

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

Anonymous Referee #1

Received and published: 4 January 2019

In this study, the authors document the climate sensitivity and feedbacks of the Brazilian Earth System Model, ocean-atmosphere coupled version 2.5 (BESM-OA2.5) and compare those characteristics to the CMIP5 ensemble.

There are really two papers co-existing in this study: one focuses on BESM, the other on the CMIP5 ensemble. The first paper appears underdeveloped and is fairly diagnostic, so needs to be deepened. The second paper is essentially an incremental extension of the Andrews et al. (2012) and Vial et al. (2013) studies. The results are interesting but seem out of scope in a GMD paper that really ought to focus on BESM. For these reasons, I recommend major revisions.

The main changes I would like to see are:

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- 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.
- 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 (i.e. in addition to the aspects listed in Table 1, it should briefly refer to the other elements of the model: Boundary layer, aerosols, convection, dynamical core, gravity waves, large-scale clouds and precipitation)
- The paper spends too long discussing CMIP5 models when it really should be discussing BESM. Three changes would fix the balance. 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. 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? 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, as indicated in my next point.
- 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

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deep understanding of why BESM behaves like it does. 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? Then, why is the $2\times\text{CO}_2$ radiative forcing at the higher end of the range? Is it an issue for the radiative transfer code? Then, why is BESM an outlier in terms of cloud feedbacks? The reader is told that the answer lies in the high latitudes (Page 8 line 35 – Page 9 line 1), but what are the mechanisms? Change in low-cloud cover? Change in phase from ice to liquid? Finally, regarding the “warming hole” in the North Atlantic, does BESM simulate it for the reasons listed by Drijfhout et al. (2012)? This is not an exhaustive list: I may have missed other responses that need discussing more deeply.

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.
- 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).
- Page 3, line 10: Is the model hydrostatic or not?
- Page 3, line 21: What microphysical processes? Clouds?
- Page 3, line 24: The 2m subscript is confusing. Are the authors talking of diagnostic or prognostic variables here?
- Page 4, section 3.1: It would be useful to refer to the CMIP6 DECK here (Eyring et al. 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.

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- 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.
- Page 5, line 27: Would be useful to refer to Soden et al. doi:10.1126/science.aau1864 (2018) here.
- 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?
- Caption of Figure 2: Please make figure captions standalone by defining all acronyms and variables.
- Figure 4: It would be helpful to put a dashed line at $\lambda = 0$ on each panel, to make easier to see where the feedback parameters switch sign.

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

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