Review for Maraldi et al.,

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.

Before detailing our response we want to take advantage to clarify that HR is not the highest (9km) resolution, as we feel the reviewer might have interpreted it that way in some cases. We are aware that this labelling can be misleading. For that reason we now explain that more clearly in the text.

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.

This is a very key point for the evaluation of the horizontal resolution impact on the simulated MJO.

The Maritime Continent (MC) barrier effect usually arises from biases in total precipitation over the MC and in tropical ocean ssts, which then reflect on the circulation leading to the degradation of moisture advection across the MC ((Kim et al., 2016)). There are two main expected benefits of the horizontal resolution increase for the MC barrier issue. The first is that the improved topography could correct some biases in the precipitation diurnal cycle as well as in the heat fluxes. The second one relates to the overall higher granularity of the simulations, which enhances (potentially improving) the ocean-atmospheric coupling.

However, resolutions higher than 10 km are needed to capture the primary convective-organization dynamics that distinguish the MJO, which are more likely to tackle the observed errors.

Few studies have employed convection-resolving horizontal resolutions to address the Maritime Continent problem. For example, Weber et al. (2020) used a 3-km mesh grid global convection-permitting model (CPM), and compared it to two different 15 km counterparts with parametrised convection optionally active, to reproduce the evolution of 4 target MJO events. The CPM version was found to outperform the other configurations, displaying an eastward propagating MJO over the MC. However, similar resolutions do not systematically solve the MC bias. For example, Wedi et al. (2020) found great agreement between an eddy-rich simulation (9 km) and a storm-resolving counterpart at 1.4 km of horizontal resolution for the MC problem. On the other hand, most climate model simulations use lower resolutions than those of the studies mentioned above. The fact that overall, CMIP6 simulations with higher horizontal resolution than CMIP5 ones improved the MC barrier problem (Ahn et al., 2020) does not necessarily imply that resolution is the only factor responsible for this improvement. In particular, when comparing two models with the same physics but different resolutions, improvements in MJO behaviour cannot necessarily be attributed to resolution alone (different resolutions have different tunings which could lead to different mean states), nor can a linear relationship between resolution and model performance be assumed (Jia et al., 2008). For instance, Pilon et al. (2016) stress the importance of the correct shallow and deep convection parametrizations on top of the increase in resolution, showing similar biases between high and low resolutions when deep convection is not parametrized.

We now discuss some of these aspects in the text.

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 hypotheses.

We welcome the reviewer's suggestion by including the following paragraph in the new version of the manuscript:

The MJO and tropical waves dynamics are intertwined and studies have attempted to describe the mechanisms responsible for the misrepresentation of tropical waves and their impact on the MJO. In fact, this relation is thought to be crucial to understand the "slow" eastward propagation speed of the MJO. The relative strength of ER and Kelvin waves is thought to affect the correct eastward propagation of the MJO (Wang and Lee, 2017; Chen and Wang, 2020; Heath et al., 2021) since it directly impacts the horizontal moisture gradient and its consequent advection.

From the perspective of horizontal moisture advection, over-representation of ER wave activity is associated with excessive moistening on the westward side of the convecting center. This bias derives from the overestimation of low-level convergence which leads convection to develop on the westward side of the main center of convection (Heath et al., 2021). The spectral signal of Kelvin waves is usually weaker in models than observations (Bartana et al., 2023). Heath et al. (2021) also associate this bias to poor horizontal moisture advection to the East of the main convective center.

In the case of IFS-NEMO, significant power peaks are correctly placed in the ER and Kelvin waves domain. However, westward propagating ER waves are over-represented while the power of eastward-propagating Kelvin waves is slightly underestimated. This feature is usually associated with models that simulate a stagnant or even westward propagating intraseasonal signal (Heath et al., 2021). However, Fig. 3 clearly shows a significant eastward propagating signal in both IFS-NEMO versions, very much comparable with observations. Taking into account the mechanisms highlighted here, the reduced E/W ratio, together with the over (under)-representation of ER (Kelvin) waves could explain the lower simulated propagation speed compared to observations.

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 a large ocean SST cold bias compared to ER over the oceans? Or is there something more to it?

We take this comment as an opportunity to better clarify our choice for the naming of the two IFS-NEMO resolutions.

The 25-km resolution version of IFS-NEMO has been labeled “-HR”, even though it is the low resolution configuration in this study, to reflect that it has higher resolution than the traditional models with ~100 km grid spacings. We used “-ER” for the 10 km version to refer to the fact that it is an “Eddy-rich” configuration, its main process-defining characteristic. We imagine the reviewer meant that IFS-NEMO-ER should be more convective, which would be in line with the assumption of expecting IFS-NEMO-ER “ to have more precipitation/humidity as it ideally should be more convective”.

If this is the case, the reviewer’s assumption is indeed correct, as IFS-NEMO-ER alleviates the precipitation and specific humidity mean biases with respect to ERA5.

Indeed, large cold sst bias in the Indo-Pacific Warm Pool are responsible for the underestimation of total MJO activity in IFS-NEMO and are also linked to the wrong MJO initiation over the early phases (with colder oceans less moisture is charged into the atmosphere). Specifically, IFS-NEMO-HR has a colder bias than IFS-NEMO-ER.

We now more explicitly explain in the text that the HR version is coarser than the ER, and the reasoning to label it as HR.

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).

The point raised here by the reviewer is crucial and potentially interesting for future analysis.

In fact, previous works have already suggested that resolution might not be the only factor limiting the MJO representation in models, even when reaching resolutions that explicitly resolve deep convection (Pilon et al., 2016; Wedi et al., 2020; Becker et al., 2021). Over-frequent precipitation is often associated with the lack of deep moisture convection. Without further analysis and sensitivity tests, it is challenging to say whether IFS-NEMO bias is only due to deep-convection parametrization deficiencies rather than dynamical features linked to different MJO preconditioning. Instead, we discuss here which specific features of the IFS convection scheme could be responsible for the MJO variance bias as well as small differences between the two resolutions.

We first note that IFS convective closure is scale aware, as the convective adjustment time scale depends on the horizontal grid spacing (Becker et al., 2021). However, deep convection is assumed to take place at subgrid scales and is fully parametrised at both resolutions in the same way. However, even under the same parametrization there are factors that can dynamically change at higher resolutions. Air entrainment during the vertical motion is crucial for the correct moisture build up that preconditions deep convection during an MJO event. Hirons et al., (2013a, b) have shown how improving the entrainment from moisture convergence-dependent to environmental moisture-dependent drastically enhanced the simulation of the MJO in IFS. Known biases of IFS include early biases in precipitation either involving an early trigger of precipitation with an anticipation of the diurnal cycle (Zeman et al., 2021), or unrealistic duration which leads to a bias in the organization of deep convection at the meso-scale (Becker et al., 2021). The authors do not see the options proposed here by the reviewer (early convective precipitation triggering - weak entrainment) as mutually exclusive. In fact, the first can be seen as a symptom of the second, as a bias in the entrainment rate could trigger deep early precipitation by not letting the rising air parcel interact with drier surroundings. This could lead to less spatially and temporally organized convective precipitation. On the other hand, a correct entrainment would allow for proper moistening of the atmospheric column, improving the MJO preconditioning. This can be improved at mesoscale resolving resolutions (9 km) such as the one considered in IFS-NEMO-ER, as deep convection does not only rely on parametrization. However, we cannot say whether the improvements seen at 9 km come from a better dynamics-parametrization interaction, or are merely a result of an improved background mean state.

Finally, we note that an additional simulation with IFS-NEMO-ER has been tested with a correction for the cloud base mass flux on top of the standard deep convection parametrization. We compared the result to the two simulations analysed in the manuscript. Only by evaluating the Wavenumber-frequency power spectra we did not notice remarkable improvements on the simulated MJO, but the representations of Equatorial Rossby and Kelvin waves are improved.

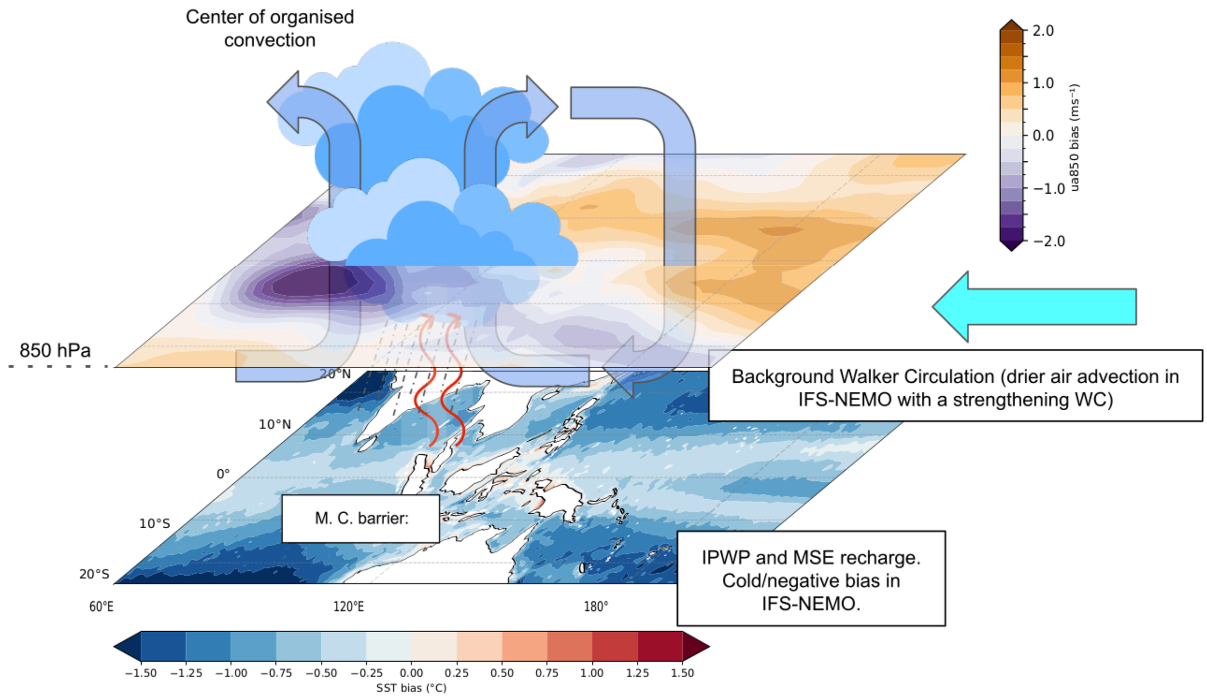
5. Figure 11: Regarding the interpretation of the Weighted Permutation Entropy (WPE) trend: while WPE effectively quantifies the complexity and chaotic nature of the signal, a decrease in WPE over time primarily indicates that the MJO is becoming more regular and less chaotic (i.e., higher potential 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

We thank the reviewer for raising this point. The authors agree that directly translating WPE with “predictability” could be misleading. While we relied on the work of Du et al. (2024) for this definition, we take the opportunity to further clarify in the text that this metric is not linked to model skill but rather to an intrinsic feature of the MJO, encapsulated by the Realtime Multivariate indexes timeseries.

A deeper explanation of what WPE effectively describes has been added to the manuscript.

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.

A schematic figure will be added to the final version of the manuscript. We include here only a draft of the final figure. In the final version, we aim to include the results of the additional Moist Static Energy analysis suggested by the other reviewer, which is currently pending due to missing data.



New Figure 14: Overview schematic of the model biases relevant for the development of the MJO. Convection develops over the Indo-Pacific Warm Pool (IPWP) after moist static energy (MSE) is accumulated in the atmosphere, latent heat fluxes (red arrows) are crucial to sustain deep convection in this phase. The model shows a negative bias in the IPWP intensity (in contours in the bottom layer), as well as in the total charged MSE, which explains the under-representation of MJO activity in the model. This bias is also reflected in a pronounced Maritime Continent (MC) barrier effect in IFS-NEMO. The second layer shows the mean ua850 wind bias. The long term changes of MJO activity in IFS-NEMO are linked to a misrepresentation of the mean background Walker Circulation.

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. Maybe authors can educate the readers on their honest opinion and way forward.

We thank the reviewer for the suggestion. We included an outlook on the merits and shortcomings of eddy-rich models in simulating the MJO. Specifically, we discuss how the minor improvements in MJO representation do not fully justify the much higher computational cost of the 9 km resolution for studying MJO variability and predictability. We note, however, that additional efforts to fine tune the key parametrizations relevant for the MJO might be needed to realize the full potential of both resolutions. As previously mentioned, even kilometer scale simulations do not always show relevant MJO improvements, although results from simulations using the super-parameterization confirmed that cloud

-permitting schemes for moist convection alleviate moisture budget biases linked to the MJO (e.g., Thayer-Calder and Randall, 2009; Stan et al., 2010)

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. Changed in the text
2. Line 21: 10 Km -> 9 km. Changed in the text
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. Has been changed to clarify the association of the IPWP (ocean) and the WC biases with the total MJO variance and its trend respectively.
4. Line 19. I would say both simulations, not both versions. You use the same version of IFS with different resolution, I guess. Changed in the text
5. Line 40: Tropics -> tropics Changed in the text
6. Line 45: Increases -> increase Changed in the text
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. Thanks for the comment. These are the aspects that help motivate better our study.

Recent works have leveraged km-scale simulations to test the storm-resolving model performance when it comes to simulating the deep convection that characterizes the MJO, as well as its propagation (Wedi et al, 2020; Yano and Wedi, 2020). Additionally, such resolutions are expected to help overcome the Maritime Continent barrier issues by better resolving coastlines and topography, possibly leading to improved surface heat fluxes and more realistic convective events. While these works shed light on the advantages of explicitly resolving deep convection, they are limited to a few example cases based on relatively short simulations (mainly because those global configurations are very computationally demanding).

It is of particular interest to establish whether eddy-rich models, which are more computationally affordable and allow for longer simulations, could help alleviate the typical biases affecting MJO representation in coarser model configurations. In particular, benefits could arise from improved scale interactions, specifically by better resolving mesoscale convective systems that characterise active phases of the MJO (Zhang, 2005). At the same time, Ahn et al. (2020) report that increased horizontal resolution reduces the dry tropical bias in CMIP6 models relative to CMIP5 ones, as reflected in more realistic horizontal moisture advection.

Some of these aspects are now discussed in the introduction.

8. Section 2.1. A few more details on the model settings, model computational speeds, parametrizations (especially convective parametrization) and frequency of calling would be nice to have here.

Details about integration time-step and convective parametrization details are furtherly discussed in the text. We also note that a documentation paper of this and other IFS-NEMO configurations, including an extensive evaluation of its performance will be submitted between the end of September and October, and we will encourage readers to refer to it for further details.

9. Sometimes you write u850 and U850. Be consistent across figures, and manuscript text. Changed in the 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. Changed in the text

11. Figure 1: Can increase the legend and perhaps figure size too.

Changed in the text

12. Figure 7: Can increase the legend and perhaps figure size too.

Changed in the text

13. Figure 8: Can increase the legend and perhaps figure size too. Changed in the text

14. Figure 8: Can increase the legend and perhaps figure size too. Changed in the text

15. Line 487: “WC” comes as a surprise or after a long time it was defined in the manuscript. Maybe just write the full form here. Changed in the text

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. Changed in the text

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