Dear editor,

We sincerely thank all the reviewers for their reviews of our paper. The aim of this letter is to detail how we addressed their comments in our manuscript.

Review of the manuscript
Modelling turbulent vertical mixing sensitivity using a 1D version of NEMO
by G. Reffray, R. Bourdallé-Badie and C. Calone

General Comments:

This paper investigates the responses of the four most popular two-equation turbulence closure models (k-kl, k-ε, k-ω, GLS) and the TKE mixing scheme. The schemes were used to simulate the Kato-Phillips idealized experiment case (1969) and the PAPA real case (15 June 2010 - 15 June 2011) in the Pacific ocean. Both these configurations are well adapted for a 1D approach. They were carried out with the one dimensional version (NEMO1D) of the 3D NEMO model. Thanks to its low computational cost, NEMO1D is a suited tool for testing the 5 turbulence schemes according to 4 global configurations in terms of vertical grid and time step of NEMO (L75, Δt=360s ; L75, Δt=1200s ; L31, Δt=3600s ; L75, Δt=3600s). As consequence, the results explore a wide time and space resolution and should be of great interest for modellers. Having said this, I have several major concerns that the authors need to address before the article can be accepted for publication.

Major Comments:

The text could be more concise and the conclusions clearer. Indeed, the authors test 5 parameterizations of vertical mixing without any modification or improvement. As consequence, the presentation of the GLS and TKE parameterizations should focus on the basics equations (k and ψ) and on main differences.

The 5 turbulent models considered in this study are those present in NEMO and are implemented as described in the associated papers. We hope that the reorganization of the paper allows highlighting the presentation of these turbulent models and the main differences between them.

In particular the constraints used for determining the 6 model constants should be clearly exposed in order to understand the "philosophy" of the GLS approach. The GLS closure assigns m=1, n=-0.67 and p=2 (Table 1). The question is how these parameters are defined ? Umlauf and Burchard (2003) speak of the polymorphism nature (exponents m and n considered as variables) of the GLS model for calibration. To understand the basic idea, please develop these aspects, not as completely as in Umlauf and Burchard (2003), and reject the expanded expressions of the stability functions and the details of the surface boundary conditions in annexe.

The description of the model calibration for GLS has been developed.
However, the sections devoted to the stability functions and surface boundary conditions have not been rejected to Annexes for two mains reasons:

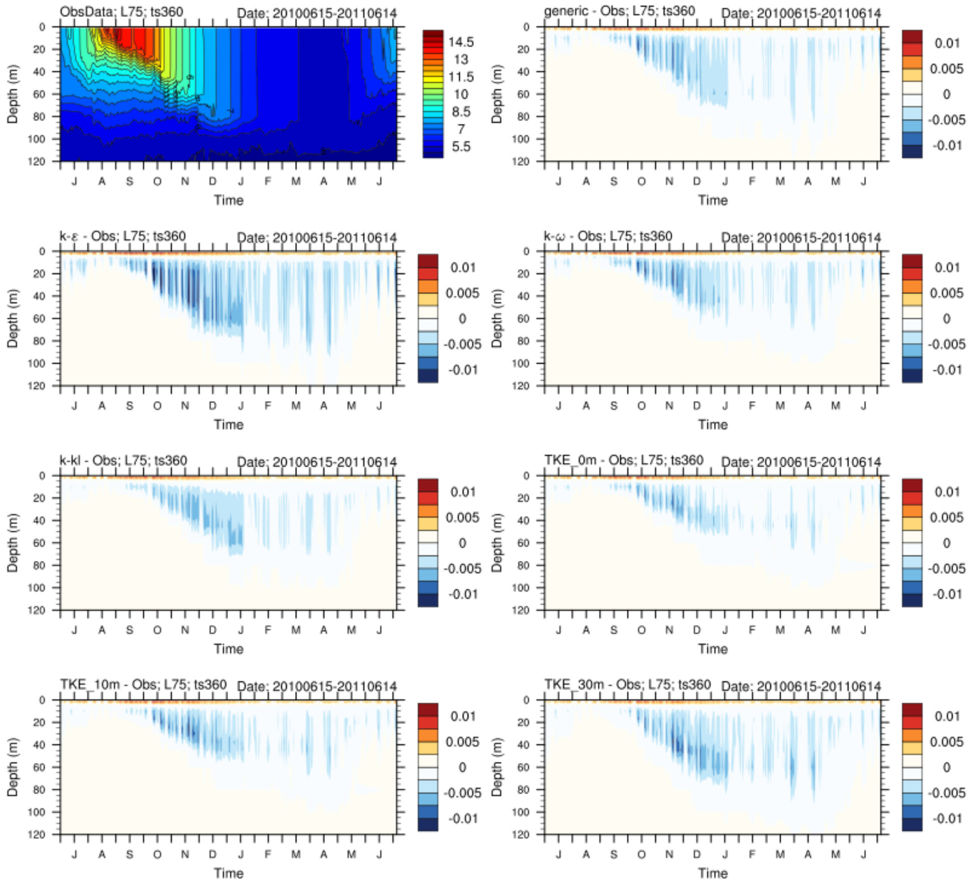
- *The logarithmic boundary layer and the shear-free turbulence are two boundary conditions necessary to calibrate the GLS model. A widest explanation of the model calibration can not be done without sufficient details about these surface boundary constraints.*

- *In light of the results presented in this study, we think these components of the turbulent model are of first importance in the results. For example, in TKE case, the way in which the energy is introduced at the surface changes drastically the results.*

The section devoted to the surface boundary condition has been streamlined.

As the paper focuses on turbulence models, I suggest to present the models first, then the NEMO1D framework. A section describing the numerical implementation (description of the explicit and implicit terms) of the turbulence parameterizations in NEMO would be interesting for readers of GMD and for understanding the possible causes of sensitivity of the results to the time-step obtained in the idealized and real cases. Finally the results presented on Figures 7-10 are not sufficient to identify the fundamental differences between the turbulence schemes. These differences could be highlighted by showing the vertical profiles of heat (w'T'), salt (w'S') and momemtum (w'u';w'v') fluxes computed by the different turbulent schemes. I do not ask but further validation would be to compare these models with LES simulationns.

The plots of the heat and momentum fluxes have been done (see figure bellow for w'T, the plots of the others quantities could be send to you if you wish). The conclusions are very close to what we can deduce from temperature or salinity profiles: the fluxes are higher with k-ε and act more deeply. We think these plots would complicate the discussion and it is better not including them in the paper. The main objective of this present paper is to evaluate the performance of the GLS closures and the standard TKE model in their actual implementation in NEMO. Anyway, we agree that this investigation could be a very interesting further study and NEMO has now enough numerical material to carry out this type of study. We think this paper could be a good starting point to lead such studies.



Observed temperature (top left) and the evolution of the temperature turbulent fluxes (w'T') for each closure in NEMO1D.

To streamline and clarify the paper, I suggest to reorganize the paper as follows:

2) vertical turbulence models
  2.1) TKE model
  2.2) GLS models
    2.2.1) Determination of model constants
3) The NEMO1D framework
  3.1) Equations system
  3.2) Space and time discretization
  3.3) Numerical implementation of the turbulence parameterizations
4) Experimental Design
  4.1) Idealized case
  4.2) PAPA station
    4.2.1) Second-order moment distribution
  …

Following your request and taking into account the recommendations of the other reviewer, the paper has been reorganized. A section devoted to the numerical implementation of the turbulent models has also been added as asked. The new plan is:

2) Model description
2.1) Equations of motions
2.2) Vertical turbulence model
2.2.1) Turbulent Kinetic Energy
2.2.2) Mixing length
2.2.3) Stability functions
2.2.4) Surface boundary condition
2.2.4.1) Logarithmic boundary layer
2.2.4.2) Mixing induced by breaking waves
2.2.4.3) Surface roughness
2.2.5) Model constants and calibration
2.3) The NEMO-1D framework
2.3.1) Equations system
2.3.2) Space and time discretization
2.3.3) Numerical implementation
3) Experimental Design
3.1) Idealized test case: the Kato-Phillips experiment
3.1.1) Description
3.1.2) Reference simulation
3.1.3) Results
3.2) PAPA station
3.2.1) Description
3.2.2) Numerical configuration
3.2.2.1) Input files
3.2.2.2) Model setting
3.2.3) Results with the pair [L75, 360 s]
3.2.4) Spatiotemporal sensitivity
4) Conclusion and perspectives

Minor comments:

1) All the Figures are really too small and it is very difficult to read the colored lines and captions. Please enlarge them.

We increased the size of the figures.

2) Equation 8 is not the original mixing length described in Gaspar et al. (1990). Include the computation l=sqrt(lup*ldown) in the paper.

The TKE model in NEMO is a simplified version of the Gaspar et al. (1990). In this model, the integrals are simply resolved by considering that the fluid is linearly stratified. This simplification leads to only one mixing length formulation instead of 4, since lup=ldown and then ld=le=lup(=ldown). We have decided to be more concise in the description of the TKE model and we just described what is available in NEMO. A full explanation of the model of Gaspar may introduce complexity for the reader. Moreover, introducing the concept of one mixing length for dissipation and one other for mixing would also change the description of Eq. 4 and Eq. 8.

3) Put the stability functions in annexe

For this point, the reasons to keep them in the main text have been explained in the "major comments" part.

4) Surface boundary conditions should be streamlined

The description of the surface boundary conditions has been divided in 3 parts: logarithmic boundary layer, surface boundary conditions in the case of breaking wave mixing and surface roughness.

5) Change "Relation (21)" into "relation (21)"

The modification is carried out directly in the text.

6) Unit of equation (24b)? l in m and u* in m/s

There was a mistake in this equation, we have corrected it.

7) Page 5264: "NEMO 1D has no restriction on the time step" Although there is no CFL condition, there is still a restriction on the time step associated with the vertical diffusion which is $K\Delta t / \Delta z^2 < 1$. Check if the sensitivity of the results to the time-step for a given vertical resolution is not associated with this constraint.

The sensitivity of the results to the time-step is not due to the CFL of the diffusive terms because these diffusive terms are solved implicitly. More details are carried out in the point 14 of this answer.

8) Page 5265: Change "Eq. (27)" into "Eq. (26)"
9) Page 5266: Change "focussed" into "focused"
10) Page 5266: Remove "in passing"
11) Page 5269: Change "15 June 2011" into "15 June 2010 "
12) Page 5270 : Change «108000» Pa into «100800 Pa»

The modifications are carried out directly in the text.

13) Page 5270: Figure 7 and 8 are presented at the beginning of Section 5.3 and suddenly the discussion skips on Figure 9 (In order to focus on two major steps ...). Please be linear in the discussion. As Figure 9 is used to evaluate the capacity of the turbulence schemes to capture the MLD deepening, replace the profiles of temperature by the density.

The temperature profiles are now replaced by the density profiles. We guess that the trouble was caused by the paragraph of 4 lines (5270 L26-27 and 5271 L1-2). We have deleted these lines to clarify the discussion.

14) Page 5273: For a given vertical grid, the k-kl and k-ω schemes display a significant sensitivity to the time-step in the idealized and real cases. It is quite disturbing because it shows a lack of robustness of the schemes. Isn't it induced by the numerical method used? That is why a

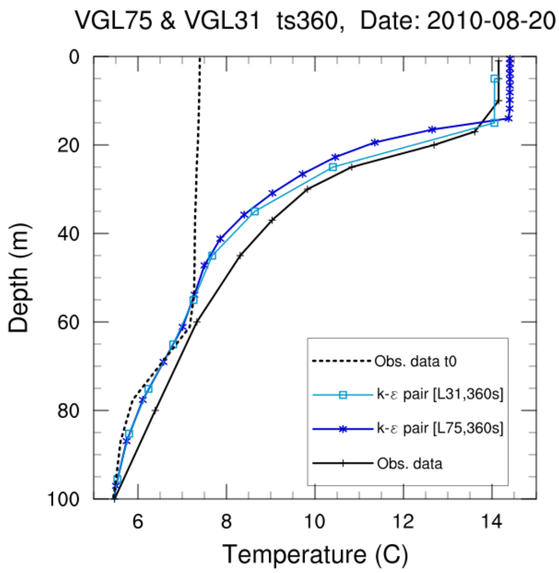
section on the Numerical implementation of the turbulence parameterizations would be fruitful.

The other reviewer has also done the same remark. We agree that this lack of robustness of the schemes is very worrying. Unfortunately, we do not have an accurate understanding of the numerical phenomenon. This point is also under discussion on the GOTM forum but without clear explanation: https://groups.google.com/forum/#!topic/gotm-users/dOpd4waVvtc. Nevertheless, supplementary tests (not shown here) have shown us that we can improve the results with k-kl and k-ω by modifying the background values but the dependence on the time-step is not totally removed.

15) Figure 11: The best result obtained with L31 versus L75 for Δt=360s is attributed to numerical dilution associated with L31. I am not convinced. With no advection the temperature below the thermocline should not change compared to the initial profile. As consequence, I assume that the observed temperature below the thermocline in August is close to the temperature in June, that is confirmed in Figure 7 (top left). Hence, I do not understand why the simulated profiles L31 and L75 do not fit the observed profile below 20m depth (Figure 11).

Please find in this section a reprocessed plot where the one hundred first meters have been plotted (instead of 60m) and we have added the initial temperature profile in dashed line (June 15). For August 20, the observations (black line) show MLD values close to 20 meters. The model tends to overstratify and with L75 grid it exhibits a too weak MLD of only 15 meters. The minimal value for modelled MLD is 2 vertical cells. Then, with L31 grid, the MLD can not be shallower than 20 meters. This is the reason why the modelled MLD with L31 is closest to the observations than the profiles obtained with L75, but it is just due to numerical reasons. The RMSE computed for these profiles are then smaller with L31 than those computed with L75.

On the initial temperature profile (black dash line), we observed that the MLD is 60m. The change, between 60 and 100m, of the observed temperature between June 15 and August 20, should be due to advective processes. In NEMO1D, there are no advective processes, which is why the modeled temperature does not fit the observations on August 20 bellow 60m. The modeled profiles are just slightly smoothed by the background mixing. The differences between 20 and 60m are induced by numerical, physical, forcing errors of the simulation during this period of stratification (June 15 to August 20).



16) Conclusions: It is in the perspectives but why did you not test K-profile models, for instance the formulation of Large et al. (1994) in this study ? After reading, my conclusion is that the most robust parameterizations are k-ε and TKE_10m. Nevertheless the authors make no recommendation on the use of either parameterization in NEMO, yet expected by the NEMO user. Numerous prospects are proposed. Only one or two main aspects should be proposed.

The aim of this paper is double. The first objective is to present one and two-equation models available in NEMO and to expose their performances. To test KPP model (or other models like Paconowski and Philander also available in NEMO) could be done in a further study.
The second objective is to introduce and to present the new reference configuration of NEMO (1D component of NEMO which was run at the PAPA station). It is true that many prospects have been proposed at the end of the paper. But, we think that it is important to provide the NEMO users with a significant list of potential applications feasible with this new configuration.
Recommendations on the choice of the turbulent model have been added in the conclusion as suggested by both reviewers.

Dear editor,

We sincerely thank all the reviewers for their reviews of our paper. The aim of this letter is to detail how we addressed their comments in our manuscript.

Reviewer comments for
"Modelling turbulent vertical mixing using a 1-D version of NEMO"
G. Reffray, R. Bourdalle-Badie, and C. Calone

General comments

A comparison of two types of turbulent closure is presented using a 1D configuration of the NEMO ocean model: the one-equation TKE model of Gaspar et al. (1990) and the two equation models encompassed by the GLS framework of Umlauf and Burchard (2003) are considered.

A brief explanation of the model primitive equations in a 1D context and then a summary of the main differences between the two types of turbulent closure are given. The performance and numerical behaviour of these closures are assessed in an idealized Kato-Phillips (1969) test case and a realistic test case based on observations from OS PAPA. The numerical behaviour is explored through the use of two vertical grid discretizations (31 and 75 levels), three time step sizes (360s, 1200s and 3600s) and an 'ideal' spatiotemporal discretization (1000 levels and a 36s time step).

The results present a very useful insight into the comparative performance and numerical behaviour of two similar types of statistical turbulent closure, for a range of spatial and temporal discretizations. Furthermore, the results are easily and directly reproducible by virtue of the configuration being immediately available from the NEMO repository. The derivation of NEMO1D from the full 3D domain also allows the performance of the discussed turbulent closures to be applicable to users of the full 3D NEMO model.

I am generally happy with the content of the manuscript but have a few minor revisions that I think should be addressed before publication.

Specific comments

1. I think that the description of the turbulent closures in section 2.2 needs to more clearly contrast the differences between the two types of closure:

> 2.2- I think it would be better to introduce the two types of closure presented in the paper at the end of this section, so that in each subsection the treatment by both closures of the solution for k, l, Cμ and C'μ can be compared

To follow the recommendations of both reviewers, we have modified the plan. We hope that the description of each turbulent model is now clearer.

> 2.2.2- The principal difference between the two types of closure presented is that the length scale of the former is an algebraic formulation, while the latter uses a prognostic differential equation to calculate a length scale-related quantity. Often these types of closure are referred

to as one and two-equation models (see for example Burchard et al., 2008). I think the clarity of this section would benefit from a concise summary of this key difference at some point.

The section has been clarified and we have followed your recommendations. The suggested reference has also been included in the text.

2.2.3- I think this section should more specifically be about the determination of Cμ and C'μ, contrasted with their constant values in the TKE model (for the TKE scheme I don't think they are defined elsewhere in the manuscript other than in table 1)

The section devoted to the stability functions has been rewritten with a subsection for the TKE model and one other for GLS models.

2.2.4- Cμ0 is missing a defined value here, and I think the reasons for the difference in its value between the models should be briefly described. I'm not sure Pr belongs in this section either- it is not a constant and might be better placed in 2.2.3 since it appears in the TKE model definition of Cμ'

The definition of Cμ0 has been highlighted as being the limit value of the stability function inside the logarithmic boundary layer. Moreover, the value of Cμ0 depends only on the stability function choice and so the value must be the same for all GLS closures considered in this study. By consequence, corrections have been carried out in Table 1. Moreover, the stability functions have also been corrected according with the model description (division per the factor $C\mu 0^3$).
The note on the definition of Pr is relevant and the definition has been naturally moved to the stability functions section devoted to the TKE model.

2. I think some caution is needed when describing "realistic" values of Hp (P5262, line 17) in section 2.2.5 for the TKE model without explaining why the 0.5m to 30m profile is realistic. I think this must either be physically justified or else not described as "realistic"

We agree with you and the sentence has been modified:
"In most cases, H_p varies as a function of the latitude (0.5 m at the equator to a maximum of 30 m at the middle latitudes)".

3. At the end of section 2.3 the purpose of the paper is described as providing "feedback on different turbulent closures available in NEMO". However the KPP and simpler models are not considered; rather the "Algebraic Stress Models" of NEMO are put forward. This is acknowledged in section 6, but I feel that a brief explanation of this choice of turbulent closures from those available in NEMO would be useful.

This point has been highlighted at the beginning of the description of the turbulent model. We have explained that only one or two-equation models are studied in this paper.

4. An interesting result from section 4.3 is that there is a sensitivity to the time step to varying degrees for all closures (figures 3a-c), but this is not really acknowledged. It would be interesting to hear some thoughts on why this might be the case.

The same remark has also been done by the other reviewer.

We agree that this lack of robustness of the schemes is very worrying. Unfortunately, we do not have an accurate understanding of the numerical phenomenon. This point is also under discussion on the GOTM forum but without clear explanation: https://groups.google.com/forum/#!topic/gotm-users/dOpd4waVvtc. *Supplementary tests (not shown here) have shown us that we can improve the results with k-kl and k-ω by modifying the background values but the dependence of the time-step is not totally removed.*

5. It would be useful for the paper to deliver a recommendation on which of the presented turbulence closures should be used, particularly as the OS PAPA test case is presented as a readily available configuration: what is the turbulence closure used by PAPA1D and why? This could be presented as a brief discussion item in section 6.

Recommendations on the choice of the turbulent model have been added in the conclusion as suggested by both reviewers.

6. Figure 10 is too small to clearly interpret
We have increased the size of this figure.

Technical corrections/suggestions

All modifications have been taken into account and are carried out directly in the text.