

**The Madden Julian Oscillation in high-resolution coupled climate simulations:
mean state evaluation and historical variability in IFS-NEMO**

General Comments:

This work evaluates the Madden-Julian Oscillation (MJO) representation in IFS-NEMO. The authors conduct two simulations for the historical period at two different horizontal resolutions and apply robust MJO methodology. The objective of the study is to (a) evaluate the impact of horizontal resolution in historical periods and (b) quantify the linear trend of MJO and its predictability. The authors apply the methodology from Wheeler and Hendon (2004) to assess the MJO performance against ERA5 and ERA5 in combination with NOAA OLR observations.

Results show a nuanced impact of resolution in the MJO representation, while the trends revealed by the model do not necessarily agree with the observed ERA5 trends. Next, the authors dive into process-based reasoning on why such behavior exists. The methodology used in the manuscript is robust with further evidence of statistical significance. The manuscript is easy to follow and well-written. I recommend the manuscript for WCD but would encourage the authors to address my comments/questions and some clarifications.

Comments:

1. One of the main motivations for using high resolution is to improve the representation of Maritime Continent dynamics, including topography and land-sea interactions. While the HR simulation brings the MJO propagation speed closer to observations (Fig. 2), the classic "MC barrier effect" (convective suppression over/after the MC) persists. Is 9 km resolution still insufficient to properly resolve these island dynamics, or is this behavior driven more by underlying air-sea coupling and SST biases in IFS-NEMO? Brief physical context on this would be helpful.
2. Fig.4, I would encourage the authors to report some of the processes responsible for under-representation of tropical waves (eastward propagating), and some hypothesis.
3. Looking at Fig.5 and Fig.6, the ER simulations show precipitation and specific humidity anomalies comparable with ERA5. Particularly it has higher precipitation and high specific humidity compared to HR. I was expecting HR to have more precipitation/humidity as it ideally should be more convective (Line 391). Is this because HR ocean has large ocean SST cold bias compared to ER over the oceans? Or there is something more to it?

4. Line 400: Could the authors expand briefly on which specific aspects of the IFS convection scheme (e.g., the Tiedtke–Bechtold mass-flux parameterization) might be responsible for the underestimated MJO variance? For instance, do the authors suspect this is driven by over-frequent convective triggering (leading to unorganized deep convection and light rain bias), or rather by sensitivity to environmental humidity/entrainment rates during the pre-conditioning phase? Having some physical intuition on why higher resolution (HR) behaves differently than standard resolution (ER) in this regard would add great value to the discussion. (I am not expecting any additional analysis for this comment).
5. Figure 11: Regarding the interpretation of the Weighted Permutation Entropy (WPE) trend: while WPE effectively quantifies the complexity and chaotic nature of the signal, a decrease in WPE over time primarily indicates that the MJO is becoming more regular and less chaotic (i.e., higher potential or intrinsic predictability). However, conflating this directly with "predictability" in a numerical modeling context can be misleading. A less chaotic real-world MJO signal does not automatically translate to improved forecast skill or practical model predictability, as that remains heavily constrained by model biases and initial condition errors. I suggest the authors clarify this distinction in the text—for example, by specifying that WPE measures intrinsic potential predictability or signal regularity, rather than absolute forecast skill or practical predictability
6. Regarding the authors comment on large-scale biases in IFS potentially leading to unrealistic evolution of MJO, and other less important factors such as ocean biases, I advise authors to think of an Overview schematic figure representing processes influencing MJO in the IFS. This would give a better and simple understanding of the manuscript's overall objective.
7. I would advise the authors to add a short outlook. Conducting such expensive simulations did not improve MJO, so perhaps it is a bit more than resolution. May be authors can educate the readers on their honest opinion and way forward.

Further improvement of text/Figures:

1. Abstract: I see many shorthand notations here, which ideally should have full forms followed by short forms. Eg., MJO, IFS-NEMO, BSC, EERIE, MIP, ERA5.
2. Line 21: 10 Km -> 9 km.
3. Line 24: Here you mention cold ocean and circulation, but in the discussion, you emphasize it has more to do with WC circulation and associated biases. Please be consistent in the manuscript.
4. Line 19. I would say both simulations, not both versions. You use same version of IFS with different resolution, I guess.
5. Line 40: Tropics -> tropics

6. Line 45: Increases -> increase
7. Line 76: Before diving into the scope of the manuscript, I would encourage the authors to motivate the readers on why horizontal resolution is important and justify why you want to conduct highres simulation. A strong hypothesis would enhance the motivation of the manuscript. Yes, the authors did mention few lines, but more weight was given on to explain about RMMs, but not in terms of the motivation on why one should investigate resolution impact of MJO.
8. Section 2.1. Few more details on the model settings, model computational speeds, parametrizations (especially convective parametrization) and frequency of calling would be nice to have here.
9. Sometimes you write u850 and U850. Be consistent across figures, and manuscript text.
10. Line 225: What do you mean by previously interpolated? I would find a better word than “previously”. Also, it was used multiple times.
11. Figure 1: Can increase the legend and perhaps figure size too.
12. Figure 7: Can increase the legend and perhaps figure size too.
13. Figure 8: Can increase the legend and perhaps figure size too.
14. Figure 8: Can increase the legend and perhaps figure size too.
15. Line 487: “WC” comes as a surprise or after a long time it was defined in the manuscript. May be just write full form here.
16. At times the authors call IFS-NEMO HR as just HR and IFS-NEMO ER as ER in the manuscript. I advise replacing the HR and ER with full names, such that they are easy to read and go with the flow.