

Interactive comment on “Description and basic evaluation of BNU-ESM version 1” by D. Ji et al.

Anonymous Referee #2

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Major Comments:

This study evaluate the coupled model performance of BNU-ESM. The authors described several important aspects of model simulated fields. However, a systematic way to evaluate the model may be necessary. For example, a standard set of metrics and diagnostics for climate model performance evaluation is needed (see comments below). Also, the authors mention the carbon-climate feedbacks. Yet, the evaluations of global carbon cycle or land model performance are not included in the present manuscript.

Another important aspect is the future development plan of the model which is barely mentioned. I suggest the authors can spend one section to address the model development plan on (1) near term focus of model parameterizations improvement, (2) vertical and horizontal resolutions of model components, (3) development or improvement of

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dynamical core of atmospheric or oceanic models. For example, most of the parameterizations in the atmospheric model, such cloud macro-, micro-physics had changed significantly from CAM3.5 to CAM5. Some well know model biases such as clouds have been improved from CAM3.5 to CAM5. How to address this issue in the BNU-ESM is important for these paper. Based on these and comments below, I recommend major revision for the current manuscript.

Specific comments:

1. A figure showing the time series of global net energy budget at TOA and surface are necessary to indicate whether the model is in energy balance or not. Also, another figure of time series of global mean sea surface temperature to indicate the climate drift would be necessary.
2. Standard metrics of several simulated global fields on a Taylor diagram to summarize model performance is recommend as shown in Fig.1 of Gleckler et al. (2008), Journal of Geophysical Research, Atmospheres.
3. A few sentences to describe the reason why only focus Tropical Pacific SST is necessary.
4. A power spectrum of the tropical precipitation is recommended.

Minor Comments:

1. Page 1607, line 3-5: It's not clear which version of the CAM was initially used for the atmospheric model (3.5?). Was it CAM3.5 used and then the convective scheme, chemistry component, and dynamical core were changed from the truck version the CAM. The authors should indicate them clearly.
2. Page 1608, line 19-21: Please provide an explanation why change the visible and near infrared albedos for thick ice and cold snow to small values.
3. Page 1609, line 20: Is one coupler utilized in the ESM for all the component? Or

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difference components are coupled through difference coupling codes?

4. Page 1610, line 21: Is the pre-industrial run of BNU-ESM an atmospheric only simulation? Should indicate this in the text.
5. Page 1611, line 12: "is" is missing in the sentence (Note the there is no land cover change....)
6. Page 1611, line 24: it's worth mentioned that the positive temperature bias consistent with low cloud fraction, precipitation and excessive net shortwave at TOA is documented in Ma et al. (2014), Journal of Climate. The positive temperature is even larger over the central US during northern summer.
7. Figure 1 & 2: include shading to indicate the interannual variability (standard deviation).
8. Figure 3: statistical test (e.g., T-test) is necessary to show the significance of the SST biases. Also, it should be biases rather than differences.
9. Figure 4: same as comment 8. Also, the GPCP also have values over land, why not also show the biases over land?
10. Page 1613, line 2: So, the BNU-ESM model actual produce too much cloud fraction? How about the total water path? A figure is probably not necessary but a sentence or two would be better to describe the performance of simulated cloud liquid and ice over Southern Ocean. This is interesting since most of the climate models produce too few clouds and too much net shortwave radiation at the surface.
11. Page 1613, line 19: references?
12. Figure 5a: include the shading for the standard deviations of the monthly mean SSTs to indicate the interannual variability.
13. Page 1615, line 9: delete "much" from "The much too extensive...".

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14. Page 1615, line 12: Although the reason for the long heat transport may be true, another observations/reanalysis rather than NCEP reanalysis should be used for comparison.
15. Figure 11, the power spectra are too noisy. Some smoothing function for the power spectra to better show the interannual band is necessary. Only three year peak is evident. The 7 year is not obvious in the current plot.

References:

1. Gleckler, P. J., K. E. Taylor, and C. Doutriaux, 2008: Performance metrics for climate models. *J. Geophys. Res.*, 113, D06104, doi:10.1029/2007JD008972.
2. Ma, H.-Y., S. Xie, S. A. Klein, K. D. Williams, J. S. Boyle, S. Bony, H. Douville, S. Fermepin, B. Medeiros, S. Tyteca, M. Watanabe, and D. Williamson, 2014: On the correspondence between mean forecast errors and climate errors in CMIP5 models. *J. Climate*. 27, 1781–1798.

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