Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-209-AC6, 2020

Geoscientific Model Development Discussions

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

Vinicius Buscioli Capistrano et al.

vcapistrano@uea.edu.br

Received and published: 6 January 2020

Reply to the Reviewer 2

Review of "Assessing the performance of climate change simulation results from BESM-OA2.5 in comparison to a CMIP5 model ensemble" by V.B. Capristrano et al. (revised manuscript)

Overall assessment and recommendation

Printer-friendly version

Discussion paper

I regret to conclude that this paper has not been sufficiently improved by the revision process to be acceptable. While I appreciate that the authors have tried to bring the characteristics of BESM-OA2.5 more in focus of their presentation (rather than discussing the general performance of CMIP5 models), the results is still a clumsy and partly dis-organized concatenation of results and result comparisons that do not lead to a clear assessment of the suitability of BESM for specific purposes. New text often has been insufficiently harmonized with the previous text, making reading through the manuscript still an extremely arduous task.

As I stated in my original review, my impression is that BESM is a reasonable model that could be useful for specific applications at least. Hence, I am reluctant to reject this paper once and for all. The authors should be allowed to make one more attempt to create a straightforward paper with a coherent message. To this end (as I have proposed before) the focus of future use of BESM should be made clear, considering the merits and shortcomings of this model. The authors should intensify their attempts to interlink the evidence arising from individual parameter evaluation. This already has been tried in a number of cases, but it too often results in circular reasoning, not approaching the roots of characteristic BESM features. Finally, I emphasize that just executing through my list of technical and language suggestions alone will not do! The author team apparently does not include an English native speaker, hence assistance in producing a proper English text ought to be given by either the editorial office or from some other consultant. Otherwise, I fear that I will be reluctant to read through this paper once again.

Reply: We thank all the reviewer suggestions. The manuscript was rewrite to make each the paragraph message clearer. Furthermore, as requested, an English proof-reading was hired (editing certificate is attached here).

General remarks

1) Section 2.1 still contains elements of a comparison between BESM-OA2.5 and BESM-OA2.3 (e.g., p. 3, l.32) though a dedicated section (2.2) is supposed to cover such differences.

Reply: This comparison was removed and a new discussion was added in the section 2.2.

2) It is on occasions still hard to reproduce what has actually been done and why (e.g., p. 6, l.25).

Reply: Please see the section about Language and Technical Remarks below.

3) No reason is given on p. 8, 2nd paragraph, why only 11 rather than 15 CMIP5 models are included here. Or are sometimes 11, sometime 15, models used, as could by read out of p. 8, l. 6?

Reply: Andrews et al. (2012) used 15 CMIP5 models and we used 26, which means that we added 11 models. We reorganized the paragraph.

4) Occasionally, I still miss a comment on the specific performance of BESM, even if it's well consistent with the CMIP5 ensemble (e.g. Figure 3).

Reply: The requested information were added.

5) Page 9, 1st paragraph: This has been reformulated, but is now even more confusing than before. Please reconsider, what is the intended message here, with focus on BESM. Then stick to specific reasoning to underpin that message.

Reply: The paragraph was rewrite to make the main message clear.

6) Page 9, 2nd paragraph: Here, too, the line of reasoning remains badly organized: What is the message: Does BESM simulate a stronger Arctic amplification than the CMIP ensemble (suggested by the more negative Planck feedback)? This could simply explain more snow/ice melting. Evidently the lapse rate feedback in BESM is exceptionally positive at Arctic latitudes, pointing at a enhanced vertical gradient in the temperature response. Can this be discussed in the context of the Veiga et al. paper (atmospheric temperature response)?

Reply: The paragraph was rewrite to make the main message clear.

7) The last paragraph of section 4.2, with much newly introduced text, is very hard to understand both concerning the weak use of English language and a confusing inherent logic. I have read through this paragraph three times, but then gave up, being unable to reconcile the statements in the text with what the

Printer-friendly version

Discussion paper

figures display.

Reply: The paragraph was rewrite to make the main message clear.

8) Scatter Diagrams in Figures 8 and 9: Do you conclude anything from the apparent correlation between precipitation in piControl and abrupt4xCO2 on one side, and missing correlation for respective surface temperature levels on the other side? Does this have implications for the BESM model performance.

Reply: BESM results were included and linked previews discussions.

9) In the last paragraph of the conclusions an outlook to what is planned with BESM-OA2.5 (future research focus) is still lacking. However, this would be the logical outcome of the assessment of its merits and shortcomings, which I assume is what the present paper has been written for.

Language and Technical Remarks

p. 1, l. 7 (Abstract): " ... the CMIP5 ensemble mean value .. "

Reply: The climate feedback responses were estimated for 25 CMIP5 models individually and for BESM, no just for the CMIP5 ensemble. This strategy as adopted in order to visualize where BESM in comparison to a distribution of climate response.

Printer-friendly version

Discussion paper

p. 1, l. 8 (Abstract): " ... BESM simulation show zonally average feedbacks, estimated from radiative kernels, that lie within the ensemble standard deviation ..."

Reply: Done.

p. 1, l. 11 (Abstract): "... BESM also features a strong positive ..."

Reply: Done.

p. 1, l. 12 (Abstract): As this sentence mentions a merit of BESM, while the preceding sentence comments on a disagreement with CMIP, "moreover" makes quite an unlucky connection. By the way, "consistent" with what?

Reply: "Moreover" was changed to "However". The BESM results are consistent with the CMIP5 ensemble mean. Changes were done to clarify this point.

p. 2, l. 7: "... results in a temperature rise ..."

Reply: Done.

p. 3, l. 7: "... models, also discussing peculiarities in the BESM climate response."

Printer-friendly version

Discussion paper

Reply: Done.

p. 3, l. 16: ”... same as used by Veiga ...”

Reply: Done.

p. 3, l. 17: “... model, with its dynamical core being based on ...”

Reply: Done.

p. 3, l. 22: “... of physical parameterizations between BAM (as used in this paper) and BAM NWP ...”

Reply: Done.

p. 3, l. 24: “... 28 layers, unevenly spaced, in the ...”

Reply: Done.

p. 3, l. 29: “... is able to capture ...”

Reply: Done.

p. 3, l. 30: "... with a double ITCZ ..."

Reply: Done.

p. 3, l. 31: "improvement", despite of the "substantial biases" addressed in the preceding sentence?

Reply: This sentence was adapted to "Comparison to previous version" section as requested in the general considerations.

p. 3, l. 33: "... decadal climate variability patterns." This is meant, isn't it?

Reply: Yes. It is correct now.

p. 4, l. 4: I understand that AMOC is a circulation structure rather than a parameter. So, what "value" a you referring to? If required, please give an absolute or relative difference of the parameter you have in mind.

Reply: The AMOC strength simulated by the model in the piControl is around 14 Sv (for 1000 years). The AMOC strength observed by the RAPID project is roughly 17 Sv (McCarthy et al. 2015).

(see the supplementary material) Fig1. - Maximum AMOC simulated by the piControl from the beginning of the simulation up to 1000 years. The red dash-dot lines shows the linear trend.

p. 4, l. 9: "... are determined, which are important ...". Anyway, the content of this sentence to me resembles what is given below (p. 4, l. 14), with the sentences in between (starting with "The total energy balance ...") causing an awkward logical break.

Reply: In order to avoid this apparent logical break, the sentence "which are important in the coupling between atmosphere and ocean" was removed. The section emphasizes the main differences between BESM versions, that are in the atmospheric model parametrization, specially in the way the diagnostic surface layer variables are calculated. The general differences in the atmospheric model are discussed first, and just after this is introduced more details about surface layer variables (where was found a repetition about the importance of these variables for the ocean-atmosphere coupling).

p. 4, l 19: This sentence again repeats what is given in p. 4, l. 9 ...

Reply: Please, see the immediately above answer.

p. 5, l. 6: "... which means a spin-up of 150 years." Does this mean that the 150 yrs of abrupt4xCO2 are regarded as a spin-up here (due to their non-equilibrium character)? Or are 150 yrs of abrupt4xCO2 swapped as a spin-up, and another

Printer-friendly version

Discussion paper

150 yrs evaluated as some kind of quasi-equilibrium? Please, clarify.

Reply: The piControl spin-up of 150 years means that the piControl run for 150 years before the analysed period. Therefore, after the 150 yr run, two new simulations of 150 yr are started: 1) the piControl continuation run; 2) the Abrupt4XCO2 run. New informations are added to manuscript text.

p. 5, l. 6: "... commonly employed ... for climate change assessment"; please, be careful to distinguish between "climate change assessment" and "climate sensitivity assessment"! In my view, "climate change" in the CMIP context is rather assessed through historical simulations and future scenario simulations.

Reply: "...climate change assessment" is changed to "...climate sensitivity assessment" in the new version.

p. 5, l. 12: In this paragraph the "forth and back" jumping in addressing the merits of the regression and kernel method is somewhat confusing but could be easily avoided.

Reply: The "forth and back" jumping was avoided in this version.

p. 5, l 27, 28: There's still something wrong with the sentences here. Suggestion: "As G can be approximated by backward regression towards ΔTas=0, ECS can be estimated as ECS=$-$G/λ."

Reply: The alteration proposed was done. The intention with the original sentence was emphasize the computation economy that the regression method allows, avoiding a simulation in a order of millenia. The following sentence was removed: “For this method the ECS can be estimated as ECS=-G/λ in a shorter simulation (typically of 150 year) without reach the thermodynamical equilibrium.”.

p. 5, l 30: “... it is common to divide the result derived from 4xCO2 simulations by 2 (Andrews ...”

Reply: Done.

p. 6, l. 9: “... is used next, in order to partition the ...”, as “next to” is confusing. By the way, “separate” or “split” may be preferred to “partition”.

Reply: Done. It was used “decompose” instead of “partition”

p. 6, l. 14: “integraly” -> “fully” (or “necessarily”)

Reply: Done. “integrally” was replaced by “necessarily” .

p. 6, l. 16: “This, however, assumes that ...”

Reply: Done.

p. 6, equation 3: Tas is the near surface temperature (p. 5), but what is then Ts? I tried to clarify this by looking into Vial et al. (2013), without success. Please, be precise in citing, or explaining what you have done, and why.

Reply: Ts means surface temperature whereas Tas means near-surface atmospheric temperature. In order to avoid misunderstandings, besides Vial et al. (2013) was cited Soden and Held (2006, page 3356), which has a good compatibility with the variables presented in the equation 3 of our work.

p. 6, l. 25: Confusing: As q is in the data base, why should it be approximated based on the assumption of constant relative humidity? To my knowledge, this is not common in feedback analysis. Is it possible that you are misinterpreting the cited references here?

Reply: The assumption of constant relative humidity is associated with how the water vapor kernel is obtained. For water vapor kernel, it is computed the specific humidity change corresponding to a 1-K increase (holding relative humidity constant). Please see Soden et al. (2008) page 3509 and Shell et al. (2008) page 2271. Additional information about the necessity of this assumption was included in consonance with what was requested in the general comments.

p. 7, l. 5: "... changes are not accounted ..."

Reply: Done.

Printer-friendly version

Discussion paper

p. 7, equation 5: It is not immediately obvious, what the indices “a” and “k” mean. The index “k” means the change in ΔCRE due to the noncloud feedbacks, while the index “a” means the ΔCRE adjusted (to obtain the cloud feedback).

Reply: Additional information was provided to this new manuscript version.

p. 8, l. 4: “... were assessed as it was performed ...”; I do not understand this sentence. Are the data not from the ESGF data base (p. 5, l. 10) ??

Reply: The sentence is to inform the reader that we used the same method (analysis, assessment or evaluation) realized by Andrews et al. (2012). This is not supposed to be link with a mention to ESGF.

p. 8, l. 6: “... e inmcm4 “, did you intend “... and inmcm4”?

Reply: Done.

p. 8, l. 13: witch -> which

Reply: Done.

p. 8, l. 29: Please, explain how Figure 4 is related to Figure 3. Is it simply an average over the latitudinal profile of Fig. 3? Your discussion of the Planck feedback is casting doubts concerning this: If it's constant by about -4 Wm-2K-1 (l.29) with mostly negative deviations at polar latitudes, how can this result in a global mean of -3.6 Wm-2K-1 (l. 26)? Please, cross-check the numbers.

Reply: a) The Figure 3 shows the global mean for the climate feedbacks, where is possible note the models dispersion. The Figure 4 shows the same feedbacks (and the Planck feedback) but for the zonal average.

b) The ensemble Planck feedback is about -3.39 W/m-2K-1 at the Equator (as well as in the Tropics). It has values below -10 near North Pole, however, we can not forget that the global mean is calculated considering the areal weight for each latitude, which is smaller for polar zones. Therefore, the global mean features a value around -3.6 W/m-2K-1.

p. 8, l. 32: "stronger vertically homogeneous warming". This is a strange reasoning, as the Planck feedback is essentially the surface warming, constantly extrapolated upward through the depth of the troposphere. Can the message of this sentence be reconciled with Figure 8?

Reply: a) We totally agree with the comments. By definition the Planck feedback assumes that the temperature change is vertically uniform throughout the troposphere with respect to surface (Soden et al. ,2008, page 3515). This is in the Eq. (4) of the manuscript:

This also is in accordance with what is stated by Jonko et al. (2013): "The Planck feedback is the response of longwave (LW) TOA flux to a perturbation in

Printer-friendly version

Discussion paper

surface temperature that is applied to each vertical layer of the troposphere". On the other hand, the lapse-rate feedback is related to the radiative response to changing the vertical temperature structure. Therefore, it was added more information regarding the relation of the Planck feedback and surface temperature.

b) It add more information mentioned the link between Figure 8 and results from figures 3 and 4.

p. 9, l. 20: The partly revised text in this paragraph (see also major comments) contains some sensible elements, but is also moving in circles, explaining stronger sea-ice melting with stronger surface warming and vice versa. More re-organisation of the text is necessary.

Reply: Modifications were performed as requested in the major remarks.

p. 9, l. 22: "Those negative values ...", it is unclear which values are addressed.

Reply: It is about negative Planck feedback. Such paragraph was reorganized.

p. 9, l. 30: "The highest positive values ...", I would expect that backscattering increases if ice turns into water, driving the shortwave cloud feedback to more negative values. However, your later discussion (Figure 7, see also below) seems to suggest that the longwave cloud feedback is the dominant component.

Reply: This whole paragraph was rewrite. Since the ice has a greater albedo than water, when occurs sea-ice melting the albedo decreases, consequently, the outgoing shortwave radiation at the TOA also decreases. Two aspects are highlighted in the high latitudes for BESM cloud feedback: a weak increase in total cloud cover, which contributes to a negative SW cloud feedback (Figure 6a-b); and a low-level clouds upward shifting that is responsible for a gain of LW energy, which is related to sea-ice melting and indirect linked to albedo feedback cloud mask (Figure 6 c-d).

p. 9, l. 31: I feel that the following text (until "... outlier for the cloud feedbacks.") is mainly repetitive.

Reply: I was rewrite.

p. 10, l. 4: "λa, λac", are you referring to an analytical framework that is given in Cess et al. (1989)? Otherwise the reader is rather left in the dark here.

Reply: They are in the Equation (5). "λa, λac" are the albedo feedback and the albedo feedback for clear-sky, respectively. More information is added to clarify the discussion.

p. 11, l. 4: "models with ... apparently do not show ..."; please also replace "present" by "show" on many occasions thereafter.

Reply: Done.

p. 11, l. 24: "...quadrupling of atmospheric CO2 with the piControl pre-industrial CO2 concentrations ...": meaning what? The two first sentences of this paragraph appear to transport the same statement.

Reply: Real meaning is: "..quadrupling of atmospheric CO2 with respect to the piControl pre-industrial CO2 concentrations . . ." . It was changed for the new version.

p. 11, l. 28: "... precipitation increase is not governed ..."

Reply: Done.

p. 11, l. 31: Does this have in any way implications for the use of these somewhat "outlying" models?

Reply: The fact that a model is an outlier in one feature does not invalidate that model in others features. For example, HadGEM2 is widely recognized for having a good representation of precipitation in many parts of the globe; however, it is on the list indicated in the manuscript that models do not have a linear fit between global warming and precipitation change. Such behaviour may be due to chosen tuning in physical parameterization.

p. 12, l. 13: "...regions with the strongest increase of westerly winds at all levels indicate a southward jet displacement ..."

Reply: Done.

p. 12, l.18: Is "omega" something different from "vertical velocity"? Anyway, "omega" isn't self-explaining, so please adjust the text.

Reply: Omega is related to vertical velocity, but is not the same variable. Omega is Dp/Dt (isobaric coordinates), while vertical velocity is w=Dz/Dt (height coordinates). For hydrostatic approximation Dp/Dz = - â■ťg with â■ť constant, Omega = -â■ťg w. In order to clarify the sentence we changed "vertical velocity" to "omega vertical motion".

p. 12, l. 31: "... radiative code transference ...", do you mean "performance"? Is there any indication of that particular feature for BESM's radiative transfer model?

Reply: It is related to BESM's radiative transfer model. The correction was done.

p. 12, l. 31: "... rapid adjustments ..."; the rapid adjustment process is included in the CMIP5 model results as well, per construction. You apparently did not calculate the rapid adjustments for BESM, but do you have any indications that there might be a systematic bias with respect to CMIP (see Smith et al., 2018).

Reply: We did not integrated the BESM (atmosphere-only: BAM) model with climatological SST and ice cover doubling CO2 in order to evaluate the rapid adjustments. However, we think that this could be done a future study.

Printer-friendly version

Discussion paper

p. 13, l. 4: "Two regions indicate enhanced inter-model standard deviation for Planck, lapse-rate and albedo feedback"; also in the rest of this paragraph the use of English language is very weak, making the meaning nearly incomprehensible for me.

Reply: The entire paragraph has been rewritten and a third party English proof-reading service has been performed.

References (only if not already cited in the paper):

Smith, C.J. et al., 2018: Understanding rapid adjustments to diverse forcing agents, Geoph. Res. Lett. 45, 12023-12031.

References cited in the responses:

Andrews, T., Gregory, J. M.,Webb, M. J., and Taylor, K. E.: Forcing, feedbacks and climate sensitivity in CMIP5 coupled atmosphere-ocean climate models, Geophysical Research Letters, 39, n/a–n/a, https://doi.org/10.1029/2012GL051607, 2012.

McCarthy, G.D.; Smeed, D.A.; Johns, W.E.; Frajka-Williams, E.; Moat, B.I.; Rayner, Darren.; Baringer, M.O.; Meinen, C.S.; Collins, J.; Bryden, H.L. (2015): Measuring the Atlantic Meridional Overturning Circulation at 26°N, Progress in Oceanography, 130:

91-111. doi:10.1016/j.pocean.2014.10.006

Jonko, A. K., Shell, K. M., Sanderson, B. M., and Danabasoglu, G.: Climate Feedbacks in CCSM3 under Changing CO 2 Forcing. Part II: Variation of Climate Feedbacks and Sensitivity with Forcing, Journal of Climate, 26, 2784–2795, https://doi.org/10.1175/JCLI-D-12- 00479.1, 2013.

Shell, K. M., Kiehl, J. T., and Shields, C. a.: Using the radiative kernel technique to calculate climate feedbacks in NCAR's Community Atmospheric Model, Journal of Climate, 21, 2269–2282, https://doi.org/10.1175/2007JCLI2044.1, 2008.

Soden, B. and Held, I.: An Assessment of Climate Feedbacks in Coupled Ocean – Atmosphere Models, Journal of Climate, 19, 3354–3360, https://doi.org/10.1175/JCLI9028.1, 2006.

Soden, B. J., Held, I. M., Colman, R., Shell, K. M., Kiehl, J. T., and Shields, C. A.: Quantifying Climate Feedbacks Using Radiative Kernels, Journal of Climate, 21, 3504–3520, https://doi.org/10.1175/2007JCLI2110.1, 2008.

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

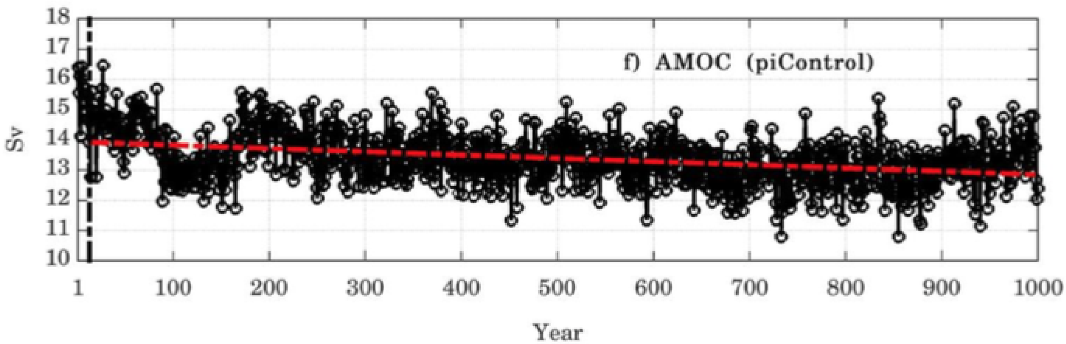

Fig. 1. Maximum AMOC simulated by the piControl from the beginning of the simulation up to 1000 years. The red dash-dot lines shows the linear trend.

Printer-friendly version

Discussion paper