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Interactive comment

Interactive comment on "Modelling a tropical-like cyclone in the Mediterranean Sea under present and warmer climate" *by* Shunya Koseki et al.

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Review of the article "Modeling a tropical-like cyclone in the Mediterranean Sea under present and warmer climate" by Koseki et al.

I read the article with great interest and I found the methods and the topic timely and important. I believe that the paper is adding to our understanding of medicanes under climate change and I support the idea of being eventually published in NHESS. Nevertheless, I have several major concerns about the content, the presentation and interpretation of the results. I hope that several of my comments below will be helpful to improve the paper.

Major comments 1) My first major concern is on the definition and interpretation of

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Discussion paper

the results. Throughout the introduction it is given the impression that medicanes are sharing same dynamics with tropical cyclones. However, this is not the case at least for the majority of cases. Therefore, I strongly suggest to the authors to revise especially the introduction as well as other parts of the paper, taking into account the following comments:

Lines 90-92: Please note that the detection of cyclones through a cloudless "eye" is a phenomenological criterion and lacks of physical content. Up to now all well known medicane cases are only defined using this subjective, arbitrary criterion. Physical criteria have been used earlier, e.g. by Cavicchia et al. (2013) and recently by Zhang et al. (2020). Nevertheless these criteria include Hart diagrams, wind speed and pressure gradients and thus they are highly dependent on the dataset properties (e.g. resolution as it was stressed by Gaertner et al., 2018). At this point, I strongly suggest to discuss the lack of physical content in the definition of medicanes (please also refer to next comments).

Lines 103-106: An intrusion of trough-like systems or cut-offs over the Mediterranean is a typical event that precedes the formation of medicanes. This is also mentioned in the cited publications of Fita et al., 2006 and Chaboureau et al., 2012 (line 106), but also the more recent ones of Bouin and Lebeaupin Brossier (2020) and Fita and Flaounas (2018). Consequently, medicanes are subject to a baroclinic forcing as other extratropical cyclones. This is also discussed in the results of Fita et al., (2006) and Chaboureau et al., (2012). In fact, the formation of medicanes is not expected to be different from other intense Mediterranean cyclones (Flaounas et al., 2015). This is an important difference from tropical cyclones along with the SST difference from the empirical threshold of 26C (as correctly stressed in lines 97-102). Both of these differences should be discussed along with the fact that there is no physical criterion to qualify a Mediterranean cyclone into a tropical-like system.

Line 109. Please note that Fita and Flaounas (2018) show that deep convection took place while the cyclone was asymmetric and cold core. Moreover, the mature stage of the cyclone coincided with absence of deep convection or at least with weaker convection than in its initial stages (i.e. during cyclogenesis, when it was a "cold core" system).

Lines 109-114. Please revise this part. Miglietta and Rotunno (2019) show that air-sea interactions are important for the development of only one out of the two analysed medicanes. Similar results were also reached by Carrió et al., (2017) for another case of medicane. In fact, Miglietta and Rotunno (2019) discuss that out of three "kinds" of mechanisms for the formation of medicanes, only one is related to WISHE.

Line 132: I believe that Cavicchia et al., (2014) performed their analysis using a simulation of 10 km of resolution. /if so, please revise.

Line 149: Is it possible to acquire additional information from the fact that Rolf is the first cyclone followed by NOAA as a tropical-one in the Mediterranean? Does it mean for instance that no other cyclone or Medicane before Rolf is to be considered as a tropical-one (at least by NOAA)? How many other Mediterranean cyclones were followed by NOAA after Rolf? Is the NOAA's criterion for tracking tropical cyclones also phenomenological (e.g. tracking spiral clouds in satellite pictures), or does it implicate physical criteria?.

Line 147: I strongly suggest to explain in more detail why Rolf was chosen. Actually the cited studies show a very important presence of deep convection in its centre. In addition, Rolf was related to a rather weak upper tropospheric disturbance. This comes in contrast to other medicanes. Rolf is indeed a far "better" candidate to be considered as "tropical-like", (in the sense that Rolf may unlikely be subject to baroclinic forcing and more plausibly it was driven by convection, thus complying with the WISHE mechanism). Such an entry in the text would make a reasonable connection with previous parts of the introduction on the still uncertain physical definition of medicanes, but also with the validity of the interpretation of the results in the context of climate change (see major comment #4).

Lines 241-242: Please note that Fita and Flaounas (2018) show that warm core and axisymmetry may be achieved due to warm seclusion and not due to the development of convection. This suggests that convection or WISHE could not sustain the cyclone on itself, i.e. tropical transition does not apply to that case study. This is also discussed in Miglietta and Rotunno (2019). Please revise.

To summarize, I suggest to explicitly mention that all known medicanes, if not most, are identified using arbitrary, phenomenological criteria such as the observation of a spiral cloud coverage and a cloudless "eye". Many of these known cases, as shown in previous studies, are not sharing similar dynamics with tropical cyclones in the sense that an upper tropospheric forcing is potentially strong. It is thus important to mention why Rolf is different and how representative it is, when compared to other medicanes (or other intense cyclones).

2) My second major comment goes on the use of English. In several parts, language is understandable but in many parts it is quite familiar and its overall level must be improved. Several minor comments below point towards this direction highlighting several awkward phrasings.

3) After reading the paper, my impression is that the size could be substantially reduced. In fact, I strongly suggest a relatively strong editing by reorganising the two main sections. It seems that paragraphs in sections 3 and 4 are each devoted to a single variable. Both of these sections include a rather long and continuous text where the detailed description of the figures is difficult to be retained. In addition, the focus of the results is often alternated between the different experiments and between ERA5 to PRS. I propose to insert more subsections and to provide to these subsections a content which is based on physical mechanisms rather than physical variables. After all, several paragraphs -especially in section 4- tend to point to the same conclusion, but from the point of view of different variables: how and when the medicane tends to attains a more or less tropical-like structure. Finally, I suggest to omit ERA5 throughout section 3. This would make reading more straight forward.

4) My final major comment goes on the interpretation of the results in the context of climate change. Main results show that higher SST drives Rolf to become stronger, while drier atmosphere is weakening the cyclone. However, as shown in previous studies, upper tropospheric disturbances are constantly interacting with medicanes (as it happens for other intense Mediterranean cyclones). These upper tropospheric systems are usually products of wave breaking over the Atlantic and therefore, the future of Mediterranean cyclones strongly depends on large scale circulation. In addition, the Atlantic Ocean functions as a major source of water vapour (Flaounas et al., 2019) for Mediterranean cyclones and this is not taken into consideration here. Indeed, the boundary conditions only prescribe a background value of relative humidity and not whether water vapour transport towards the Mediterranean will be more (or less) significant in future cyclogenesis events. Therefore, I suggest to be more precise that the results may only relate current cyclones with a background forcing of climate change, rather than reflect the future dynamics of medicanes. However, I find it interesting to stress that Rolf seems to be a system that is least affected by large scale circulation. Consequently, understanding the background forcing of climate change on Rolf's development is of crucial interest for other similar medicanes that might occur in the future.

Minor comments: Line 121: misses "et al"

Line 179: "Miglietta"

Lines 260-276. This paragraph is very detailed and the reader's focus is somewhat shared between NOAA, ERA5 and WRF. I guess that WRF's accuracy in reproducing the track is the important message. I suggest you shorten dramatically this section by providing the most important information as supported by the figure.

Lines 282-283: "develops more vertically", awkward phrasing, please rephrase.

Lines 284-285: I am not sure that cyclone phase diagrams are anyhow related to cyclones intensity. Please explain better this part.

Line 291: The terms presented in Fig. 4 are representative of warm/cold advection and thus they are both expected to be very sensitive to models horizontal resolution. I am not sure if the phrase "stronger warm core" has a "solid" physical interpretation, or if observed differences are mostly due to resolution differences. Would it be more fair to say that PRS reproduces Rolf in a way that cyclone phases match accordingly the ones of ERA5?

Lines 277-293: I am not sure if Figures 4a and 4c provide more information than the ones provided by this paragraph.

Line 304-305: What is meant by "development of the cyclone"? For the period of 3 of November and until the 8 of November, the SLP and latent heat in Fig. 5 seem to be correlated in PRS. Shouldn't an increase of latent heat lead to a stronger cyclone due to a stronger convection and therefore to a decrease of SLP as in PGWSST? Does this mean that Rolf is not behaving as a tropical cyclone (i.e. does not comply with the WISHE mechanism) and thus another physical agent is driving its intensification.

Line 315: "huge amount". Is it possible to quantify this result and compare it to values of previous studies of Mediterranean cyclones and/or other cyclone categories? Is it more than normal? Is it comparable to cyclones developing over open oceans.

Line 341: Make landfall.

Lines 329-352. This large part of section 4 is thoroughly descriptive. It could be shortened by presenting directly the most important differences. After all, the track is also described in the previous section.

Line 353: Figure 7a shows...

Line 358: What is meant by "strength of deepening"?

Line 358: If Figure S1 (also for S2) is indeed important for the presentation of the results then please move it to the main article.

Line 360-361: "warmer climate tends to deepen the centre of the medicane". Please relate cyclones intensity with processes. Also this statement is contradictory with the results in Fig. 7a. It is not the deepening rate or minimum SLP that is different, but the gradient of SLP.

Lines 362-363 and 374: Awkward phrasing, please rephrase.

Line 379: This conclusion seems to overgeneralise the situation where a drier atmosphere is weakening a cyclone and a warmer SST is intensifying it. I suggest to rephrase (see also major comment #4).

Line 381: Figure 7b shows...

Line 385: "Correspondingly to the more rapid decay of the cyclone". Awkward phrasing.

line 385 and throughout the manuscript: "much more". Please quantify your results and compare them to other experiments or previous studies.

Line 392: here and elsewhere (e.g. line 414) what is meant by "inactivated"?

Line 398: Maybe it would be better to move the entire presentation of PRS in the previous section?

Lines 399-400: Could you please verify with the model outputs?

Line 407: "amplitude". Please change to amount; "much larger", as previously mentioned quantify your results and put them into context e.g. by comparing with previous studies. You may compare results of 7c with Figure 8 from Flaounas et al. (2019). It seems that the 2.7 mm/h places Rolf indeed as an outlier system when compared to other intense Mediterranean cyclones (maybe this information is also useful for the introduction).

Line 409. It is here (and in other lines, e.g. 442) quite clear that S1 is important for the presentation of the results. I suggest you move it into the manuscript.

Line 413: "along the cyclone track", or "during cyclone lifetime".

Lines 416-417: Familiar language.

Line 421: I suggest you show the 95th or 98th quantile of wind speed of all grid points within the 250 km radius. This is more objective and will also smooth the plot; In the caption of Figure 7d: "250 km".

Line 434-437: This part was difficult to understand, please clarify. Also please rearrange the narrative or the order of figures so that the important conclusions are complete.

Lines 441-442: I am not sure I understand how warm or cold core (i.e. temperature advection in cyclone phase diagrams) is related to intensity. Is there a straight forward relationship between thermal advection and cyclones intensity. Does for instance the same stand for extratropical cyclones?

Lines 443-456: I am not sure I understand this part. Language could certainly be improved.

Line 473-474: How is size defined? Actually, I am not sure that I understand how the size is related to cyclone phase diagrams. Continuing my previous comment, cyclone phase diagrams correspond to a rather simplistic diagnostic about cyclones core being warmer or colder than its surrounding. However, these diagrams are used here to interpret cyclone dynamics and relationship with other variables. I understand that there are underlying mechanisms that force cyclone phases to coincide with e.g. peaks of precipitation. Could you please be more analytical on these mechanisms.

Line 475-476: This is a very arbitrary comment. I suggest to remove it.

Line 477: Please correct caption of Fig. 10 ("maximum")

Line 487: "similar" instead of "identical".

Lines 485 & 496: "Vigorous". Please rephrase; also avoid familiar language throughout the text. Such phrasings are open to interpretation. Maybe rewording could help in guiding the reader to focus on the figure details that merit more attention and better support the results, "e.g. the areas where precipitation exceeds XX mm is more narrow in PGWSST and perfectly encircles the cyclone centre. On the other hand, in PGW...".

Line 491-492: Phrasing gives the impression that there is only an arbitrary observation.

Line 497: "still survives". This is only a time frame of rainfall spatial distribution. What if in later or later times the rainfall is more symmetric but weaker? (e.g. Fita and Flaounas, 2018).

Lines 499-500: Does this mean that Rolf as in PRS may not be classified as a hurricane? Actually the whole paragraph from 477 to 500 seems to be based on arbitrary observations. This seems more appealing to a discussion section. I would suggest to use parts of the text for discussing earlier paragraphs.

Line 508-509: Awkward phrasing.

Lines 500-526: This part introduces a new variable (CAPE). It seems to be a continuation of the same motive as in previous sections, i.e. every paragraph is devoted to a single variable. In these lines, the text is very descriptive, lacks of quantification of the results and includes many arbitrary observations. In addition, use of English should be improved.

Line 530: Background humidity is identical only in the boundary conditions but not in the centre of the cyclones in the two experiments. Therefore I do not believe that there can be such a straight forward interpretation of the difference between the two experiments.

Line 536: Please remove "feedback".

Lines 527 to 537: You may omit this part. It basically describes the WISHE mechanism.

Line 537: "consumes CAPE more rapidly": This is not shown in the figures. Also I am not sure that I understand why this "indicated that the WISHE mechanism works more effectively".

Line 545: "inhibit", maybe "reduce"?

Emmanouil Flaounas

Athens, 20 July 2020

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