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Ocean Science Discussions

Interactive comment on "The analysis of large-scale turbulence characteristics in the Indonesian seas derived from a regional model based on the Princeton Ocean Model" *by* K. O'Driscoll and V. Kamenkovich

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Reviewer #1 comment:

(1) I have reviewed this manuscript previously. The editor and I thought it was unsuitable for publication in that journal and could not think of any it was suitable for.

Answer:

(1) We are very grateful to our reviewer for critical comments concerning our paper. We chose the journal Ocean Science because it gives us an opportunity to discuss

reviewer's comments. The editor of the journal, to which this paper was previously submitted, did not, unfortunately, provide us such an opportunity. Thus, we are happy to outline our point of view. All our clarifications, induced by reviewer's comments, will be inserted in adequate form in the reworked paper.

Reviewer #1 comment:

(2) A major objection I have is they clarify what turbulence they are characterizing or simulating. I would have liked to see a good discussion of what the turbulence they are getting out of the model; otherwise it is just mystery turbulence. So the question is what turbulence does the model represent either through simulation or parameterization Horizontally, at this scale, they will simulate large eddies and shear flow around topographic features, like islands. They won't be able to simulate small eddies or the filaments coming off eddies. Vertically, they will get a boundary layer. They could get a surface mixed layer, if they put it in. But they won't get internal waves or lee waves. Will they get mixing induced by flow over sills? Probably. Are there other sources of mixing. Yes, lots. And many of them are local and some are episodic or periodic. So although they say they will not do lee waves and only turbulence scales of 100 km or more, I think they need a clear discussion of which turbulent processes they intend to include in their simulation and which are excluded.

Answer:

(2) First of all, this paper is not intended to analyze the internal structure of turbulence. In this paper, we analyze the turbulence characteristics provided by Mellor-Yamada's scheme of parameterization, which is incorporated into the POM (and this is stressed in the heading).

There are many general definitions of turbulence. All of them are essentially reduced to the statement, that velocity, pressure, temperature, salinity, and other characteristics are chaotically pulsated both spatially and temporally. So, for our convenience, we reformulate your question as: what types of fluid motions are responsible for these

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pulsations? Horizontal mixing is parameterized by Smagorinsky's formula, which takes into account eddies of sub-grid scales. The specifics of these eddies are not elaborated. This formula is widely used in atmospheric and ocean modeling and is considered as very effective. The analysis of vertical turbulent mixing is based essentially on the consideration of the turbulence kinetic energy equation, (1/2)q2. Nobody questions the usefulness of this equation nowadays. From the standpoint of this equation, we consider turbulence that is essentially generated by the shear of large-scale currents. So, our focus was on turbulence associated with basin scale motions in the Indonesian seas. This is the main reason why we call the analyzed turbulence large-scale turbulence. The effect of shear is balanced by the work of buoyancy forces, dissipation of the energy, by vertical and horizontal diffusion, and advection. The contribution of lee waves into the shaping of q2 is not considered, because they occur at a much smaller scales, order 100m. The effect of internal waves is recognized separately but parameterized very crudely by the introduction of background mixing. We did not analyze the surface boundary layer since this was not our focus.

Finally, the last comment. Currently, there are no GCMs that are able to simulate simultaneously large scale features of the circulation and such motions as small eddies, the filaments coming off eddies, internal waves or lee waves. We understand that the study of such motions is extremely important from the standpoint of the internal structure of turbulence. But all known GCMs are using some kind of parameterization. This does not mean that characteristics of turbulence provided by GCMs are of no interest. For example, the simple Munk model based on the 1D temperature equation is used by many researchers to obtain an estimate of basin scale turbulence mixing. Let's look at our approach as an advanced Munk's approach.

Reviewer #1 comment:

(3-1) At their scales, they are averaging out turbulence. By it's nature turbulence fluctuates and occurs at sub-grid scales. So their estimates will be averages that don't actually represent what is occurring.

(3-2) And before doing the Indonesian seas, it would be good if they could point to a place and say our model's estimate of turbulence matches what was observed. Then move to another spot and you will know your estimate is likely to be correct, if the same processes are occurring.

Answer:

(3-1) We disagree with the statement 'So their estimates will be averages that don't actually represent what is occuring'. Averaging is the only possible approach to the analysis of turbulent motions. The point is how to do the averaging. In the derivations of basic relations, Mellor and Yamada used ensemble averaging, which is absolutely valid.

(3-2) This is reasonable advice. But such a comparison has been done already by Mellor and Yamada in the paper 'Development of a turbulence closure model for geophysical fluid problems' published in 'Review of Geophysics and Space Physics', 1982, vol. 20, No 4, 851-875. They applied their model to a wide variety of engineering and geophysical flows and by comparison of simulated data and experimental/observed data unambigously demonstated its validity. Thus, we are absolutely sure that the coefficients of vertical mixing, kinetic energy and master scale of turbulence provide some useful information about turbulence and thereby should be analyzed. This was the main task of our paper.

Reviewer #1 comment:

(4) Another objection is that the model uses Mellor Yamada 2.5 vertical mixing to estimate the vertical turblence. This only acts in the boundary layer. Vertical mixing occurs at other depths in the real ocean and this is neglected.

Answer:

(4) First, we studied basically the bottom boundary layer. But it has been demonstrated in many papers that the POM can be used in the analysis of the circulation in the main

depth of the ocean as well.

Reviewer #1 comment:

(5) I don't think that the Indonesian Seas is a good place to study mixing at a scale of 100km or so. This is an area of extremely complex topography and currents, with strong currents flowing through narrow straits. It is likely that topographic features generate much of the mixing and influence the scales at which it occurs.

Answer:

(5) Yes, we agree, it is indeed 'an area of extremely complex topography and currents, with strong currents flowing through narrow straits'. We also agree that 'topographic features generate much of the mixing and influence the scales at which it occurs'. That is one reason why we have focused on turbulence and circulation at these scales. Again, we stress the importance of deep overflows, and particularly over the Lifamatola Sill, on Indonesian seas circulation and the global thermohaline circulation. Our goal was to advance our understanding of the turbulent processes contributing to these overflows. This is why we think it is very important to study mixing at the scale of topographic features in the Indonesian seas.

Minor comments:

p. 4 - assuming KM and KH are reasonable because currents and T & S are reasonable is dangerous.

Answer:

We agree that it is a rather shaky argument if a model has a set of tuning parameters. But all parameters in the MY turbulence closure scheme are fixed (they were determined by the comparison of some simulated and experimental data, see answer 3-2), so our argument can be considered as a minor argument, but still an argument, giving an additional validity to our calculations. We will rework this statement in the text of the paper.

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p. 5 - the part about eq 8 is confusing. This equation is what Mellor-Yamada is founded on so if it is unfounded, so is your mixing scheme, ... and your turbulence, ... and this paper. I would remove this seciont until you have better arguments for or against it.

Answer:

Equation (8) is part of the MY turbulence closure scheme and Mellor and Yamada themselves did not consider this equation as well founded. But the replacement of equation (8) (it is not clear by what) will require substantial change of the whole model. Our task was to check the adequacy of this equation in all layers. We came to the conclusion that the calculation of l is not adequate in some layers. We will rework this part in the paper.

p. 6 - How can the model recommend anything?

Answer:

These are the POM's authors (Blumberg and Mellor) recommendations for sigma-coordinates models to avoid contamination of the calculation of the horizontal pressure gradient due to large slopes in bottom topography. We followed these recommendations, i.e., slope not to exceed 20%.

p. 6 - I don't think 29 levels will resolve the vertical well enough for turbulence.

Answer:

Do you mean to resolve the vertical structure of pulsation? We aimed at the vertical resolution of large-scale characteristics. We put 7-sigma levels close to the bottom to allow for a reasonable description of a bottom boundary layer.

p. 7 - What are the POM recommended boundary conditions?

Answer:

Model boundary conditions are given in O'Driscoll & Kamenkovich (2009), Appendix b.

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For your convenience, we reproduce here a full list of POM BCs:

Horizontal boundary conditions at rigid walls for the depth-averaged motion:

1. Impermeability condition (normal component of velocity is equal to zero).

2. A special expression for the viscous flux of momentum in the direction normal to the wall. This is sometimes called the 'half-slip' condition. This condition has been chosen because of the simplicity of its formulation in the C-grid.

Horizontal boundary conditions at rigid walls for the 3D motion

1. Impermeability condition (normal component of velocity is equal to zero.

2. A special expression for the viscous flux of momentum in the direction normal to the wall ('half-slip' condition).

3. Diffusive fluxes of temperature and salinity in the direction normal to the wall are set to zero.

4. Diffusive fluxes of turbulence energy and length scale in the direction normal to the wall are set to zero.

Vertical boundary conditions for the 3D motion

Surface:

1. The vertical turbulent flux of horizontal momentum is set equal to the prescribed wind stress.

2. The kinematic boundary condition for the vertical velocity.

3. The prescribed temperature and salinity are set equal to the climatological value at the surface

4. The turbulence energy is set proportional to the wind stress.

5. A special relation is assumed between the turbulence length scale and energy at

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the second sigma-level. The turbulence length scale is set equal to zero at the first sigma-level.

Bottom:

1. The bottom stress is set proportional to the square of the bottom velocity.
2. The kinematic boundary condition for the vertical velocity.
3. The vertical fluxes of heat and salt are set equal to zero.
4. The turbulence energy is set proportional to the bottom stress.
5. The turbulence length scale is set equal to zero.

p. 7 - Nudging of T & S to maintain climatology. Doesn't this artifically add a fake unknown mixing into your simulations and estimates.

Answer:

Nudging of T and S to corresponding climatological values was introduced to take into proper consideration mixing processes in the upper layers. It is known that the Mellor-Yamada 2.5 turbulence scheme with monthly climatological winds results in underestimation of mixing in the upper boundary layer, see, e.g., Martin, P.J. 1985. Simulation of the mixed layer at OWS November and Papa with several models. J. Geophys. Res., 90, 903-916, and Ezer (2000). It is also known that to reach an established temperature and salinity distribution in the deep ocean, models must be integrated for 1000 years (Bryan, K. 1984. Accelerating the convergence to equilibrium of ocean-climate models. J. Phys. Oceanogr., 14, 666-673). But we know that both temperature and salinity tend to climatological values, so weak nudging is introduced to accelerate the process of establishment (which is common practice in many general circulation models, see, e.g., Sarmiento, J.L. and K. Bryan. 1982. An ocean transport model for the North Atlantic. J. Geophys. Res. , l87, 394-408; Oey, L-Y, and P. Chen. 1992. A model simulation of circulation in the northeast Atlantic Shelves and Seas. J. Geophys. Res., 97, 20,087-20,115). See O'Driscoll & Kamenkovich (2009, Appendix) for further discussion.

p. 8 - Using a background diffusivity of 10x-5 m2/s. How can you say KH and KM are scaled with all these additions?

Answer:

This is the suggestion of the Mellor-Yamada parameterization. Looking at Figs. 12 and 13 (Km and Kh), you see that this is essentially the value outside of the boundary layer. It is at least 2 orders of magnitude smaller than values calculated in the boundary layer, an average value of 15 – 20 x 10 ˆ-4mˆ2/s, so it has little or no effect on the boundary layer dynamics.

p. 8&9 - These validations of Mellor Yamada are for boundary layer mixing. So you only have boundary layer mixing too.

Answer:

Yes. Please see answer to (4) above.

p. 13 - How good is the agreement? Some numbers would be good here. Numbers are given for Van Aken, but not for the model or for the difference between the model and observations.

Answer:

We reproduce here the paragraph from our paper, which we hope answers your questions.

'Model simulated deep water transports across the sill below 1200 m are in good agreement with those given by van Aken et al. (1988) who calculated a deep southward transport of ∼1.5 Sv from a current mooring deployed between January and March 1985, while model deep southward transport is approximately 1.4 Sv in February (Kamenkovich et al. (2009). In the same paper, van Aken et al. (1988) estimated an annual deep transport of the order of 1 Sv in good agreement with the model (see Kamenkovich et al. (2009). But later on van Aken et al. (2009) calculated a long-term mean deep transport of 2.4-2.5 Sv for 34 months between 2004 and 2006 from a mooring containing current meters and ADCPs at the sill as part of the INSTANT program (see Sprintall et al. (2004). van Aken et al. (2009) state that the reduced transport estimated in van Aken et al. (1988) is due partly to the shorter observation period and partly to the smaller thickness of the overflow layer estimated from linear extrapolation. The simulated structure of velocity profile is in very good agreement with that observed by van Aken et al. (1988). All models have some tuning parameters to make compatible some separate values. Therefore we argue that the comparison of structures of velocity profiles is more important than that of some separate values. Fig. 2 is a vertical section of v-velocities through the Lifamatola Sill, and shows a southward flow extending from below 1200 m to the bottom through the sill. At the sill, southward velocities increase with depth to a maximum value of over 0.15 m/s at ∼1700 m. Model velocities across the sill are in very good agreement with those of Broecker et al. (1986) who found mean velocities of ∼20 cm/s just above the sill over a 28 day period in August-September 1976. However, they are less than those measured by van Aken et al. (1988) who found mean velocities of 61 cm/s at 60 m above the bottom in January-March 1985, and van Aken et al. (2009) who found maximum velocities of 65 cm/s at ∼70 m above the bottom. Simulated v-velocities are also southward below sill depth, north and south of the sill, to a depth of ∼2400 m. Velocity is northward below ∼2400 m both north and south of the sill. A weak northward current is seen to extend from 1200 m into the thermocline across the entire section. Note that all model results presented are from the 15th of August after 15 years of model run.'

p. 16-18 - This discussion and these plots confirm my understanding that this is benthic boundary layer mixing only.

Answer:

Yes. Please see answer to (4) above.

Conclusions - Much of the conclusions appears to be from a previous version or another paper. There is material here is not covered in the paper.

Answer:

We disagree. We find that everything in section ‘4 Summary and concluding remarks’ has been discussed earlier in the paper.

Plots - I am not convinced that all these profiles are needed. Possibly these figures could be combined as panels on one or two plots. And some might be removed.

Answer:

We think all plots are necessary. We would be prepared to combine some plots (if necessary).

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