

Atmos. Chem. Phys. Discuss., author comment AC2
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Reply on RC2

Enrique Pravia-Sarabia et al.

Author 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-AC2>, 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.

Thank you very much for your kind words and for the invaluable time spent in a detailed and insightful reading of our manuscript.

Major corrections:

- **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.**

There is a misspelling in the description of the indices prior to the formula definition. It should read "For each i in $1:(Nt-1)$, and each j in $(i+1):Nt$, find pair $[i,j]$ such that:". Thank you for noticing out.

Besides, we will keep the compact set definition and use it in the discussion, provided that a discussion of the track length is what it was conceived for in the first place.

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?

Yes, these differences are preserved if we do an azimuthally-averaged radius-height plot, as shown in the figure below. We will include these figures in the new manuscript version. Thank you very much for the interesting suggestion.

Please see Figure 1 in the supplement provided with this comment.

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.

A figure showing the time-averaged (over the times in which a medicane structure is present) vertical extent of the warm core of the pairs no nudging/spectral nudging simulations for both PA and IA are included below for Rolf (starting on 2011 Nov 05) and Cornelia (on 1996 Oct 05) simulations (Celeno is discarded provided that no nudging and spectral nudging simulations of Celeno reproduce a totally different system). From this figure, it is clear that the warm-core is not as well developed and continuous over the vertical direction in the nudging runs as in the no nudging ones. We hope this sufficiently proves that, although nudging simulations may produce "better" tracks, the vertical structure of the reproduced cyclone is not as well-developed as in the cases where no nudging is used. Indeed, for Cornelia it can be seen that spectral nudging cools the core in the 700 hPa level and breaks the deep convective structure.

Please see Figure 2 in the supplement provided with this comment.

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.

Both notes are interesting and worth a discussion. First, it is certainly true that Celeno tracks are far from real when simulated without nudging. However, a more robust and symmetric MTL structure is formed in this simulations. It would probably mean an entire second paper to address this fact and provide a feasible explanation on why this happens, since no easy interpretation seems to exist. Besides, literature suggests the structure break of Cornelia medicane before reaching the Tyrrhenian Sea. But, in fact, only the eye-like feature seems to have been lost (see Cavicchia and Von Storch, 2012 and Mazza et al, 2017), contrarily to what we wrote in section 2.2, and we will correct. Indeed, it seems like Cornelia did not entirely lost its tropical-like nature.

Moreover, while it is true that a discussion of the tracks divergence from the "real" ones may be interesting, we found at least three problems that made us not including it: i)

NOAA Tropical Bulletin Archive is available from 2007 to present. Thus, Celeno and Cornelia are not contained on it. ii) We were not able to find any paper providing an objective reproducible manner of tracking medicanes from satellite images. iii) We found no paper providing the tracks of these three medicanes. iv) As we argued in the response to Referee 1, it doesn't seem like a valid approach to use three tracks from three different works obtained with different methods.

Hence, in order to introduce the discussion suggested by both Referee 1 and 2, we have included in the KDE figures the tracks of the three medicanes obtained as a result of running the tracking algorithm on the ERA5 reanalysis dataset. These tracks will serve as reference for the discussion we will introduce in the new manuscript version.

Please see Figure 3 in the supplement provided with this comment.

Minor corrections:

- **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.**

You are right. Thank you for your comment.

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

What we meant here is that when single-moment microphysics is used, the effect of an interactive aerosol calculation is ignored, and no coupling is allowed by definition of a single-moment approach. We will include the sentence "Double-moments microphysics (progn=1) has been used for all the simulations object of this work" in the next manuscript version.

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

The model top is fixed at 1000 Pa, and the model vertical levels are 40 (not fixed by us, but instead selected by the model).

4) line 94: Please explain the acronym WPS geogrid, which is a technical term of WRF not known to the users of other models.

You are right. Thank you for the comment.

Moreover, what data sources were used to define land use, soil category, topography and land-sea mask?

The data sources are the ones downloadable from the WRF Users Page (WPS V4 geographical static data downloads), accessed on 4th December 2019.

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

Right, thank you.

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

Thank you for pointing it out.

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

Right, thank you.

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.

You are right, we will review this specific data and correct them as needed.

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.

We will take these notes into consideration for the next manuscript version. Thank you for your recommendations and comments.

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.

The radius was variable, calculated as the mean distance from the center to the zero-vorticity line in eight directions, with a minimum of six non-infinite directions. The vertical layers were 900-600 and 600-300, being the vertical levels in which the thermal wind parameters is calculated: 900, 850, 800, 775, 750, 725, 700, 675, 650, 625, 600, 550, 500, 450, 400, 350, 300.

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.

That is absolutely true. Thank you for noticing.

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.

Yes, we will make the correction, thank you.

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.

We will try to use a more accurate term. Thank you for pointing out.

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

Yes, thank you.

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

Yes, thank you.

16) Figure 1, 3, 4, 5, 6, S1-S12: The labels must be enlarged because they cannot

be read in the printed version.

We are aware of this. Thank you for pointing out.

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

As mentioned before, we have overplotted the medicane tracks as a result of running the tracking algorithm with the ERA5 reanalysis dataset.

18) lines 207 and 208: It is suggested to use "medicane duration" instead of "medicane tracks".

Thats right.

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).

When we use the term "track length", we refer to its temporal length, which seems to be an equally valid and frequently used measure of its duration. Thank you.

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.

Your recommendation will be implemented in the new manuscript version. Thank you.

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.

This case accomplished the best equilibrium between robustness and long-lasting MTLC structure among all cases. Besides, it has been the most widely studied medicane in the literature. The initial time was chosen provided that it produces the longest-lasting and most intense MTLC structure.

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.

Right, thank you for the note. We will study this in more detail.

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

It is an error; it should read UTC+00:00 and will be corrected in the next manuscript version.

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

Yes, thank you.

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).

Right, thank you.

Please also note the supplement to this comment:

<https://acp.copernicus.org/preprints/acp-2020-1312/acp-2020-1312-AC2-supplement.pdf>