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

Major comments

1: *Given a baroclinic growing cyclone, I would expect maximum precipitation (rates) on average around the time of maximum intensification (e.g. Papritz et al., 2021). This is also reflected in the precipitation composites in e.g. Figure 8, where the maximum precipitation for the extreme wind composites (Fig. b and c) is before the minimum pressure is reached. That the precipitation maximum is still at the minimum pressure for the extreme precipitation composites, is probably due to the sampling on the maximum precipitation, as the authors also remark themselves. That the cyclones grow on average baroclinically is visible in some of the composites a westward tilt of the minimum pressure with height. Can the authors argue why they did made this choice?*

We agree with this comment that it is well-known that cyclone-related precipitation peaks before the cyclone reaches its mature stage (i.e. when the cyclone reaches its lower core pressure) (Booth et al., 2018, Papritz et al., 2021). We decided to look at precipitation at the time of lowest core pressure to let it be consistent with wind speed. However, we do agree that this is not the best way to select extreme precipitation cyclones. Therefore, we will repeat the analysis for extreme precipitation cyclones by selecting those cyclones which have the highest precipitation rate independent of the core pressure. Still, the cyclone state with the highest precipitation rate must be located within one of the corresponding boxes of Fig. 1, defining the central and eastern Mediterranean. Our expectation is that, for example, Fig. 8 a,d,g, and Fig. 9 a,d will change and show, e.g. that the highest precipitation rate is reached before the minimum core pressure.

2: *The minimum pressure is not the best way to assess cyclone strength, since it depends on both the latitude (since there is a equator to pole gradient of pressure) and the size, since a minimum pressure does not necessary indicate a strong gradient of pressure. Can the authors argue why this would be a sensible choice, and have they tested for example using the gradient of gpm per 1000 km, since given the cyclone detection algorithm they have this information available?*

We fully agree with the reviewer and can see that the reviewer might have misinterpreted our results. We use wind speed as an indication of strength. We only use minimum core SLP to illustrate the life cycle and the mature stage of a cyclone. In Fig 8 d,h, we show the minimum core SLP to give the community the opportunity to compare our results with existing literature, as still most of the studies use minimum core SLP as a strength measure.
Note that the strength of the cyclone in Fig 8 is based on its related cyclone-related precipitation and wind speed at the time of minimal core pressure. As shown by Pfahl and Sprenger (2016), this works well for wind speed. For precipitation, this method has drawbacks, as explained in the reply above.

As a side remark, the issue of minimum core SLP versus gradient might not be so relevant here in this study, as our region does not span over a large range of latitudes. We show the gradient pressure relationship to illustrate this here in the response (see below Fig. S1).
To adapt this point we will revise the text accordingly.

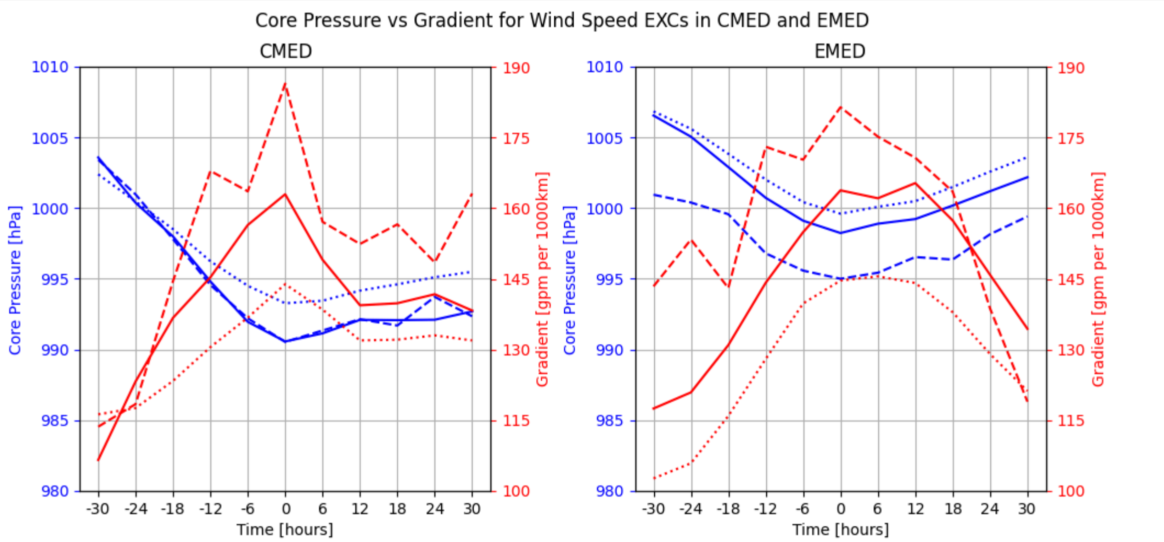

Fig S1: Core pressure (blue) versus gradient (red) for the 10 (striped lines), 100 (normal lines) and 1000 (dotted lines) most extreme cyclones in the central Mediterranean (left) and the eastern Mediterranean (right).

In Fig. S1, we show the relation between gradient and core pressure for EXC10, EXC100 and EXC1000, similar to Fig. 8g–i in the manuscript. The relation that the highest gradient is achieved when the core cyclone pressure is the lowest is evident for the central Mediterranean and is also true most of the time in the eastern Mediterranean. We assume this will be the same for other measures like cyclone depth. Hence for individual tracks, using the time of minimum core pressure is a good indication of cyclone maturity.

3: *In several parts of the paper (e.g. line 50) the authors argue that the modes of variability influence the occurrence and strength of Mediterranean cyclones. This would suggest some causal link. However, I would argue that it is merely a correlation, as the authors also write for e.g. the link to the NAO (line 48). I would therefore suggest rewriting the text at these points a bit.*

This is a fair point. We agree that our wording is too strong and the general atmospheric circulation modes are not a cause of cyclones but merely a correlation. These points will be addressed in the revised version.

Minor comments

Line 8: Does this variability refer to the variability of cyclone frequency?

We agree that this was not fully clear. We refer to a set of cyclone characteristics, not just frequency. The variability of cyclone frequency, 90th percentile cyclone-related precipitation and 90th percentile cyclone-related wind speed are in the order of 5%.

To clarify this, we will change it to:

"We found that Mediterranean cyclone characteristics exhibit pronounced multi-decadal variability in the order of 5% throughout the entire late Holocene with respect to several cyclone-related properties."

Lines 47-49: This is an almost exact repetition of the sentence before, so I would suggest rewriting it and remove one the lines. I think this sentence is clearer than the previous, so I would suggest keeping this sentence.

We agree, we would rather keep the latter sentence.

Line 70: Does they refer to proxies?

Yes, "they" refers to proxies. This sentence should be rewritten anyway to make it flow better, for example:

"Yet, since proxies are usually only sensitive to temperature and precipitation, and not to wind and pressure, it is complicated to reconstruct cyclone activity directly (Raible et al., 2021)."

Line 92: The focus on extremes could be motivated a bit more since it is a vital part of the manuscript.

Agreed, some more background literature will be provided.

Line 103: 1.9 x 2.5 degree longitude and latitude respectively?

Yes, indeed, this will be clarified in the text.

Line 119: Given that you use the Z1000, you probably detect minima in the geopotential (height) and not pressure directly?

Indeed, the sentence should be changed a bit to make it more consistent, for example:

"The algorithm identified local minima in the Z1000 field"

Line 121: Local minimum or averaged over the 1000 km?

It should be the mean gradient averaged over 1000 km. This will be changed to:

“A minimum mean gradient of at least 20 geopotential meters (gpm) per 1000 km is achieved”

Line 124-125: I do not understand it completely, since as far as I understand the authors track the cyclones on a 6-hourly resolution, so why is this criterion applied daily?

It means the new cyclone centre cannot be more than 1000 km away from the previous centre if the time difference were to be 24h. However, since we have 6-hourly data, this basically means that the new cyclone centre cannot be further away than 250 km. We agree this is not very clear, though, and this will be clarified in the text as follows:

“The new minimum of the cyclone must be within 250 km of the previous cyclone minimum.”

Line 143: Are these eruptions used, independently, where they occurred on earth?

Yes, they can occur anywhere on Earth. Most of the major eruptions have a tropical origin and thus can affect both hemispheres. This will also be emphasized in section 2.3.

Line 144: Why is there a time frame of 5 years used before an eruption and only a time frame of 2 years after the eruption?

The time frame of 5 years before the eruptions will provide enough years to characterize internal climate variability (note that we do this for 20 eruptions, which equals 100 years of “undisturbed climate variability”). We only use the first two years after an eruption as these are the years when the impact of the forcing is strongest, so we expect to see the strongest signals of volcanoes. Our results already indicate that the impact is not strong, so extending it to longer periods after an eruption will decrease the signal-to-noise ratio.

Line 154: This region could be indicated in one of the Figures.

Since the region we chose to define the PC-based NAO is a standard region as defined by Hurrell et al. (1995), and since we don’t really see a suitable figure to include a region this large, we think our paper will remain more structured if we just leave the boundaries of the region in the text as defined in line 154.

Line 167: See above major remark, why not selecting on the time step of maximum intensification, since this is one would expect strongest precipitation rates?

See response to major comment 1.

Line 176: How many cyclones are detected in total, or in other words, what is the fraction of selected ‘extreme’ cyclones?

That’s a fair point, and we will include the numbers in the text.

Lines 200-205: The authors argue that the storm tracks are too zonal compared to ERA5 in the CESM model. However, if I would for example look at the DJF climatology, I would almost argue the opposite: there are relatively more cyclones detected at the northern side of the storm tracks, and less at the southern side. Can the authors explain why they argue that the storm tracks are too zonal in CESM?

The comment that CESM is too zonal mainly refers to the Atlantic, where, in our opinion, the lack of cyclones penetrating the subtropics is evident, and we allocate this to a zonal bias. However, as the other reviewer pointed out, this is probably also a consequence of a northern shift in the storm tracks. This will be accounted for in the corrected version.

Line 210: See previous remark, isn't it more a northward shift of the storm tracks (away from the Mediterranean)?

See previous comment for line 200-205.

Line 229: I might have missed this, but the 850 hPa temperature related to the cyclones is calculated in a certain area/radius around the cyclones? And the plotted temperature in Figure 4 is then the average over all cyclones occurring in a certain year?

Fig 4a refers to the 30-year running mean T850 anomalies for the two boxes in Fig 1b combined. Fig 4a is not related to any cyclone-related metrics and just acts as a proxy for the state of the climate of the last 3500 years. We will write this more explicitly in the updated version of the manuscript.

Line 259: See remark above, I would be careful to describe these modes as 'drivers of the circulation'.

See reaction to major comment 3.

Line 268: See previous remark

See reaction to major comment 3.

Line 287-288: I think this sentence could be moved to the methodology section, since it is not related to results shown.

Given that we find no relevant correlation at all and the fact that this sentence does not fit into the rest of the narrative, we have dedicated to remove this sentence from the manuscript.

Line 300: To what does the three different EXC types refer too? I assume it is the extreme precipitation, wind and compound composites? This could be further clarified.

Although they are mentioned in section 2.5, we agree it would be useful to note them down here again for the sake of clarity. This will be included in the revised version.

Lines 320-321: I do not understand what the authors try to argue here, can the authors clarify their argument here?

When observing the wind speed composites Fig. 7, visually, there are wind speed and compounding EXCs that are much stronger in the central Mediterranean compared to the eastern Mediterranean (Fig. 7b vs 7e and Fig. 7c vs 7f). This is also evident from the dots indicating statistical differences between the two. The argument here is that grid cells with statistical significance have a much more coherent pattern for the wind speed composites (which mostly coincide with the areas with the highest wind speeds) than the precipitation composites. The statistical significance pattern for the precipitation compounds is much less coherent. We understand, however, where the confusion comes from and this part should be clarified in the text.

Lines 337-338: Given this possible preselection bias, I would strongly suggest the authors to look at the sensitivity of this choice.

See reaction to major comment 1.

Line 352: Is Figure 8 then DJF?

Yes, this obviously should be highlighted.

Line 369: The authors write that the jet stream remains at certain position, which suggests that the jet stream remains at the certain position over a time period, but I don't think that is what the authors mean.

This is indeed not what we mean, and we appreciate the reviewer's sharp observation. What is meant here is that the jet stream is located southeast of the cyclone during the mature phase. Hence, we will change the sentence to the following:

"Also, the jet stream is located southeast of the EXC100 centre in the eastern Mediterranean, whereas the jet stream in the central Mediterranean is only located south and southwest of the EXC100 centre."

Line 378: This already suggests that the (detected) cyclones grow baroclinically

Thank you for the suggestion. We will include the sentence.

Line 385: What is meant with an areawise more negative anomaly?

This simply means that EXCs in the central Mediterranean are accompanied by a larger negative Z500 anomaly with respect to size (i.e. a larger trough in size). We will clarify this.

Lines 459-465: I would suggest writing the abbreviation EXC in full, since this probably would clarify the text.

We agree this will be changed.

Caption Figure 4c: I think it is a precipitation rate (in mm/6h), as also described in the label of the y-axis?

Yes, this is an error, and will be corrected.

Figure 7 and elsewhere: I would suggest to make the text of the regions Central and Eastern Mediterranean bold, the first time I read the figure labels I was confused because I read them as 'central Mediterranean longitude'

We thank the reviewer for the suggestion, and we agree that the suggested correction would make the labels less confusing.

References

Booth, J. F., Naud, C. M., & Jeyaratnam, J. (2018). Extratropical Cyclone Precipitation Life Cycles: A Satellite-Based Analysis. In Geophysical Research Letters (Vol. 45, Issue 16, pp. 8647–8654). American Geophysical Union (AGU). https://doi.org/10.1029/2018gl078977

Hurrell, J. W. (1995). Decadal trends in the North Atlantic oscillation: regional temperatures and precipitation. *Science*, *269*(5224), 676–679. https://doi.org/10.1126/science.269.5224.676

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