General Remarks

Both the Editor and the Referee #2 find that the manuscript does not yet present the important points well. In the current revision, we have much strengthened the points stated in the abstract and in the final section, while sharpening the narrative and the presentation.

The abstract has been completely restructured, in particular L5–11: "Contrary to the original motivation, emulating a free dynamics with an operational forecast model turns out to be rather difficult, because forecast performance sensitively depends on the specific type of friction turned off. The result suggests a need for theoretical investigations that much more closely follow the actual formulations of model physics: a naive approach with a dichotomy of with or without friction simply fails to elucidate the rich behaviour of complex operational models. The paper further exposes the importance of physical processes other than convection for simulating the MJO in global forecast models."

The discussion section has been revised and a new paragraph has been added (L397–409: see also L88–94 in the introduction).

Especially, the last two sentences of the new paragraph emphasise: "Though we are short of making any definite conclusions from our sensitivity study of the MJO on the momentum dissipation processes, the study suggests a critical importance of examining the physical sensitivities of a phenomenon with more detail rather than simply switching off the entire physical mechanism as has been done in past sensitivity studies." We believe that this is an important message that needs to be widely appreciated in the community.

Please also see the final paragraphs in L458–465.

We would like to thank the Editor and a referee for their comments and suggestions: we have made an extensive re-configuration of the text following the suggestions of the Editor closely.

We hope that the present revision is satisfactory to make the manuscript publishable.

In the following please find detailed responses to the comments and suggestions.

Individual Responses

Reply to Editor:

We would like to thank the Editor for his review and suggesting "to reconsider the present manuscript after major revisions".

We also much appreciate the comments by the Editor where he agrees with our claim that "a paper containing negative results could be valuable — if the work was clearly presented."

We further interpret an acceptance recommendation by one of the referees, even if without any remarks, to be a positive sign that our previous efforts in revising the manuscript have addressed some of the concerns. Unfortunately, another referee (Referee #2) is still negative, as the Editor also remarks, to which we respond below separately.

The Editor is inclined to agree with the critical referee that there continue to be many aspects of the paper that are unclear; in principle, a paper can be a valuable contribution even if it contains only 'negative' results; nevertheless, the present paper falls short of providing a clear presentation of a set of interesting numerical simulations that will be a genuine help to others who are planning this sort of work.

We have strengthened in response the main points of the paper as summarised both in the abstract and in the final discussion section.

The Editor concludes that the paper may be suitable for publication subject to further revisions. For this step, the Editor asked to consider carefully the comments of the referee as well as the Editor's own, to which we respond below. The Editor suggests to focus much more on a clear description of the simulations and what exactly can be deduced from them — perhaps with some discussion of how the simulations might be modified further to resolve some of the questions that are currently unresolved.

In revision, the abstract has been revised to make the key points clear and revised the discussion section with an additional paragraph (L397–409: see also L88–94 in the introduction). This paragraph explicitly points out an inherent limitation of existing mechanism–denial studies, while emphasising the importance of detailed sensitivity studies of specific physical processes, as in the present work. It would be an important guidance to those who are planning similar work in future.

Though not explicitly stated in the text, we also believe that many of the currently unresolved problems of MJO prediction can be addressed with more extensive, careful sensitivity studies, given more detailed physical constraints. The latter is particularly relevant

in the context of emerging tools based on machine learning for extracting "causal" MJO information from data.

On the other hand, we believe that the description of the simulations is already clearly presented. For this reason, we have mostly addressed the suggestions by the editor by reconfiguring the text as described below. A critical future direction is already stated in the previous version (L426–428): to repeat the present sensitivity study by a more elaborate numerical setup (with an explicit ambient state) under development (Kühnlein *et al.* 2019).

Two Particular Concerns:

The following major modifications have been made in response to the Editor's two particular concerns.

Firstly, more recent publications have been added as references. The Editor has kindly suggested several papers, and all of them are cited in revision. The two recent reviews (Zhang *et al.* 2020, Jiang 2020) are added (L25–26). As more specific references, Ma and Kuang (2016) is cited in discussing the difficulty of maintaining a relevant basic state when certain physical processes are turned off (L406–409). We have also emphasised (L425–428) a conceptual difference of our sensitivity study from the so–called mechanism–denial studies (*e.g.*, Kim *et al.* 2011, Ma and Kuang 2016). Furthermore, the question of interactions between the MJO and higher–latitude Rossby–wave activities is better placed in more general modelling contexts by citing Hall *et al.* (2017: L53–55).

In responding to the second major concern, the section plots have been collocated in revision based on the fields (OLR, 250-hPa stream function) and the periods (standard 20–day forecast, 40–day extended forecast, 20–day forecast from 1 Feb.): in all collocations, the first frame is the analysis field, thus it much facilitates comparisons of forecasts with the analysis.

DETAILED COMMENTS

Abstract:

L3: The editor suggests here that though the motivation was to seek support for the 'free wave' theory, no support is found. However, the precise conclusion is that perhaps the 'naive' approach taken here does not allow us to draw any conclusions on the theory of the MJO that has motivated this study.

L4-5, 'The reduction in friction tends to improve the MJO forecasts, but hardly in any additive manner.' : This sentence has been modified in revision to: 'The reduction of

friction sometimes improves the MJO forecasts, but without any systematic tendency.'

Main text:

L15: 'the pre-existing MJO' has been changed to 'a pre-existing MJO' as suggested

L16: 'especially, in' has been changed to 'especially in' as suggested

L21: "or short 'physics' hereafter" has been changed to "or 'physics' for short" as suggested

L24: 'Yano and Plant 2015' has been corrected to 'Plant and Yano 2015' as suggested

L24-25: We believe that references are still a metric used in evaluating science, so reviews cannot replace original work. Notwithstanding, the editor is right and we have added the references to Zhang *et al.* 2020, Jiang *et al.* 2020 as suggested (L25–26).

L39: 'dissipations' has been modified to 'dissipation' everywhere as suggested

L40, 'A classical work by Chang (1977) makes this point by invoking surface friction as a mechanism to slow down the propagation speed ...': Indeed, this is an important one of the earliest theoretical papers on the MJO, and also a good example to demonstrate how the role of friction has been considered to be important in the MJO dynamics. The main line of research on "convectively coupled waves" is already discussed in an earlier part (L24–26). Note that the purpose here is to establish the potential importance of friction in context of historical studies of MJO.

L45, 'When physical forcings are switched off from a model, an alternative mechanism for generating MJOs must also be considered.': This sentence was "puzzling" due to its unfortunate incompleteness. The sentence 'A shortcoming of the free–wave theory of the MJO is that it does not explain by itself an MJO initiation.' is added in revision (L47).

L49: The reference to Hall *et al.* (2016) is added in revision (L54–55), but as a reference for suggesting an importance of lateral forcing in simulating a MJO under an equatorial channel configuration. Note that this paper itself does not investigate a role of higher–latitude Rossby waves in the MJO dynamics in any explicit manner.

L57: The second question here has been modified in revision to "To what extent can the simulated MJO be interpreted in terms of free Rossby–wave dynamics?" In this context, the question of interactions of the MJO with higher–latitude Rossby waves also naturally arises as remarked over the paragraph of L70–75.

Table 1, 2nd row: The original description 'OFF selected or total physical tendency for the momentum (due to shallow and deep convection and the vertical eddy diffusion)' was rather confusing. However, change it to 'OFF selected for total physical tendency' does

not solve the problem, because tendencies are turned off either in selective manner or in totality. In revision, it is changed to 'OFF selected physical tendencies for the momentum (*e.g.*, shallow and deep convection, vertical eddy diffusion)'.

L67: As already responded at L57, the question 2) has been reformulated in revision.

L90, 'and it is likely also model specific': This non-essential remark has been removed in revision.

L103: Following the suggestions by the editor, the original lead sentences (L102–103) of this paragraph have been integrated into the paragraph L30–38 in the introduction. Though we considered to refer Virts and Wallace (2014), we found that this paper does not, in fact, use the stream function for their analysis.

L119: As suggested, this description (L118–120) has been integrated into the paragraph L30–38 in the introduction.

L122: This problematic sentence has simply been removed in revision.

L130: The phrase has been revised as "higher than the corresponding linear grid at the same spectral truncation".

L134: 'for the water substance' has been modified to 'for water substance' as suggested.

L136: 'the MJO in concern' has been modified to 'the MJO event considered here' by taking the suggestion

L140-141: 'how can we improve it' has been modified to 'how can we improve them', and 'in introduction' has been modified to 'in the introduction' as suggested.

L144-147: These two key questions here are simply whether it is possible to forecast those (two) key features found in the MJO evolution in these two forecasts cases. A phrase "on the other hand" (L155) has been added to suggest a relative separation of two questions, also in response to the Referee #2.

L175-180: The title of this subsection is re–named to "250-hPa Stream Function". Discussions about lower tropospheric rotational flow, etc are moved and merged to the beginning of Sec. 2.1.

L185: In revision, the section plots have been collocated based on the fields (OLR, 250-hPa stream function) and the periods (standard 20–day forecast, 40–day extended forecast, 20–day forecast from 1 Feb.): in all collocations, the first frame is the analysis field, thus it hopefully facilitates direct comparisons of forecasts with the analysis.

L195-205: The figure 2 (figure 6 in revision) has been revised following the suggestions.

L206-213: As the Editor correctly points out here, it is very difficult to interpret the plots of RMM as a measure of forecast skill. In fact, we believe that applicability of the RMM is much limited due to those inconsistencies (*e.g.*, mismatch of an initial point depending on forecast periods, etc). The point was already suggested in the previous version (L439–440: L448–449 in revision) by quoting Straub (2013), but it has been more explicitly stated in revision: "This design exactly becomes a key limitation of RMM" (L198–199); "However, afore–mentioned mismatching fundamentally limits the applicability of the RMM analysis in the following"(L215). Note that the RMM coordinates (patterns to project) are fixed in all the analyses. The real problem is in applying the RMM index, originally introduced to characterize the MJO in a long data set, to a short forecast run: the definition of temporal anomaly, to be projected, changes depending on the period considered.

L224, 'the model simulates those interactions features rather well' : To discuss forecast improvements by modified physics, the performance of a standard forecast (control runs) must first be established. In this case, all the forecasts considered simulate the interactions between the MJO and higher–latitude Rossby waves rather well, but without finding any noticeable improvements by modified physics.

L269, 'apparently in support of a free nonlinear-wave theory': This phrase has been removed as well as other similar phrases that may confuse the reader.

L402, 'failure of emulating the free dynamics ... maintaining a realistic mean state as detailed further below': A reference to Ma and Kuang (2016) has been added (L409) in revision. In the above sentence, "mean" has also been replaced by "background" in revision for consistency of the terminology. Note a subtle difference between "mean" (climatology) and "background" as discussed in revision (L425–428).

L421: Though conclusions regarding initial conditions, discussed here, are based only on two different initial conditions, we believe it worthwhile to mention, because, as already stated in the text, the finding is consistent with that of Kim *et al.* (2016), thus more likely to be generally the case than otherwise.

L441, 'The present study has also elucidated active interactions of MJOs with higher-latitude Rossby wave activities.': We believe that this statement itself stands with the present study, because the word "to elucidate" does not necessarily suggest any "systematic conclusions", which are missing as the Editor points out. The discussion has been much shortened in revision by removing remarks concerning 'westward dispersion of the original dipole' as well as 'Overall, forecasts of these interactions are found to be rather robust ...'.

Respond to the Referee #2

Overall opinion

As the Editor summarises, the present Referee thinks the paper is clearer, but that it is still difficult to find the important points and to find any connection between the scientific questions posed and the results obtained. For this reason, the Referee suggests rejection.

We are glad to know that the present Referee finds significant improvements in the manuscript by stating "scientific questions become clearer, and the explanations become more detailed."

However, the Referee also remarks that "the main results are not sufficient to support two scientific questions". Indeed this negative result was clearly stated in the beginning of the final section in the last version of the manuscript (L393–396 in revision): "the present study does not support the proposed free nonlinear Rossby–wave theory in any consistent manner. Details on the forecast behaviour based on the choice of physical configurations of the model have been carefully documented to record the unexpected but nevertheless important impact on MJO forecast skill."

The "main points" were also already clearly stated in the previous version of the abstract: "A change of the forecast performance rather sensitively depends on the type of friction turned off. The behaviour is in contrast to many theoretical studies based on a rather simple Rayleigh–friction formulation under a dichotomy of with or without. By reporting the details of those physical sensitivities on the MJO forecast, the present study suggests a need for theoretical investigations that much more closely follow the actual operational formulations of physics. An important lesson to learn from the study is an inherent difficulty to emulate a free dynamics with an operational forecast model. The study also exposes the importance of other physical processes than convection for simulating the MJO in global forecast models."

To address the difficulties and sharpen those important, main points better, the above has been modified in revision to (L6–11):

"Contrary to the original motivation, emulating a free dynamics with an operational forecast model turns out to be rather difficult, because forecast performance sensitively depends on the specific type of friction turned off. The result suggests a need for theoretical investigations that much more closely follow the actual formulations of model physics: a naive approach with a dichotomy of with or without friction simply fails to elucidate the rich behaviour of complex operational models. The paper further exposes the impor-

tance of physical processes other than convection for simulating the MJO in global forecast models."

The discussion section has been revised and a new paragraph has been added (L397–409: see also L88–94 in the introduction).

Especially, the last two sentences of the new paragraph emphasise: "Though we are short of making any definite conclusions from our sensitivity study of the MJO on the momentum dissipation processes, the study suggests a critical importance of examining the physical sensitivities of a phenomenon with much detail rather than simply switching off the entire physical mechanism as has been done in past sensitivity studies." We believe that this is an important message that needs to be widely appreciated in the community.

Please also see the final paragraphs in L458–465.

This together with the re-structuring of the text following the Editor's suggestions, directly address the main objection of the present Referee that "it is hard to figure out the main points": we believe that this is now resolved in the present version of the manuscript.

Major comments

1. L143-147 "From a dynamical point of view, this is before the anticyclonic activity begins to develop over the Indian Ocean (Fig. 1(d)). Thus the key forecast question is whether the model can predict the onset of this activity". The Referee points out that these sentences are "not logically connected" with the previous sentences. This is correct, because we are making two independent statements. For clarity, in revision, we have added "On the other hand" in the beginning of the second pair of sentences (L155).

2. L279-281: Here, we suggest merely from a morphological basis that "the emission of the Rossby wave energy from west during 22-28 January is suggested as a major source for initiating the anticyclonic signal associated with the MJO". The word "suggested" was probably too strong, which has been replaced by "speculated" in revision (L281). However, yes, we speculate that the Rossby–wave energy is the source.

3. L316-317: Here, "additive" means that we can reproduce the same result of a run turning–off two processes both A and B by adding changes form two runs tuning off processes A and B, separately. Our result shows that it is clearly not the case. Yes, one of the possible consequences is that longer runs will lead to different results.

4. L297: The sentence in concern has been removed in revision, also by following a general suggestion by the Editor (his comment on L269).

5. L337-343: The word "significant" has been removed in revision. The role of the Rossby wave in this process is merely our speculation, thus we have added the phrase "we speculate that" in revision (L338).

6. Second scientific question and Figure 9: As the present Referee correctly suggests, the idea "that the Rossby-wave train is really important in this event" is not substantiated in any concrete manner by the present study, and we do not make any definite conclusion. Discussions concerning Fig. 9 merely focuses on morphological aspects of simulations, thus the Referee's suspects and objections about the presentation do not actually apply. [The second question itself has been re–phrased by following a suggestion of the Editor in revision.]

Note that NQ, QF, and Ma experiments are important, because we base the experimental setups on the hypothesis in the introduction that by realising a state closer to the free dynamics, interactions between the MJO and Rossby waves would also be enhanced. In this respect, sensitivities identified in Fig. 9 are rather intriguing, although we are short of making any definite conclusions. [Longitude–latitude plots were extensively examined, but we found it hard to choose selective snap shots to make a point: simply there are too many things going on in a field.]

Minor comments:

1. L5:"but hardly in any additive manner": This phrase has been modified to "but without any systematic tendency"

2. L32: The paragraph of L30–38 has been substantially expanded in revision, also by following the suggestion of the Editor.

3. L73 & L197, 4. L75 & L184: Please note that the present manuscript follows the British spelling rather than American, to which the present Referee appears to be more familiar with.

5. Figs. 1, 4, and 5: Please note that periods are not the same for all the plots, thus depending on extents of the periods, the y–axis also appears differently for obvious reasons. An aesthetic aspect of this problem has been removed in revision by re–collocating all the plots by the experiment periods and the physical fields in concern.

60. L266 : Probably, the phrase "this overall aspect" was obscure. It has been changed to "the overall aspect" in revision (L267). Of course, it means the overall aspect on the sensitivity on the choice of momentum dissipation terms, as clearly remarked in the proceeding sentence.

7. Page 26: The Caption for Figure. 6 was indicated as “Figure. 5” by editorial mistake in the previous version. The mistake has been corrected in revision, as now seen as Figure 3.