Authors' Response to Review of "Description and basic evaluation of BNU-ESM version 1" *by* D. Ji et al.

We thank four reviewers for their constructive comments, which helped us clarify and greatly improve the paper. Comments from the reviewer are in black, and our responses are in blue.

Reviewer #1

General comments:

In this manuscript the authors document the Beijing Normal University Earth System Model and its climate simulation performance. The model consists of components adopted from various modeling centers in the world, with a number of modifications. The simulations of the climate mean and temporal variability from intraseasonal, annual, interannual to decadal scales demonstrate that the model performs reasonably well. The major problems that exist in other models also appear in this model, including double ITCZ, weak MJO and warm SST biases in the eastern part of the oceans. Putting together a comprehensive model, even with existing model components, is a tremendous effort. The BNU-ESM is a participant of the CMIP5 project, and its simulations have been examined in a number of studies as referenced in the manuscript. Thus, it is very useful for the global modeling and climate change communities to have a thoroughly documented reference in the literature. This study is timely for this purpose, and is suitable for publication in GMD. The paper is well organized and well written. I suggest publication with minor revision.

Minor comments:

1. I suggest using the full name of the model in the title, i.e., change "BNU-ESM" to "Beijing Normal University Earth System Model".

Agree. Revised title is "Description and basic evaluation of Beijing Normal University Earth System Model (BNU-ESM) version 1".

2. I suggest adding the climatological mean fields from observations in Figs. 3

and 4. This will give a better sense of the model simulation performance.

Agree. Student t-test was also performed to show the significance of the SST and precipitation biases according to the minor comment 8 and 9 from reviewer#2. Please see Figure 8 and 9 in the revised paper.

3. The simulations of basic fields such as temperature, specific humidity, circulation and clouds are an important metric for GCMs. I suggest that the authors add a sub-section 4.2, which describes the zonal mean T, q, zonal wind from reanalysis and deviations from that of the model simulation (height-latitude cross section), and global distribution of cloud fraction compared with some observational products.

Agree. We added a section to describe the mean atmospheric state and its deviations from reanalysis. Please see Section 5.1 at Page 15 Line 8 – Page 17 Line 20.

4. P. 3, L2. Change "much cooperation" to "collaboration".

Agree. See Page 3 Line 3.

5. P. 4, L3. Add "Zhang, 2002;" after "Zhang and McFarlane, 1995;"

Agree. See Page 4 Line 7.

6. P. 4, L21-22. Add "Data for" before "all" and change “published” to “stored”.

Agree. See Page 5 Line 1.

7. P. 7, L9. Replace the reference “Zhang and McFarlane, 1995” by “Zhang, 2002”. Zhang (2002, JGR) first modified the Zhang-McFarlane scheme.

Agree. See Page 7 Line 18.

8. P. 8, L22. Add “,” after “that is”. L24, change “a little” to “slightly”.

Agree. See Page 9 Line 9.

9. P. 11, L2. Add “is” after “there”.

Agree. See Page 12 Line 18.

10. P. 13, L15. Change “coast” to “coastal”.

Agree. See Page 18 Line 24.

11. P. 13, L19. Add “Oceans” after “Pacific”.

Agree. See Page 19 Line 4.

12. P. 16, L20. Change “averaged” to “average”. L21, delete “anomalously”.

Agree. We also compared the surface wind stress with another reanalysis according to reviewer 2 minor comment 14. See Page 23 Line 10-12.

13. P. 17, L10. Delete “to” after “reach”.

Agree. See Page 24 Line 11.

14. P. 19, L1. Change “demonstrated in the simulation;” to “simulated,”

Agree. See Page 26 Line 2.

15. P19, L14-16. “While. . .40 days.” This is not a complete sentence. One way to change it is to combine it with the preceding sentence: As with BNU-ESM. . .(Kim et al. 2009), while . . .

Agree and thanks. See Page 26 Line 13-17.

16. P 19, L17. Suggest changing the sentence to “. . .climate model to simulate realistic MJO depends not only on its convective parameterization, but also on interactions between...” It’s incorrect to say it does not depend on convective parameterization because it DOES.

Agree and thanks. See Page 26 Line 17-20.

17. P. 21, L10. Add “that” between “with” and “from”.

Agree. See Page 29 Line 5.

18. P 25, L22. A model is not a diagnostic tool. You can change “diagnostic” to “modeling”.

Agree. See Page 37 Line 8.

Reviewer #2

Major Comments:

This study evaluates 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 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.

Agree and thanks for these insightful comments.

(a) Performance evaluation for BNU-ESM with a standard set of metrics and diagnostics is added as Section 4 in the revised paper. Please see Page 13 Line 2 – Page 15 Line 5.

(b) Section 7 is added to describe the model performance on terrestrial land carbon cycle. Please see Page 30 Line 24 – Page 33 Line 24.

(c) The future model development plan is added in the discussion Section 8. Please see Page 37 Line 7 – Page 38 Line 8.

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.

Agree and done. We added the following sentences in the revised paper. See Page 12 Line 9-12 and Figure 1.

> "In terms of energy balance and model stability, the global mean TOA net radiation flux over *piControl* period is 0.88 W/m^2, while the global mean surface net radiation flux is 0.86 W/m^2. The global mean sea surface temperature over *piControl* period is 17.69 ^{o}C with a warming drift of 0.02 ^{o}C per century (Fig. 1)."

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.

Agree and thanks for this constructive suggestion. The model performance summary based on Taylor diagram is included as Section 4. Please see Page 13 Line 2 – Page 15 Line 5 and Figure 2 in the revised paper.

3. A few sentences to describe the reason why only focus Tropical Pacific SST is necessary.

Agree. The following sentence is added. See Page 21 Line 8-10.

> "The tropical Pacific SST is closely associated with the El Niño–Southern Oscillation (ENSO), and exerts a strong influence on the East Asian monsoon (Change et al., 2000; Li et al., 2010)."

4. A power spectrum of the tropical precipitation is recommended.

Agree and Done. See Page 27 Line 3-20 and Figure 16.

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.

Agree. To indicate it clearly, we revised it as following. See Page 7 Line 11-15.

> "The atmospheric component in BNU-ESM is based on Community Atmospheric Model version 3.5 (CAM3.5), which is an interim version of the Community Atmospheric Model version 4 (CAM4) (Neale et al., 2010, 2013). Here, the main difference of the atmospheric component in BNU-ESM relative to the original CAM3.5 model is the process of deep convection."

Also, we revised Page 1607 Line 12-18 as following to state the original schemes being used. See Page 7 Line 22 – Page 8 Line 4.

> "BNU-ESM uses the Eulerian dynamical core in CAM3.5 for transport calculations with a T42 horizontal spectral resolution (approximately 2.81°× 2.81° transform grid), with 26 levels in the vertical of a hybrid sigma-pressure coordinates and model top at 2.917 hPa. Atmospheric chemical processes utilize the tropospheric MOZART (TROP-MOZART) framework in CAM3.5 (Lamarque et al., 2010), which has prognostic greenhouse gases and prescribed aerosols."

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.

Agree. The albedos for sea ice and cold snow were used as tuning parameters during model control simulation. See Page 9 Line 8-11.

3. Page 1609, line 20: Is one coupler utilized in the ESM for all the component? Or difference components are coupled through difference coupling codes?

Only one coupler utilized in the BNU-ESM for all components, it's based on the coupler in CCSM3.5. We made the following revisions to indicate it clearly. See Page 10 Line 12-17.

> "The coupling framework of BNU-ESM is largely based on the coupler in NCAR CCSM3.5 (an interim version of NCAR CCSM4), with changes on grid mapping interpolation to allow for the identical tripolar grids used in both ocean and sea ice components. The time evolution of the whole model and communication between various component models are all synchronized

and controlled by the coupler in the BNU-ESM."

4. Page 1610, line 21: Is the pre-industrial run of BNU-ESM an atmospheric only simulation? Should indicate this in the text.

The pre-industrial run of BNU-ESM for providing physical quantities to off-line carbon cycle integrations was done with the whole coupled model but turning carbon cycles off. We made the following revision to indicate it clearly. See Page 11 Line 19-22.

"In these off-line integrations of the first step spin-up, surface physical quantities such as winds, temperature, precipitation, moisture, and radiation flux are taken as the climatology of a pre-industrial run of the fully-coupled BNU-ESM with carbon cycles turned off."

5. Page 1611, line 12: "is" is missing in the sentence (Note the there is no land cover change....)

Agree and thanks. See Page 12 Line 18.

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.

Agree and thanks for this suggestion. See Page 19 Line 12-17.

7. Figure 1 & 2: include shading to indicate the interannual variability (standard deviation).

Agree and done. Please see Figure 6 and 7 in revised paper.

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.

Agree and done. The climatological mean field from observations and biases of surface air temperature over continents are also added according to review#1 minor comment 2. Please see Figure 8 in revised paper.

9. Figure 4: same as comment 8. Also, the GPCP also have values over land, why not also show the biases over land?

Agree and done. Please see Figure 9 in revised paper.

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 two few clouds and too much net shortwave radiation at the surface.

Sorry we were wrong on this conclusion. BNU-ESM model actually produces less cloud fraction. In South Atlantic and South Indian Oceans, the shortwave cloud radiation effect even has a small positive bias in the cold band between 40^{o}S and 50^{o}S. We deleted this sentence (Page 1613, line2) from the revised paper. In the revised paper, Figure 4a on total cloud fraction bias and Figure 5b on shortwave cloud radiation forcing bias can help explain it.

11. Page 1613, line 19: references?

The following references mentioned it. But in the revised Figure 8 the dipole bias is not significant, we deleted this line from the revised paper.

Liu, L., Yu, W., Li, T.: Dynamic and Thermodynamic Air–Sea Coupling Associated with the Indian Ocean Dipole Diagnosed from 23 WCRP CMIP3 Models, J. Climate, 24, 4941–4958. doi: http://dx.doi.org/10.1175/2011JCLI4041.1, 2011.

Cai, W., and Cowan, T.: Why is the amplitude of the Indian Ocean Dipole overly large in CMIP3 and CMIP5 climate models?, Geophys. Res. Lett., 40, 1200–1205, doi:10.1002/grl.50208, 2013.

12. Figure 5a: include the shading for the standard deviations of the monthly mean SSTs to indicate the interannual variability.

Agree and done. See Figure 11 in revised paper.

13. Page 1615, line 9: delete "much" from "The much too extensive...".

Agree and thanks. See Page 23 Line 7-8.

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.

Agree. We also compared the surface wind stress with ERA-Interim reanalysis and revised to the following. See Page 23 Line 10-12.

> "One notable bias is that the annual average zonal wind stress from about 35°S to 55°S latitudes over ocean is 23.2% stronger compared with ERA-Interim reanalysis and 42.8% stronger compared with NCEP reanalysis…"

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.

Agree and done. To clearly indicate the 3-7 years range from observations, we also added two vertical dashed lines at 3-yr and 7-yr and one horizontal line in the figure. See Figure 18 in the revised paper.

Reviewer #3

General comments:

This study documents BNU-ESM's setups and performance. As the authors mentioned, BNU-ESM has participated in CMIP5 and its results have been analyzed in many studies. A thorough documentation like this study would be beneficial and relevant to the climate science community and GMD's readers. I recommend its publication after some revisions.

Along the line of publishing a through documentation for the scientists that analyze CMIP5 data and the model developers in other centers, I have a few suggestions that hopefully would further improve the manuscripts:

Specific comments:

1. Before evaluating BNU-ESM's internal variability, a systematic analysis of mean state would be helpful. In the current manuscript, only surface temperature and precipitation over the ocean are shown. In order for the readers to compare BNU-ESM's performance with those in other models, more fields are required. A good reference for thorough evaluation would be Chapter 9 in the IPCC report (Flato et al. 2013), which includes annual mean surface air temperature, precipitation (over land and ocean), shortwave and longwave cloud radiative forcing, and the seasonality of surface air temperature. Trenberth and Fasullo (2010) also show some great figures that demonstrate models biases in terms of annual mean and seasonality.

Agree and thanks for this suggestion. We response these comments in three sections:

(a) On mean state, we've done further analysis on zonal mean temperature, specific humidity, zonal wind from BNU-ESM and deviations from that of the ERA-Interim reanalysis, and global distribution of cloud fraction compared to observational ISCCP D2 products. Please see Section 5.1 at Page 15 Line 8 – Page 17 Line 20. We've also added a systematic analysis of model performance with Taylor diagrams for selected 24 fields by comparing to ERA-Interim and JRA-55 reanalysis products and various observations. Please see Section 4 at Page 13 Line 2 – Page 15 Line 5 and Figure 2.

(b) Figures on annual mean surface air temperature, precipitation and their biases have been improved to include biases over land, statistical significance tests and climatological annual mean of observations according to referee#1 minor comment 2, referee#2 minor comment 8 and 9. See Figure 8 and 9 in the revised paper.

(c) On shortwave and longwave cloud radiative forcing (SWCF and LWCF), we added a paragraph in Section 5.1 on atmospheric mean state. See Page 17, Line 8-20 and Figure 5.

2. Following the previous comment, comparing with figures in Flato et al. 2013, there are some biases in BNU-ESM that are commonly shared in many other CMIP5 models, whereas some biases seems to be unique in BNU-ESM. The authors have identified some of these in the text; however, it would be worth elaborating more. A few features that catch my eyes:

(1) Most models have SST over Southern Ocean being higher than those in observations. However, BNU-ESM has cold biases in the region. The authors have mentioned two possible reasons: ACC strength and clouds. It would be helpful to show shortwave cloud radiative forcing biases. A band of excessive precipitation over Southern Ocean (and east of South America) seems to be related with this cold bias, whereas other models have deficient precipitation in the region.

Agree and thanks. BNU-ESM model actually produces less cloud fraction over Southern Ocean (see Figure 4a in the revised paper). We were wrong on concluding clouds are one possible reason for cold SST biases, and deleted the relevant sentence (Page 1613, line2) in the revised paper. Figure 5b (in the revised paper) indicates the shortwave cloud radiation effect even has a small positive bias in the cold band between 40°S and 50°S and is consistent with less total cloud fraction here. We addressed this issue in the revised paper as following. Please see Page 19 Line 6-11.

"In South Atlantic and South Indian Oceans, a tendency for negative SST biases along the northern flank of the Antarctic Circumpolar Current (ACC) are mostly due to insufficient southward transport of heat out of the tropics and a positioning error of the ACC caused by equatorward shift of the westerlies; although there is a small positive bias of the shortwave cloud

radiation effect at the cold band between 40°S and 50°S (Fig. 5b)."

The band of excessive precipitation over the Southern Ocean between the southernmost of Southern Africa (about at 35°S, 30°E) to southwest of Australian is more consistent with the spatial pattern of warm SST biases and is along the northern flank of a cold SST bias, which probably produces more convective precipitation. We stated it more clearly in the revised paper. See Page 20, Line 13-17.

(2) There are a few different aspects of the double ITCZ problem, and the current manuscript doesn't articulate this clearly. Some models simulate too much precipitation off equator (in both NH and SH) and too little precipitation at the EQ, but BNU-ESM only shows significant excessive precipitation at around 5N. The SPCZ being to equaterward and too horizontal is another aspect of the double ITCZ problem, which appears in BNU-ESM and many other models. It would be helpful to articulate these similarities and differences comparing with other models, as the descriptions for AMOC in line 1~5 on page 1616. (A few references for the double ITCZ problem: Li and Xie 2014, Hwang and Frierson 2013, Lin 2007)

Agree and thanks for this suggestion. The description of the double ITCZ problem has been rewritten as following. See Page 19 Line 23 – Page 20 Line 11.

"In common with many climate models (e.g. Li and Xie, 2014, Lin, 2007), we note a bias in precipitation, characterized by a double Intertropical Convergence Zone (ITCZ) structure over much of the Tropics. This produces excess precipitation over the Northern Hemisphere's ITCZ, Southern Hemisphere's South Pacific convergence zone (SPCZ), the Maritime Continent and the tropical Indian Ocean, together with insufficient precipitation over the equatorial Pacific. BNU-ESM displays the characteristic pattern of the double ITCZ problem with too much precipitation in the central Pacific near 5°S and too little precipitation in the west and central Pacific between 15°S and 30°S which is similar to CCSM4 (Gent et al., 2011). BNU-ESM underestimates precipitation at 5°N latitude but overestimates it along the 5°S parallel in the tropical Atlantic. Compared with observations, the BNU-ESM develops too weak a latitudinal asymmetry in tropical precipitation and SST over

the eastern Pacific and Atlantic Oceans."

3. If the authors see fit, some comparisons (in terms of mean state) with CAM that have similar schemes as in BNU-ESM would be interesting for readers, as those in line 25 p. 1617. For example, how do changes in convection schemes affect clouds, precipitation, or SST?

Thanks for this suggestion. A comprehensive comparison between the atmospheric component of BNU-ESM and CAM is beyond the scope of this paper, although the main difference is in convection schemes. We prefer to summarize this study in another future manuscript with dedicated experiments. In the revised paper, we added a paragraph on intensity distribution of precipitation from BNU-ESM *historical* simulation. Please see Page 20 Line 20 – Page 21 Line 5 and Figure 10.

4. Again, if the authors see fit, an analysis of monsoon would be very relevant. Base on Figure 1 & 2, the model seems to simulate monsoon pretty well. A monsoon index diagnostic (as in Flato et al. 2013, which follows kim et al. 2011) together with a 2-D map of temperature seasonality might be relevant to the paper, but this is a whole new set of analysis and I leave decision of including it or not to the authors.

Thanks for this suggestion. We prefer to summarize this work in future.

5. p.1611, line 13, There "is" no land cover change ....

Agree and done. See Page 12 Line 18.

6. Figure 3 & 4, it might be worth showing values over land for readers that are interested.

Agree and done. See Figure 8 and 9 in the revised paper.

7. In the sea ice section, similar to the major suggestion 2.2 above, it would be worth comparing Figure 6 with Figure 9.22 and 9.23 in Flato et al. 2013, especially that BNU- ESM has an ice scheme that's slightly different from CAM4.

Agree and thanks. We added a paragraph to describe the seasonal cycle of sea ice

extent. See Page 23 Line 15-23.

Reviewer #4

General comments:

This paper describes the configuration of Beijing Normal University Earth System Model. Authors also evaluate the performance to simulate the mean climate and climate variability using CMIP5 simulations of BNU-ESM. I think that the description and results from new earth system model are sufficiently interesting to merit publication. However, there are a number of issues that require attention as described below. Addressing these issues could make the paper more publishable. So I have recommended that this manuscript could be accepted after minor revision.

Minor comments:

(1) Although there is a model description, some explanation to enlighten about the basic philosophy and logic to choose components of BNU-ESM will be helpful to understand the goal of development of BNU-ESM (or main goal of this new development). I wonder why only some components are chosen differently from CCSM4.0 (or CESM). Please remark how to keep up this model under circumstances of constant upgrades of original modules (e.g. CAM, MOM, CICE).

Thanks for this suggestion. The development of BNU-ESM was prompted by foundation of a new multidisciplinary research center committed to study global change and earth system science in Beijing Normal University. The components of BNU-ESM were chosen based on the specific expertise and experience available to the research center, and furthermore with an eye to how the research strengths of the center can improve and develop it. We indicated this point in the revised paper. Please see Page 3 Line 6-12. We also discussed future model developments in response to referee#2's major comment. Please see Page 37 Line 10 – Page 38 Line 8.

(2) To add a plot showing zonal mean OLR at TOA is recommended to show the global net energy balance. To add basic fields including vertical structure of zonal mean temperature, zonal wind, and specific humidity, cloud water/ice content is recommended.

Agree and thanks for this suggestion.

a) The global net energy balance was shown with TOA net radiation. Please see Page 12 Line 9-12 and Figure 1 on energy balance and model stability in the revised paper.

b) Evaluation on basic fields including vertical structure of zonal mean temperature, zonal wind, and specific humidity, cloud water/ice content was added as Section 5.1 on describing atmospheric mean states. Please see Page 15 Line 8 – Page 17 Line 20 and Figure 3, 4 and 5 in the revised paper.

(3) Since this model simulate stronger interannual variability to the observed, to add a plot to show the amplitude of response of circulation fields to the interannual variability of SST anomalies is recommended. (e.g time series of SOI, regressed field of circulations by NINO3.4 or time series of leading EOF mode of SST).

Agree and done. We added time series of SOI to show the amplitude of response of circulation fields to the interannual variability of SST anomalies. See Page 29 Line 14-23 and Figure 20 in the revised paper.