Ocean Sci. Discuss., 10, C257–C259, 2013
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Interactive comment on "Tidally-induced lateral dispersion of the Storfjorden overflow plume" *by* F. Wobus et al.

Anonymous Referee #4

Received and published: 28 May 2013

This manuscript describes the numerical simulations of Storfjorden overflow with and without tidal forcing. The authors described that tidal simulations induce horizontal mixing in the gravity current especially around the Sorkapp headland. Although the manuscript is interested, I do not recommend the manuscript for publication as of now. It needs significant changes and additional simulations. My concerns are the following; 1) This is a sigma layer model with 3km resolution and they use 50 sigma layers. In terrain following models generally a horizontal smoothing is applied to make sure that Haney number is less than 0.4. The authors performed this smoothing but there is also additional criteria Beckmann & Haidvogel which requires that the ratio between horizontal and vertical resolution shouldn't be high. That's the reason why most of the sigma grid models use only 25-30 sigma layers in their simulation. The authors should describe what the effect of this high vertical grid spacing in spurious mixing is. They

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should run an additional experiment with 30 sigma layers and compare the results. 2) In fact, the authors should mention spurious mixing in a different subsection. In the overflow cases, numerical diapycnal diffusion is very important and it should be handled properly. They should describe if they can measure the numerical mixing or not. They use k-epsilon vertical mixing parameterization followed by Warner et al. However they should cite Ilicak et al. (2008) paper which describes performance of difference two-equation turbulence closures in Red Sea overflow case. 3) In the model description, they say "South of the sill the flow forms eddies and fills the depressions in the Storfjordrenna". The model has 3km horizontal resolution, what is the Rossby radius of deformation in that latitude? How can they resolve these mesoscale or sub-mesoscale eddies? I cannot see any evidence from the figures, there might be some filaments but not eddies. They should describe eddies in details if they resolve them. 4) In Figure 2, they should also plot the southern boundary model ssh, so we can compare the sea levels between the tidal forcing and model results. 5) In Figs. 3 and 4, the discrepancy between model results and observations are very significant. The authors seem very happy with the results but I would strongly recommend them to adjust their wording. 6) In Fig. 5, showing the last sigma layer tracer concentration might be misleading. The last sigma layer is very thin. They should plot the vertical averaged or sum of total passive tracer concentration in the gravity current. 7) The authors only showed the difference of horizontal spreading between tides and no-tides simulations. However, the most important thing in overflows is the diapycnic mixing. They should describe and show that how much amount of vertical mixing is changed between simulations. 8) I read their previous paper where they implemented no-slip boundary conditions. That flow was much smaller scale than the current study. In a 3km horizontal resolution model, using a no-slip boundary condition, just because you have higher vertical resolution, is insane. The authors claim that they resolve bottom boundary layer! They should perform an additional experiment with drag coefficient parameterizations and compare the results. Otherwise this manuscript should not be accepted.

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References; Ilicak, M., T.M. Özgökmen, H. Peters, H.Z. Baumert and M. Iskandarani, 2008: Performance of two-equation turbulence closures in three-dimensional simulations of the Red Sea overflow. Ocean Modelling, 24, 122-139.

Interactive comment on Ocean Sci. Discuss., 10, 691, 2013.