Response to the Referees

Revisiting the parameterization of dense water plume dynamics in geopotential coordinates in NEMO v4.2.2

Response to Reviewer 1

This is an interesting and pragmatic approach to improving the parameterisation of dense water flows in NEMO. The evidence of a significant improvement is not definitive but there is sufficient to encourage further investigation. The premise of the paper is to use an analytical model of plume dynamics to derive a parameterisation. The assumptions underlying the analytical model are discussed but the suitability of this approach in a wider range of applications is not made clear. For example, at what model resolution do the assumptions breakdown even for the same IFR/FBC area?

We want to thank reviewer 1 for constructive comments. Your latest remark regarding the resolution at which assumptions break down is difficult to reply, as it depends on the characteristics of the dense overflow. Our DPLUME-Full parameterization assumes quasi geostrophic equations describe the dense water plume, but as mentioned in the manuscript this assumption breaks down either if the plume is very dense compared with its environment or if the width of the plume is small. If we consider only the IFR/FBC area, a dense plume of thickness roughly 100m, and the density anomaly at the FBC sill, then we get a baroclinic Rossby radius of about 3.6km. So the quasi-geostrophic dynamics start to be described with a resolution of at least 1.3 km. We have now mentioned it in the manuscript (Line 134). However, as already mentioned, our first stage of parameterization named DPLUME-PRGR, only includes the downslope pressure gradient that results in the inclusion in an additional baroclinic pressure term, which is always physically consistent regardless of the resolution and of the plume characteristics.

The parameterisation adds momentum to the model and the authors should consider implications for energy balance diagnostics.

Very good remark. The parameterisation actually adds an extra pressure gradient. So if the flux of pressure anomaly is considered in the energy balance diagnostic, then it should work. We now specify this point in the manuscript (Line 158).

The implementation is unlikely to have impacted the computational performance of the model greatly but the authors should specify the cost of adding their parameterization. The results sections appear to have been rushed and could be presented better to improve readability (see comments below). The code changes appear well-contained and using a recent enough version of NEMO that replicating this work in the latest releases should be straight-forward.

> We now provide an estimate of the computational cost of this new BBL parameterization (Line 289). We provide further reply to your detailed comments hereafter.

line 50 'only this' instead of 'this only'
line 74 'the conclusions of which' instead of 'and which conclusions' line 114 'from which' instead of 'which yields' line 256 'a slight density increase' instead of 'a light density increase'

> This is now changed.

line 83 Not sure turbulence is the best example here. Turbulence closures often include empirical relationships derived from observations and laboratory experiments and are not simply analytical studies.

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Paragraph starting line 172. I find the discussion of u and v as alongslope and downslope velocities confusing in this section relating to the model grid. Why should the slope always be aligned such? Surely, any real world application could have the roles reversed or any orientation in between?

Indeed, there was confusion here. When describing the system of equations on which the parameterization is based, we use u and v as alongslope and downslope velocities but when implemented in NEMO, this notation does not matter anymore as alongslope and downslope directions can take any direction. We have now made this point clear in the manuscript (Line 143).

Section 3.1 The sensitivity experiment is of insufficient length to evaluate the effect of the parameterisation in the target application of climate forecasting. Much longer tests in a global domain featuring all of the major overflows will be required before the universality of the approach and any impact can be assessed.

Indeed, we have made this point clear in the text (Line 209).

The presentation needs to be improved however. Simple steps such as renaming DIFF as BD_DIFF (to make easier association with Beckman and Döscher) and ADV1 as CG_ADV1 (to make easier association with Campin and Goose) for example, would help. Figure 4 does not make for an easy comparison between the schemes and would benefit from addition panels showing DIFF-NOBBL, ADV1-NOBBL and ADV2-NOBBL. Likewise, the comparisons of sections in figure 5 would benefit from a supplementary figure with columns NOBBL, DPLUME-FULL minus NOBBL, DIFF minus NOBBL, ADV1-NOBBL and ADV2 minus NOBBL.

We have now changed the figures consistently. Figure 5 shows the effect of DPLUME-Full/BD/CG_ADV1/CG_ADV2 parameterizations against NOBBL. Figure 6 shows the effect of DPLUME-Full parameterization against BD/CG_ADV1/CG_ADV2 parameterizations. Effect of each parameterization against NOBBL, for each section, is also presented in the appendix.

I see little value in appendix A as it stands. The only comparison is between NOBBL and BOM which achieves no new insights. It is already known that sigma coordinate models better preserve dense overflows (often too well). Unless equivalent comparisons with DPLUME-FULL are available, this appendix, and references to it, should be removed.

Indeed, it was removed.

Response to Reviewer 2

The authors are proposing a new parameterization of dense water plume in NEMO. This parameterization is based on an additional momentum term which results in increasing bottom alongslope velocities. This approach as mentioned in the manuscript do not affect the tracer concentration but the effect on the energy budget of the model could be questioned.

We thank reviewer 2 for interesting comments. Regarding the energy budget, this comment was also made by reviewer 1, and is indeed relevant. We now address it in the manuscript (Line 158).

While the introduction and implementation are clear and easy to follow, considering the results, the choice of showing only differences (mainly Fig5) between then make difficult to really appreciate the results and the benefits of this new parameterization. Although we understand that the objective is to increase the plume densities a reference simulation is missing. using BOM is a good idea but then it should be integrated in the results and not used as an appendix. Furthermore considering the comparison with BOM why is BOM bathymetry represented with stair cases while it is supposed to be sigma model.

We have now added extra results, which show the effect of each parameterization compared with the total absence of BBL parameterization (Figure 5), and compare our new parameterization with the previously existing ones (Figure 6). Reviewer 1 noticed that the comparison with BOM is actually difficult, which your remark regarding the vertical coordinate system confirms (we had interpolated BOM to the Nemo-NAA10km vertical grid to allow the comparison). We have chosen to remove this appendix.

A thing which is not discussed and should be is the relevance of the parameterization in regards of the horizontal and vertical resolution of the model.

We have now mentioned this point in the manuscript (Line 134) regarding the horizontal resolution. The relevance of the parameterization when it comes to the vertical resolution is mentioned in the manuscript (Line 170).