Response to reviewer #2

General comment

This manuscript investigates the impact of different vertical mixing schemes and convective adjustment in a regional Mediterranean Sea model. The topic is relevant for ocean modeling, and the experimental design comparing several turbulence closure schemes with and without convective adjustment is useful. However, the manuscript mainly presents a sensitivity comparison between different parameterizations, while the mechanisms driving the simulated differences are not sufficiently analyzed. A deeper analysis of these mechanisms would significantly improve the manuscript. In particular, the role of convective adjustment in relation to turbulence closure schemes is not clearly justified. Overall, the study is potentially valuable, but major revisions are required before it can be considered for publication.

We thank the reviewer for the constructive and thoughtful comments, which significantly helped improving the manuscript. We have addressed the points raised and implemented the requested revisions throughout the paper. During the revision process, we have also identified and corrected a minor bug in our simulations, which does not affect the results or conclusions of the manuscript; the figures have been updated accordingly.

A detailed response and explanation are provided below (reviewer's questions are in blue, our answer in black). Line numbers refer to the revised manuscript with track changes enabled, in No Markup view.

Major concerns

1. The title *"**Untangling the effects of vertical mixing schemes and convective adjustment in the Mediterranean Sea**"* suggests that the manuscript aims to clearly separate the roles of these two processes. However, the current presentation appears closer to a sensitivity comparison between parameterization choices and combination, and the extent to which the individual effects are truly disentangled remains somewhat limited. If the title is to remain unchanged, the analysis may need to be strengthened with clearer process-oriented diagnostics and attribution analysis, in order to better identify the respective roles of vertical mixing and convective adjustment.

   Thank you for this comment. In the revised version of the manuscript, we have substantially strengthened the analysis and expanded the discussion to provide a more in-depth and process-oriented comparison among the experiments, moving beyond a primarily descriptive assessment toward a more mechanistic interpretation.

   The revised manuscript now places stronger emphasis on the underlying physical mechanisms, highlighting how the different vertical mixing schemes and convective adjustment impact local water mass properties and temporal evolution. We now first define the key physical processes occurring in the region and then explicitly relate the discrepancies among the simulations to differences in the physics represented by each mixing scheme in the presence and absence of convective adjustment. This improvement has been achieved through a through revision of large parts of the Results (especially for what concerns the deep and intermediate water formation regions) and the conclusive section, as well as with the addition of several new figures (Figs. 1d, 1f, 2, 10, 11, and 12) which support a cleared attribution of the observed differences.

In particular, we have:

- Expanded the description on **how the different mixing schemes handle convection** (section 2.2.1, lines 205-213, 225-229, 256-263) and explicitly linked the results to the physical description of the mixing schemes, ultimately moving from a descriptive analysis to a mechanistic one;
- added a **seasonal analysis of Argo profiles in key deep and intermediate water formation regions**, namely the South Adratic Pit and the Rhodes Gyre region through the addition of two figures (Fig. 1d and 1e) and a discussion in the manuscript (lines 129-142);
- **expanded the discussion of deep-water formation in the South Adriatic**, both in the text (Section 3.3.1) and through the addition of a new figure (Fig. 10) showing the temporal evolution of the salinity field, highlighting how the different vertical mixing schemes and convective adjustment affect local water mass properties and their temporal evolution. We also further extended this analysis to **include the role of Levantine Intermediate Water intrusion** and its **evolution over time**, providing additional insight into the mechanisms driving the differences among the experiments (lines 564-597);
- **reshaped the entire paragraph on the Rhodes Gyre** (Section 3.3.2) by: a) slightly enlarging the region (by 1 degree east and south) to capture not only LIW formation but also the beginning of its spreading (lines 604-610 and Figure 1a); b) moving Fig 11 from a representation of the mean bias to the absolute value of salinity, which allows us to discuss the physical processes rather than just comparing the model output to observations; and c) discussing the results by first defining the physical processes observed in the region and then explicitly linking the discrepancies in the results to the differences in the physics of each mixing scheme (lines 614-641).
- **expanded the discussion of how summer convection is represented** by the different mixing schemes and the role of convective adjustment, through the addition of a new figure (Fig. 12) showing the **temporal evolution** of vertical mixing and summer convection in terms of eddy diffusivity coefficient; a detailed discussion of the underlying mixing processes in this context has been added in lines 665-695.
- **Reshaped the entire conclusive section** (Section 4) to summarize our findings on the connection between the internal logic of the schemes and the resulting physical response of the basin.

Overall, the manuscript now more explicitly addresses the "why" behind the results, linking them to the physical behavior of the parametrizations. Considering these substantial changes, and in order to better reflect both the scope and the methodological framework of this study, while specifying that it is based on sensitivity experiments, as the reviewer suggested, we propose the following revised title: **Untangling the effects of vertical mixing schemes and convective adjustment in the Mediterranean Sea: insights from a sensitivity study.**

2. The manuscript discusses the combined use of turbulence closure schemes and convective adjustment. However, turbulence closure schemes such as TKE and GLS already represent convective mixing through buoyancy production when stratification becomes unstable**.** In this

context, introducing an additional convective adjustment does not represent a separate physical process, but rather an alternative term of the same mechanism. Therefore, the manuscript should clarify the physical rationale for applying a convective adjustment together with these turbulence closures. In particular, it should explain whether the closure schemes alone fail to adequately represent convective mixing, and why an additional adjustment is considered necessary.

We thank the reviewer for raising this important point, which was not sufficiently explained in the original manuscript and indeed deserves further clarification. We agree that turbulence closure schemes such as TKE or GLS include buoyancy production terms and therefore, in principle, are able to represent convective mixing when the stratification becomes unstable. However, in practice, these schemes may not always remove static stabilities rapidly enough, particularly at the temporal and vertical resolution typically used in regional ocean models. As a consequence, convective mixing may be underestimated, and this potential issue remains an open question.

For this reason, many ocean circulation models combine turbulence closure schemes and a convective adjustment parameterization (e.g. the physical component of the Mediterranean Forecasting System of the Copernicus Marine Service). The convective adjustment acts by stabilizing the water column, allowing for a rapid vertical redistribution of tracers and a prompt restoration of stabile stratification. In this sense, it does not represent an independent physical process, but rather a numerical parameterization designed to mimic the fast overturning associated with unresolved convective events.

In this regard, the literature still provides limited assessment of the combined effects of turbulence closure schemes and convective adjustment, and whether a mixing scheme alone (i.e., without convective adjustment) is sufficient to represent the vertical structure and variability of the upper ocean remains unclear. Our results suggest that this capability may depend on the specific closure scheme used, as we find that GLS is able to reproduce these features without convective adjustment, while TKE benefits from its inclusion. To clarify this aspect, we have revised the manuscript by adding a short explanation of the physical rationale for applying a convective adjustment together with physics-based vertical mixing schemes in the Introduction (lines 51-67 and 78-80) and in the conclusive section (lines 719-722).

Minor comments

1. In several places, the manuscript refers to the “**interaction**” between turbulence closures and convective adjustment. This terminology may be misleading and could be clarified.
   We thank the reviewer for this comment. We agree that the term *interaction* could be misleading in this context and have replaced it throughout the manuscript with a more appropriate wording to emphasize that we are studying here the *combined effect* of these two parameterizations.

2. The introduction identifies the combined use of turbulence closure schemes and convective adjustment as a research gap. However, the physical motivation for this question is not entirely clear, since turbulence closures such as TKE and GLS already represent convective mixing through buoyancy production.

As explained above (point 2 of the Major Comments), to clarify the physical motivation for combining turbulence closure schemes with convective adjustment, we have revised the manuscript by adding new paragraphs in the Introduction (lines 51-67 and 78-80) and in the Conclusions (lines 79-722).

3. The manuscript does not clearly explain why convection is parameterized through enhanced vertical diffusivity. Convection is a non-hydrostatic overturning process that differs from shear-driven turbulent mixing, and the rationale for representing it simply by increasing the vertical eddy diffusivity should be clarified.
We thank the reviewer for pointing out the need to clarify the rationale behind using the enhanced vertical diffusivity to represent convection. In the revised manuscript, we have added the following paragraph to Section 2.2.2 to clarify this point:

Since convection has the effect of effectively homogenizing the upper ocean boundary layer, it can be parameterized by applying a high eddy diffusivity which has a similar effect. Although this procedure does not accurately represent the physics of convection with its narrow and dense downward plumes compensated by a broader upwelling in between, leading to non-local fluxes, it effectively mimics the net impact of convective processes, including the rapid vertical redistribution of properties associated with convective overturning. The increased eddy diffusivity coefficient stabilizes the water column, allowing the model to maintain realistic stratification while enabling the development of large-scale circulation patterns.

4. The description of the model configuration and parameterization schemes in Lines 75–80 would be more appropriate in Sections 2.2–2.3.
We agree that the description of the model configuration is more appropriate in the "Data and methods" section rather than in the Introduction. Accordingly, we have moved the description of the numerical models (formerly lines 75-80) to Section 2.2 and the description of the parameterizations to Section 2.3.

5. Line 130, Please provide more details on the vertical grid distribution (e.g., layer thickness near the surface and within the mixed layer), as this may influence the representation of mixed-layer processes.
We added a new figure to the manuscript (Fig. 2) showing the vertical distribution of the layer thickness, with a zoom in the upper 50 m. The discretization is particularly dense in this region (2 m near the surface to 4.6 m at 50 m depth) to accurately capture processes within the mixed layer. A brief description of the figure and comment on this dense upper-ocean discretization have been added (lines 173-176).

6. The manuscript states that the model is nested into the "**daily analysis and forecast data of the Copernicus Marine Global product**". It is unclear whether the boundary conditions are taken from the analysis fields, the forecast fields, or a combination of both. Please clarify this point. In addition, it would be helpful to explain why this product is used instead of the available reanalysis product, which is commonly used for ocean model boundary forcing
We thank the reviewer for pointing this out and we apologize for the confusion. We had included the name of the Copernicus product we used, but in this study, we only use analysis

data. Therefore, we have removed the word *forecast* from this sentence and clarified that *Global Ocean Physics Analysis and Forecast* is the name of the Copernicus product as listed in the online catalogue.
The reason we use this product at the Atlantic boundary is that our reference model for this study is the physical component of the Mediterranean Forecasting System of the Copernicus Marine Service, and we therefore use the same input datasets. This has also been specified in the manuscript (lines 296-299).

7. The authors should justify the choice of the typical value of 10 $m^2 s^{-1}$ for the convective adjustment used in the simulations.
   Typical values recommended in the NEMO manual range from 1 to 100 $m^2 s^{-1}$. A value of 10 $m^2 s^{-1}$ was chosen based on previous sensitivity tests and is the value currently used in the physical component of the Mediterranean Forecasting System of the Copernicus Marine Service. We added a sentence in the manuscript (lines 275-276) to include this information.

8. Figures 4 and 5 could be combined into a single figure like figure 1b.
   Following this recommendation, Figures 4 and 5 have been combined into a single figure (similarly to Figure 1b), now Figure 5. Presenting the results together makes the differences between the temperature and salinity misfits more evident and allowed us to expand the related discussion in the manuscript. We thank the reviewer for this suggestion.

9. Figure 8 shows a basin-averaged MLD from the model together with an "**observed MLD**" derived from Argo observations. However, it is not clear how the comparison is performed. In particular, Argo profiles do not provide coverage over the Mediterranean basin, please clarify whether (1) the model MLD is averaged over the entire basin and compared with an average observation directly from the available Argo profiles, or (2) the model fields are first sampled at each Argo location, and then averaged all the profiles. A clear description of the methodology used to construct the basin-averaged time series would be helpful.
   We thank the reviewer and agree that this point was not clearly explained in the manuscript. The method used here corresponds to the second approach hypothesized by the reviewer: the model fields are first sampled at each Argo location, MLD is computed for each profile in both models and observations and then averaged over all the available profiles. We have improved the description of this methodology in the manuscript (lines 471-473).

10. The purpose of Figure 10 is not entirely clear. Since the water mass formation rate is computed only from model outputs without observational reference, it would be helpful to clarify what additional insight this diagnostic provides.
    We thank the reviewer for this comment and agree that this diagnostic does not provide substantial additional information beyond what is already presented in the manuscript. After careful consideration, we decided to remove the figure and the related text to improve the clarity and focus of the manuscript.

11. The analysis of "**summer diurnal convection (Section 3.4)**" mainly shows temperature profile evolution at a single location. It remains unclear how convection is diagnosed in this section. Additional diagnostics or clearer explanation would help support the interpretation.

We thank the reviewer for this comment. We agree that Section 3.4 benefits from additional diagnostics and explanation regarding how summer convection occurs and how it translates into differences in temperature. To address this, we have added a new figure (Fig. 12) showing Hovmöller diagrams of the hourly eddy diffusivity coefficient for the six simulations at the same location and over the same three-day period. We also added a new portion of the paragraph (lines 665-684) describing how summer convection is represented by the different schemes, both with and without EVD, and linking these patterns to the differences in temperature observed in the following figure (lines 691-695 and lines 710-713).