*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 »*

*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 attendibility, 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 severeimpacts 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 : Schermann 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 Schermann et al. Can you correct or explain/motivate your statement further ?"*

We thank the reviewer for their suggestion in this matter. Indeed Schermann 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 inconsistence 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."