Geoscientific

Model Development

Discussions

[GMDD](#)

Interactive

comment

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

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 paper is severely out of balance in that it dwells too much on discussing (and interpreting) CMIP5 model results, while entering too less into the potential origin of BESM-OA2.5 peculiarities. [...] the focus needs to be on the BESM results and their proper appraisal.**
   Reply: Please see the specific and technical remarks.

2. **In the current text I find the statements in the last paragraph (p. 12, l. 11ff.) rather strange. The main objective of BESM is not supposed to "show climate sensitivity and thermodynamical responses similar to ... CMIP5" but rather "to study the climate system [with a model able] to reproduce changes that are physically understood". Besides, that the latter objective should be pursued by any climate model activity, what does this mean for the present paper and its priorities?**
   Reply: Please see the specific and technical remarks.

3. **The authors use (or rather combine) two ways of calculating radiative feedbacks, viz. the regression method from Gregory et al. (2004) and the individual feedback calculation method from radiative kernels. This is quite recommendable, on principle. However, the methods are not equivalent as the phrase "seemingly redundant" (p. 4; l. 27) is suggesting... The kernel methods includes [...] rapid adjustments directly induced from the CO2 forcing. More severe, the regression method implies that the radiative feedbacks are consistent with the actual radiative transfer module used in the climate model, while this is not true for the kernel method, if another than the radiative kernel from the actual climate model is used (as is the case here). The authors are apparently aware of this fact (p. 5, l. 25, p. 7, l. 16),**

but repeatedly fail to appreciate it when interpreting results.
Reply: The manuscript was changed to include this concerns. Please, see answers in the specific remarks section.

4. **In the same context the authors might also consider to refer to Forster et al. (2016) and Smith et al. (2018) here (beyond Vial et al., 2013), with respect to the options of calculating and interpreting effective radiative forcings, radiative adjustments and feedbacks, and climate sensitivity parameters. Are there abrupt4xCO2 simulations with fixed SST from BESM-OA2.5 that could be included in the discussion? Or are those intended to be analyzed in further BESM studies?**
Reply: We have not performed an abrupt4xCO2 with fixed SST. We know that it is important to find the rapid adjustment of the troposphere and surface. Therefore, we intended to analyze this issues in a next BESM version.

5. **Further, I have some concerns about deriving the ECS (which is for 2xCO2) from 4xCO2 simulations by using a factor 2 (p. 6, l. 22). Is this really a standard method? Then it's certainly at odds with available knowledge (e.g., Boer et al., 2003; Knutti and Rugenstein, 2015). However, the authors could argue that they used the same approximation, crude or not, for all evaluated models.**
Reply: We used the same method of Andrews et al (2002), that obtained the ECS (for 2xCO2) from comparison between piControl and Abrupt4xCO2.

6. **Even if the focus of the paper were redirected towards the BESM performance, I still suggest a modified title, for example: "Assessing the performance of climate change simulation results from BESM-OA2.5 in comparison to a CMIP5 model ensemble".**
Reply: Done

7. **Specific and Technical Remarks**

- **p. 1, l. 8 (Abstract): For the following two sentences I would rather expect a general assessment of BESM rather than pure repetition of specific parameter results. While it is obviously true (and worth mentioning) that BESM-OA2.5 is not an outlier off the CMIP ensemble, its appraisal ought to be more process directed.**
  Reply: The abstract was modified in order to attend what was requested.

- **p. 2, l. 1: "..., commonly referred to as", I think this is rather a simplification for less developed models, so "..., sometimes given as" may be preferable.**
  Reply: Done (p. 1 l. 18)

- **p. 2, l. 20: There is a formal contradiction here: "... is robust from ... models" does not fit with "... uncertainty is likely to arise from ... inter-model spread", please reformulate.**
  Reply: After changes in the paragraph, the sentence became out of context, then was removed.

- **p. 3, l. 3: "Differences ...", this sentence may be omitted as it is essentially repeated at p. 3, l. 3.**
  Reply: Done.

- **p. 3 l. 16: "... uses BAM ... with simpler and computationally cheaper parameterizations"; Does this mean that BESM-OA2.3 uses the original BAM? Why has this been changed and could there be consequences of the simplification for the response behavior of the model as addressed in the present paper?**
  Reply: As required by the other Referee, more information about physical parameterization was included in the manuscript. Moreover, it was men-

C4

tioned that BAM is the atmospheric component of the climate model BESM-OA2.5. For the current study we used a different parameterization set from that of the evaluation paper of BAM (Figueroa et al. 2016), mainly because a computationally cheap set is desirable in a long simulation. However, changes in those sets result in different climate change response. For instance, a different radiative scheme probably will lead to a different radiative forcing (p. 3, l. 11).

- **p. 3, l. 20: From the preceding text, it is puzzling that the simplified model should have a better representation of the ToA radiative budget. I assume, however, that this is a result of more careful parameter tuning (but this is not mentioned). Like referee#1, I also wonder whether this relatively large ToA radiative balance bias leads to a considerable present-day surface temperature bias. Does the coupled atmosphere-ocean model use a flux correction?**
  Reply: A simulation with the atmospheric component only (BAM) presents a imbalance of 0.25 W m^{-2}. The imbalance of -4 W $^{-2}$ is related to higher loss of energy at TOA both from the outgoing long-wave radiation and outgoing short-wave radiation, compared with AGCM stand-alone simulation. Despite of this constant imbalance, the surface temperature is in thermodynamic equilibrium in a piControl run. BESM adopted the coupling strategy of pass variables through the surface interface instead of flux. It means that ocean component receive atmospheric variables and calculate the fluxes from atmosphere to ocean, then return variables for atmospheric component in order to calculate fluxes from ocean to atmosphere. Moreover, we do not apply flux correction for our simulations.

- **p. 3, l. 22: "surface layer"; I assume you mean the "planetary boundary layer", don't you? Or does tis refer to pure diagnostics, as suggested by the following sentence.**
  Reply: The surface layer is the lowest layer of the planetary boundary layer.

- **p. 4, l. 4: "general mean present-day climate state"**
Reply: Done.

- **p. 4, l. 7: "BESM-OA2.5 also is capable ..."; this sentence is rather vague, are you talking about ocean variability here? Or does this include the leading modes of long-term atmospheric variability like NAO, PNA etc. ?**
Reply: It is about leading modes of long-term climate variability. The sentence "manly that related to Atlantic Ocean", that can contribute to this misunderstand, was removed.

- **p. 4, l. 11: "overturning"**
Reply: Done.

- **p. 4, l. 12: "slightly"**
Reply: Done.

- **p. 4, l. 14: You might wish to address the matter of storm track variability here, but only if this is supposed to be a field of BESM application in the future. And if it has been actually studied, of course.**
Reply: The Storm Track variability of BESM has not been studied yet. This issue will be investigated in a future work.

- **p. 4, l. 30: "... the Gregory et al. (2004) method ..."; from various reasons it may be preferable to introduce (and refer to) the respective method as "... the regression method ...". Mainly, because using the terms "regression" and "radiative kernel" directly points to the methodical differences.**
Reply: Done.

- **p. 5, l. 17: "... we extract the clear sky radiative flux components from the BESM and CMIP data bases in order to ..."**
Reply: Done.

C6

- **p. 5, l. 25: (see major remarks above) – as the assumption is not necessarily true a remark should be made on the consequence for interpretation in case that there are substantial differences between the radiation modules.**
  Reply: As suggested, new information about radiative kernel limitation has been written in the Methods section.
- **p. 6, l. 4 (and l. 11): No information is given on how stratospheric temperature (and water vapour) changes are accounted for when calculating the feedback parameters. I recommend at least making a statement, if those contributions are included in the Planck feedback, or if they are shifted to the residuum Re (which I guess is, what you actually did). See also Rieger et al. (2017, their Fig. 5).**
  Reply: Differently from Rieger et al. (2017), the stratospheric adjustment was not investigated here. All feedback calculation was obtained integrating from the surface up to the tropopause. Thereby, it is mentioned that the stratospheric changes are shifted to the residuum (p. 7, l. 5).
- **p. 6, l. 17: "... cloud feedback is approximated using ..."; I'm aware that this is a standard method, so the authors are not responsible for the quality of this approximation.**
  Reply: Done.
- **p. 6, l. 22: I expect that the respective 30 year periods are not fully stationary as the deep ocean components of the various models have not reached equilibrium. If your analysis allows, please give some information on the remaining trend in the evaluated periods. Or have the data been de-trended before using them as an input to the radiative kernels?**
  Reply: The time-scale to a coupled model reach a thermodynamic equilibrium is more than 1000 years. Therefore, it is true that the stationary phase is not reached in the analyzed period. We proceeded similar to Vial et al.

(2013), that used a 10-year period centered around the 130th year after the CO_2 quadrupling in abrup4xCO2. The models data were not submitted to de-trended in the feedback estimation through kernel method.

- **p. 7, l. 5: "... the spatial inner product ..."; the authors might like to introduce the term in this way, but I assume they compute what is elsewhere called the 'Pearson correlation coefficient', hence I recommend to use the latter term through the rest of the paper.**
  Reply: The "spatial inner product" is similar to the "Pearson correlation coefficient" applied to space instead of time as it is commonly used. Therefore, to distinguish between the application for space and time we used the term "spatial inner product".

- **p. 7, l. 9: "These linear regressions ..."; this sentence is hard to read and needs rewriting. With the current formulation, it is not possible to unravel for which purpose all-sky or clear-sky data haven been used.**
  Reply: The application of all-sky and clear-sky is explained in Method section: "... we decompose the feedback parameter into shortwave (SW) and longwave (LW) radiation components and we extract the clear sky radiative flux components from the BESM and CMIP data bases in order to estimate the cloud radiative forcing or cloud radiative effect CRE defined as the difference between the all-sky and clear-sky feedback parameters Andrews et al. (2012)" (p.6 l. 4).

- **p. 7, l. 11: The values given are at odds with what is written p. 5, l. 12, concerning G, λ, and ECS. Please, give an explanation (which is probably to be found in the fact that no actual equilibrium has actually been reached).**
  Reply: New information was added in Methods section (p. 5 l. 26-30)

- **p. 7, l. 13: "... similar to those of Andrews et al. ..."; in fact, the reader certainly expects no less than this, as those authors used CMIP5 data**

as well. Where does the difference come from? Interpolation as mentioned on p. 4, l. 23?

Reply: Small differences are found in the analysis, which we attribute to the interpolation of the data. It was better explained in the manuscript (p.8 l. 3-8)

- **In the simulations with BESM, has there any form of "radiation double calling" been used to calculate radiative forcings or feedbacks? That could help to assess whether the radiation parameterization within BESM produces results (largely) consistent with the GFDL and NCAR kernels.**

Reply: There is not a "radiation double calling" module in BESM. We agree that it is could be a important implementation to include in future versions.

- **p. 7, l. 28: "Both radiative kernels are used ..."**
Reply: Done.

- **p. 8, l. 4: The following discussion (of Figure 4) is an example in a text flow that is largely out of scope with the paper's focus. Most of this is established knowledge from a multitude of previous papers. A clear change of perspective towards the specific features of BESM is advisable.**
Reply: The figure description was change to include the new perspective requested (p. 8 l. 29)

- **p. 8, l. 6: "The faster increase ...", I assume you mean "stronger", don't you?**
Reply: It was changed to "stronger" (p. 9 l. 1).

- **p. 8, l. 7: The two sentences discussing the possible cause-and-effect relation of water vapor and lapse-rate feedbacks is somewhat confusing. The general notion, I think, is the different degree of turbulent mixing in tropical, mid and polar latitudes. I recommend referring to,**

e.g., Po-Chedley et al. (2018), who draw a lucid and consistent picture of the latitudinal differences.
Reply: A paragraph was changed to include a more clear explanation (p. 9 l. 1-11).

- **p. 8, l. 16: "... as noted in yellow and blue shaded areas in Figure 4", this hint would better be given when the discussion of Figure 4 starts (l. 4) or, alternatively" in the figure caption.**
Reply: It was removed.

- **p. 8, l. 22: This paragraph is either too short (different cloud feedback results from different methods being a highly complex issue) or too long (as these general issues are not necessarily within the scope of the paper). Please focus on what could be a reason for the specific behavior of BESM in this particular case.**
Reply: Parts with comparison between regression and kernel methods was removed.

- **p. 8, l. 35: "This is due to ...", a rather technical reasoning (which continues throughout this paragraph). The reader would rather be interested in the physical reason. I the cloud cover response over sea (60°S) and over sea ice (Arctic) less well simulated by BESM compared to land areas? Or could it by that there is a problem with the cloud phase feedback (e.g., Mitchell et al., 1989; Tan et al., 2016) in BESM? I would find it sufficient, if some ideas could be formulated, with hints to future research.**
Reply: (p. 9, l. 29) It is evident from figures presented in the manuscript, that BESM is an outlier for the cloud feedbacks. This is due to a strong short-wave component response over both the Arctic and the Southern Ocean near Antarctica. Considering the SW CRE/ðİŽěTas [as 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 [-$(\lambda_a - \lambda_a^c)$], as shown in Figure 1. 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 such 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 principally over the (Figure 2a). As consequence, a negative SW CRE change is present in those regions (but not stronger for BESM comparatively to other models), that is the response to the increase in sun shading (Figure 2b). However, the SW cooling is smaller than the heating provided by LW radiation, as presented in the net effect (Figure 2d). The net radiation heating change is more intense around 60oS, that can be related to the more intense surface albedo change as well as the low-cloud lifting.

- **p. 8, l. 6: "stratocumulus region", this is presumably a different entity and not connected to the BESM peculiarities showing up in Figure 4.** Reply: This part was deleted.

- **p. 9, l. 10: The section 4.3 with its figures 6 and 7 (the scatter plots) is not very insightful to me. What are these correlation diagrams (especially Figure 6) supposed to teach the reader? Is this a standard diagnostic? Does the placement of BESM in the third quadrant reveal anything about this model in a physical sense? Please, give some reasoning for the figure's usefulness in the present paper. Interpretation of precipitation change patterns is more lucid; yet, it would be fine to know whether, e.g., the southward shift of the SPCZ in BESM does occur in other CMIP models too (even if not in the ensemble mean).**

C11

Reply: The diagrams helps understand the models temperature and precipitation dispersion. Moreover, it was used to answer if there is some general behaviour, such as: Do warmer/wetter models in piControl run present also a warmer/wetter in the abrupt4xCO2? Are there some physical limitations? Maybe the way it was presented is not clear, so we decided rewrite the paragraph.

- **p. 9, l. 27: "... near the equator compared to the subtropics ..."; ("as opposed to" suggests that the subtropics grow colder)**
  Reply: Done.

- **The statement beginning on p. 10, l. 21 "This increase ..." sounds somewhat counter-intuitive and is, in my opinion, an oversimplification of what the cited papers actually say. Rather, the non-linear increase of water vapor available for condensation, as suggested by the Clausius-Clapeyron relation, is limited towards a more linear relation by tropospheric radiative cooling (Mitchell et al., 1987).**
  Reply: New information was provided in order to make the sentence clear (p. 11 l. 26): "The slope of the linear regression is 2.5

- **p. 10, l. 25: "ACCESS1-0 and HadGEM2-ES use ..." up to the end of this paragraph: that may all be true, but the reader would rather be interested whether this implies anything for BESM.**
  Reply: Some discussion about ACCESS1-0 and HadGEM2-ES were suppressed and new information about BESM was included (p. 11 l. 34).

- **p. 10, l. 32: "... (SLP) pattern ..."**
  Reply: Done.

- **p. 10, l. 30: This whole paragraph gives a lot of (by no means unfounded!) physical reasoning on tropospheric variability patterns, but in the end takes a simple similarity of the SLP mean response patterns from BESM and from the CMIP ensemble to indicate that BESM may**

well represent such variability patterns. This is a bold conclusion, which in my view would need backing from actual variability pattern analysis. Is such analysis planned?

Reply: The comparison between BESM and ensemble was removed. The BESM variability change is planned to be discussed in a future work.

- p. 11, l. 23: "It is shown ...", this a very odd 'conclusion', as this statement is common knowledge motivating any research on global warming, and it is certainly not "... shown in this study". Even "... confirmed by this study" would be a summary much too weak for motivating publication of this paper. Please, find a more specific main conclusion that is directed towards the BESM performance.

Reply: New information was added.

- p. 11, l. 31: Here, some information about the BESM radiation module and its evaluation would emphasize that the radiative feedbacks calculated from BESM output within the CMIP range indeed indicate a good representation of such feedbacks inside that model (see major issues).

Reply: New information was added.

- p. 12, l. 5: You might delete "However,"; I see no contradiction of this sentence with the preceding one.

Reply: Done.

- p. 12, l. 12: "... is not the aim for the BESM development", this whole paragraph is a very puzzling wrap-up of your paper (see general remarks).

Reply: New information was added.

- Figure 3, Figure 8: Please ensure that this figure will appear larger in the eventual paper, otherwise it will be hard to decipher.

Reply: Done.

- Caption of Figure 6: "Shaded areas"; this return in several other figure

captions, too. You mean the white areas, don't you?
Reply: We mean areas fill in with colors.

Complete Figure Captions

Figure 1. SW Cloud feedback and the albedo and SW humidity feedbacks cloud mask-ing for the CMIP5 multi-model ensemble-mean (solid line) and BESM-OA2.5 (solid linewith 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 latitudeamong the models, not including BESM-OA2.5.

Figure 2. 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-AC2-supplement.pdf

---

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-209, 2018.
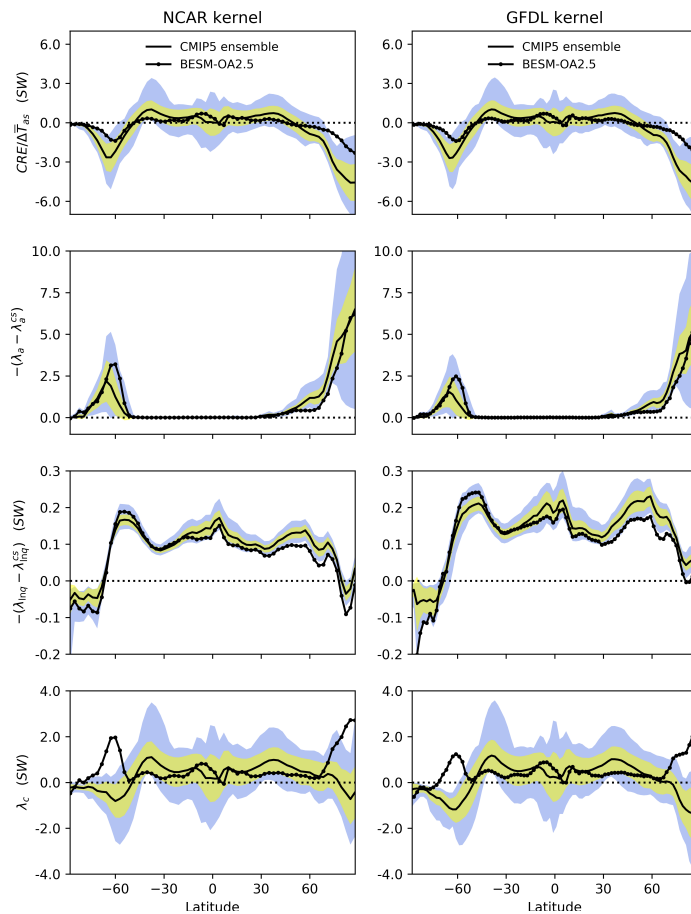
Fig. 1. SW Cloud feedback and the albedo and SW humidity feedbacks cloud mask-ing for the CMIP5 multi-model ensemble-mean (solid line) and BESM-OA2.5 ...
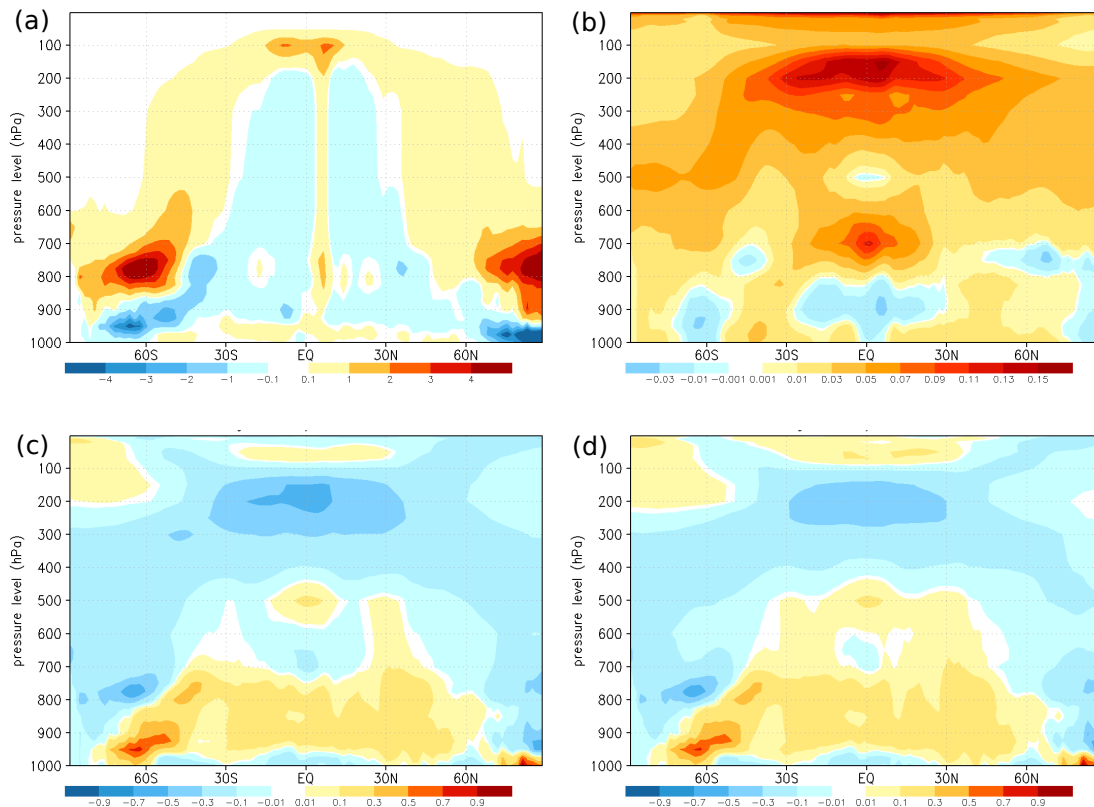
Fig. 2. Vertical profiles of the zonal mean of the 4xCO2 - piControl mean differencefor the following variables: (a) Cloud fraction, Radiative heating-cooling rate (dT/dt) of (b) shortwave, ...