

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

Reviewer 1

Review of manuscript

« Extreme Mediterranean cyclones under future climate change »

This manuscript analyses the most extreme precipitation, wind and precipitation-wind events induced by cyclones in the past and future climate using the regional downscaling of a long climate simulation. The results are interesting, and partially in accordance with previous literature on the topic, although the regions and periods of the analysis do not overlap with other studies. The topic is of strong interest, but lacks a more precise comparison with existing literature and a systematic assessment against observational reanalysis. For these reasons, I believe the work must undergo a major revision before being reconsidered for publication in WCD.

General Comments

“The paper analyses changes in Mediterranean cyclone extremes in the future climate against the past climate (XIX century). This should be emphasised in the title and in the abstract, as the present version may be understood as future changes compared to the present climate.”

“In the title and in the text you should also specify you are considering « extremes induced by/associated with Mediterranean cyclones », rather than « extreme cyclones ». The second formulation can be read as a measure related to the intensity of the pressure low rather than the intensity of the cyclone impacts.”

We agree with the comment that the title should be more specific and highlight the fact that we compare the future with the past climate. The new title is

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

we also changed the second sentence in the abstract to

“Thus, the aim of this study is to investigate the impact of future climate change with respect to pre-industrial conditions on cyclones which induce extreme wind, precipitation and compounding precipitation-wind events. ”

“How do you know that the changes in the extremes (specially in wind) are not driven by changes in cyclone depth, and hence in the intensity of the gradients around the cyclone center ? Have you verified this ?”

Maybe we do not fully understand the comment of the reviewer. Wind speed and cyclone depth are tightly linked to each other via the geostrophic wind relationship. We decided to do the analysis with wind speed rather than cyclone depth or gradient in pressure as e.g. in applications such as the insurance industry, one is more interested in wind speed than in depth.

“In my opinion, for the applicability of the results to the real context of the Mediterranean climate, it is not sufficient to say that « WRF reproduces the main cyclone hotspots and these results are in line with previous studies ». (Lines 366-367). To assess the model performance and attendability, it is necessary to :- verify cyclone density in the model against ERA5.- verify the cyclone-centered analysis of extrema against ERA5. This should be done using the overlapping period between model simulation and ERA5.”

We fully agree with the reviewer that just concluding the performance of WRF is sufficient because it represents the main cyclone hotspots is not solid enough. Still, we do not expect to have a perfect agreement between ERA5 and the WRF simulation as WRF is not forced by ERA5, but by CESM. Therefore, WRF is expected to:

- Improve process-level realism (e.g., convection, orography, mesoscale structure)
- But still inherit large-scale biases from CESM

Nevertheless, in the supplementary section of the paper, we provide the comparison between cyclone frequency in WRF and ERA5. Moreover, we also provide the comparison between ERA5 and WRF for the cyclone-centered analysis with respect to precipitation and wind speed. We do this for the period 1981-2010 for a couple of reasons.

- It is the standard 30-year period for climatology as defined by the WMO. A 30-year overview should be sufficient to cover the variability.
- It fully covers the simulations that are downscaled using the CESM historical simulation. After 2012 the future RCP8.5 simulation is used, and since the RCP8.5 simulation is not branched off from the historical runs, the future simulation is initially colder than the historical simulation which would not be ideal for a fair comparison.

The comparison between WRF and ERA5 can now be found in Figs S1–S3. Also, we briefly mention how we compare ERA5 against WRF at the end of the methods section, we dedicate a paragraph for figures S1, S2 and S3 each in the results, and we briefly summarize how WRF and ERA5 compare in the discussion.

“Section 2.3 is not clear in describing the definition of cyclone-related extremes. For more details see the related Specific Comments.”

We understand that the method is too packed and confusing. We decided to split up the individual steps into bullet points and more thoroughly explain the method. The revised version can now be found in section 2.3.

“Sections 3.2-3.4 : You can compare the position of extremes/compound extremes with the results by Rousseau Rizzi et al 2024.”

We thank the reviewer for the suggestion. Indeed, this is an excellent paper to compare our results more thoroughly with. Yet to strictly keep the separation between results and discussion we discuss the work by Rousseau Rizzi et al (2024) in the Discussion section in line 463

Specific Comments

“Line 1 : You use the term « intense » although, as you say in second sentence of intro, Mediterranean cyclones are usually less intense than Atlantic cyclones.”

We appreciate the suggestion of the reviewer. We propose a sentence with a more general statement (line 1):

“The Mediterranean is a major extratropical cyclone hotspot and heavily impacted by climate change.”

“Line 5 : Could you change the EXC acronym? EXCY or CYEXTR or other? EXC is a bit confusing as it is used as an abbreviation for excellency, except, exception, ..”

Although we agree with the reviewer that EXC is not the most straightforward acronym, we would like to keep it as we used the same acronym in our previous paper studying extreme Mediterranean cyclones to avoid confusion (<https://doi.org/10.5194/cp-21-1305-2025>).

“Line 10-11 : Could you rephrase this sentence to improve clarity ?”

We propose the following sentence to clarify (line 10-12):

“This intensification is linked to the positioning of future wind speed EXCs at the left exit region of the jet streak, which is itself projected to strengthen under RCP8.5.”

“Line 21 : windstorms are « strong » rather than « heavy ».”

Agreed and also flagged by the other reviewer. This sentence has been changed to (line 20-23):

“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).”

“Line 25-26 : I find this sentence is unrelated with the previous. You could maybe say instead that climate change may play a rôle in aggravating the intensity of these extremes.”

Yes, we agree and decided to remove this sentence.

“Line 27 : « influence » rather than « impact ».”

We reformulated the sentence to “So far, changes of Mediterranean cyclones and their characteristics due to future climate change are extensively studied in global circulation models (GCMs; e.g. Lionello et al., 2002; Raible et al., 2007; Nissen et al., 2014; Hochman et al., 2020; Doensen et al., 2025).” (lines 29-31)

“Line 37 : « a reduction in the intensity or in the frequency of cyclones ».”

This should be a reduction in the cyclone frequency. We rephrased this section significantly.

“Lines 47-50 : I would exchange the order of the two sentences, to first introduce PV streamers, and then talk about how these are reinforced by diabatic processes in the low levels.”

We agree with the reviewer that switching the order of the sentences is more logical, thus these two sentences become (lines 50-53):

“An important process is the intrusion of stratospheric high potential vorticity (PV) air, which is a primary trigger for cyclogenesis in the Mediterranean (Raveh-Rubin and Flaounas, 2017; Scherrmann et al., 2024). Moreover, diabatic processes substantially enhance the cyclonic circulation in extratropical cyclones by low-level PV production (Davis and Emanuel, 1991).”

“Line 53 : Scherrmann et al does not support the statement that « Low-level diabatically produced PV is often the dominant source of PV in Mediterranean cyclones ». I would rather say that upper-level PV forcing is the dominant source in Mediterranean cyclones. Then diabatic PV contributes to the lower-level PV as specifically discussed in Scherrmann et al. Can you correct or explain/motivate your statement further ?”

We thank the reviewer for their suggestion in this matter. Indeed Scherrmann et al claim that low-level diabatically produced PV close to the cyclone center is the dominant source of low-level PV, and not total PV as we claimed. Thus, we changed the sentence to the following (lines 56-57):

“Diabatically produced PV close to the cyclone centre is the dominant source of low-level PV in Mediterranean cyclones (Scherrmann et al., 2023)”

“Lines 54-56 : Could you specify whether the works you cite refer to all extratropical cyclones, Mediterranean cyclones or cyclones from a specific geographic region ?”

We appreciate this suggestion. Büeler and Pfahl (2019), focuses on intense cyclones in the Northern Hemisphere and using idealized aquaplanet simulations. Zhang et al (2018) focuses on cyclones on the East Coast of North America. However, we believe the underlying dynamical mechanism is sufficiently general that it applies for cyclones in general. Specifying the setting of the original study could cause confusion for the reader. We clarified this (lines 56-62):

“Diabatically produced PV close to the cyclone centre is the dominant source of low-level PV in Mediterranean cyclones (Scherrmann et al., 2023). Hence, increased latent heating in a warmer climate can lead to cyclone intensification due to increased low-level diabatic PV production as shown in idealized simulations under aquaplanet configurations (Büeler and Pfahl, 2019). This intensification process is

responsible for enhanced wind speeds in the warm sector of a cyclone (Dolores-Tesillos et al., 2022, Dolores-Tesillos and Pfahl, 2024). Moreover, Zhang and Colle (2018) suggested in an analysis of the East Coast of North America that future extratropical cyclones are less intense in their initial phase due to lower baroclinicity in the atmosphere, but more intense in their mature phase due to increased diabatically produced PV.”

“Line 55 : Is the wind speed in the warm sector reported by Priestley and Catto, 2022 linked to an increase in the low-level diabatic PV, as your sentence suggests ?”

We thank the reviewer again for their suggestion. This statement is supported by:

Dolores-Tesillos, E., Teubler, F., and Pfahl, S.: Future changes in North Atlantic winter cyclones in CESM-LE – Part 1: Cyclone intensity, potential vorticity anomalies, and horizontal wind speed, Weather Clim. Dynam., 3, 429–448, <https://doi.org/10.5194/wcd-3-429-2022>, 2022.

Though it is discussed in Priestley and Catto (2022), they do not explicitly show results that support this claim. Thus, we changed the reference to the Dolores-Tesillos et al. (2022) who do discuss it. Moreover, also Dolores-Tesillos and Pfahl (2024) support this statement.

“Line 66 : One big caveat using GCMs to... Could you complete by saying what you use the GCMs for ?”

We decided to change the sentence as follows to improve readability (lines 72-73):

“GCM resolutions are often too coarse to fully represent these processes and to resolve the complex topography of the Mediterranean (Flaounas et al., 2013).”

“Line 84 : Does the global CESM model present any dynamical biases in jet position, intensity, or other features affecting the Mediterranean region ?”

As is previously shown in:

Doensen, O., Messmer, M., Kim, W. M., and Raible, C. C.: Characterization of the mean and extreme Mediterranean cyclones and their variability during the period 1500 BCE to 1850 CE, Clim. Past, 21, 1305–1322, <https://doi.org/10.5194/cp-21-1305-2025>, 2025.

the storm tracks in the CESM simulation are too zonal and underestimate the cyclone frequency in the Mediterranean. However, the bias is not discussed and we added this in the discussion in the same section where we also discuss the biases with ERA5. (lines 405-406)

“Line 125 : As in the article title, « extreme cyclones » is confusing, as readers may think of cyclones which are extreme in intensity rather than cyclones which lead to extreme weather in terms of wind and precipitation.”

We agree with the reviewer that the subsection title is confusing, thus we changed it to “Definition of cyclone-related extremes”. (line 137)

“Line 126 : Is it correct that for every cyclone, at time of slp minimum, you find the grid value corresponding to the 95th percentile among the grid points within the 500 km radius ? And then do you use this value to select the 50 cyclones with most extreme wind ? Please describe more clearly.”

That is how it works exactly. We hope it is clear now from the methods section.

“Line 129 : For timing of peak precipitation in Mediterranean cyclones see also : Flaounas, E., V. Kotroni, K. Lagouvardos, S. L. Gray, J.-F. Rysman, and C. Claud. 2018. “Heavy Rainfall in Mediterranean Cyclones. Part i: Contribution of Deep Convection and Warm Conveyor Belt.” Climate Dynamics 50: 2935–2949.”

We thank the reviewer for the suggestion. We included this publication as suggested. (line 144)

“Line 130 : It is not clear what you mean with « the rolling summed 6 hourly 95th-percentile precipitation within the 500 km radius ». Please improve the description. Do you mean the 95th percentile value of 6h-accumulated precipitation among the grid points within the 500 km radius ?”

This should be the 95th percentile precipitation within the radius. We then accumulate the 95th percentile precipitation over 6 hours. We hope this is clearer now in the text (see also general comment).

“Line 132 : Is t_precip the time when the 95th percentile in the 500 km radius is the highest ? Again this is not clear from the text.”

t_precip is the time where the 6-hour rolling sum (3 hours before and 2 hours after corresponding hour) is the highest. We hope this is clearer from the text now. See also general comment.

“Line 134-136 : I don’t understand the reason for transforming the data. Can you explain ?”

When initially analysing all precip_6h values at t_precip, the distribution does not have a Gaussian shape (due to 0 mm/6h being a physical limit). Thus, we apply the mentioned power transformation to make the shape Gaussian. This is eventually important to determine the joint distribution for the compounding cyclones. We included this clarification in the fourth bullet point of section 2.4. See also general comment

“Line 143 : Do you mean for the « seasonality » instead of « climatology » ?”

Does the reviewer mean line 147 instead of 143? If so, yes this should be “seasonality” instead of “climatology”, we changed it accordingly. (line 169)

“Line 152 : I think you should specify here that you select the 5 % most extreme cyclones in terms of the 95th percentile of the impact variable within the 500 km radius around the cyclone at reference time. If this indeed corresponds to your methodological response.”

Yes, we identify roughly 1000 cyclones per period and region and we select the 50 most extreme cyclones, defined with respect to *the 95th percentile of* precipitation, wind speed, or wind-precipitation. We changed it to the following: (lines 173-177)

“Finally, a cyclone-centred composite analysis is performed on the 50 most extreme EXCs found in the WMED and EMED, respectively. We identify roughly 1000 cyclones per period and region. For the composite analysis, we select the 50 most extreme cyclones, defined with respect to the 95th percentile of precipitation, wind

speed, and wind-precipitation compound EXCs. The analysis is performed for cyclones associated with cyclone-related precipitation, wind, and compound extremes, separately.”

“Line 378-380 : Can you describe the regional dependences of these results and how they relate to the results of your work ? In fact, Zappa et al 2014 and Reale et al 2021 show many regional details, which may or may not be in accordance with your projections for EMED/WMED.”

Zappa et al. (2015a) analysed the effect of climate change on Mediterranean cyclone frequency and cyclone-related precipitation in 17 different CMIP5 models using the RCP8.5 scenario. Reale et al. (2022) analysed the effect of climate change on Mediterranean cyclone frequency, precipitation and wind speed in 7 different RCMs using the RCP8.5 scenario. Both studies draw the same main conclusion: cyclone frequency decreases in the entire Mediterranean. Whereas in Reale et al. (2022) mean cyclone-related precipitation increases in the northern (or central) part of the Mediterranean, this increase does not appear in the CMIP5 models used by Zappa et al. (2015a). In Zappa et al. (2015a) the increase in precipitation intensity does not overcome the decrease in cyclone frequency. Yet, both studies show a cyclone-related precipitation reduction in the eastern Mediterranean. Moreover, Reale et al. (2022) found an increase in cyclone-related wind speed in the central part of the Mediterranean and a decrease in the eastern part of the Mediterranean.

The regional differences for cyclone-related precipitation are already mentioned in the introduction in line 73-76 for Reale et al. (2022), however not for Zappa et al. (2015a). This is changed to:

“Using regional climate models, Reale et al. (2022) showed in contrast to GCM results by Zappa et al. (2015) that mean cyclone-related precipitation is projected to increase in the northern part of the western Mediterranean and decrease in the eastern Mediterranean, indicating a regional dependence of the response of these events.”

We also agree with the reviewer that the regional differences in these studies are not sufficiently discussed. To connect our results better to these two studies we extended the discussion in lines 378-380 to: (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 386-388 : Can the increase in PV be associated with a greater occurrence of the cyclones over the surface of the sea ?”

We find more future precipitation EXCs over sea in the EMED, but not so much for the WMED (Fig. 3). We show the increase in PV only for the WMED since that is the only region with a significant increase in precipitation. Thus, we think it is unlikely that the mechanisms suggested by the reviewer play a significant role in the PV production.

“Line 400-401 : Can you outline how your results for wind extremes relate to previous literature on the topic ? How do you accommodate your results showing an increase in the intensity of cyclone-associated wind with the literature you cite in lines 43-44 ?”

Despite highlighting the uncertainties around future cyclone wind speed projections, we do agree with the reviewer that a discussion of the available future projections for cyclone-related wind speed is to some extent missing. The paper that seem most relevant for comparison is again Reale et al (2022). Also Chericoni et al. (2025) provides a future projection for cyclone-related wind speed in their supplementary. Thus, we added the following sentence in the discussion: (lines 445-448)

“Reale et al. (2022) found an increase in mean cyclone-related wind speed around Italy, and a decrease in the rest of the Mediterranean. However, Chericoni et al. (2025), found an increase in mean cyclone-related wind speed for intense cyclones for almost all regions in the Mediterranean, which agrees with our results.”

“Line 403-404 : Unclear how this aspect impacts your results.”

We mainly want to highlight the uncertainties around future climate change. Still, we agree that the sentence does not really connect to our results and we think it fits better in the introduction. Thus, we moved it there (lines 76-77)

“Line 426-427 : I think there is a logical inconsistency here. The number of extreme cyclones you consider does not depend ONLY on the number of ensemble members, as suggested by your sentence.”

We appreciate the suggestion of the reviewer, we rephrased the sentence (lines 473-474)

“Still, one shortcoming is that we only use a single member simulation.”

Reviewer 2

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 do 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 θ_e 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 θ_e 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.”

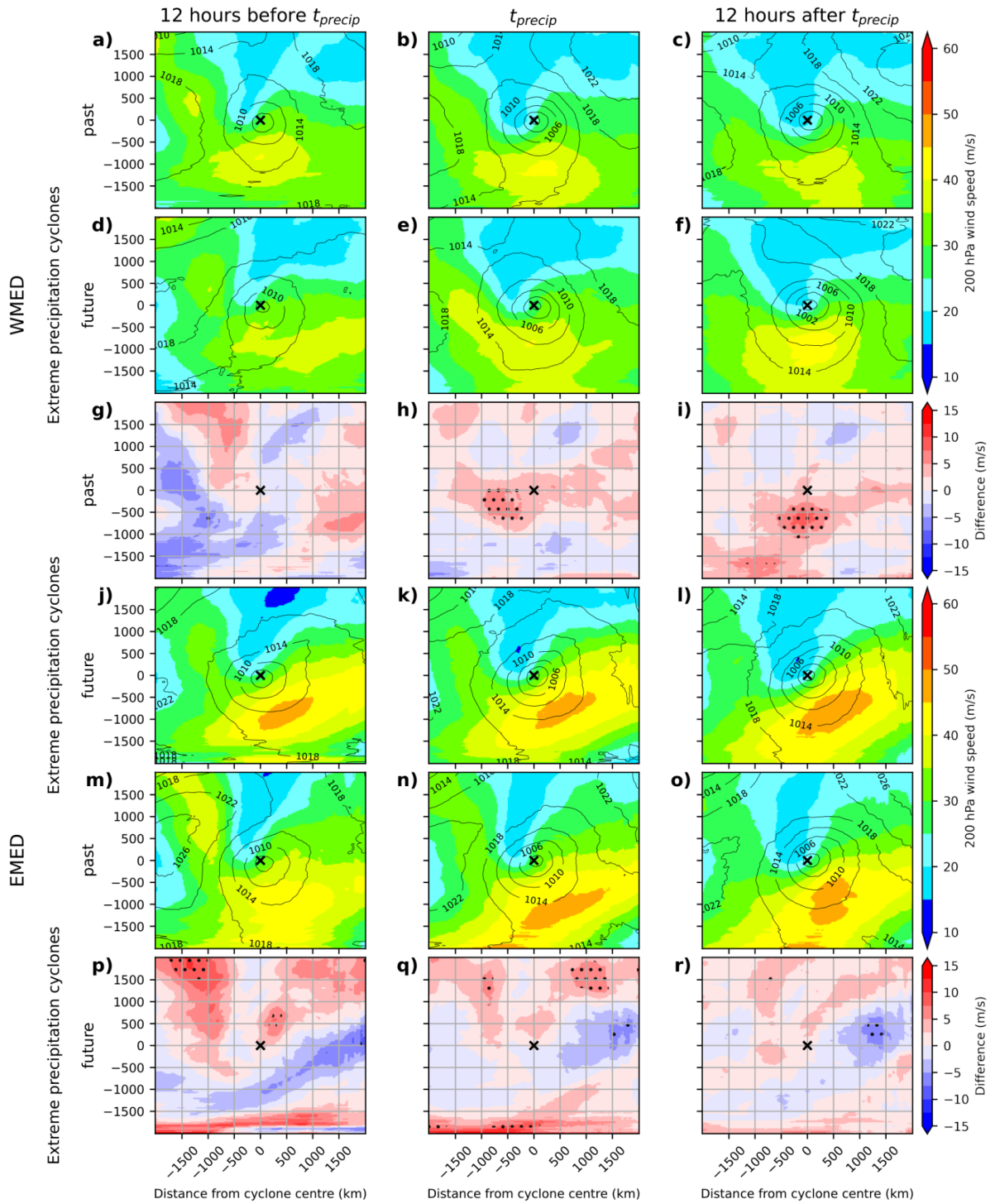
“Lines 254–259: Several studies have shown that stronger diabatic forcing can lead to smaller, more compact cyclones. Could this help explain the positive–negative rainfall pattern in Fig. 5? Could it also explain the more uniform pattern mentioned in line 305 (perhaps “more axisymmetric” is what is 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 interaction 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.”

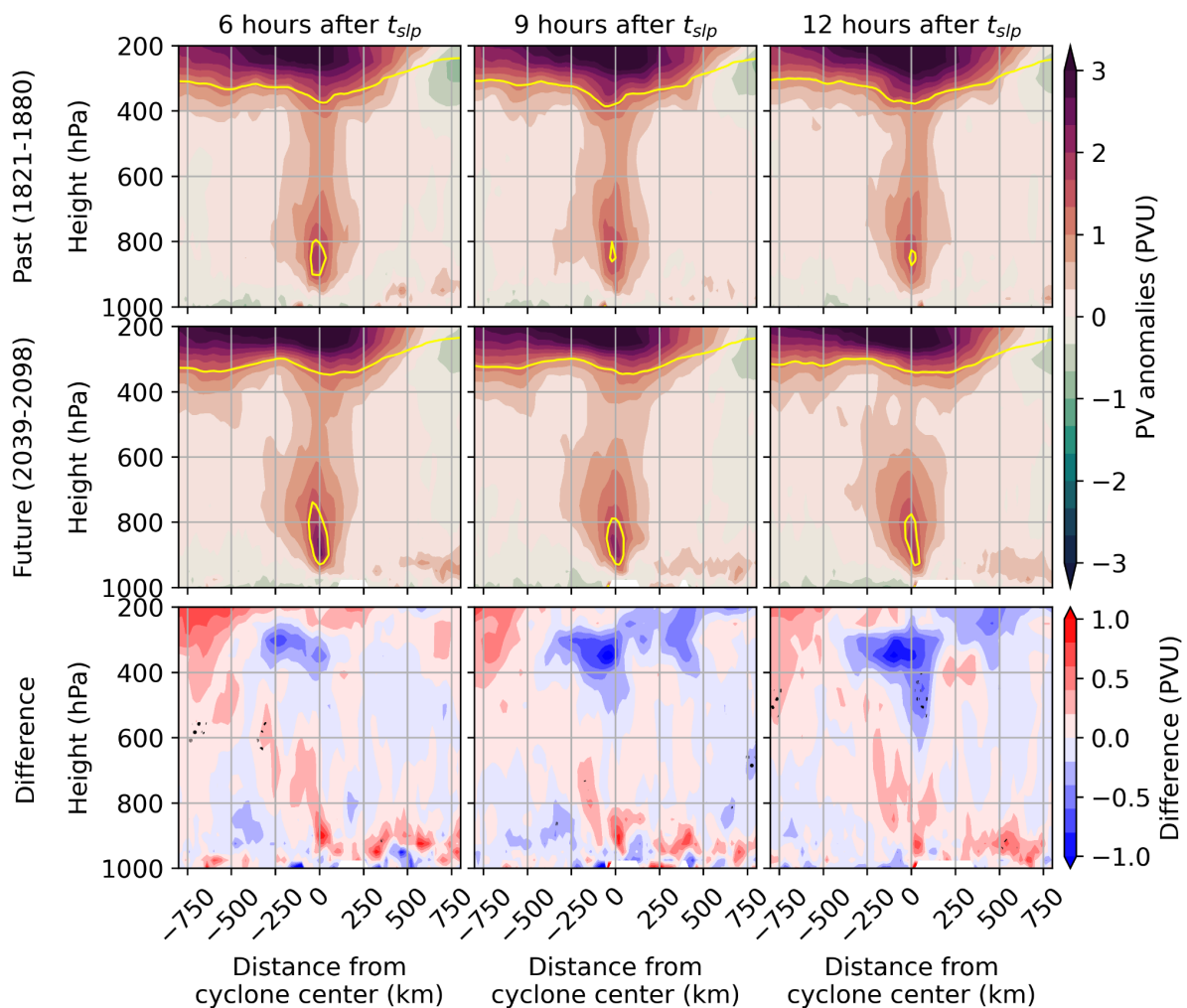
Fig. 6 explicitly only discusses precipitation EXCs. In lines 321-329 we try to find an explanation for the increase in wind speed. Thus, we look at two different kinds of events and it is not trivial to connect lines 321-329 with Fig. 6. Also, as mentioned in one of the comments below, there is no significant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

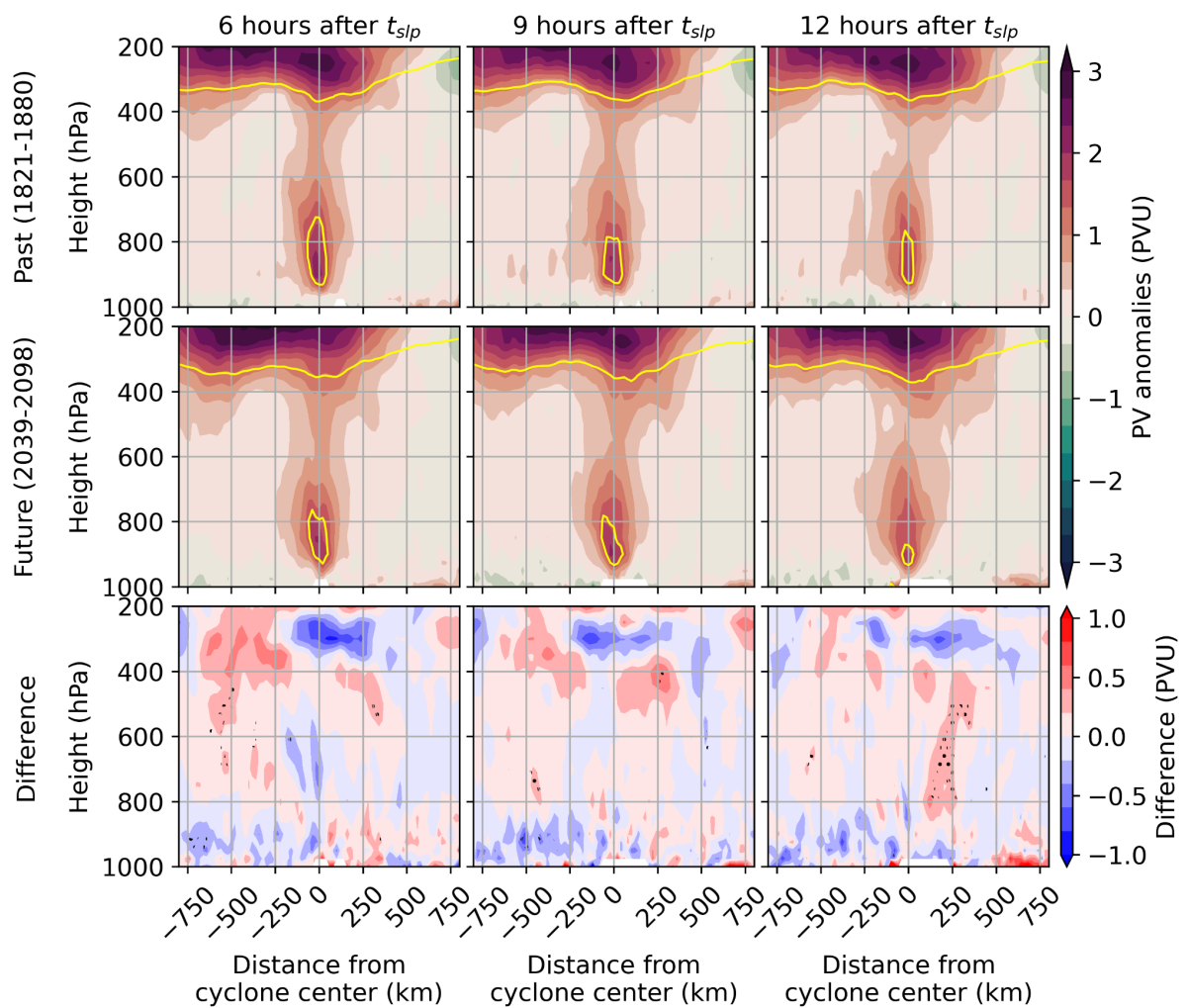
We do not find a significant increase in diabatically produced PV for wind speed EXCs in either the WMED or EMED (See Figures below). Yet, we do not mention this in the text and to avoid confusion we add: (lines 351-353)

“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.