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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivities of the MJO Forecasts on Configurations of Physics in the ECMWF Global Model" *by* Jun-Ichi Yano and Nils P. Wedi

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Thank you for your comments posted on 3 March 2020.

I respond to those as follows:

General Introductory Remarks:

An explorative nature of the present manuscript is emphasized. As well summarized as items 1) and 2) by the present Reviewer, the scientific questions of this study are well posed. We strongly believe that presented results are rich in implications. However, as the present Reviewer suggests, we are short of developing full interpretations of

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Discussion paper

the results. It is a major reason that we decided to submit the present manuscript to ACP, thus by going through the discussion session, we can obtain various useful feedback. Regardless of the amount of feedback we may receive, we also believe that the materials presented herein strongly invite for theoretical interpretations, that must further be developed. Development of such theories must be a common effort of the community by placing these materials in public domain. This is the main reason that we believe that the present materials are worthwhile to publish in the present form. As stated in the current version, we believe it important to present those details of the model sensitivities so that the theoretical community will be aware of the real issues of the operational MJO modelling.

Major Comments:

1. We believe the difference of different simulations are already carefully described. If the Reviewer believes that further details are required, please be specific. The physical details of IFS are available on Web, and the Web address will be provided in the final version. On more specifics,

i) The momentum dissipation is expected to suppress a "free dynamics", thus we expect that the MJO would also be enhanced by turning it off, if it is described as a free dynamics to some extent. This very basic point will be more explicitly stated in the final version.

ii) However, the most fascinating aspect of the result is that the change of MJO behaviour is hardly monotonous by simply turning off various moment-dissipation terms. In other words, the role of momentum dissipation is highly nonlinear in the MJO dynamics, as already suggested in the manuscript. This point will also be more clearly stated in the final version .

iii) I agree that convection in Fig. 4(a) is stronger than that in Fis. 4(b) and (c). This point will be explicitly stated in the final version.

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2. No ensemble run is considered in the present study. Every run is initiated with an initial condition for the operational standard run. Thus, the model is initialized by the most-likely state, and the resulting forecast is also the most-likely evolution under a given physical setting. We do not understand why an ensemble is important for the present purpose, because the most-likely evolution is the main result that we want to know, though ensemble information may provide supplementary information.

In the final version, the term "correlation analysis" will be replaced by "pattern correlation analysis", as suggested.

3. In Fig. 4(a), the most remarkable improvement is the clear-sky area behind MJO. A mechanism for this change is hard to identify, though the present Reviewer may like to speculate. Nevertheless, it does not prevent us from pointing out this most remarkable improvement. Convection associated with MJO is too strong with this setting, and this is hardly considered an improvement as dramatic as the clear sky.

4. As already remarked in response to the item 1, the effect of turning off a physical process is hardly linear, but the MJO evolves in nonlinear manner in response. This is just an example of such a nonlinear that turning off the convective friction leads to a sudden deterioration of the forecast towards the end of the forecast period. Data sampling may only artificially remove those nonlinearities that are actually present.

Minor Comments:

1. Against to what the present Reviewer suggests, there is no line for the CF in Fig. 4. In any case, such a line must be drawn somehow in a subjective manner, because the MJO is hardly a simple linear propagation process. In our opinion, it rather hinders us form more objectively see a change of the forecast by a change of physics.

2. This is a very good speculation to make: indeed, if the MJO is a free wave to a good extent, too strong convection will hinder a proper propagation tendency. This remark will be added in the final version.

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3. The y-axis here is correct. Note that the extended Mbb case is run for only 30∼days, as stated in Sec. 2.3. The figure caption will be modified for a better clarity in the final version .

4. Thank you for pointing us errors in figures. These errors will be corrected in the final version.

5. "Emission of an anticyclonic Rossby-wave train from the Eastern Pacific towards higher latitudes"

6. The verb "forecast" can be either "forecast" or "forecasted" in past participle form. According to whatis.techtarget.com: Although both are used, forecast is the preferred form.

7. Thank you for picking up a typo: "Wang et al. 2018" will be corrected to "Wang et al. 2019" in final form.

Finally, thank you for suggesting various additional references. However, it would be much helpful, if you could provide us full references, rather than just years and authors' names.

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