

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

E. de Boissésou et al.

deboiseric@yahoo.fr

Received and published: 17 October 2012

Dear Dr Webber,

First I would like to thank you again for your comment. I attached to this comment an up to date revised version of the manuscript which take into account comments from the other referee. This reply is made on behalf of all my co-authors and can be considered as an official revision of the paper. In the following, the answers to your comments:

In the following, the answers to your comments (some of them are the same as in my previous reply):

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General Comments

> "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."

As you suggested, I added diagnostics using the AVHRR-only SST reanalysis from NOAA.

> "Alternatively, the authors could focus on the temporal resolution by comparing monthly, weekly and daily SST data from the same data set."

Additional experiment using either OSTIA or AVHRR-only SST were conducted at daily, weekly and monthly temporal resolution for a fair comparison.

> "As an aside, could the authors explain why daily SSTs from the ERAInterim data set aren't used, when they appear to be available to download from the ERA-Interim website?"

As mentioned in the manuscript, over the 1992-1993 period, daily ERA interim SST are, in fact, produced by a linear interpolation of the weekly NCEP 2Dvar SST reanalysis.

> "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"

The phase relationship diagnostic has been performed with AVHRR SST. The relationship appears to be even more distorted than when using OSTIA. This is consistent with the MJO scores (see attached PDF file). The comparison with coupled MJO hindcasts give more weights to this result (see Section 4.3).

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Specific Comments:

> "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!"

Yes, this diagnostic is very sensitive to the SST fields that we use. Zhang et al from NCEP also reported that (in an internal note) when computing the phase relationship between SST and precipitation. I can send it to you if you want to have a look at it.

> "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."

New paragraphs have been added to give more details on air-sea interactions in the context of MJO.

> "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."

You are right. As you suggested, additional details on the reanalysis process have been added in Section 2.1.

> "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

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models entirely and so on. It would be worth suggesting some of these at the end of the conclusion section."

We are aware that our study is not exhaustive and some of the hints you mentioned have been added in the conclusion.

> "Could these experiments be done with sub-daily SSTs? Avenue for future work"

That would be very interesting to see what happens. However, you should be aware that a skin layer scheme simulating the diurnal cycle of SST is present in the ECMWF model. It is mentioned in Section 3.1.

Technical comments:

> "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"

These errors have been corrected. Some of the sentences you mentioned are not present anymore in the revised version.

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

We could not use TMI data as they are not available in the period we were interested in (1985-2006). However these data are mentioned in the conclusion as avenue for future works.

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

This paragraph has been removed.

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End of my answer to your comments.

I would like to thank you again for your comments.

Best regards,

Eric de Boisseson

Please also note the supplement to this comment:

<http://www.ocean-sci-discuss.net/9/C1161/2012/osd-9-C1161-2012-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., 9, 2535, 2012.

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