

## ***Interactive comment on “Validation and intercomparison of two vertical-mixing schemes in the Mediterranean Sea” by V. Fernández et al.***

**V. Fernández et al.**

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### RESPONSE TO REVIEWER #2

1- The paper is rather an internal technical report than a scientific paper. It describes the mechanical comparison of two mixed layer schemes, their own model vs. other well-known model, without proper physical interpretation of the result. This kind of job, which should not take more than a few days, can be done within a group during the model development process, but a scientific paper should provide more general information on the parameterization of the mixed layer process.

REPLY: Our paper compares the two most wide-spread and successful turbulence models for 3 physically quite different sites in the Mediterranean with the help of the best available hydrographic data - namely those from the ARGO project. Even though

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the models are as different as two mixing models can be our main result is that their performance is surprisingly similar. If the reviewer thinks he/she knew this before - congratulations. We think, however, that most of the potential readers of this article didn't, for the simple reason that, to our knowledge, there are no published references describing a KPP-SOC intercomparison for the Mediterranean. Such information, however, should be highly welcome given the considerable current interest in the Mediterranean modeling community for 'upgrading' circulation models with state-of-the-art mixing models (most models in operational systems applied to this region of the world ocean still work with quite simplistic turbulence closures).

2- They simply give the conclusion that the performance of both models are good, because they correctly predict the onset of stratification in spring, the maintenance in summer, and the thermocline erosion in autumn (p. 1952). Is there any mixed layer model which cannot? What is the criterion for good prediction? Both models predict too shallow mixed layer depths under the surface heating and too weak convective deepening. Too shallow mixed layer depths under the surface heating in both models are certainly due to the improper near-surface mixing arising from the neglect of wave breaking and Langmuir circulation effects. Similarly, too shallow convective deepening of k-epsilon model under the surface cooling is certainly related to the neglect of non-local mixing.

REPLY: The reviewer is obviously right with his/her remark on the performance of mixing models. But to claim that our observed shallow mixed layers are "certainly" due to wave effects is pure conjecture, in particular with regard to the fact that these shallow mixed layers essentially occur in the presence of weak winds and the virtual absence of wave breaking. On the other hand, the reviewer may be right that the absence of non-local effects in the second-moment closure causes to shallow mixed layers under convection. However, as also pointed out by the other reviewer, the difference is not very large. We did a sensitivity study to check this point in the GL site and the result is that both runs give similar results. We modified the paper accordingly.

3 - There is no review at all on the current status of the mixed layer model development. There are few references other than those from their own group. What are current major problems regarding the mixed layer model? For example, how to handle the major mixing phenomena such as wave breaking, Langmuir circulation, and convection? Why did they choose these two particular models among many other models? What are the merits and demerits of these two particular models compared to other models? Which typical oceanic condition do those particular observation sites represent? Without this information the results in the present paper have no general validity.

REPLY: Our manuscript was not meant as a review article. As said above these two models have been picked because we and our colleagues consider them the most widely used in numerical ocean modeling, at least among those models with 'advanced' mixing schemes. We agree with the reviewer that the relative merits of the model deserve a greater emphasis. This point has also been raised by the other reviewer. The first parts of the paper were completely re-written to take the reviewer's remarks into account.

4 - Authors spent most of introduction in order to explain two models, and the explanation was repeated again in section 2.2 (Vertical mixing schemes). Nonetheless, I certainly believe that most readers cannot have an idea what basic physics is in the mixing parameterizations in both models. For example, they describe unnecessarily the general principle of second moment closure (p. 1947) or a very specific modification of a model parameter (the length scale) (p. 1950), but there is no explanation how various mixing processes are parameterized in the model.

REPLY: We agree, and tried to emphasize the large differences in the two modeling concepts in the new version of the manuscript. As said above, large parts of the paper have been re-written.

5 - Authors used the term 'the second closure model' to represent their own model. This gives readers a wrong idea that the model error shown in this paper is the typ-

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ical symptom of the second closure model. More specific name should be used to represent their own model. The terminology they used to refer to this model was very confusing (statistical closure model, second-order statistical model, second-moment closure model, etc.), and inaccurate. What kind of statistics was used in the 'statistical' closure model (p. 1947)?

REPLY: We agree with the reviewer, the terminology was indeed unlucky. We renamed the second-moment closure to emphasize its particular character. The same point has been raised by the second reviewer who also suggested using the acronym 'GISS' (apparently after the name of the institution where this model was developed).

6 - Authors suggested that discrepancy between model and observations comes from the atmospheric forcing parameterization (abstract, conclusion), but they did not provide any evidence that it is due to surface forcing rather than improper mixing parameterization. The agreement in the average heat content (Fig. 2) implies that not only horizontal advection is negligible but also the surface forcing is correct.

REPLY: The only atmospheric forcing parameterization inaccuracy that is pointed out in the paper is about the radiative component of the heat flux (shortwave radiation). In the Appendix there is a discussion showing that the model SST resulting from the use of direct shortwave radiation from NCEP is closer to satellite than using bulk formulae (using the same mixing parameterization). Anyway, the paragraph about the atmospheric forcing as a 'main source' of the error in the model has been taken out from the abstract.

7 - The only attempt by authors to explain the model error in terms of the parameterization of mixing is about the mixing below the mixed layer depth (Fig. 11), where they suggest the improvement of the model can be obtained by including the internal wave mixing parameterization in KPP. However, the comparison with observed temperature reveals that both models produce too shallow mixed layers, and the difference between models is much smaller than that from observation, which illustrates clearly that it is

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not the main problem of the model. In the KPP model, the parameterization of mixing below the mixed layer is the most irrelevant and inaccurate part of the model. The mixed layer process of the model is mainly determined by the K profile within the mixed layer and the critical Richardson number at the bottom of the mixed layer.

REPLY: We agree with the referee that below the boundary layer, the KPP model uses a simple representation of the internal wave mixing which has little effect on the mixed layer depth. However, it does have an effect on the structure of the temperature and salinity fields below the mixed layer which are modeled completely wrong if no internal wave mixing is implemented, and this is why we discuss the internal wave model. Similar arguments hold for the second-moment closure. We hope to have made this point clearer in the revised manuscript.

8 - They explained that the reason of the fluctuation of observed temperature is due to horizontal advection. What kind of advection can generate only small-scale fluctuation without generating the mean drift? Do they expect that the turbulent mixed layer shows a smooth variation of temperature?

REPLY: The origin of the more noise structure of the data in figs. 4, 5 and 6 is probably due to three dimensional ocean structures as internal waves or mesoscale eddies (big oscillations in Fig. 6). which are lateral processes not included in the 1D model. We changed this accordingly in the paper.

9 - What is the conclusion? The performance of their own model is poorer than the KPP model, even though it is more complicated? Of course, it does not mean that the second order closure model works poorer than the K-profile model.

REPLY: The main conclusion is that the two models considered here (two examples of different column models) have similar capabilities to represent some of the upper mixed layer process involved in the real ocean stations modeled here (wind deepening, night time convection). Our point is obviously not that the second-moment closure is nicer because it is more 'complicated' whatever that means. But we believe that in

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many respects it is physically more sound (e.g. not involving dimensional constants, being frame-invariant, being based on general principles valid for any high-Re turbulent flow) and, above all, it contains only very few adjustable parameters. To the contrary, the KPP model has dozens of adjustable parameters most of which are not well constrained. We hope to have made these points clearer, in particular in the re-written sections describing the model properties.

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Interactive comment on Ocean Sci. Discuss., 3, 1945, 2006.

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