

Interactive
Comment

Interactive comment on “Impact of the sea surface temperature forcing on hindcasts of Madden-Julian Oscillation events using the ECMWF model” by E. de Boisséson et al.

B. Webber

b.webber@uea.ac.uk

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General Comments: This paper sets out "to explore the sensitivity of the prediction of the Madden-Julian Oscillation to different aspects of SSTs... The impact of temporal resolution of SST on the MJO is first evaluated via a set of monthly hindcast experiments". The paper presents a set of interesting results from atmospheric model experiments and accompanying analysis of observational data sets, and these results are worth publishing. However, in its present form, the paper falls short of addressing the aims stated in the abstract, and I believe that further work is necessary before publication.

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The key results of the paper could be summarised as follows: 1. Switching SST boundary conditions from one dataset used to initialise the model to another for the hindcast simulation degrades the accuracy of the hindcast due to the shock of changing the boundary conditions. 2. The OSTIA SST data have a different lag relationship to convection when compared with the ERA-Interim SSTs. 3. These two differences overshadow any influence of temporal resolution. 4. Weekly SST boundary conditions lead to better hindcasts than monthly.

The problem is that although the first two findings are interesting, they do not answer the questions posed, but instead undermine the generality of the conclusions without further work. In order to fully investigate the impact of different aspects of SSTs, the authors could use more SST products and not compare between SST data sets where the experiment is always biased towards the one used to initialise the model. Comparison between different SST data sets where neither were used to initialise the model (or where both are) would be more enlightening. Alternatively, the authors could focus on the temporal resolution by comparing monthly, weekly and daily SST data from the same data set. As an aside, could the authors explain why daily SSTs from the ERA-Interim data set aren't used, when they appear to be available to download from the ERA-Interim website?

The findings regarding the relationship of OSTIA SSTs to convection are interesting, but would be greatly improved by repeating the analysis with at least one other independent data set. Such analysis would give some insight into which SST data set has a phase-relationship to convection that is more realistic, rather than simply showing that this differs between data sets. If the model could be forced with this third SST data set, it would also allow a better analysis of whether this is the key difference between the SST data sets that leads to the difference in model performance.

If the above additions to the paper would be too onerous to complete, then I feel the authors must substantially re-write the paper to tone down the conclusions. The results should be better placed in context and described in terms of what is actually proven.

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In addition, the the limitations of the study should be laid out clearly and avenues of further work suggested. However, I would hope that the authors can address the issues outlined above, since I believe that the aims of the paper are very worthwhile and the results presented interesting.

Specific Comments:

1. The abstract states that “capturing the correct SST-convection phase relationship is a major challenge for the MJO predictions,” but it seems that this is also a challenge for observations!
2. In the introduction, the authors would do well to describe better the existing literature on atmosphere-ocean interaction within the MJO, and the impact this has on simulations (probably two further paragraphs). The authors only reference two papers on this, and there are a very large number of others.
3. In Section 2.2, the authors refer to the two SST data sets used as being independent, but then describe each one as having a substantial amount of the same data used in creating the reanalysis. Therefore, it would be better to state that it is only the reanalysis process that is independent (and give more detail about how this differs), and that there is a lot of common data going into each reanalysis.
4. The paper as it stands asks a lot of questions, and much further work is clearly needed to fully understand the impact of different aspects of SSTs on simulations in the ECMWF model. The authors would do well to outline some of these in the conclusion section. For example, evaluating further SST products, repeating the experiments for different study periods, using a different version of the atmospheric model, comparing the results to coupled model experiments, repeating these experiments with other models entirely and so on. It would be worth suggesting some of these at the end of the conclusion section.
5. Could these experiments be done with sub-daily SSTs? Avenue for future work

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perhaps?

Technical corrections: 1. The paper requires improvement to grammar and language use throughout - careful editing required. For example:

pg 2536, line 9: "would in principle allow to establish...": change this to "would in principle facilitate the establishment of" or "would in principle allow us to establish".

pg 2536, lines 14-15: "Such result suggest..." should be "such results suggest"

pg 2536, line 16: "challenge for the MJO predictions": omit "the".

pg 2536, line 19: "Eastward" should not be capitalized

pg 2536, line 26: "... of the MJO in the General Circulation Models (GCM)": omit "the", and change GCM to GCMs.

pg 2537, line 1: "... in term of" should be "in terms of"

and other similar errors

2. In the introduction, compare the SST products used to others, e.g. TMI data.

3. The first paragraph in section 3.2 is confusing and could be better written.

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