We thank the reviewer for their constructive and helpful comments, which will provide a more robust approach and increase the readability of our manuscript.

Review of manuscript

« Extreme Mediterranean cyclones under future climate change »

"I read the paper by Doensen et al. (2026) with great interest. The study examines the effect of climate change on the most intense Mediterranean cyclones and quantifies the associated changes in induced extreme weather. The manuscript is generally well written, and the methods and overall scientific approach appear appropriate for addressing the stated objectives. I have only minor concerns regarding some of the results themselves; however, my main impression is that the discussion needs to be improved. In these regards, I would like to highlight two main issues:"

General Comments

"Use of vague language.There are several points in the manuscript where the language lacks technical precision. As a result, important details are not sufficiently highlighted, which in turn affects the significance and clarity of the paper. I would advise the authors to revise the text carefully in order to improve precision and scientific accuracy."

We appreciate the feedback by the reviewer. We revised the text carefully, and aimed for making the manuscript more accurate and understandable. We thank the reviewer for highlighting individual phrases that were especially vague.

"Interesting results, but limited discussion. The results are interesting and insightful, but their discussion remains underdeveloped. Much of Section 3 appears to describe the figures, without sufficiently connecting the findings to previous studies and without clearly showing how the present work expands existing knowledge. In my view, a stronger discussion would considerably enhance the value of the paper. For instance, several previous studies, some of which are already cited in the manuscript, have statistically described the effects of climate change on extreme cyclones and, more generally, on extremes in the Mediterranean region. By contrast, the three-dimensional composite structures -provided here- represent a more original contribution. However, the manuscript does not clearly reveal how these results build upon and extend previous knowledge."

We agree with the reviewer that we do not discuss our work in enough detail. This has partly also been flagged by the other reviewer, thus we included more literature work to compare our results with. For example the location of extreme cyclones and the future changes are discussed in more detail.

"My recommendation is major revision, mainly because addressing these issues requires more than minor editing. The following specific comments mainly outline issues related with the two concerns right above."

We thank the reviewer for their comment. We agree that the paper can be discussed in more detail, as also suggested by reviewer 1. Nonetheless, we would like to stick to the distinction between results and discussion in two different sections.

Specific Comments

"Title: At first glance, it is not entirely clear whether "Extreme Mediterranean cyclones" refers to weather extremes associated with cyclones or to cyclones of extreme intensity. Please revise the title for clarity."

As also stated by the other reviewers we have specified the title to the following:

"Mediterranean cyclones from pre-industrial to future climate: impacts on extreme wind, precipitation and compound precipitation–wind events"

"Line 22: What is meant by "heavy windstorm"?"

We mean to highlight intense winds caused by extratropical cyclones. The word "heavy" has been poorly chosen. As also suggested by the reviewer 1 we changed the sentence to:

"Yet, these cyclones can have severe impacts on the Mediterranean region causing heavy precipitation events (Pfahl and Wernli, 2012; Flaounas et al., 2015a; Raveh-Rubin and Wernli, 2015), intense winds (Nissen et al., 2010; Raveh-Rubin and Wernli, 2015), and coastal floods (Lionello et al., 2019; Ferrarin et al. 2021)."

"Lines 34–65: These paragraphs are informative, but they are somewhat disconnected. It would help if the phrasing were more clearly articulated and if some synthesis were added to guide the reader regarding what should be retained and how it connects to this study."

See general comment 1. We agree with the reviewer, and concluded that the introduction needs more restructuring in general. Therefore, we reorganised the introduction to connect individual sentences and cited literature better by topic. We also made sure to use less sloppy language.

"Lines 51–52: Please articulate this more clearly with the following sentence."

See previous comment and general comment 1

"Line 178: grid point"

We changed grid cell to grid point as suggested.(line 204)

"Lines 181–183: How do these results compare with previous studies, for example Reale et al. (2021)?"

This has been discussed in section 4 (lines 397-401). Nevertheless, we have not compared this in a detailed way, and we now added how the changes in our study compare to the changes in the mentioned studies.

“Line 193: subpanel → panel”

We thank the reviewer for their suggestion. This has been change throughout the document

“Lines 194–196: The language is familiar here. Please be more specific.”

See general comment 1.

“Lines 193–207: These lines seem somewhat superficially descriptive of the figures. Please discuss these results in relation to previous studies and provide a clearer conclusion to Section 3.1.”

We agree with the reviewer the climatology and seasonality of the EXCs is not extensively enough discussed. We added in the discussion some extra literature on this and compare our results with the existing literature.This can be found in lines (410-421)

“The location and seasonality of the EXCs are consistent with previous literature. We find that precipitation EXCs occur predominantly over the Mediterranean Sea, whereas wind speed EXCs also occur frequently over land. This agrees with Raveh-Rubin and Wernli (2015), who found that precipitation extremes in the Mediterranean are most likely located over the sea while gust extremes are more likely over land. Similarly, the precipitation–wind compound EXCs occur mainly over the sea, in agreement with Portal et al. (2024). The seasonality is likewise consistent with earlier work. Khodayar et al. (2025) showed that precipitation-related damages peak in autumn, plausibly reflecting cyclone-related precipitation, although we do415 not observe the peak in precipitation extremes in winter in the eastern Mediterranean as shown by Raveh-Rubin and Wernli (2015). Doiteau et al. (2024) found that deep Mediterranean cyclones, capable of producing intense winds, are distributed evenly from November to March. The latter matches the even distribution of wind speed EXCs we detect across the winter half year. Despite no significant change in EXC seasonality, precipitation and compound EXCs in the EMED are shifted southward over the warmer Mediterranean Sea in the future, whereas the average latitude of precipitation EXCs in the WMED shows no significant change.”

“Line 211: “general structure” is too vague. Please be more specific. It would also be useful to discuss whether this structure is systematic across the systems considered. Is there any indication of how many medicanes, as recently defined by Miglietta et al. (2025), are included in your dataset? Miglietta, M. M., and Coauthors, 2025: Defining Medicanes: Bridging the Knowledge Gap between Tropical and Extratropical Cyclones in the Mediterranean. Bull. Amer. Meteor. Soc., 106, E1955–E1971, https://doi.org/10.1175/BAMS-D-24-0289.1.”

We agree with the reviewer here that the used language is vague. We changed the sentence to the following (lines 240-241)

“In all composites, we see the structure of a cyclone with a clear minimum in sea level pressure and precipitation bands north and southeast of the EXC centre”

Since we are mainly aiming for the mean impact of the cyclone, our interest is not on the behaviour of all the individual cases making up the cyclone. Thus, we only show the mean structure. However, to constrain our results, we do compare the composites with ERA5 for an overlapping time period.

On the medicane issue, the definition provided by Miglietta et al. (2025) is hard to apply in climate model data as it requires for example the detection of an “eye-like” structure which is hard to detect in our model at best, and it is also questionable whether such an eye-like structure can be resolved at 20 km resolution. As focussing on medicanes would require a whole new tool to detect them in the first place, we made the decision not to separately analyse them. Although the number of medicanes in our composite is unknown, this does not change the impact of the cyclones that make up the composite.

“Section 3.2: Could you please compare and discuss your findings with previous studies showing an overall decrease in cyclone-related precipitation under climate change in the Mediterranean (e.g. Reale et al., 2021; Chericoni et al., 2025)?”

As also highlighted by the other reviewer, this has now been added to the discussion (lines 422-430):

“Our findings show that cyclones associated with extreme precipitation respond differently in the future in the two subregions with an increase in extreme precipitation in the WMED and no significant difference in the EMED. This is remarkable, since CMIP5 simulations project a significant future decrease in mean winter-time precipitation over most of the Mediterranean (Zappa et al., 2015b). Yet, the increase in precipitation we find for EXCs in the WMED matches the findings by Reale et al. (2022) who also found an increase in mean cyclone-related precipitation for the northern Mediterranean. However, in our work the precipitation EXCs in the EMED show no significant change in precipitation. This does not coincide with the decrease in mean cyclone-related precipitation found by Zappa et al. (2015) and Reale et al. (2022) Nevertheless, extreme cyclone-related precipitation in the Mediterranean responds differently to climate change than mean seasonal precipitation (Chericoni et al. 2025).”

“Line 257: I think that the vertical gradient of θe is related to potential instability, that is, it may help explain the stronger link between frontal structures and precipitation. If so, please revise accordingly.”

We thank the reviewer of their insights here. Indeed, the instability matches the location of parts of the fronts. Thus, we specified the sentence to the following:

“Besides, the negative vertical gradients in θe in the lower part of the troposphere (e.g. in the right-hand side of Fig. 6e) points to convective instability, which can contribute to precip_6h that we observe in Fig. 5.”

“Lines 254–259: Several studies have shown that stronger diabatic forcing can lead to smaller, more compact cyclones. Could this help explain the positive–negative

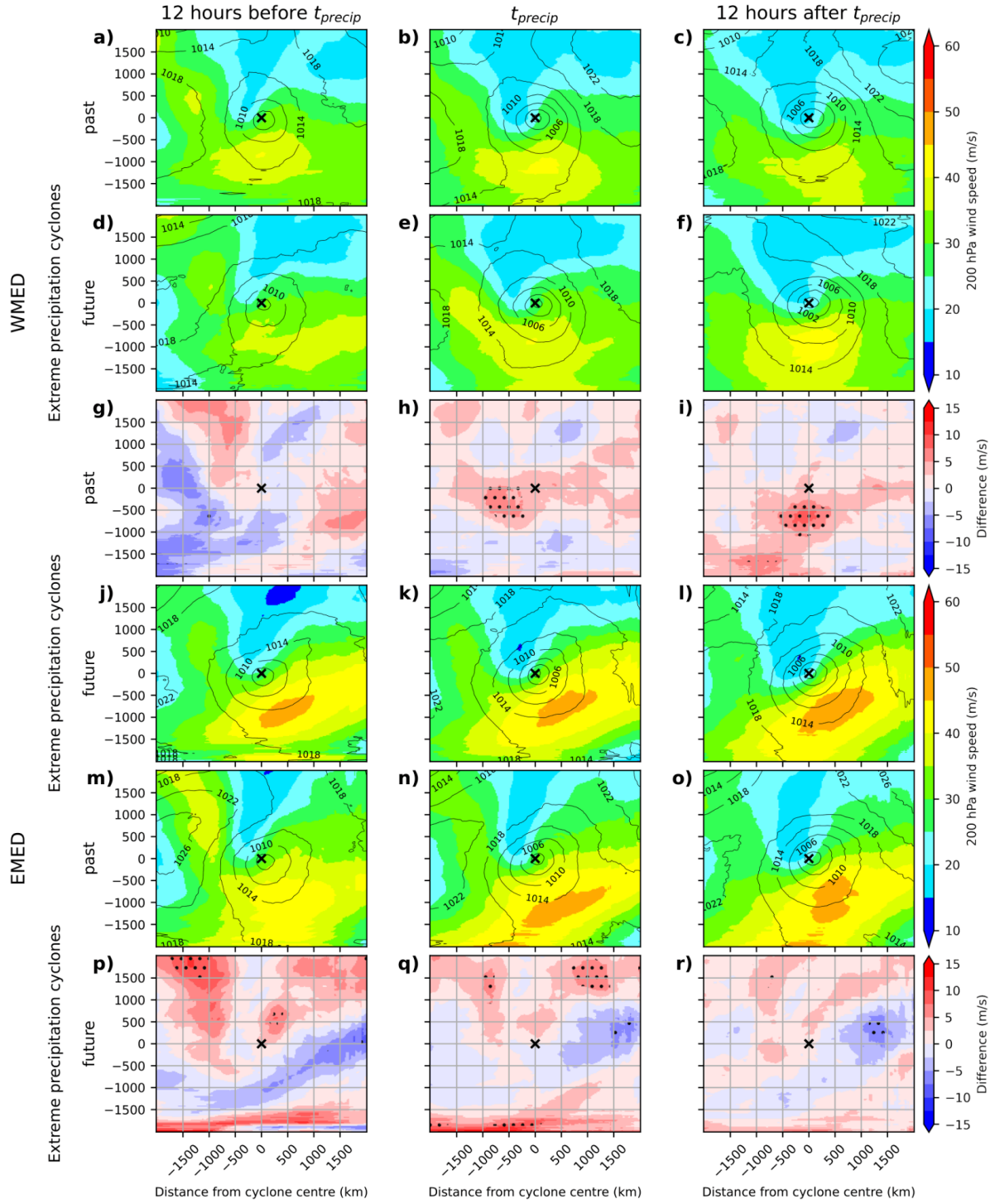
rainfall pattern in Fig. 5? Could it also explain the more uniform pattern mentioned in line 305 (perhaps "more axisymmetric" is what is meant)?"

Although the radius for which we consider the precipitation, and wind speed is set to 500 km, we also have an estimate of the cyclone radius from the tracking algorithm. We tested whether there is a significant change by applying a t-test on all the 50 cyclone radii for the past against the future for each event type in each region. None of the EXC types (wind, precipitation, compounding) in neither the WMED nor the EMED show a reduction in cyclone radius size at minimum sea level pressure. Given this result and to avoid confusing of the readers we do not mention this in the text.

Axisymmetric is exactly the word we are looking for, we thank the reviewer for their suggestion.

"Lines 266–267: Givon et al. (2024) showed that different clusters of PV intrusions are related to the mature stage of Mediterranean cyclones (their Fig. 3). Is it possible that future cyclones may fall into, or preferentially favour, different clusters? Perhaps PV could also be included in Fig. 8 so that the reader can better assess whether the vertical cross-sections in Fig. 6 are comparing similar three-dimensional structures.Givon, Y., Hess, O., Flaounas, E., Catto, J. L., Sprenger, M., and Raveh-Rubin, S.: Process-based classification of Mediterranean cyclones using potential vorticity, Weather Clim. Dynam., 5, 133–162, https://doi.org/10.5194/wcd-5-133-2024, 2024."

We appreciate the reviewer's suggestion, and the paper by Givon et al. (2024) is an excellent paper that could be of great interest to this work. But there are a few complications. First of all, we do not show PV for wind extreme cyclones because the future change in low-level PV is insignificant (see comment to lines 321-322). For consistency, we would need to make Fig 8 also for the precipitation EXCs, and there we do not see any significant intereaction with the cyclone and the jet stream, nor an increase in the jet stream (see below for precipitation EXCs). Thus we refrain from adding this to Fig 8



"Line 268: "When looking ..., we see .. general decrease..." Please avoid overly vague language throughout the paper. At times, this comes at the expense of precision regarding the results and the points that the reader should focus on most."

See general comment 1.

"Lines 321–329: My impression is that this part should be linked more directly to Fig. 6, or perhaps the two discussions could be merged."

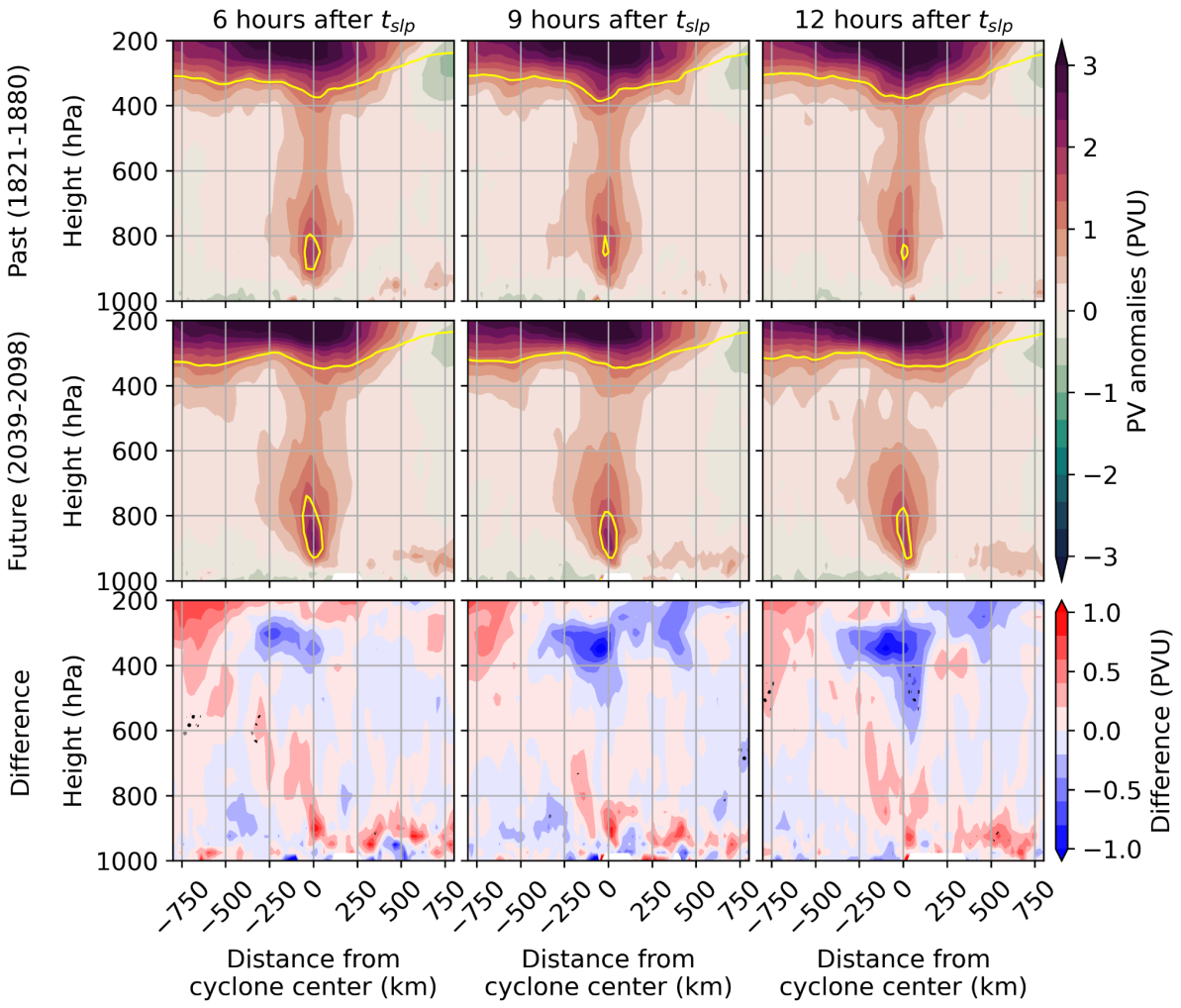
Fig. 6 explicitly only discusses precipitation EXCs. In lines 321-329 we try to find an explanation for the increase in wind speed. Thus, we look at two different kinds of events and it is not trivial to connect lines 321-329 with Fig. 6. Also, as mentioned in one of the comments below, there is no significant increase in low level PV for wind EXCs.

"Lines 321–322: Could you please explain why? A stronger cyclone could also be related to stronger diabatic forcing."

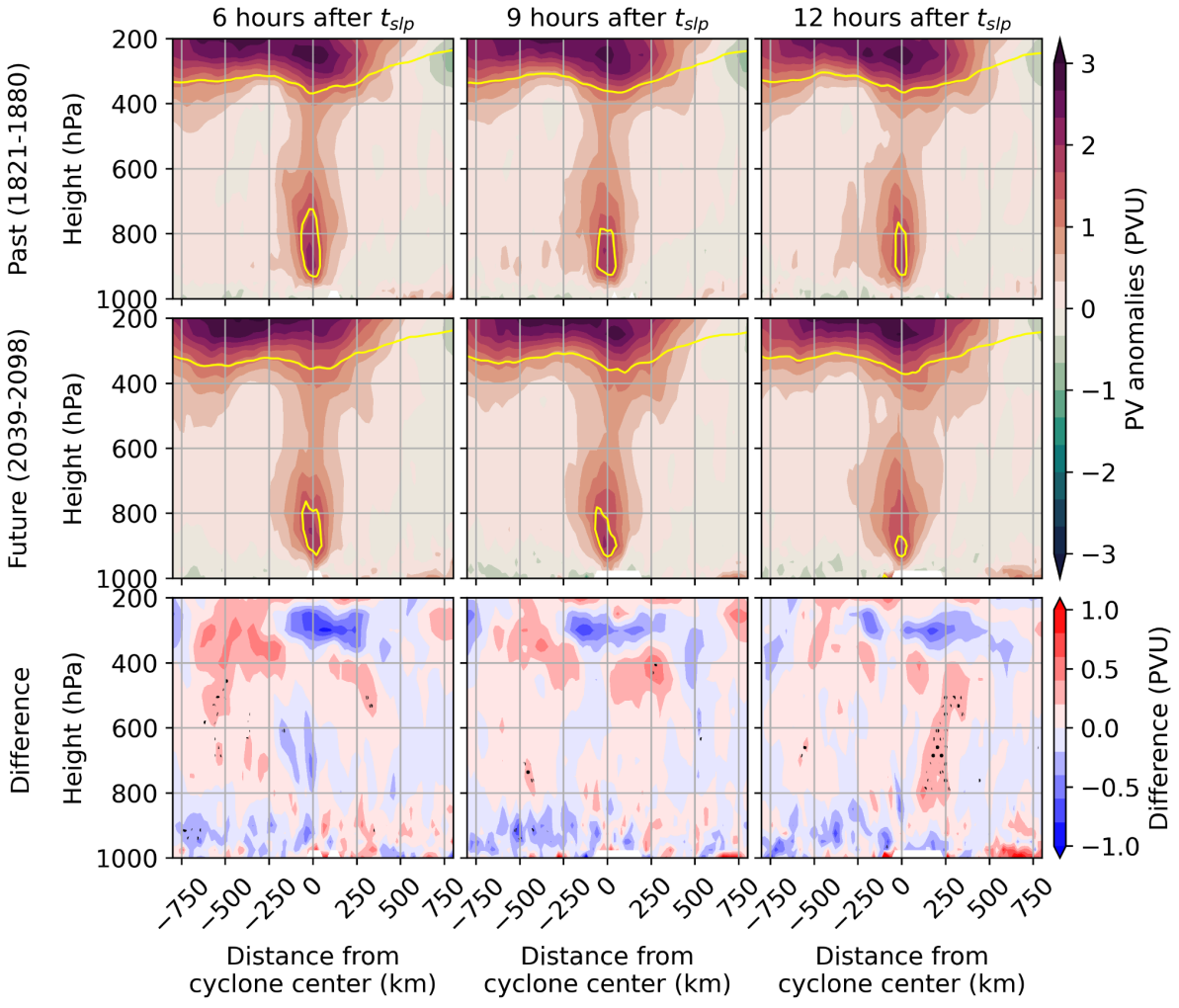
We do not find a significant increase in diabatically produced PV for wind speed EXCs in either the WMED or EMED (See Figures below). Yet, we do not mention this in the text and to avoid confusion we add: (lines 351-353)

"For wind EXCs we do not find a significant increase in low-level PV that could explain the future intensification observed in Fig.7 (not shown). To further understand why wind EXCs in the future are stronger in their most intense and mature phase, we investigate the jet stream as a potential driver"

Vertical cross sections wind EXC WMED



Vertical cross sections Wind EXCs EMED



“Line 326: “This hints...” is again somewhat informal. Also, the merging of the jets appears to be atypical in intense Mediterranean cyclones; see Fig. 10 of Flaounas et al. (2015).Flaounas, E., Raveh-Rubin, S., Wernli, H. et al. The dynamical structure of intense Mediterranean cyclones. Clim Dyn 44, 2411–2427 (2015). https://doi.org/10.1007/s00382-014-2330-2”

For the use of language see general comment 1

We thank the reviewer for raising this point. However,we do not fully agree with the reviewer that the merging of the jets is atypical in intense Mediterranean cyclones. We think Fig. 10 of Flaounas et al. (2015) is very similar to the pattern we observe in Fig 8, with a branch of high 200 hPa wind speed just west of the cyclone, which we think is very likely related to the midlatitude jet. Also, Raveh-Rubin and Wernli (2015) (https://doi.org/10.1002/qj.2531) stated that extreme weather events can be related to the merging of the midlatitude and subtropical jet (albeit for precipitation extremes).

“Line 379: “quite some spread” is another example of overly familiar language. Please be more precise.”

See general comment 1.