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Comment on acp-2020-1312

Anonymous Referee #2

Referee comment on "Influence of sea salt aerosols on the development of Mediterranean tropical-like cyclones" by Enrique Pravia-Sarabia et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1312-RC2, 2021

This is an interesting modelling study about the influence of sea salt aerosols on the development of Mediterranean tropical-like cyclones (medicanes). The effects of different initialization times and spectral nudging are also investigated. The results were based on a large ensemble of 72 experiments, produced by the simulations of 3 medicanes (Celeno in 1995, Cornelia in 1996 and Rolf in 2011) x 2 aerosol approaches (prescribed and interactive aerosols) x 6 different initialization times x 2 nudging approaches (no nudging and spectral nudging). The use of English is very good and the abstract is concise. The figures are necessary and with good quality, although some minor corrections are suggested. However, I have some concerns about some points in the methodology and the depth of the analysis.

It is suggested that this article may be acceptable for publication after a number of major corrections is performed.

Major corrections:

1) lines 165-170: A) I agree with the notion of “compact set”. However, the authors have to justify the need of the compact set in this article, since it was not actually used here. The compact size was only shown in Figures 1 and S1-S6, but without being discussed or used. Moreover, it was not used in the summary diagram of figure 3, in which the individual times with a medicane were used. In the current form of the paper, I would suggest to remove the compact size calculation and description. The following 3 corrections (B, C, D) are relevant if the compact size is actually utilized and discussed in the paper. B) line 163: it is suggested to use “output steps (e.g. hourly)” instead of “time steps”, because they may be confused with the model time steps. C) In the formula of Q (line 167), why does m start from i+1, instead of i? (i) This is not properly defined when i=Nt. In this case, the sum starts from m=Nt+1 (i.e. outside the simulation), and (ii) when j takes its first value (i.e. =i) the sum is counted backwards (from m=i+1 to

m=j=i). D) line 166: why does j ends at Nt-1, instead of Nt? Why is the final time of the compact set not allowed to be Nt, even if there is a continuous period with a simulated medicane? This appeared in Figure 1, in the IA simulation with run-up time equal to 108 hours.

2) Discussion of figure 6: Does an azimuthally-averaged, radius vs height, plot of these variables (that takes into account the whole storm) produce the same differences (at least qualitatively) between PA and IA runs?

3) lines 264-266: No figures were shown in the article in order to support the physical explanation which is provided here. It is suggested to provide and discuss these results about the effects of spectral nudging.

4) Although section 5 states that this paper did not attempt to assess the simulated medicanes against the actual ones, I need to mention that (i) the medicane positions of PA runs in figure 2 seem to be closer to reality than the ones of IA experiments, since section 2.2 showed that Cornelia temporarily lost its structure before it moves over the Tyrrhenian Sea and strengthen again, and (ii) the tracks of Celeno in figure S12 do not agree with the track of the actual system in the literature. Therefore, some basic comparison of the simulations with the literature is necessary, because this is not an idealized modelling study.

Minor corrections:

1) line 62: The phrase "No physics suite (…) is used for the model run…" is confusing. It is suggested to state that no physics suite is widely accepted for the simulation of medicanes.

2) line 84: " Single-moment microphysics …"

3) section 2.1.1: Please provide the number of model vertical levels and the model top.

4) line 94: Please explain the acronym WPS geogrid, which is a technical term of WRF not known to the users of other models. Moreover, what data sources were used to define land use, soil category, topography and land-sea mask?

5) lines 118, 120, 122, 143, 217, Figure 1, etc.: It is suggested to use UTC, instead of GMT, throughout the manuscript.

6) line 130: Please define NOAA and any other acronym at the first time it is used in the manuscript.

7) line 138: It is suggested to use hPa, instead of mbar.

8) lines 142-143: Please provide the location and the time of Meteor's measurement and the time of the SLP measurement at northern Malta. Was this SLP measurement at Malta associated with the medicane or its parent low? The center of actual Celeno did not pass close to Malta.

9) lines 148-149: a) The sentence should become "The lowest model estimated atmospheric pressure …", b) I think that no such information about the lowest pressure of Cornelia is found in Pytharoulis et al. (2000) and Cioni (2014), c) The paper of Cavicchia and Von Storch was published in 2012. This correction must also be made in line 310.

10) section 2.3.1: Please specify the vertical layers and the radius used in the Hart diagrams. Was the radius constant (provide the value for each medicane) or variable? Although this information may be available in Pravia-Sarabia et al. (2020), some basic information must also be provided in the current article.

11) section 2.3.2: The intensity must be removed from the title of this subsection because it is not discussed in 2.3.2.

12) line 160, last word: Do you mean the number of time points, i.e. the number of output times? Please make this correction throughout the article, to avoid confusion with grid points.

13) line 161: The phrase "… support will be the total length calculated …" is not clear to me. The term "support" was not used in the article.

14) line 162: "... serves as an objective …".

15) line 182: "… for IA simulations without spectral nudging.".

16) Figure 1, 3, 4, 5, 6, S1-S12: The labels must be enlarged because they cannot be read in the printed version.

17) Figures 2 and S7-S12: It is suggested to overplot the track of each actual medicane.

18) lines 207 and 208: It is suggested to use "medicane duration" instead of "medicane tracks". Figure 3 shows the duration of medicane conditions and not the track's length. A longer duration does not imply a longer track (because the translation speed may change).

19) Figure 3, caption: it is suggested to replace "upper half" and "lower half", with "outer half" and "inner half", respectively, because the orientation of all ring portions is not the same.

20) lines 215-218: Please justify the choice of this case (Rolf) and this initial time (00:00 UTC, 5 November 2011) for analysis of the SSA-wind feedback in section 4.

21) line 224, figure 5: I think that the strongest effect in mid-low levels (800-500 hPa) equivalent potential temperature appears in the center and seems to be related to 'eye' dynamics.

22) Figure 4: The label "UTC+01:00" is not clear to me. Does this figure use local time? Why?

23) line 243: "... an ensemble of simulations has been …"

24) References: The link "https://doi.org" appears twice and must be removed from the references of Dafis et al. (2018), Gong (2003), Miglietta et al. (2013), Miguez-Macho et al. (2004), Pytharoulis et al. (2000).