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Interactive
Comment

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 REVIEW #1

1. The authors compare two different mixing models: KPP and the GISS model against data in the Mediterranean Sea. In principle, this is a worthwhile exercise provided the reader is being made aware of the key physical difference the two models. This is not done properly in this paper. It is demonstrably untrue that, as the authors write “There is a plethora of SOC" (second-order closure). Here the authors may be confusing SOC models with models for the dissipation which may indeed assume a variety of forms. The freedom in the SOC models stems from the way one chooses the form of the pressure correlations.

REPLY: The reviewer is evidently right claiming that the modeling of the pressure terms

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essentially determines the SOC. But we still think that there are quite a few choices for these terms, including (a) models of the Rotta-Komogrov type (e.g. Mellor-Yamada), (b) models of the Gibson-Launder type (e.g. all models using the isotropization-of-production concept) and (c) the most recent generation using the complete linear tensor representation for the pressure terms (e.g. GISS and very similar models used in engineering). Non-linear tensor representations for the pressure terms have also been suggested, not to talk about the numerous different parameter sets available for each of these models. We have modified the paper to account for the special character of the second-moment closure used in our study, and to clarify the principle differences with KPP.

2. The original MY (Mellor-Yamada) models chose a set of values which led to the prediction of a very small (0.19) critical Richardson number above which turbulence mixing subsides. In an important paper in 1985, Martin showed that to reproduce the measured depth of the ML, Ri(cr) had to be of order unity, four times as large as what MY predicted. The SOC community did not step up to the plate and the impression naturally arose that there was something intrinsically wrong with the SOC approach and thus KPP was proposed as an alternative.. In 2001-2002 the GISS model was proposed (JPO, 2001, 31,1413; 2002, 32, 240, cited here as Cheng et al., 2002 which is not quite proper) and it showed that the improvements made since the 80' in understanding/ modeling the pressure correlations naturally produced a Ri(cr) of order unity, as required by martin. It is fair to say that if such work had been done in the early nineties there would probably been no need for a KPP-type model. To that, one must add a further decisive ingredient: the RNG renormalization group technique offers the possibility to compute some key dissipation time scales which would otherwise be undetermined. The sum of all these ingredients is that the GISS model is almost free of adjustable parameters. To say that KPP has a "large number of empirical adjustable parameters" without a juxtaposition with what happens with the GISS model is incomplete at best.

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REPLY: We agree. The paper has been modified to account for (a) the different ways parameters are found in both models and (b) the importance of having a large critical Richardson number in mixed layer modeling. We keep Cheng et al. (2002) as our reference to GISS, because this is the model we've used for our computations, and because this model is structurally identical to that of Canuto et al. (2001), with only marginal differences in some of the parameters (as the reviewer pointed out correctly, most of them are fixed from RNG anyway).

3. The KPP model must adjusts the coefficients below the ML, while the GISS model predicts them and such predictions were compared with the NATRE data. The GISS model includes DD (double diffusion) processes and the salt-heat diffusivities predictions were compared with observations. The authors say that DD are excluded in their module for KPP. It would have been useful to know why, considering that in many places in the Mediterranean Sea one would suspect that DD processes are relevant.

REPLY: The reviewer is certainly right that DD plays a role in the Mediterranean. We are aware that GISS has been extended in order to include different diffusivities for salinity and heat but there are two reasons why we did not use this option: Firstly, the DD version of GISS is not yet available in GOTM. In contrast to the original publication by Canuto et al. (2002), JPO, we use their SOC together with two dynamic equations for the TKE and the dissipation rate which turned out to involve a number of tricky and not yet fully resolved issues regarding stability and realizability of the model (similar to those discussed by Burchard and Deleersnijder, Ocean Modeling, 1, 33-50, 2001). Since the focus of this paper is the application of a model (rather than model development), we decided not to discuss these issues in our paper. Secondly, the focus of our paper was on the mixed layer where DD plays a much less important (essentially negligible, we think) role compared to the thermocline region. Having made this decision, it would have been 'unfair' to compare GISS without DD with KPP with DD even though this option would have been available.

4. An important issue is non-locality especially when convective events set in, as for

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example in the GL region. KPP is being used in its non-local version which means that in those regions, the KPP depth of the ML as a deep as one can get. On the other hand, the GISS model in its present formulation is local and thus under-predicts the ML depth in convective regions. And yet, Fig. 6 shows that KPP ML depth is only slightly deeper than GISS, which is not what one would have expected. It would have been quite useful to have more discussion on this point since previous studies (Ocean Modelling, 2004, 7, 75) show the opposite behavior. This brings about another point. Which variable must be treated non-locally? Convective flux, eddy kinetic energy, eddy potential energy? KPP adopts a non-local model for the convective flux, but it is not clear if that is sufficient. Since a clear understanding of which variable must be treated non-locally is not yet available, the GISS model has not been made non-local. In summary, I think that the high quality data the authors have at their disposal can and should be used to test KPP and GISS, two models that differ in a quantitative way. Key processes like DD, non-locality, etc can then be analyzed. This is a first attempt and the hope is that the next time around the authors will concentrate more on specific items and their physical meaning and implication.

REPLY: We have tried to shed some light onto the question whether non-locality is important at our sites, or not. To check the effect of non-locality we have done sensitivity studies comparing runs with and without non-local mixing in the KPP model for the GL station. From the results we can see that there is almost no difference between both runs in the GL site.

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