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ability. Nature, 484(7393), 228-32. Nature Publishing Group. doi:10.1038/nature10946

Escudier, R., Mignot, J., & Swingedouw, D. (2012). A 20-year coupled ocean-sea ice-atmosphere variability mode in the North Atlantic in an AOGCM. Climate Dynamics, (2011). doi:10.1007/s00382-012-1402-4

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