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Geoscientific Model Development Discussions

Interactive comment on "Overview of climate change in the BESM-OA2.5 climate model" *by* Vinicius Buscioli Capistrano et al.

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First of all, we would like to thanks the extraordinary review. It is evident the importance of your suggestions, which is associated with the quality and relevance of all information for GDM reader. The original manuscript was planned to intercompare BESM climate model with CMIP5 ensemble, documenting the well-known physical responses to increased CO2. Therefore, many analysis (tables and figures) were proposed with this view, having side-by-side BESM and CMIP5. We agree with the main issue pointed out by both reviewers, that the GMD reader would not be interested if BESM has climate sensitivity within ensemble dispersion. Thinking in this way, we rewrite parts of the manuscript (following the reviewers' suggestions) where comparisons BESM vs. CMIP5 were mentioned, bringing more discussion about BESM response. Moreover,

new figures focusing on BESM results was added, however the original figures and tables remained without change.

1. **The title is too vague. The paper is not about BESM-simulated climate change in general – that would imply showing results from historical or projection simulations. The paper is in fact about BESM simulated climate sensitivity and feedbacks.**
   Reply: According to the suggestion of the anonymous Reviewer 2, the article title was changed to "Assessing the performance of climate change simulation results from BESM-OA2.5 in comparison to a CMIP5 model ensemble".

2. **A re-organisation of Section 2 Model Description. At the moment, it has only one subsection, which is a mixture of model description and comparison to the previous version. This should be split cleanly into two subsections focused on each aspect. The model description should be more complete.**
   Reply: The Section 2 was split in two parts as requested. Moreover, the model configuration was more detailed. Please, see page 3 lines 3.

3. **The paper spends too long discussing CMIP5 models when it really should be discussing BESM. Three changes would fix the balance**

   (a) **First, Section 3.2 needs to be shortened because it is essentially a re-telling of Andrews et al. (2012) and Vial et al. (2013). In the context of the paper the reader is only interested in the physical meaning of the different variables estimated by the Gregory and kernel methods.**
   Reply: As far as we have two different methods, we decided explicit all calculations. It worth noting that the first technique is the same as Andrews

et al. (2012), however the other does not share the same methods with Vial et al. (2013). The radiative kernel method applied here is similar to its origin paper (Soden et al., 2008), whereas Vial et al. (2013) separated the feedback and the rapid adjustment using different protocols run (see next question).

(b) **Second, the results presented in Sections 4.1 and 4.2 need to be compared to the original papers: are the results replicated? How many models have been added/removed compared to the original papers?**
Reply: As mentioned by the reviewer, the climate sensitivities of 26 CMIP5 coupled models (including BESM-OA2.5) were assessed using the Gregory et al. (2004) linear regression between net radiation in TOA and surface temperature changes, as well as it was performed by Andrews et al. (2012) for 15 CMIP5 coupled models. In the present work, we included the following models: ACCESS1-0, ACCESS1-3, bcc-csm1-1, BESM-OA2.5, BNU-ESM, CCSM4, FGOALS-g2, FGOALS-s2, GISS-E2-H, GISS-E2-R, e inmcm4. For the 15 same models, we found similar results with respect to Andrews et al. (2012). Such small difference may we can attribute grid interpolation as explained in lines 3 of page 8. In order to partitioned the feedback agents we used the radiative kernel described in Soden and Held (2006) and Soden et al (2008) and Shell et al (2008). In turn, Vial et al. (2013) adapted this previous methodology to consider the tropospheric adjustment to CO2 (comparison between abrupt4xCO2 and sstClim4xCO2, instead of abrupt4xCO2 and piControl).

(c) **Third, a lot of the analysis in Section 4.3 is about CMIP5 models in general (page 10 especially), and that has been said already in other papers so could just be repeated briefly. Instead, the space could be used to deepen the analysis of the BESM simulations.**
Reply: New information was added to include what was requested.

4. **The authors frequently compare BESM to the CMIP5 average, or say that it is within the CMIP5 range (which is often large), or note where BESM is an outlier. But such statements are only mildly useful. After all, it may not be a good thing to be close to the CMIP5 average. Instead, readers need evidence for a deep understanding of why BESM behaves like it does.**

   (a) **Why is there a radiative imbalance of 2 W m-2? That is a large value. Does that cause a model drift? Does the model conserve energy?**
   Reply: The AGCM stand-alone run shows a net radiation at TOA of 0.25 W m-2 during 20 years of simulation (Fig. 1a). Such radiative imbalance is within the range simulated by different atmospheric models. However, in the coupled simulation, the net radiation imbalance at TOA is amplified up to -4 W m-2 (Fig. 1b). The reason for such imbalance is related to higher loss of energy at TOA both from the outgoing long-wave radiation (OLR) and outgoing short-wave radiation (OSR), compared with AGCM stand-alone simulation (Fig. 1c and 1d). In Fig. 1c and 1d, the solid lines represent the coupled model and the dashed lines represent the AGC. The higher loss of energy through the outgoing short-wave radiation is potentially duo to enhanced cloud formation in the coupled model run.

   (b) **Then, why is the 2xCO2 radiative forcing at the higher end of the range? Is it an issue for the radiative transfer code?**
   Reply: BESM-OA2.5 was integrated with UKMET radiative code for SW and LW in order to compare the imbalance of the first year, which is a proxy to the Instantaneous Radiative Forcing.

   (c) **... why is BESM an outlier in terms of cloud feedbacks? The reader is told that the answer lies in the high latitudes, but what are the mechanisms? Change in low-cloud cover? Change in phase from ice to liquid?**
   Reply: It is evident from figures presented in the manuscript, that BESM is

an outlier for the cloud feedbacks. This is due to a strong shortwave component response over both the Arctic and the Southern Ocean near Antarctica. Considering the SW CRE/ðİŻěTas [described by Cess et al. (1989)] and the individual components of feedbacks cloud mask, we can note that those higher values cloud feedback are mainly consequences of the sum of SW CRE/ΔT_{as} and the cloud masking for albedo feedback [-(ðİIJĘa-ðİIJĘac)], as shown in Figure 2. For Arctic region, the major contributor for BESM be an outlier is the SW CRE, while for over the ocean near the Antarctic is the albedo feedback cloud mask. In this latter, since the radiative kernel for both all- and clear-sky are the same throughout the models, the difference among them is due to the albedo change [$\Delta a/\Delta T(K_a - K_a^{cs})$]. Over the both regions (Arctic and near Antarctic), an increase in cloud fraction above 850 hPa and a decrease below that level for BESM is observed, which means a low-level clouds upward shifting . Moreover, the increase in cloud cover above 850 hP is stronger than the reduction below (Figure 3a). As consequence, a negative SW CRE change is present in those regions, that is that response to the increase in sun shading (Figure 3b). However, the SW cooling is smaller than the heating provided by LW radiation, as presented in the net effect (Figure 3d). The net radiation heating change is more intense around 60oS, that can be related to the more intense surface albedo change. We could not investigate the change in phase from ice to liquid because we did not designed the experiments to have the liquid and ice water content in their outputs. We pretend develop a new analysis about it in a next work.

(d) **Finally, regarding the "warming hole" in the North Atlantic, does BESM simulate it for the reasons listed by Drijfhout et al. (2012)?**
Reply: A new work about the "warming hole" is in preparation by Nobre et al (2019), which will have more information about BESM transient responses to radiative forcing in that region.

5. Other comments:

- **Page 2 line 3: The main result of the "trapping" of infrared radiation is an increase in ocean heating content, since this is the Earth system component with the largest heat capacity.**
Reply: Done (p.2 l.2)
- **Page 2 line 20: The wet-gets-wetter etc. is probably too simple and more subtle descriptions are now preferred, see for example Marvel and Bonfils, doi:10.1073/pnas.1314382110 (2013).**
Reply: Done (p.2 l.20)
- **Page 3, line 10: Is the model hydrostatic or not?**
Reply: It is hydrostatic. This is information was add in the Model description section (p. 3 l. 15)
- **Page 3, line 21: What microphysical processes? Clouds?**
Reply: It is about the microphysical parameterization of precipitation.
- **Page 3, line 24: The 2m subscript is confusing. Are the authors talking of diagnostic or prognostic variables here?**
Reply: Those variables are diagnostic for the atmospheric model, however it is important in the ocean-atmosphere coupling (p. 4 l.15).
- **Page 4, section 3.1: It would be useful to refer to the CMIP6 DECK here (Eyring etal. doi:10.5194/gmd-9-1937-2016, 2016) since piControl and abrupt4xCO2 are both mandatory simulations within the DECK. Referring to CMIP6 would make the paper more current.**
Reply: Done (p. 5 l. 7).
- **Page 4, lines 26–31: Need to move the statements on page 5 lines 27–28 and page 6, lines 15–16 here to list the advantages and limitations of both methods in one place.**

Reply: As far as we decided maintain a separated description of those methods (as discussed previously), we also let the limitation and advantages in different sections.

- **Page 5, line 27: Would be useful to refer to Soden et al. doi:10.1126/science.aau1864 (2018) here.**
  Reply: Done (p.6, l. 16)
- **Page 7, lines 24-25: That statement needs to be clarified and referenced. Perhaps Zelinka et al doi:10.1175/JCLI-D-12-00555.1, 2013?**
  Reply: New information based on the Methods section was provided.
- **Caption of Figure 2: Please make figure captions standalone by defining all acronyms and variables.**
  Reply: Done.
- **Figure 4: It would be helpful to put a dashed line at lambda = 0 on each panel, tomake easier to see where the feedback parameters switch sign.**
  Reply: Done.

Complete Figure Captions

Figure 1 – Net of the radiation of TOA simulated by (a) stand-alone AGCM for 20 years and (b) BESM-OA2.5 Historical for the first 20 years (1850-1870). (c) and (d) are outgoing long-wave radiation and outgoing short-wave radiation, respectively. In (c) and (d) the solid lines represent the coupled model and the dashed lines represent the AGCM. Units are in W m^{-2}.

Figure 2. SW Cloud feedback and the albedo and SW humidity feedbacks cloud masking for the CMIP5 multi-model ensemble-mean (solid line) and BESM-OA2.5 (solid line with dots). Inter-model standard deviations for each latitude are in yellow. In blue

are the feedback limits based on the maximum and minimum values for each latitude among the models, not including BESM-OA2.5.

Figura 3. Vertical profiles of the zonal mean of the 4xCO2 - piControl mean difference for the following variables: (a) Cloud fraction, Radiative heating-cooling rate (dT/dt) of (b) shortwave, (c) longwave and (d) sum of shortwave and longwave.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-209/gmd-2018-209-AC1-supplement.pdf

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-209, 2018.

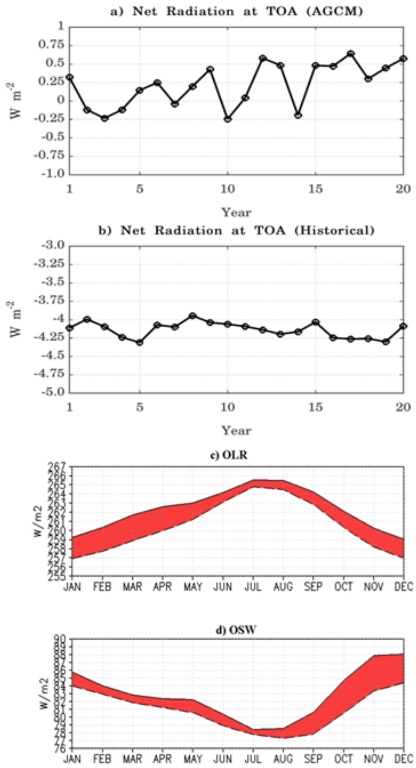
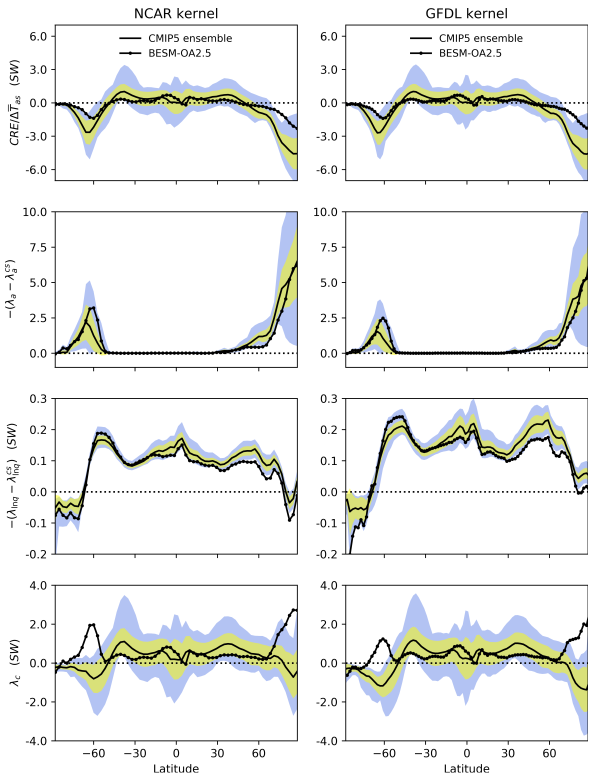


Fig. 1. Net of the radiation at the TOA by (a) AGCM and (b) BESM-OA2.5 Historical. (c) and (d) outgoing LW and outgoing SW radiation, respec...



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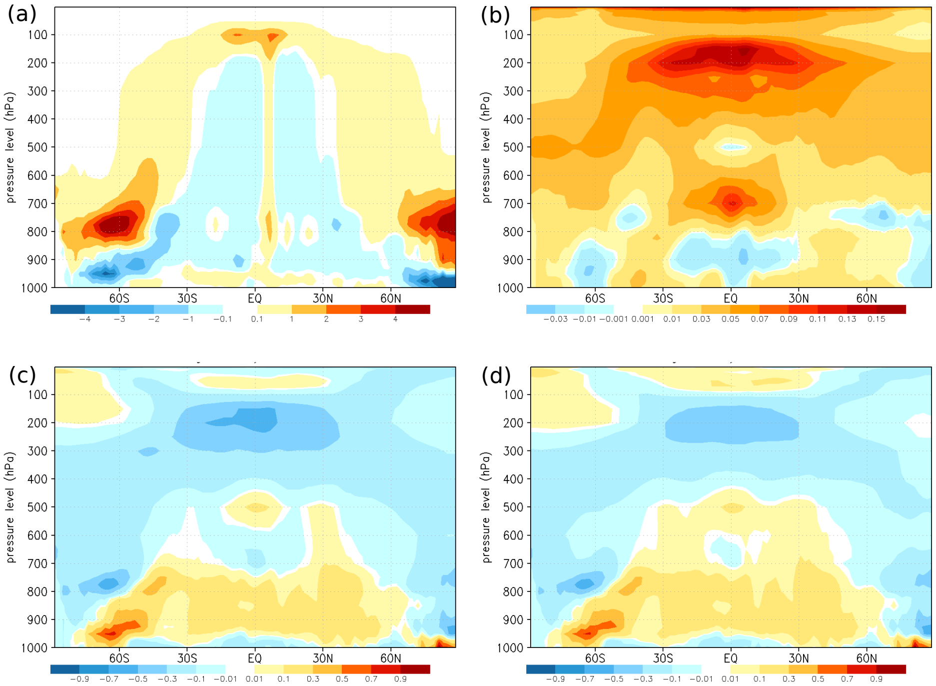

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