Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-983-RC1, 2019

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

Received and published: 21 November 2019

RECOMMENDATION: Major revisions

The Medicane of November 2014 is analyzed here using a coupled modelling approach, focusing on the role of surface processes for the development of the cyclone. This part of the analysis is very detailed and provides a clear investigation of the role of different surface parameters involved in the evolution of the cyclone. Also, results show that the coupling with the ocean model appears not necessary for a proper simulation of this Medicane.

Results are interesting and worth of publication, in particular Section 3.2 and 4 are full of interesting insights in the mechanism of development of the cyclone in terms of air-

sea interaction processes. Also, bibliography is detailed and provides a very updated description of the state-of-the-art in the field.

In contrast, I have some concerns on the discussion of the NOCPL simulation (Section 3.1), for which I ask to reconsider, at least in part, the analysis provided in the paper. Thus, my recommendation is for a major revision.

Major points:

- The distinction among the three different phases in the cyclone lifetime is subjectively defined, thus not completely convincing. I recommend you find an objective way, e.g. based on the Hart (2003) diagram, or on the methodology discussed in Fita and Flaounas (2018).

- The discussion is Section 3.1 is not convincing in many points (see also minor points in 3.1.1 and 3.1.2):

L280-285: the bands of colder air may not be due to the evaporation of precipitation. In particular, the one to the left may be due to cold-air advection, which secludes the cyclone warm core, turning around the center within the cyclonic circulation, as suggested by Fig. 6a. To clarify this aspect, one should perform numerical simulations without the evaporation of precipitation: this would demonstrate whether the cold air is due to a long-range transport toward the center of the cyclone or really to the evaporation of precipitation.

L286-288: I understand that your focus is on surface fields, but you should also consider what happens in the levels immediately above (e.g., 950 hPa, 900 hPa) to support your considerations and better identify the origin of the different air masses and thermal gradients. The vertical extension of the cold air masses in North African may be limited to a few meters, so you should demonstrate more clearly that the "advection of cold and dry air . . . from the Tunisian and Libyan continental surface . . . (L296-297, L448-449)" is relevant. For example, you can use backtrajectories, going earlier in time than those shown in Fig. 9, to clearly illustrate the origin of the air parcels.

Minor points: Lines 9-12: "The deepening . . . of the cyclone is due . . . then to low-level convergence and uplift of conditionally unstable air masses by cold pools, resulting either from rain evaporation or from advection of continental air masses from North Africa.": the deepening can be due to WISHE mechanism and/or to baroclinic processes, the way precipitation is generated is related to convergence and uplift by cold pools, so the sentence should be reformulated.

Line 14: . . . due to a sea surface cooling . . .

Line 27: at least up to the mid troposphere . . .: really, even in the version of Hart diagram modified by Picornell et al. (2014) you used, the vertical structure is analyzed also in the upper troposphere, up to 400 hPa.

Line 33: note that the 26°C threshold of tropical cyclones cannot be applied to tropical cyclones developing from tropical transition processes (see McTaggart-Cowen et al., 2015).

Line 41, 274: I image you are referring to lee cyclones (see Tibaldi et al., 1990), not to lee waves. Lee cyclones are not only the effect of a wave, but theory provides a comprehensive way to describe the process of cyclogenesis.

Line 119: . . . its initiation as a baroclinic storm . . .

Section 2.1: I understand that 19 Figures are a lot, but please consider the possibility to include additional 1-2 figures to make easier to understand the results in this Section, or refer to Figures published in previous papers.

Line 144: please refer to Fig. 1 to help following the text.

Line 170: Is convection treated explicitly? Please add this piece of information.

Line 171: grid spacing instead of resolution.

Line 189: what is an ORCA grid?

Section 2.2.3: what is the rationale behind the choice of this domain extension? Have you tried with different domains? Apparently, only a small domain extension is favorable for a proper simulation of the track - cfr. Cioni et al. (2018) and your tracks with those in Carriò et al. (2017) and Pytharoulis et al. (2018). Please, comment on this.

Figure 3a: it would be helpful to add at least the maximum wind recorded in Lampedusa, Malta, Pantelleria in panel a.

L274: . . . by the lee cyclone induced by the North African relief . . .

L279-280: note that it is not the convergence between SE flow and S-SW flow responsible for the most intense precipitation at sea in Fig. 6b, but the one associated with the low-level southwesterlies and the northwesterly flow behind.

L284: northwestwards?

L285: deep convection in . . .

L292: The low-level virtual . . .

L300: in the southeasterly low-level flow . . . (in Fig. 7b, the wind component seems from the south).

L302-305: did you check these points in the simulation? May you be more quantitative?

L306: the high CAPE is not obtained by extracting heat and moisture! The surface fluxes modify the low-level features, determining an environment more favorable to instability (i.e., CAPE increased).

L316-318: again, this sentence should be less qualitative in order to be more convincing.

L323: Mazza et al. (2017) does not refer to the December 2005 Medicane.

L331: you really show the role of air-sea interaction processes; the role of diabatic processes may be inferred from theory, you do not show that it occurs in this case.

L342-343: I suggest to change “the evolution of the SST” with “sea currents”.

L360: an important conclusion you could mention here is the negligible role of currents.

L384: the gradient of wind speed . . . between which levels?

L385: I suggest to reformulate as “humidity at saturation with temperature equal to SST”.

L394-398: this part is already discussed and can be omitted.

L410-411, L421: This is partly due to the conditional sampling . . .: explain better.

L424-425, L439: The sentences “The time evolution of the distributions of LE and the SST are opposite to each other” and “parameters controlling LE (and evaporation) are the SST . . . with very strong positive correlations” do not contradict somehow each other?

L442: please change “globally” into “positive”.

L442-443: this sentence is not clear; my interpretation is that you should rearrange in something like: “The fact that r is low in the whole domain, and higher in EF600 suggests that strong evaporation controls specific humidity and temperature”.

L448-449: see major point.

L485: in contrast . . .

L485-486: again, see major point.

L531: . . .lack diabatism . . .: really, the second case discussed in Miglietta and Rotunno (2019) does not lack diabatism, but contains both diabatism and baroclinic processes. Please clarify this point here and later in the discussion (L561-564 should be changed, as the Medicane apparently belongs to the second category of Medicanes).

L555: This is consistent with the observations in Miglietta et al. (2013) and Dafis et al. (2018).

Figure 6 is very confusing: the coastline can be hardly identified; latitude and longitude are not reported; the extension of the cross section in b) is not clear; finally, the same color scale should be used in both panels. Similar considerations apply to Figure 7.

Figure 9: how long do the backtrajectories go back in time?

Figure 10: the cyan and blue columns are difficult to distinguish.

Figure 11: the triangles are difficult to identify.

REFERENCES:

Dafis, S., Rysman, J.-F., Claud, C. and Flaounas, E. (2018) Remote sensing of deep convection within a tropical-like cyclone over the Mediterranean Sea. Atmospheric Science Letters, 19(6), e823.

McTaggart-Cowan, R., Davies, E.L., Fairman, J.G., Jr., Galarneau, T.J., Jr. and Schultz, D.M. (2015) Revisiting the 26.5âŮęC sea surface temperature threshold for tropical cyclone development. Bulletin of the American Meteorological Society, 96, 1929–1943.

Tibaldi, S., Buzzi, A., and Speranza, A.: Orographic cyclogenesis, in: The Palmen Memorial Volume, edited by: Newton, C. and Holopainen, E. O., Am. Meteorol. Soc., Boston, 107–127,1990.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-983, 2019.