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.

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.
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.

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.
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.

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.
4. The description of the model configuration and parameterization schemes in Lines 75–80 would be more appropriate in Sections 2.2–2.3.
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.
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.
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.
8. Figures 4 and 5 could be combined into a single figure like figure 1b.
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.
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.
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.