

Interactive comment on “The American Monsoon System in HadGEM3.0 and UKESM1 CMIP6 simulations” by Jorge L. García-Franco et al.

Anonymous Referee #1

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The submitted manuscript represents a useful contribution to the assessment of the American monsoon systems (both its North and South America components spanning boreal and austral summers) using the latest CMIP-class versions of UK Met Office models. The novelty of the work partially stems from the fact that these monsoon systems are rarely formally assessed in European models such as the UKMO models, and to do so at such an early stage in the CMIP6 lifecycle is a valuable contribution to the literature for this ill-studied monsoon system.

With some corrections the paper should be acceptable for eventual publication in WCD, although it is a matter for the Editors as to whether this paper suits the remit of WCD or would be better served as a model evaluation paper in the sister journal GMD.

Specific comments: The abstract is generally well-designed and describes the work

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concisely, although there are a few issues to be considered.

Line 8: The abstract describes that “[the model has] a stronger intraseasonal variation than observed”. Note that intraseasonal variability (in the way that most readers will understand the term, i.e. the Madden-Julian Oscillation or Boreal Summer Intraseasonal Oscillation) is not at all examined in this study. The authors are really describing aspects of the annual cycle (e.g. the mid-season drying). Thus, the wording here needs to be changed to avoid the language of intraseasonal variability. (See also later similar comment.)

Line 9: While the Atlantic ITCZ is assessed, what of the SACZ? Is it relevant for such a study of the South American monsoon system?

Line 12: I think it is fair to say that ENSO characteristics (amplitude, frequency, longitudinal position, meridional spread, pattern, skewness. . .) are not at all assessed in this study. Thus, a more accurate form of words is needed here in order to avoid giving the reader such a misconception, e.g. revised wording should focus on the AMS response to ENSO.

Line 15/16: Instead of “between the two model configurations”, in the abstract the sentence should be worded to emphasize the scientific (rather than technical) meaning of this, namely that Earth System processes appear to make no difference to the monsoon simulation.

Line 16: At this stage the various resolutions involved have not been described so the use of the term “medium resolution” may confuse the reader, since it naturally implies there has been a comparison made with both lower and higher resolutions. Being familiar with the model framework used, I know that there is a higher resolution version of the HadGEM3 model although it is not studied here. The authors need to rethink their terminology for this description. (See also later comments.) In addition, the resolutions used need to be explained in the abstract since they mean very different things to different readers (and their definition also changes with as this article ages).

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Line 25/26: In an analogous fashion, what about the parts of South America north of the equator and their annual cycle? Can they be aligned to the NAMS?

Line 63: How have CMIP5 models misrepresented the magnitude of the seasonal cycle? Are they systematic under- or over-estimations, or a mixture depending on the model?

Line 67: Be specific as to what the CMIP5 models have improved upon. Presumably it is the CMIP3 models.

Table 1: Which version of the TRMM algorithm is used? V7 for example is known to perform better over orography such as the Andes (see Zulkafli et al., 2014, <https://doi.org/10.1175/JHM-D-13-094.1>).

Line 96/97: What is the evidence that TRMM provides the most reliable source of information on rainfall for this region? Are there citable studies intercomparing satellite, gauge and merged datasets for the NAMS and/or SAMS?

Lines 100-124: Note that the ocean model horizontal resolutions have not been listed.

Line 129: Clarify whether this is surface temperature or surface-air (i.e. 1.5 or 2 metre) temperature that is being considered.

Line 132: The Welch t-test should be defined in the methods or referenced here. How does this differ from a student's t-test? Plus, how are the different ensemble members dealt with relative to this?

Line 137/138: Does the stronger Bolivian LLJ support a stronger seasonal cycle/monsoon in the region north of the equator in South America during boreal summer? (i.e. in the South America component of the NAMS.)

Line 147/148: The physical outcome of this needs to be made explicitly clear to the reader, namely it appears that the inclusion of Earth System processes makes no difference to the SAMS.

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Line 149/150: A better summary of the changes in historical forcing (compared to the pre-industrial) needs to be described in lines 119-124 in order for the reader to be able to understand possible changes. Clearly the reader will know that global GHG emissions have increase, but what are the relevant/local patterns of aerosol emissions, land-use change etc. between the two experiments?

Line 150-152: Given the length of the pre-industrial control integrations that are available (and given the small size of the forcing when compared to the historical experiment), the internal variability of quantities such as those listed here (and elsewhere through the results) within the pre-industrial should be considered as a means to understanding the significance of any change.

Line 175: I understand the logic, but the chosen model comparison mixing UKESM with the GC3 model appears rather unclear.

Line 193: In what way is the low-level wind structure biased?

Lines 171-222 and onwards: All of the comparisons whether maps or seasonal cycles would benefit from a table of quantitative comparisons between the various datasets, such as pattern correlations (or just correlations for the seasonal cycles) and RMSE. This is standard practice in multi-model evaluation studies.

Line 239: That the AMIP models "removed the spatial patterns" is strange wording. Did any bias remain at all? Generally, I think that this study could be significantly strengthened if a fuller comparison could be made between AMIP runs of these two models (which will be available as contributions to the CMIP6 DECK) could be thoroughly compared with the coupled historical runs. The absence of SST bias would make for improved understanding.

Line 253: Here the run is referred to as "high-resolution" yet in the abstract it was medium resolution. The consistency within the manuscript needs to be improved. Could the manuscript not also examine a higher resolution version of the GC3 ex-

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periment, e.g. at N512?

Line 262: Are there any published onset measures for the AMS that could be used to measure this? And how is the onset objectively defined from Figure 9b? E.g. 1mm/d threshold, or the maximum rate etc.?

Lines 256-287: In the tropics, and especially for monsoons, I would expect the seasonal cycle of precipitation to be discussed in the context of the lower tropospheric circulation. This doesn't necessarily need to be done in the same paragraphs (the layout here is fine), but at the very least I would expect the discussions of precipitation biases here to reference the circulation biases for consistency. This is because of the intimate connection between circulation and precipitation in the tropics: winds providing moisture to the monsoon and the monsoon heating feeding back on the circulation to bring more moisture. At present the discussion is kept very separate. This could be aided by adding wind vectors to Figures 7 and 8.

Lines 256-287: It would be preferable to have some contextual comparison with other contemporary models (or at least CMIP5). How did CMIP5 perform for the NAMS and SAMS (cite references)? Do the UKMO models here fit within that envelope or are they better/worse? This will help improve the level of interest in this study outside the single modelling group. Furthermore, can the authors state how the current UKMO model versions (especially GC3.1) have advanced upon earlier versions (HadGEM3, HadGEM2-ES, even HadCM3) with respect to the AMS? Are there any published works mentioning those models? It would be useful for the community to understand if the simulation is being improved or whether significant biases are persisting.

Line 288: In the deep tropics, OLR is not really going to tell us much more than we already learn from precipitation, since much of the convection is deep. What is the nature of convection in the regions discussed? If any particular regions are dominated by shallow convection/warm rain, then this could be highlighted by references to relevant published works.

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Line 297: How certain can we be about the tropospheric moisture in any case in a reanalysis? What level of data is assimilated in some of these remote regions? Can any ground-truthing (really air-truthing!) be performed (even if not shown) using nearby RS launches such as those publicly available from Wyoming?

Line 328: Unlike the implication in the abstract, there is no assessment made here of general ENSO behaviour in these coupled models – and if the driving point of a teleconnection is faulty then resultant impacts over the AMS will hardly do well. A summary of the behaviour of ENSO in these models with reference to a published assessment of their performance should be made.

Line 332: Is this in units of temperature (degC/K) or a normalised index in terms of standard deviations? Where is the index taken from or how have you calculated it?

Line 334: What are the years included in the observed composites of El Nino and La Nina? Has the impact of CP and EP El Nino events been considered and what does the published literature say about the different impacts of such events on the NAMS and SAMS?

Line 351: It would be very instructive if wind vectors were added to Figure 12, enabling to reader to understand something of the mechanism by which ENSO controls rainfall anomalies in the AMS. The authors should then elaborate upon this in the text.

Line 348-350: It's not immediately obvious how the NAO links described are relevant to the study at hand. The authors should either make this clear or remove this text.

Lines 365-370: The authors should consider whether the lack of nonlinearity in the modelled ENSO response reflects the lack of diversity of simulated ENSO in the model (e.g. the lack of distinct central Pacific or east Pacific events).

Line 376: The authors could be more explicit on the likely kink between cloud cover and the warm bias in the SAMS domain. If precipitation is too weak, this should be stated explicitly. (Note there would also be a soil-moisture feedback as a result.)

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Lines 376-380: Finish the sentence by making explicit how the land-sea temperature contrast may feedback on the monsoon.

Line 391: Make explicit whether the Ryu and Hayhoe study was using CMIP5 models.

Line 393: With reference to the earlier comment on the abstract, the authors should avoid the terminology of intraseasonal variability here since the MJO/BSISO have not been assessed.

Lines 371-404: In the conclusions I would want to see a more thorough synthesis of the results (e.g. how all the meteorological components fit together) than a summary of each in turn. It would also be worth reflecting upon (if possible) how these models sit in comparison to published literature on the AMS in CMIP3/5 models or on earlier versions of UKMO models.

Line 413: See earlier comments on higher/medium resolution.

Line 418: Need to see a summary of how the Earth System processes influence the response to forcing.

Figure 1: The domains used later in Figure 3 etc. need to be pictured somewhere, e.g. on this figure.

Trivia: Lines 13/14: Perhaps replace “in subtropical America” [meaning USA?] with “in the subtropical Americas”.

Line 21: Change “copuled” to “coupled”.

Line 42: “...and the dynamics the features largely characterise the MSD characteristics...”. I don't understand what is meant here, something is wrong with the grammar.

Line 43: Change “reproduce accurately” to “accurately reproduce”.

Line 51: Remove hyphen from “South-America”.

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Line 66: Space needed in “MetOffice”.

Line 119: Replace “beginning for” with “covering”; replace “that include” with “of”.

Line 142: Change “temperature” to “temperatures”.

Line 171: Second “the” is not required.

Line 184: brackets not needed around location point.

Line 195: By “a minimum” do you mean “southernmost position”? This would be easier to understand.

Line 302: Replace “indicative” with “are indicative”.

Line 304: Clarify if the decreased omega is a reduction or increase in ascent.

Line 309: Mixture of singular and plural in this line.

Line 331: By convention, “EI” is not included when referring to the “Nino-3.4 index”.

Line 362: Change “opposite sign response” to “opposite signed response”.

Interactive comment on Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2020-8>, 2020.

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