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

1: The structure of the results section is imbalanced. On the one hand, the first half presents an overview of different aspects, where many results apply to the North Atlantic rather than the Mediterranean (Figs. 2, 5, 6). They may be relevant for the Mediterranean but this is not discussed. For instance, not much impact of volcanic eruptions is found, nor much link with atmospheric modes. On the other hand, the second half describes intense cases only. It is very detailed but contained in a single section. Some reorganization is required here, with important results highlighted and others streamlined.

Thanks for the reviewer's suggestion. However, we do not fully agree with this comment. One of our intentions of including the Atlantic is because the region is upstream to the Mediterranean. In addition, we would like to argue that the Atlantic offers a possibility of a comparison of the role of volcanic eruptions and atmospheric modes of circulation in the Mediterranean.
Responding to the reviewer's comment, we can see that the arguments we attempted to deliver were not clear. So we will adjust the text to motivate the sections better and explain the relevance of the Mediterranean better than in the current form.
We also agree that some sections need a more smooth transition and some figures show too much information. For Figs 2 and 3, we will only show the results for DJF and JJA, as MAM and SON are not discussed in other sections of the paper, and move the results for MAM and SON to the supplementary section. Furthermore, we also suggest removing the middle and right columns of Fig. 6, as the discussed modes of circulation have no significant impact on cyclone-related wind speed and precipitation in the Mediterranean. These changes will help to move the focus on the Mediterranean.

2: Why are results compared between the western/central and eastern Mediterranean? Dynamical differences between these regions are not introduced, despite the large body of literature about Mediterranean cyclones. In contrast, Genoa lows, Vb cyclones, Sharav cyclones and Medicanes are introduced but not further discussed. Thus, the reader does not know what to learn from the comparison.

We thank the reviewer for raising this point. We do not justify enough why we differentiate between the regions. Partly, this is due to the differences we found in the results, but we agree that we have to justify this more in the introduction. Therefore, we will include a more solid basis (by updating the literature) for the differences between the regions with respect to cyclones in the introduction.

3: *Why is CESM compared with ERA5 for the period 1980–2010 only and not for the full period 1940–present?*

The 30-year reference periods are used to define climate averages as defined by the WMO. Also, the time period suggested by the reviewer overlaps with a strong increase in global temperature and its potential effects on global circulation. Additionally, the pre-1979 ERA5 satellite observations were sparse, which can cause the risk of including biased data with respect to the storm tracks. Note also that extending the period to the present would mean

that we compare observations with emission scenario-driven climate (CESM). Therefore, we see no reason to change this time period.

4. Methods are not always clear and definitions are sometimes repeated, which blurs the interpretation of results.

We agree that some definitions in the methods section are defined vaguely or repeated unnecessarily. With the reviewer's suggestions in the minor comments below, we will address them and make sure the methods section overall becomes clearer and easier to understand.

5. In the results section, please systematically refer to the figure and panel that is discussed. Currently, one has to guess where to look at. Also, figures tend to contain many panels, which are not all discussed and could likely be removed to focus on the main results.

We agree that a lot of panels are not properly referred to, especially in the last few figures - we thank the reviewer for pointing them out. Additionally, in the reply to major comment 1, we suggested changes to some figures to make the information conveyed in the paper as a whole more concise.

Minor comments

l. 3 “their variability in the late Holocene is poorly understood”: more precisely?

More precisely, their spatial and temporal variability in the late Holocene. This will be changed to:

“their spatial and temporal variability is poorly understood”

l. 8 5% in what?

The variability of cyclone frequency, 90th percentile cyclone-related precipitation and 90th percentile cyclone-related wind speed are in the order of 5%.

This will be changed to:

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

l. 9 the relation is described as “weak” in the conclusions

We will add “weak relation” in the abstract to clarify.

l. 24–25 What kind of variability and connection? This is the main motivation for the paper, thus requires (way) more details. Perhaps it is discussed below but it is unclear at that point.

We agree this sentence is unclear. We propose rewriting it in the following way:

"However, the factors driving the variability of Mediterranean cyclone characteristics, especially extreme cyclones, are not fully understood.”

l. 40–41 References are expected here

The paper by Cavicchia et al. (2013) will be included to refer to the Medicane climatology, and the sentence will be changed to the following (including that the peak also takes place in winter:

“ a special type of Mediterranean cyclone is the so-called Medicane, which is a hybrid system between tropical and extratropical storms and often occurs in autumn and winter (Cavicchia et al., 2013)”

l. 42 I don’t fully agree: see, e.g., the devastating cyclone Daniel of September 2023

This is a fair point, but we argue that extratropical cyclones are much more likely to occur in the Mediterranean, and their cumulative impact is much larger. We will weaken the statement to:

“However, due to their rarity, their overall socio-economic impact is not as large as that of extratropical Mediterranean cyclones”

l. 43–44 Feser et al discuss North Atlantic cyclones whereas Flaounas et al discuss Mediterranean cyclones

The reference of Feser et al. (2015) is with respect to the decadal variability this study observed in extratropical cyclones in NW Europe and the Atlantic. The reference of Flaounas et al. (2022) is with respect to section 2.2, where the relation between Mediterranean cyclones and teleconnection patterns is discussed. However, discussing the relation for extratropical cyclones in a more general sense may make more sense. Therefore, instead of Flaounas et al. (2022), we will include the references of Seierstad et al. (2007) and Walz et al. (2018) in the text.

l. 46 Wintertime precipitation correlates negatively with NAO but positively with cyclone frequency

This is an error in the structure of the sentence. The sentence should be:

“The NAO correlates negatively with the frequency of cyclones in the Mediterranean (Raible et al., 2007) and also negatively with wintertime precipitation over the Mediterranean (Brandimarte et al., 2011; Montaldo and Sarigu, 2017).

l. 65 What scales are referred to by short/long periods?

This should be of a smaller spatial scale:

“ Mediterranean cyclones, which are usually of a smaller spatial scale than other extratropical cyclones, suffer even more from the low resolution in GCMs (Flaounas et al., 2013)”

l. 84 What scale is referred to by low frequency?

With low-frequency decadal to multi-decadal time scales are meant. Low frequency will be replaced in the text with the latter.

l. 88 It would be worth citing and discussing the few studies, as the impact of volcanic eruptions on cyclones is investigated in the paper

There are very few studies directly investigating the link between volcanic eruptions, and the ones we found are mentioned in the two sentences before. Though with some extra research, we did find an earlier study by Fischer-Bruns et al. (2005), who found no link between volcanic eruptions and storm activity. This study will be included in the introduction as well.

l. 100 missing “in” Kim et al

Brackets will be included (Kim et al., 2021).

l. 115 why not 1940–2024?

See response to major comment.

l. 125 I don't fully understand the condition "across" 24 hours

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. 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."

l. 134 missing "in" Raible et al

Brackets will be included (Raible et al., 2018).

l. 134–138 What is the meaning of fitting a Gaussian function to the geopotential field on a single grid point? Also, rather than describing first the "traditional" selection and then the adaptation, I recommend describing straight away what is actually used here.

We agree that this part is a bit confusing, and we want to highlight that we do not fit a Gaussian function to one grid cell, as that would be impossible. What we meant is that sometimes the radii are so small that they do not extend over more than one grid cell. Still, to clarify, we decided to rewrite the last paragraphs of section 2.2 as follows (shortening it significantly):

"The cyclone identification and tracking method provided a variety of cyclone characteristics, such as the cyclone position, the radius of a cyclone, the cyclone depth, the core (central) pressure, and cyclone-related mean and extreme precipitation and wind speed. To compute the cyclone radius, first, a Gaussian function was fitted to the Z1000 field, assuming that the cyclone was azimuthally symmetric (Schneidereit et al., 2010). The cyclone radius is then defined as the distance between the cyclone centre and the point of 1.5 standard deviation (which represents the middle between the first and second inflection points), as done by Messmer and Simmonds (2021). The depth of the cyclone is defined as the difference between Z1000 in the centre of the cyclone and the Z1000 mean over the area of 1000 × 1000 km2. To calculate the cyclone-related wind speed and precipitation, the maximum value of wind speed and precipitation of all grid cells within a cyclone's radius were considered (Raible et al., 2018). We also applied the tracking algorithm to the ERA5 Z1000 field to evaluate the Mediterranean cyclone characteristics in CESM, mostly focusing on the cyclone tracks."

l. 148 typo "significance"

Will be corrected.

l. 159 rendering issue

Will be corrected.

l. 161–165 not sure what "western Mediterranean" means here; actually the CMED region in Fig. 1 largely contains the region usually referred to as the West Med. Thus, for consistency with previous studies, and considering that only two regions are compared here, I recommend naming them West/East Med.

This is a fair point, and we will change it in the manuscript.

l. 166–177 The precipitation and wind speed metrics are repeated several times in the paragraph but in an inconsistent fashion (with/out max, with/out t0); I recommend defining them once for good and then simply using precipitation and wind speed (as in l. 177). Also, which radius is used along the track for precipitation and wind speed?

According to the reviewer's comment, we updated the paragraph as follows:

"To do this, we ranked them based on the maximum 6-hourly cyclone-related precipitation (hereafter just precipitation) and 850 hPa wind speed (hereafter just wind speed) at the time when the cyclone reaches the lowest core sea level pressure within its track (hereafter t0)."

The rest of the paragraph will be corrected accordingly.

l. 173 what is the untransformed distribution?

For the selection of extreme precipitation cyclones, the assumption is that one picks the cyclones from the tail of a Gaussian distribution. This clearly is not the case for precipitation, and therefore, we compute the square root of the cyclone-related precipitation so the distribution looks more Gaussian.

To clarify this, we will explain this in more detail in the manuscript:

"The distribution for precipitation is skewed towards extreme values and values close to zero. Therefore, all 6-hourly precipitation rates below 1 mm were excluded, and we computed the square root of the remaining precipitation to assume a Gaussian distribution for precipitation."

l. 177 here and elsewhere: CMED and EMED to keep the same terminology

See comment for l. 161-165.

l. 180 t0 already defined

Agree and the sentence will be changed to the following:

"For each cyclone track, we set the reference time t0. Every time step of the track that occurred after t0 obtains a positive index, and the time steps of the track that occurred before t0 receive a negative index."

l. 185 missing "at" t0?

Indeed missing "at", will be included.

l. 186 variations in what? I don't get the point here

This refers to the "variations of the weights of the grid cell". Due to the small range of latitudes and the southerly location of the Mediterranean, we deem the differences in weights of the grid cells insignificant for the EXC composites. However, the sentence should be rephrased to make it clearer:

“Since all the cyclones were located in the Mediterranean basin t0 and, therefore, within a relatively small range of latitudes, variations due to differences in latitude weights of grid cells were considered insignificant and, therefore, negligible.”

l. 188 Why 30h? And missing “for” t0?

30h is ratheran arbitrary value. We deem it enough to capture the intensification and decaying phase of the cyclone. We also tried plotting 48h before and after the cyclone, but it did not provide any extra insights. And indeed, “for” is missing.

l. 191 are all fields averaged over the area of both regions for all cyclones?

Bullet point number 4 should be clearer. We do not mean we compute averages of all fields that we have. We will compute spatial averages of the fields noted in the paragraph below. To clarify this, we will change bullet point 4 to the following:

“For every time step within these 30 hours before and after t0, we compute spatial averages for EXC10, EXC100, and EXC1000. This was done for both regions for cyclones associated with cyclone-related precipitation, wind, and compound extreme. Cyclone tracks, which did not appear at any of these time steps, were ignored for the temporal means.”

l. 192 Cyclone tracks “that” did not appear (and no comma)

Will be changed.

l. 195 Some details are expected about the RWP amplitude. And typo: Rossby wave “packet”

Will be included, and the typo will be corrected.

l. 200ff In the discussion on Fig. 2 please indicate which panel is referred to

We will refer to the panels more consistently in this part of the text.

l. 204 Fig. 2 suggests a northward shift rather than a zonal vs wavy storm track

We agree that Fig. 2 also shows a northward shift of the jet. However, we would also argue that it is not just a northward shift of the jet, but also a decrease in waviness (e.g. CESM not capturing cyclone activity around the Azores in DJF). The sentence will be changed to:

“Thus, the biases generally indicate that storm tracks in CESM are too zonal and shifted northward compared to the storm tracks in ERA5.”

l. 209–214 Any evidence for these assumptions? Otherwise they sound speculative

In the introduction (line 31), we mention that about 20% of Mediterranean cyclones originate in the Atlantic. Now, although this does not account for the 50% underestimation we see in Fig. 2, we hypothesize that a less wavy and more northerly jet stream would contribute to fewer cyclones penetrating the Mediterranean and would also hamper conditions for lee cyclogenesis. We argue this is not merely speculation.

l. 216 “while” rather than “although”

Will be changed.

l. 221 this is speculative but should be easy to verify

To avoid speculation, we remove the sentence hypothesizing about the heat lows.

l. 224 where is it most often underestimated by 50%? it is unclear where this number comes from

This refers to the tails of the distribution of Fig. 6m–p, and the difference is about 50%. To clarify, we will rewrite it to the following:

“CESM struggles to reproduce high cyclone-related precipitation events in all four seasons, where the high-end tails of the distributions are most often underestimated by 50%”

Fig. 3 wind speed is the max value here, while it is defined as the value at t0 in Section 2.5

We appreciate the reviewer’s sharpness here. We agree it is not very consistent to use max wind speed in a cyclone track instead of the wind speed at t0, so this will be adjusted in Fig. 3. However, we do emphasize that we do not expect a significant difference here.

l. 234 the 30y running mean does not show similar patterns and is not appropriate for tendencies

Our statement in line 234 is not worded properly and, therefore, causes confusion. What we mean is that in both Fig. 4a and Fig. 4b multidecadal variability is clearly visible, not that the variability in Fig. 4a and 4b are correlated. To clarify this, we change line 234 to the following:

“The cyclone frequency in the Mediterranean (Fig. 4b) also exhibits a clear multidecadal variability.”

l. 235 is this definition of cyclone frequency different from that used elsewhere in the paper?

Slightly, as we sum the individual cyclone centres per time step in both regions for every month. However, Fig 4b does not take into account the cyclone radius as in Fig 1. To clarify the differences, we will change the label of Fig 4b to “total cyclone time steps”.

l. 255–256 Repeats previous sentences

This sentence can be shortened to:

“Only a small region with a significant increase in cyclone frequency is found in the eastern Mediterranean, but it still accounts for an increase of 30% in this region.”

l. 260 “important” driver?

This should, of course, be important.

l. 263–265 Please refer to the corresponding panels

Correct references to panels will be included.

l. 266 Fig. 6 is already presented before

Good point. The first paragraph will be rewritten to make it flow better.

l. 287 this sentence is surprising, as sea ice anomalies are not discussed in the methods

After reviewer #1 also highlighted this, and since this sentence does not really fit in the rest of the manuscript, we have decided to remove it.

l. 289 this should be mentioned earlier, and clarified that Fig. 6 shows DJF only

That is a fair point, of course, DJF should be included in the first paragraph of section 3.4

l. 303 the presence of the warm sector of the cyclone (I) could be verified and (II) does not dynamically explain the highest wind speeds (see, e.g., Raveh-Rubin and Wernli 2015, or papers for the North Atlantic)

It is true that the presence of the warm sector is not the cause of the highest wind speeds. However, it is also obvious from Fig. 9 that at t0, the location of the warm sector and the highest wind speeds are correlated. To not cause any confusion or speculation though, we will discuss this in the paragraphs where Fig. 9 is discussed. Consequently, this discussion in this section of the paper will be removed.

l. 309–311, 320--321 Please refer to the corresponding panels in Fig. 7

Will be included.

l. 312–313 not only northward but also (obviously) westward! Western/central and eastern Mediterranean cyclones have different dynamics, which should be discussed in the introduction (see, e.g., Doiteau et al. 2024, or older papers)

That is a very fair point, and as discussed in major comment 2, we agree there should be more emphasis on the differences between the two regions. Hence, we will give a more solid literature basis in the introduction.

l. 316 it is expected indeed; I don't quite get the point at showing precipitation for windy cyclones and wind for rainy cyclones in Figs. 7–9

We think it is needed to provide the full characterization of the different cyclones. Just because a cyclone has a wind extreme, does not mean that there is no precipitation. We think it is important to highlight these differences to show if different categories are unique or not.

l. 327 Fig. 8 is already referred to on l. 308

The sentence with the introduction of Fig. 8 in l 308 should not be there and will be removed.

l. 329 This is true for panels (a) and (d) but not (b) for instance

We should highlight that this is only the case for wind speed, and not for precipitation. Also, the other reviewer highlighted that selecting precipitation at t0 is problematic, and this method will change. So this section will likely change quite significantly.

l330, I do not understand: there are triangles in the plots, indicating significant differences for EXC100 (also, it should be clarified that the symbols indicate statistical significance)

The markers indicate statistical significance for EXC10 (circle), EXC100 (triangle) and EXC1000 (square) between the two regions for each timestep. The definition of the symbols is in the caption of Fig. 8.

l. 335 This contradicts l. 320 (see comment above)

See the comment for l 329.

l. 337 This questions the relevance of the definition of EXCs (see above comments on methods

See the comment for l 329.

l. 349 typo: "cyclones"

Will be changed.

l. 352–362 Why look at summer cyclones separately? This is quite a long description for a figure that is not shown in the paper

As mentioned in the review paper by Flaounas et al. (2022), there are not a lot of papers on extreme cyclones in summer and this is a research gap we deem highly significant to address. However, for the sake of space, we decided to put these figures in the supplement.

l. 365 See comment on l. 303

See the comment for l. 303.

l. 380 why "often"? Why "again"?

"often" and "again" can be removed.

l. 422ff references to specific figures are unexpected in the conclusion

This is fair, the references will be removed.

l. 439 Please explicit, and clarify whether it is your result or arises from the cited study

The latter part refers to the cited study. The sentence should be changed to something like this:

“ even though the horizontal resolution of the climate model used is too coarse, as discussed in Flaounas et al. (2013).”

l. 442 the wind and precipitation are also underestimated compared to ERA5

That is a very good point to highlight!

l. 451 an earlier study cannot confirm your current results: the other way round

Thanks, this, will be changed to “Our results confirm this”.

l. 470 Flaounas et al. (2015b)

Will be changed.

l. 471 and a very different time period!

This will be included.

l. 472 “region”: better “area” to avoid confusion with the east/west Med

Will be changed.

l. 476 max???

This was a typo andwill be removed.

l. 477 Homar et al. (2007)

Will be changed.

l. 484 5% in frequency?

See comment on l. 8. This should be “in the order of 5% from the multi-millennial mean”.

Still “up to” should be replaced by roughly, as the multi-decadal variability is roughly 5% for several aspects relating to Mediterranean cyclones.

References

Cavicchia, L., Von Storch, H., & Gualdi, S. (2013). A long-term climatology of medicanes. *Climate Dynamics*, *43*(5–6), 1183–1195. https://doi.org/10.1007/s00382-013-1893-7