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

Anonymous Referee #2

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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. The discrepancy between observation data and model results must be explained in terms of the mixing parameterization used in the model, in stead of saying simply without any justification that it may be due to inaccurate surface forcing or horizontal advection.

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

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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.

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 ciruclation, 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.

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.

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 typical 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)?

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.

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 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.

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?

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.

The main problem of this paper is that authors did not give an effort to understand the model error in terms of the mixing parameterization. They reveal in many places the lack of proper understanding of the dynamical process of the mixed layer, as mentioned

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