Reply to Reviewer#2, Dr. Emmanouli Flaounas
by
Koseki, S., Mooney, P. A ., Cabos, W., Gaertner, M. A.,
de la Vara, A., González-Aléman, J.-J.,

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.

We are very grateful for the many detailed and constructive comments in your review. We have made every effort to address these helpful comments and we believe that this has greatly improved the manuscript. Our responses to your comments are in blue for clarity.

Following the reviewer#1's comments, we have re-done the simulation wth 6 ensemble members. Consequently all plots have been also re-made and the descriptions on some figures have been re-written. Please note that these rewritings are shown in **red** in the revised version of manuscript.

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

We have modified and added the following text from lines 90-99:

"It is well known that severe cyclonic storms occur in the Mediterranean Sea, in particular, from September to March (e.g., Cavicchia et al., 2013). They generate large amount of precipitation and intense winds that severely damage regional economies and infrastructure over the coastal areas in the Mediterranean nations (e.g., Bakkensen, 2017). Although these cyclonic systems have a clear societal importance, methods to robustly detect these cyclones via physical criterion (e.g., Cavicchia et al. (2013) and Zhang et al. (2020)) have remained elusive (Gaertner et al., 2018). The cyclonic systems are typically detected via phenomenological criteria

such as the cloud-free "eye". Consequently, most known cyclonic storms in the Mediterranean Sea develop into meso-scale cyclones with a cloud-free "eye" around the cyclone centre, which is also a common feature of tropical cyclones."

***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.**

We have added the following text from lines 100-118:

"These tropical-like cyclones are called Mediterranean hurricanes or medicanes (this term is used hereafter). Although there are many similarities between medicanes and tropical cyclones, there are also clear differences between them. Firstly, the lifetime of medicanes is shorter than that of most tropical cyclones. Secondly, the development of tropical cyclones generally requires that sea surface temperatures (SSTs) exceed the empirical threshold of 26°C. However, SSTs in the Mediterranean Sea are almost never this warm with autumn and winter SSTs varying from around 18°C to 23°C in the current climate (e.g., Shaltout and Omstedt, 2014; Fig. 2a). This is thus much lower than the empirical threshold of 26°C for tropical cyclone formation and the occurrence of tropical cyclones over such cold SSTs are very rare even in the tropics (cf. Pacific and Atlantic cold tongue, e.g., Jin 1996; Caniaux et al., 2011). Another difference between medicanes and tropical cyclones is that the formation of medicanes is generally preceded by an intrusion of trough-like systems or cut-off lows over the Mediterranean (Fita et al., 2006; Chaboureau et al., 2012; Fita and Flaounas, 2018; Bouin and Lebeaupin Brossier, 2020). In particular, Fita and Flaounas (2018) suggested that some medicanes show hybrid features of tropical and extratropical cyclones, which is more similar to subtropical cyclones (cold core and shallow convection at the mature stage). Consequently, they are subjected to baroclinic forcing like extratropical cyclones (Fita et al., 2006; Chaboureau et al., 2012). As such, it is expected that the formation of medicanes is not different from other intense Mediterranean cyclones (Flauonas et al., 2015), and it should be noted that there is no physical criterion to quantify 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).

We have added the following text from lines 112-114:

"In particular, Fita and Flaounas (2018) suggested that some medicanes show hybrid features of tropical and extratropical cyclones, which is more similar to subtropical cyclones (cold core and shallow convection at the mature stage)."

Lines 109-114. Please revise this part. Miglietta and Rotunno (2019) show that airsea 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.

Thank you so much for pointing it out. We have revised the texts from lines 123-130:

"While Miglietta and Rotunno (2019) showed the importance of air-sea interactions for one medicane out of the two studied, they also suggested that the other case medicane is maintained mainly by mid-latitude baroclinic environment (air-sea fluxes and latent heat flux still help to develop the medicane). This aspect is also suggested by Carrió et al. (2017). These discrepancies on the importance of air-sea interaction in the literature may arise from the dependency of the various works on individual case studies. In particular, the importance of air-sea fluxes can be related to wind-induced surface heat exchange (WISHE) mechanism similar to tropical cyclones (e.g., Emanuel, 1986; Miglietta and Rotunno, 2019)."

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

It is correct that Cavicchia et al (2014) used 10 km grid spacings. However, the text (previously on line 132) refers to global modelling studies while Cavicchia et al. (2014) use a regional climate model. Consequently, we have not revised the statement about the limitations of Coupled GCM simulations arising from their grid spacings. So, we added Cavicchia et al. (2014) as an example of the study of medicanes with a high-resolution regional climate model. Please see lines 153-154.

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?.

We re-consider this part and decided to remove the description on it. Instead, we added more scientific details on Rolf as in response to the other comment. Please see lines 163-174.

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).

Our review of the literature suggests that Rolf is one of the more intense medicances wth long-lasting tropical-like characteristics. Additionally, Rolf occurred around the Balearic Islands where many medicanes are generated. That is our main motivation to investigate Rolf and its future change. We have revised the texts from lines 166-174:

"Since Rolf was a highly destructive medicane for coastal communities in many Mediterranean countries and is one of the most intense medicanes (e.g., Dafis et al., 2018), it is important to assess how these types of medicanes will respond to climate change in near future. Medicane Rolf generated the deep cumulus convection and persisted with tropical cyclone-like characteristics longer than other Mediterranean storms and vortices (e.g., Miglietta et al., 2013). Moreover, according to Miglietta et al. (2013), Rolf occurred around the Balearic Islands, which is a hot spot of medicane genesis. Therefore, it will be interesting and important to investigate the impacts of climate change on this type of Mediterranean storm."

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.

We revised that part. Please see lines 274-275.

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).

As outlined above, we have included a more detailed discussion on medicanes in response to the comments. According to Miglietta et al. (2013), Rolf is one of the more intense medicanes and it showed a tropical transition. We also added this rationalization for choosing Rolf as our case study. Please see lines 166-174.

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.

We have re-read our manuscript more carefully and corrected expressions in the reviewer's minor comments.

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.

Thank you so much for the comment. For the mechanism showing the plots of precipitation, latent heat flux, CAPE, and wind speed, we explain and discuss why Rolf is modified by different background in sections 4, 5 and 6. Since Rolf has more tropical-like features (deep warm core and deep convection), such plots are helpful in order to explain its mechanism of formation and maintenance. This study pays more attention to how the medicane will be changed more than its fundamental dynamics of development since previous studies have shown the dynamics of medicanes. However, in response to the other comment, we moved Fig.S2 (OLR) to Fig. 12 and provide more discussion on the differences in deep cumulus convection associated with simulated Rolf and our simulations in section 5, in combination with the discussion on CAPE. We believe that this revision gives more physical aspects. Please see lines 536-570.

On the other hand, we agree with the fact that our paper is a bit repetitive and the part of ERA5 could be omitted. However, we recognize that some readers may prefer to see the realism of our PRS simulation. Therefore, we have moved the figures of ERA5 to supplemental information and substantially reduced its description.

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

We added the following texts to emphasize "background change". Please see lines 619-627.

"The PGW technique is a powerful tool to investigate the impacts of climate change on the weather systems in the future. However, our results in this paper include only the climate changes in background such as temperature, relative humidity, SST and etc. In this framework, any changes in extratropical dynamics like wave breaking and large-scale circulation as a source of medicanes are not directly considered. Additionally, as Flaounas et al. (2019) suggest, the water vapor transport from the North Atlantic sector will be modified and significantly influences the medicane frequency and intensity. The PGW approach does not reflect directly such future change in water vapour transport. Nonetheless, we can conclude that the background change associated with global warming will have some impact on the medicane development."

Minor comments: Line 121: misses "et al"

Line 179: "Miglietta"

Corrected.

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.

Following major comment#3, we moved ERA5's figure to supplemental information and shortened section 3 more focusing on WRF's ability to reproduce Rolf. Please see the entire section 3.

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

Since Figure 4a has been removed (replying to other minor comments), this expression has also been removed.

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

Since Figure 4a has been removed (replying to other minor comments), this expression has also been removed.

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?

We rephrased it. Please see lines 304-305.

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

We agree. These figures have been removed from the manuscript since Fig.4b and d already provide sufficient information on cyclone phase.

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.

Before the SLP deepening around 00UTC-08, the latent heat flux is the strongest (after 00UTC-07) and precipitation is maximum around 20UTC-07. This could indicate that (1) the cyclone gains more water vapour and that (2) diabatic heating due to condensation provides energy for cyclone development. This is similar to the development of tropical cyclone. Please see new Fig.6.

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.

According to Miglietta and Rotunno (2018), the latent heat flux of the October, 2006 case is 1800 W/m^2 at the cyclone peak. Their other case of December, 2005 has a value of 1000 W/m^2 at the cyclone peak. In our case, at the cyclone peak, the value is about 740 w/m^2 in the 6-member ensemble.

However, this difference could be due to the geographical location of the cyclone. Their cyclones' centres of October,1996/ December, 2006 locate 38N-39N/34-35N which is more southern than Rolf (November, 2010), our case, medicane Rolf (at the peak, its latitude is 41N-42N). When the cyclone is located more southerly, the dry air can advect from the African continent and evaporation will be enhanced effectively. The underlying SST is also warmer near the African continent (the cases of Miglietta and Rorunno, 2018) than near Europe (our case).

We added this quantification and discussion. Please see lines 320-324.

Line 341: Make landfall.

Corrected.

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.

Because the plots have been remade in order to include the 6 ensemble simulations, this part has been re-written drastically to describe the remarkable differences among tracks. Please see lines 350-375.

Line 353: Figure 7a shows...

Corrected.

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

We meant the SLP gradient here. The sentence has been rephrased. Please see lines 382.

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.

As suggested, we have moved Fig.S1 to Fig.7. However, Fig.S2 is more useful for the discussion on difference in cumulus convection in Section5 in addition to CAPE. This revision is related to the reviewer's major comment#2 and other minor comment below. Please see lines 619-627.

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.

Here, we used "deepening" as SLP gradient. But, this terminology is wrong. In the ensemble simulations, SLP is slightly lower in PGW_{ALL} than in PRS, but still almost same (please see new Fig.6a). And, the SLP gradient is much stronger in PGW_{All} than in PRS (please see new Fig. 7).

To avoid misusing the terminology, we rephrased it in lines 383-386.

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

This statement has been removed from the manuscript as it does not add any value to the presented results.

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

As replied to major comment#4, we agreed that our study showed the impacts of changes of the atmospheric and oceanic background associated with global warming on medicane. Therefore, here, we have modified the text to emphasises "background" Please see line 401-402.

Line 381: Figure 7b shows...

Corrected.

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

From the ensemble simulations, the decay rate of latent heat flux in PRS and PGW_{ALL} are almost identical. Therefore, we removed this sentence.

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

As stated in the previous comment, we removed the sentence including the expression here. Also we added more quantifications throughout the manuscript.

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

We have replaced this word here and elsewhere in the revised version of the manuscript by clearer descriptions.

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

We agree on this. This part has been moved to the previous section. Also, the description on maximum wind speed in PRS has been also moved to the previous section. We modified Fig.4 by adding precipitation. Please see lines 324-338.

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

In this study, we do not aim to investigate the fundamental dynamics of Rolf's development and we understand that the mentioned statement was too speculative. Therefore, we changed our description there. However, the initial intense rainfall can still be related to the initial disturbance. Please see lines 327-328. Also, we added a figure of SLP and precipitation (for this reply) averaged from 00UTC to 12UTC on 6th-Nov in PRS's ensemble mean.

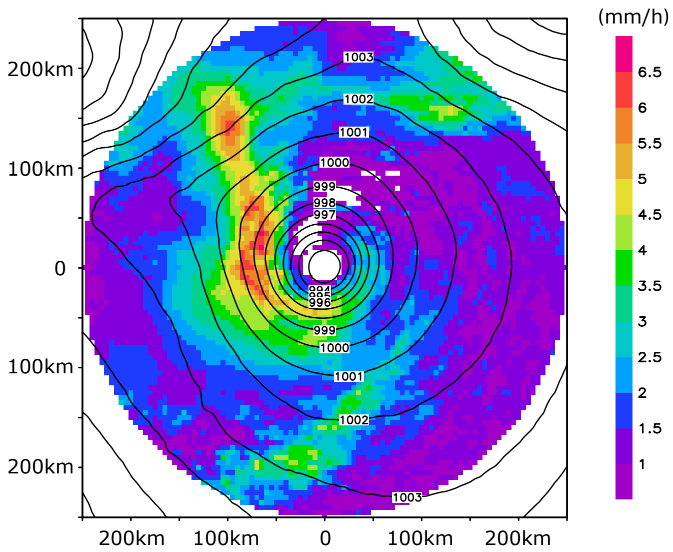

Figure R1. SLP and precipitation averaged from 00UTC to 12UTC on 6, Nov in PRS ensemble.

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).

Corrected. Thank you so much for the useful suggestion. That point is very interesting. Actually, 2.7mm/h (in ensemble, 2.6mm/h) is for PGW_{SST} and PRS has 1.5mm/h. These values are equivalent to 31.2mm/12h and 18mm/12h, respectively. Both are located in the "intense" spots shown in Fig.8 of Flaounas et al. (2019). We added a brief discussion on this. Please see lines 428-432.

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.

We moved S1 to Fig.7 in the revised version of the manuscript.

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

Corrected.

Lines 416-417: Familiar language.

Corrected.

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".

Thank you so much for the suggestion. We re-plotted that figure by showing averaged winds exceeding 95th percentile of hourly data in each simulation. Please see new version of Fig. 6d.

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.

This paragraph has been rephrased:

"In PGW_{ATMS}, during 06 and 07-Nov, the MWS is stronger than that in PRS. However, after 0000UTC on 08-Nov, the MWS in PGW_{ATMS} is weaker than that in PRS resulting in a smaller maximum amplitude of MWS during the cyclone tracking in PGW_{ATMS} is smaller than in PRS (21m/s for PGW_{ATMS} and 24m/s for PRS). In addition, as seen in Fig. S2, the ratio of grid boxes with weaker wind speeds (category of 5 to 10m/s) is larger in PGW_{ATMS} than in PRS (in particular, 12UTC-07 and 08UTC-08). That is, the area of strong winds is much smaller in PGW_{ATMS} than in PRS (the horizontal distribution of winds will be given in Fig. 9)."

Please see lines 448-455.

For this revision, we added new Fig.S2 showing a probability density function of wind speeds in PRS and PGW_{ATMS}.

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?

Our argument was too speculative without any concrete evidence. Therefore, we removed that sentence from the manuscript.

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

In the original manuscript, we described each cyclone phase space in details. However, in the revised manuscript, we show 6 lines in each simulation (please see new Fig.8). Thus, we avoid describing unnecessary details and focus more on the overview of differences among the simulations. Please see lines 456-472.

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.

Regarding the size mentioned here, we meant that the radius of maximum wind speed is smaller in PGW_{ATMS} than in other simulations. In response to an earlier comment, we added new Fig.S2 showing histograms of wind speeds. We rephrased the text with New Fig. S2. Please see lines 452-455 and 485-487.

Regarding the relationship between cyclone phase space and intensity, as replied to other comment above, our argument was too speculative and sufficient evidence. Therefore, we removed that sentence from the manuscript.

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

Removed.

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

Corrected.

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

Done.

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...".

We re-read again carefully and familiar phrasings have been corrected.

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

This sentence has been removed.

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).

We agree. This statement has been removed.

Lines 499-500: Does this mean that Rolf as in PRS may not be classified as a hurri- cane? 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.

This statement has been removed from this section and we moved Fig.S2 to Fig.12 (OLR in each simulation) to section 5 in order to discuss more details on differences in the simulated medicane. Please see lines 536-562.

Line 508-509: Awkward phrasing.

Rewritten to: "*In this section, we discuss the roles of the atmosphere and the ocean in the medicane's response to future warming"*

Please see lines 514-515.

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.

We added a new OLR figure (Fig.12) to this section to provide a more insightful discussion on the differences among medicanes in each simulation. In particular, OLR is a reasonable indicator for changes in deep cumulus convection associated with this medicane. Please see lines 536-549.

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.

Agreed. This part has been removed. Additionally, we discussed the differences of the simulated medicane between PRS and PGW_{SST}. The difference between the two experiments is related to the SST boundary condition. It is obvious that the warmer SST will modulate the humidity field entirely giving more water vapour as our figures show.

Line 536: Please remove "feedback".

Removed.

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

Omitted.

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".

This sentence has been removed.

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

Corrected.