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Interactive comment on "Surface processes in the 7 November 2014 medicane from air–sea coupled high-resolution numerical modelling" *by* Marie-Noëlle Bouin and Cindy Lebeaupin Brossier

Emmanouil Flaounas (Referee)

emmanouil.flaounas@env.ethz.ch

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I read with great interest the study by Bouin and Lebeaupin Brossier, entitled "Surface processes in the 7 November 2014 medicane from air-sea coupled high-resolution numerical modelling". The results have been reached using adequate methods and a plethora of diagnostics. They are valuable for the Mediterranean cyclones community and therefore I am certainly in favour to see this study published. Nevertheless, I have several major concerns on the analysis of the results.

Major comments 1) I find that the technical language used to describe atmospheric dynamics in several parts of the text comes at the cost of a clear interpretation of

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cyclone dynamics. Here are several examples from the abstract and introduction. I suggest a more careful revision of technical language throughout the text.

line 10: "Tropicalization". I am aware of "tropical transition" but I am afraid I am not familiar with this term. What is its physical/dynamical content? If this term has been used before, then its physical content could be very briefly explained alongside with a reference, if not then, maybe it should be defined in the text.

line 10: ".. then to low-level convergence and uplift of conditionally unstable air masses by cold pools..". Is it meant that "tropicalization" is a product of jet crossing and latent heat release? Then from the perspective of cyclone dynamics maybe it should be mentioned that both baroclinic and diabatic forcings act in synergy to deepen the cyclone.

line 12: "...feeding the latent heat release during the mature phase of the medicane..." do you mean that specific conditions favour convection, in turn leading to high latent heat release? Such phrases were found in several other parts of the manuscript.

line 40: A PV streamer is filament of high PV flow it may not be always regarded as equivalent to a trough.

line 40: "potential vorticity (PV) from higher-latitude regions.." you mean intruding stratospheric air characterized by high PV values?

lines 40-43: I am not sure how these lines help us understand the contribution of different processes in medicanes development.

line 46: what do you refer by "upper-level thermodynamics". What is meant by "limited", maybe you mean "inadequate"?

Line 49: "behaviour", "extension", "..various characteristics" please be specific by rephrasing or attributing a physical content to each term. Please also note that Fita and Flaounas (2018) that warm seclusion may be diagnosed as a warm core symmetric system even if convection is weak. This is opposed to tropical cyclone dynamics.

Line 53: "PV transfer from above" I am not sure what is meant here. Do you suggest that a cyclone was intensified due to the formation of a PV tower after aligning air of stratospheric origin with diabatically produced PV in the lower to mid troposphere?

Line 56: As formulated here it seems that the PV streamer and the upper level Jet are two different atmospheric features.

Line 60: "large diversity of characteristics and behaviours". Please provide physical content to these words.

Line 61-62: I am not sure I understand this phrase. In main lines PV anomalies in a given location can originate from advection (adiabatic process) or from momentum/heat exchanges with the environment (diabatic). Both contribute to the formation of a PV tower and thus both provide a combined baroclinic or diabatic forcing to cyclonic circulation.

Lines 74-77: This phrase is long and difficult to understand.

Line 76: please note that atmospheric processes (e.g. convection, or baroclinic instability) are expected to form, or sustain a vortex/cyclonic circulation not enthalpy or surface thermodynamical variables. What is meant here is that surface processes enhance convection and thus a diabatic forcing to cyclonic circulation?

Lines 82-83: "WISHE-type vs baroclinicity". Please rephrase. Baroclinicity is not a mechanism is an atmospheric state.

Lines 83-84: It is quite awkward to characterize a PV-streamer as "instability aloft". Do you mean that stability decreases within the atmospheric column under the PV-streamer?

Lines 85-86: I am not sure I follow the rational here. Fita and Flaounas, (2018) present a detailed analysis of the implicated dynamics in the development of the december 2005 case. Both a PV-streamer and a cut-off low were involved (the former evolved to the latter). Both features correspond to a PV anomaly of statospheric origin (cut-off or streamer) that provide a baroclinic forcing to the cyclone. I would say that the difference comes from the extent that baroclinic or diabatic forcing (basically latent heat release due to convection) prevails in provoking cyclonic circulation.

Lines 82 & 87: "WISHE-type" and "WISHE-like". Please be consistent on the terminology. If the mechanism explained here diverges from the original WISHE, please be more specific.

Lines 87-88: I do not understand how warm seclusion "replaces" the "heat and moisture to latent heating through a WISHE-like mechanism". Is it meant here that the low-level warm core in the first case is due to convection and latent heat release, while in the second case it is due to warm seclusion?

Line 90: Would you agree that the "Jet-crossing" mechanism refers to a baroclinic forcing to the cyclone? E.g. Jet-crossing could deepen a cyclone even in the absence of any diabatic processes. This seems to be similar to the upper tropospheric forcing "DΦ" on the medicane case of 2005 in Fita and Flaounas (2018), their Figure 7.

Line 98: Intensity may change due to processes such as convection, not surface heat fluxes. A warmer SST would suggest enhanced convection or at least, favour latent heat release.

Line 108: Please also note that the impact of coupling has limited effect on "all intense Mediterranean cyclones". This was shown in a simulation ensemble by Flaounas et al., (2018). This could reinforce the argument that known medicane cases are not expected to have an exceptional sensitivity to air-sea coupling than the rest of the intense cyclones.

2) Medicanes definition and relevance with this case: A major drawback in the field is the lack of a physical definition for medicanes. I am not sure that it has ever been shown that there is a "perfect vertical alignment between the sea level pressure (SLP) minimum". I could argue that in several papers there is no such alignment observed,

especially when the "eye" is fully developed. In line 30 how is radius defined? I suggest a more careful opening paragraph mentioning that knowledge comes for a very limited number of cases qualified as medicanes where no objective criteria have been used other than some arbitrary visual characteristics of cloud coverage. After all, several papers show that medicanes present a high variability of dynamical structures (e.g. Miglietta et al., 2017).

In fact, from a climatological context, medicanes are not expected to clearly stand out in terms of dynamics from several other cases that were not "qualified" as medicanes. This seems to be one of the results for several of the metrics used by Tous ad Romero (2013) and Flaounas et al., (2015) in line 37.

3) I believe that cyclone dynamics in section 3 are not convincingly addressed (this also concerns section 5). It seems to me that this is also partly due to how technical language is used (comment #1).

In summary a PV streamer intrudes the Mediterranean, it provokes cyclogenesis and wraps around the cyclone centre until it evolves to a cut-off at 300 hPa (this is not very clear due to the domain size in figure 5). This scenario seems to be fairly similar to the 2005 medicane, analysed by Fita and Flaounas (2018) and Miglietta and Rotunno (2019), but also to several other cases.

In the "transitional" phase (Fig. 5b), the main body of the PV anomaly is dislocated with respect to the cyclone centre and overall I would say that this example complies with the classic paradigm of baroclinic instability in Hoskins et al., (1985), as illustrated from a PV perspective. What do you refer to with "no trace of baroclinicity" in line 265? With such high PV anomalies in the upper troposphere, the potential temperature isosurfaces should slope accordingly, creating a barcolinic environment to the proximity of the cyclone.

The analysis in 3.1 is based on the PV-streamer evolution, which mainly refers to adiabatically advected stratospheric air masses. Why do you refer to the last phase of the section as "diabatic"? How can rainfall be indicative of diabatic vs baroclinic forcing to the cyclone intensification? Actually the presence of high PV anomaly in the upper troposphere suggests a considerable forcing to the surface cyclone.

Figures 6 and 7 are overcharged and not consistent in the plotted fields. It is also quite odd to use potential temperature at surface level. If this is a model diagnostic (e.g. as 2-meter temperature) rather than a prognostic variable, then it is not used by the model to resolve dynamics. This would create some inconveniences in the analysis. Furthermore, the field is strongly influenced by land-sea transitions. I suggest you use 850 hPa for low level baroclinic structures. Finally, the two overlaid temperature fields in Fig. 7 makes interpretation of dynamics rather difficult.

Lines 274-275 "baroclinic processes are responsible for the heavy precipitation" I am not sure I understand this phrase. Are you suggesting that large scale, quasi-geostorphic forced ascent provokes rainfall?

Section 3.1.3: I do not really understand the term "diabatism". If you refer to the mechanism of tropical transition (Davis and Bosart, 2004) then I guess that you suggest that an initially baroclinic system is now maintained due to convection (as stated in line 316). However there is still an upper PV anomaly due to the streamer. How much does it contribute to the surface cyclonic circulation (also evident in the PV profiles of Fig. 18)? Does the absence of divergence in the upper troposphere suggest that the dynamics are different from the ones expected by tropical cyclones (lines 313)?

It seems that the trajectories "confirm that the warm core of the medicane is actually due to diabatic processes. (lines 330-331)". How do high θe values (regardless the origin of these values) assure that the cyclone is maintained due to diabatic forcing? Equivalent potential temperature (θe) should remain constant if condensation takes place. Here, air masses experience a dramatic increase of θe. Does this reflect an increase in heat fluxes or moisture? Furthermore, how often do the air masses hit the ground and what is the time interval used to calculate the trajectories? Could you comment on how the method used to calculate the trajectories assures that the "same" air mass is followed in time (especially given the sudden change of altitude after leaving the Sicilian coast)?

In fact, factor separation technique in Carrio et al., (2017) shows that the development of this cyclone is due to synergy between upper tropospheric forcing and latent heat release (their figure 14). Could you compare and discuss with respect to their results?

4) I suggest to reorganise the manuscript structure to make it more attractive to the reader. Section 4 is very intensive, the importance of the results is eclipsed by a plethora of diagnostics and variables and seems quite detached from section 3. I would suggest to the authors to make a clearer and more refined presentation of the objectives in the introduction and merge the sections 3 and 4 by organising the document sections according to cyclone phases. In fact, section 5 seems to include quite interesting analysis that would be more adequate for section 3.

Minor comments: Abstract: One important message is that intense air-sea interactions have not a strong effect on the cyclone development. Please further highlight your main results, I am not sure about what message we get from lines 16-21.

Line 131: It seems from Di Muzio et al., (2019) that the occurrence of Qendresa was well predicted by ECMWF with a lead time of 7-8 days in contrast to most medicanes, where predictability seems to be possible only in erlier lead times. Could you please comment, or be more specific, on the predictability of this case?

Line 213: Typically trajectories refer to air masses and tracks to cyclones. What "best-track" refers to and what kind of observations were used?

Line 226-227: This is contradictory statement please rephrase. In fact, the northward displacement of the track only allows to confirm an agreement of the deepening phase of the cyclone between observations and model.

Line 239: I do not follow the reasoning of using 100 km radius, been "fitted to the radius

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of maximum wind". The 100 km radius used is too small, compared to the size of the cyclonic circulation. This is quite clear from the wind field in e.g. Fig. 6. Therefore it seems quite difficult for the diagnostic to capture asymmetries due to frontal structures, which in turn develop in higher length scales. Could you please comment on this? Furthermore Di Muzio et al., 2019 used the operational analyses and same set-up for cyclone phase diagrams and argues that Qendresa presented no warm-core in the upper troposphere. Could you comment on the sensitivity of phase diagrams in your case?

Figure 4 Legends in all axes seem to mention the same pressure layers.

Line 360: Could you please comment more on the reasons why cooling may be higher or lower depending on the case and/or location?

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