Reviewer #1:

The authors have addressed my comments adequately.

We thank the Reviewer for dedicating time to review our manuscript and for the observations that have raised the quality of the paper.

Below, the Reviewer will find our detailed, point-by-point, answers.

I have two minor suggestions:

Reconsider the phrasing of the following sentence (line 269): " In this context, atmosphere-ocean coupling is expected to have a stronger influence on the physical processes within the cyclone systems, and a rather weaker effect on their formation, distribution, and track characteristics." In my opinion, the differences in Figure 3d are not so small. In some locations, they are larger than 10. Considering that the largest number of cyclones in ERA5 is about 50, a difference of 10 means at least 20% and probably more (depending on the exact pixel). When plotted with the same color scale, it looks small because the difference is smaller in general than the difference relative to observations (panels 3b,3c). It would probably look clearer if you used the percentage instead of the total number.

We thank the Reviewer for raising this point. The Reviewer is correct, when looking at the differences in cyclone distribution between CPL and STD (Fig. 3d), the atmosphere-ocean coupling appears to have an impact in the location of the cyclone minima over the Sea, and a rather weak effect only on the seasonal cycle and statistics of the tracks. Based on this, we have revised our conclusions in Section 3.1 as follow (L266-273):

"*In contrast, changes in the SST distribution primarily affect the location of cyclone minima over the sea (Fig. 3d), leading to differences between STD and CPL over the Ionian and Tyrrhenian Sea. Interestingly, when compared to ERA5, the CPL model reproduces the cyclone distribution over the sea slightly more accurately than STD, with a lower root mean square error (RMSE) in the location of cyclone minima (2.16 vs. 2.17 for STD), despite having greater degrees of freedom (i.e., the ocean domain in CPL is not constrained to observed SST). In conclusion, cyclone systems arise from a combination of large-scale processes (external to the cyclone) and small-scale processes (internal to the cyclone). In this context, atmosphere-ocean coupling is expected to have a stronger influence on the physical processes within the cyclone systems, and a minor, yet significant impact on their locations*."

Following the Reviewer's suggestion, we have updated Figure 3 to display cyclone centre densities (CCD) as percentages, normalized by the total number of cyclones (the 500 most intense). The revised Figure 3 is shown below.

We have also changed the sentence in lines (251-252) to clarify the definition of the total number of CCD occurrences:

"*Consequently, the CCD maps (Fig. 3) indicate the number of cyclone occurrences at each grid point, normalised by the total number of cyclones (the 500 most intense)*."

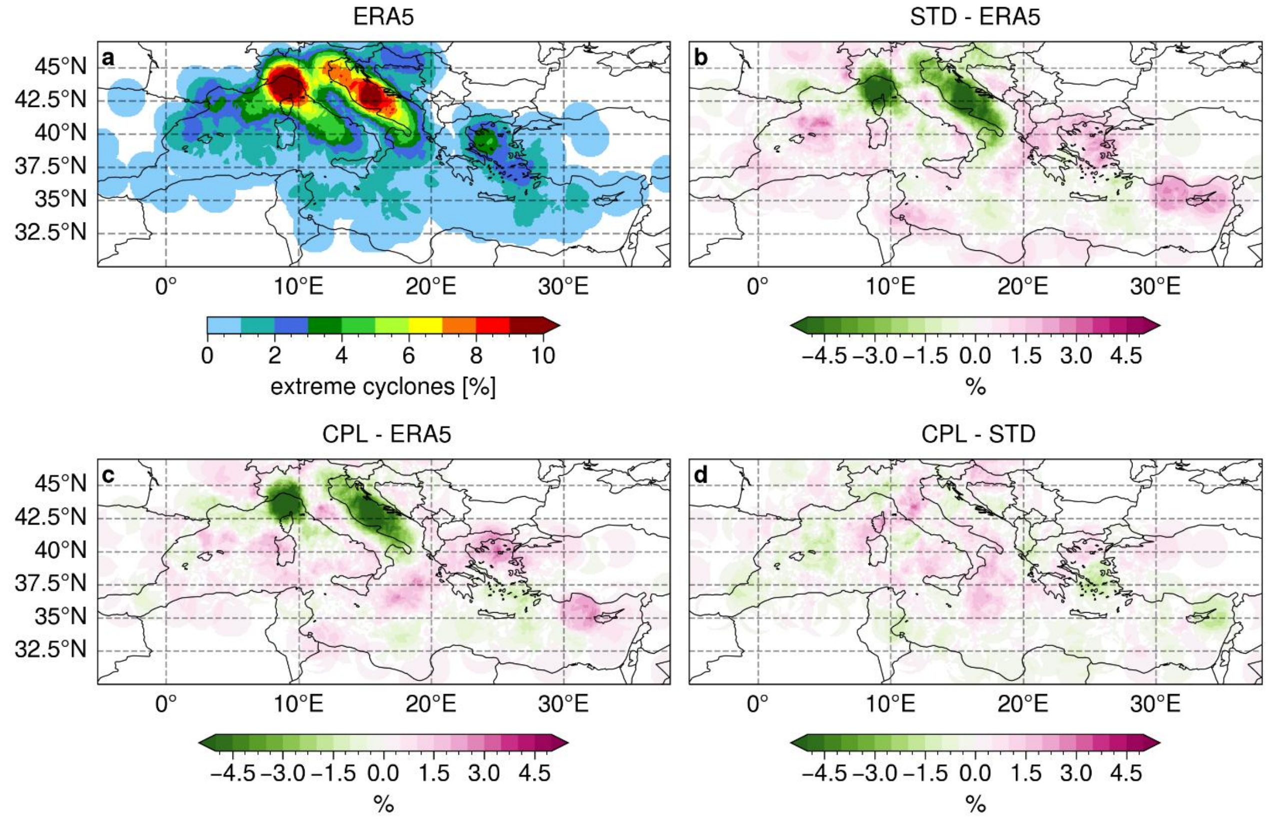

Figure 3: Cyclone centre densities (CCD) for the 500 most intense cyclones in ERA5 (a), along with CCD differences between STD and ERA5 (b), CPL and ERA5 (c), and CPL and STD (d). The values are normalised by the total number of cyclones (i.e. 500) and expressed as percentage. To highlight the cyclones' area of influence, each centre is represented by a circular area with radius of 1.5 degrees around the tracked minimum SLP point.

Also, I think that if a coupled model has a better representation of the cyclones (though it is minor, according to the calculation of RMSE of the distance between observed and simulated cyclone location at maximum intensity) is still impressive, considering it has much larger degrees of freedom (i.e., the ocean domain is not constrained to observed SST). I would say something about this in the manuscript.

We thank the Reviewer for this suggestion. In Section 3.1, we have added the following sentence (L268-271):

"*Interestingly, when compared to ERA5, the CPL model reproduces the cyclone distribution over the sea slightly more accurately than STD, with a lower root mean square error (RMSE) in the location of cyclone minima (2.16 vs. 2.17 for STD), despite having greater degrees of freedom (i.e., the ocean domain in CPL is not constrained to observed SST).*"

Reviewer #2:

Overall comment

I appreciate the authors' efforts toward the improvement of the manuscript. I did find the manuscript has been substantially improved.

Nevertheless, some of the concerns over the earlier version of the manuscript have not been fully resolved, and as a consequence, there are still a few issues to be addressed, as commented specifically below.

Therefore, I think that another major revision is required before the manuscript could be published.

We thank the Reviewer for dedicating time to review our manuscript and for the observations that have raised the quality of the paper.

Below, the Reviewer will find our detailed, point-by-point, answers.

Comments (In the following, line numbers correspond to those in the revised manuscript with a trace of modifications unless otherwise specified)

A) After I read the authors' response to my comments on the earlier version of the manuscript and the revised manuscript, I have better grasped the arguments of this study. Specifically, I understand that the SST difference between STD and CPL does not have a large impact on the cyclone density distribution. However, it leads to the impression that the main conclusions of this study are somewhat unclear. Probably I still do not fully figure out what the authors' are arguing, but it is recommended that the authors should make their standpoints and conclusions of this study clearer as readers easily can find a way to follow.

We thank the Reviewer to point out this issue. We believe that the main findings and the novelty of our work was really improved in the revised manuscript. However, the valuable feedback raised by the Reviewer helped us to improve the clarity of the main findings presented in the conclusion (Section 4, L461-467):

"*This research demonstrates for the first time the ability of the coupled model to coherently simulate the entire atmosphere-ocean system, offering novel insights into how extreme Mediterranean cyclones influence both atmospheric and oceanic processes. Specifically, it investigates how energy released at the sea surface during these events affects the atmospheric boundary layer and the ocean mixed layer. Furthermore, comparing the models allows for quantifying the impact of sea surface available energy on precipitation and surface wind speed associated to extreme Mediterranean cyclones. These findings are of crucial importance in the climate change context, since atmosphere-ocean coupled RCMs give the possibility to reduce the uncertainty deriving from coarse-resolution SSTs coming from the global models*."

B) I am still puzzled by the interpretation of the SST differences between the observations and experiments. I expect that the SST difference between CPL and STD evaluates the effect of coupling in the model's domain.

For instance, Figs. 5b and 6b tell us how air-sea coupling in the Mediterranean Sea affects cyclone-related environments in the model. However, this study compares the climatological SST in STD and CPL with the MED-REP-L4 SST, instead of ERA5 SST, which is used as the boundary condition of STD. The climatological SST difference between STD and MED-REP-L4 (Figs. 5d and 6d) is seemingly due simply to the difference in data used, and why do the authors show it? In addition, I understand that the SST difference between CPL and MED-REP-L4 (Figs. 5c and 6c) corresponds to the bias of the model (as mentioned in Section 3.3), suggesting that the CPL model has a warm (cool) bias up to 1~2K in its climatological SST. This contrasts with the SST in STD, which is much closer to the observations (MED-REP-L4) even when ERA5 SST is prescribed.

Then Section 3.4 argues that the SST difference between STD and CPL is the dominant factor shaping both the sea surface fluxes and the precipitation and wind speed differences associated with the extreme cyclones. Does this indicate that, for example, "the analysis shows the ability of the coupled model to coherently represent the dynamic and thermodynamic processes associated with extreme cyclones across both the atmosphere and the ocean" (in the abstract)? I agree with this in terms of the ocean response to cyclones, but I am afraid that this is rather not valid for the processes in the atmosphere, given the SST bias in CPL. In addition, I understand that mechanisms behind the SST difference between STD and CPL, in other words, "coupling effect", are beyond the scope of the present study (as in the authors' reply). Nevertheless, the way in which the SST difference should be interpreted needs further

clarification. The authors might also want to mention that the mechanisms for the SST difference need an additional study in the manuscript.

We appreciate the Reviewer's feedback on this point. To better highlight our findings, we have modified Figure 5 as follows:

- **Fig. 5a**: SST distribution in the MED-REP-L4 dataset.
- **Fig. 5b**: SST climatological differences between STD and MED-REP-L4.
- **Fig. 5c**: SST climatological differences between CPL and STD.
- **Fig. 5d**: SST differences between CPL and STD during extreme cyclone events.

The updated Figure 5 is provided below. We have applied the same modifications to Figure 6 (SST in SON).

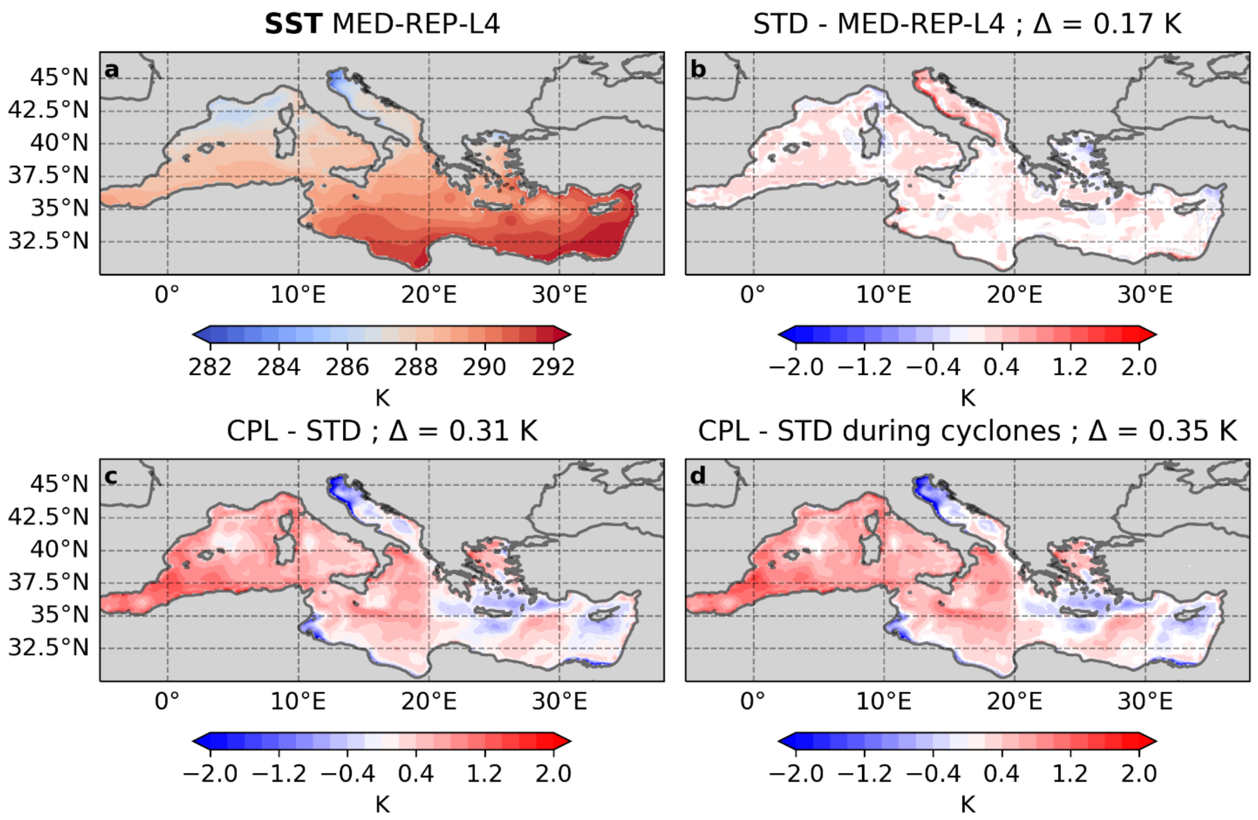

Figure 5: Map of SST from the MED-REP-L4 observational dataset in winter (a). Climatological winter SST differences between STD and MED-REP-L4 (b) and between CPL and STD (c). SST differences between CPL and STD during extreme winter cyclone events (d). The white colour indicates no significant differences at 5 % confidence level. Δ values represent the domain average of the differences where the values are statistically significant.

In Section 3.3 (SST analysis), we focus on the SST differences between CPL and STD. Since the STD simulation uses prescribed SST from ERA5 reanalysis, we firstly validate ERA5 SST against the high-resolution observational dataset MED-REP-L4 (Fig. 5b). Then, to investigate the SST differences between CPL and STD, we compare them at both the climatological scale (Fig. 5c) and during the extreme cyclone events (Fig. 5d). The similarity between these differences (Fig. 5c vs. Fig. 5d) suggests that they are not directly influenced by cyclonic activity but rather reflect a climatological bias in the MITgcm ocean model embedded in CPL compared to ERA5. Similar results are found for SON (Fig. 6c vs. Fig 6d). These findings are presented concisely in the manuscript (L323-328) as follow,

"*During extreme winter cyclone events, compared to STD, the CPL model is remarkably warmer, up to 1.5 °C, over most of the Mediterranean Sea, except for the northern part of the Adriatic Sea and, to a smaller degree,* *the Eastern Sea where the difference has opposite signs (Fig. 5d). SST differences are not associated with the occurrence of the cyclones, but rather to the climatological bias of explicitly resolved SST by the coupled model. Indeed, the same difference appears also when comparing the SST climatology in CPL with STD (Fig. 5c vs. Fig. 5d), while limited differences are found between STD and MED-REP-L4 (Fig. 5b).*"

The significant SST differences between the models motivates us to evaluate their impact on atmospheric processes during extreme cyclone events (section 3.4), when air-sea fluxes and convective processes are expected to be stronger compared to climatological values (as shown in figure S2). Finally, we examine the ocean model to investigate the ocean's response during the extreme cyclones. The findings from Sections 3.4 and 3.5 support our conclusion that the CPL model is able to coherently simulate the dynamic and thermodynamic processes associated with extreme cyclones in both the atmosphere and ocean.

In fact, in the CPL system, the atmospheric model responds to a warmer SST fostering surface latent and sensible heat fluxes, leading to modifications in atmospheric properties up to the top of the boundary layer, with impact on both 10 m wind speed and convective precipitation. At the same time, the strong winds during cyclones, enhancing evaporation and surface heat releases, lead to ocean cooling in the mixed layer and favour the turbulent mixing processes.

If the atmosphere and ocean were not coherently coupled, we would not observe significant differences between CPL and STD. Thus, the CPL model's response to a warmer SST is a direct consequence of the coupling system. However, the SST differences between CPL and STD (ERA5) stem from how the MITgcm ocean model simulates the Mediterranean Sea. Understanding these mechanisms requires further investigation and is beyond the scope of this paper. We have now clarified this point in the manuscript (L333-335):

"*Further information on the validation of the ocean system of the CPL can be found in Anav et al. (2024) across all seasons. However, the underlying mechanism responsible for CPL's climatological SST bias remains unclear and requires further investigation, which is beyond the scope of this study.*"

C) The authors discuss the influence of atmosphere-ocean coupling on physical processes (i.e., diabatic heating related to precipitation) within cyclone systems based on regional model experiments. The discussion by and large makes sense to me. Nevertheless, I consider that the authors need to be careful when discussing "atmosphere-ocean coupling" using a regional model. Diabatic heating within cyclone systems is a moisture sink, which requires a moisture supply. Since moisture is supplied mostly by evaporation from the ocean, I would say that atmosphere-ocean coupling (in any place) is pivotal for diabatic heating within cyclone systems, in the first place. It should be noted that, in regional model experiments with relatively small domains (compared to, say, the entire North Pacific), only moisture supply from the ocean within the model's domain is included in the SST's influence. By contrast, moisture supply from the ocean outside the model's domain is included in the influence of lateral boundaries. Recent studies (Papritz et al. 2021; Okajima et al. 2024) show that moisture to precipitate within extratropical cyclones comes largely from remote regions (oceanic frontal zones or subtropics). Discussing the aspect of remote moisture supply for extratropical cyclones referring to those studies would further clarify the authors' arguments for the relative importance of (rather local) atmosphere-ocean coupling to large-scale forcings.

The Reviewer is correct, the moisture during Mediterranean cyclones events comes not only from the Mediterranean Sea but also from the Atlantic, by advection processes. In our comparison, both models share the same lateral boundary conditions, meaning that the moisture entering the domain from external sources is identical in both simulations, as it is inherited from the ERA5 reanalysis. Therefore, our findings are based on the local air-sea fluxes within the Mediterranean, and their role on the atmospheric processes during extreme cyclone events.

We appreciate the Reviewer for sharing these relevant studies. In response, we have added a sentence in section 3.4 (L346-350) acknowledging the relative importance of both remote (external to the cyclone's area of influence) and local (within the cyclone) moisture sources in contributing to precipitation associated with extratropical cyclones.

"*In the Mediterranean, precipitation within the cyclones is sustained both by moisture advected from remote regions, i.e. the Atlantic Ocean, as well as by local evaporation over the Mediterranean Sea (Flaounas et al., 2016; Raveh-Rubin and Wernli, 2016), similarly to what occurs in extratropical cyclones over open oceans (Okajima et al., 2024; Papritz et al., 2021). However, since CPL and STD share the same lateral boundary conditions from ERA5, the only difference in terms of moisture supply derive from their distinct interactions with the Mediterranean Sea surface*."

Other comments

1. L278: "This upper tropospheric forcing" -> "This large-scale upper tropospheric forcing" will be better.

It has been corrected.

2. L286-287: The argument "therefore, differences from ERA5 should not be taken purely as a weakness of RCMs, but rather as a result of differences when reproducing atmospheric processes" is un convincing and thus unnecessary. While I do not mean that this particular model is not useful for investigating Mediterranean cyclones, there is a difference. I consider it sufficient to describe differences between the model results and ERA5 and to suggest potential related factors.

We thank the Reviewer for raising this point. We initially included this sentence in response to the previous Reviewers' comments on the comparison of the models with ERA5. However, we agree that it can be removed, and we have now corrected the text accordingly.

3. Fig. 9: It would be better to show numbers with 2 (or 3 if needed) decimal points.

We thank the Reviewer for this suggestion. We have updated Figure 9 (DJF) and Figure S6 (SON) to display values with two decimal points.

4. L433, L437 (in the manuscript without a trace of modifications): "Click or tap here to enter text." -> error?

We thank the Reviewer for highlighting this issue. It was an error, and we have now fixed it.

5. Regarding the ocean response to extreme cyclones: Kuwano-Yoshida et al. (2017) pointed out that explosive cyclones over the North Pacific influence the ocean interior reaching 2000 m depth by inducing anomalous cooling and upward flow, based on a case study using an eddy-resolving OGCM. It would be a good comparison for the result of the present study.

We thank the Reviewer for sharing these interesting studies. In Section 3.5, we have added a comment on the impact of cyclones over open oceans, which is significantly larger than over the Mediterranean but follows the same mechanism. Please refer to lines L420-421:

"*Interestingly, these mechanisms are similar to those over open oceans (Kuwano-Yoshida et al. 2017), although with lower magnitude*."

6. Unfortunately, there are still many expressions "storm track method", which the authors might want to replace with "cyclone tracking algorithm" in my comment on the previous version of the manuscript.

Apologies for the misunderstanding regarding your previous comment (minor comment 20 from the first revision). We agree that "cyclone tracking" is more precise than "storm track," which has a broader meaning. We have now corrected this throughout the manuscript.

References

Kuwano-Yoshida, A., Sasaki, H., & Sasai, Y. (2017). Impact of explosive cyclones on the deep ocean in the North Pacific using an eddy-resolving ocean general circulation model. Geophysical research letters, 44(1), 320-329.

Okajima, S., Nakamura, H., & Spengler, T. (2024). Midlatitude oceanic fronts strengthen the hydrological cycle between cyclones and anticyclones. Geophysical Research Letters, 51(6), e2023GL106187.

Papritz, L., Aemisegger, F., & Wernli, H. (2021). Sources and transport pathways of precipitating waters in cold-season deep North Atlantic cyclones. Journal of the Atmospheric Sciences, 78(10), 3349-3368.