Dear Editor,

Pleas find attached a detailed answer to the reviewers' comments. We have addressed all the points and made most of the changes suggested.

The reviewers' comments are printed in blue our changes made to the manuscript are given in green.

Sincerely,

Achim Wirth

Answer to referee # 1:

General comments

This manuscript describes a series of numerical simulations of a downslope dense current in a rotating system. The simulations are distinguished principally by their res- olution and vertical coordinate. Some simulations are 2.5 dimensional (no gradients in the along-slope direction) while a smaller number are fully 3 dimensional. The princi- pal conclusion of these simulations is that a lower resolution sigma coordinate model can reproduce the results of a higher resolution model provided sufficient resolution is retained in the frictional bottom boundary layer. This conclusion is of course only applicable to terrain following coordinates, where resolution can be selectively placed near the topography, whereas z-coordinate models have to have uniformly high vertical resolution throughout the whole depth in order to achieve the same resolution at the topography. While it has been known for a while that sigma coordinate simulations of dense gravity currents can be achieved with lower resolution than z-coordinate sim- ulations (e.g. Winton et al, 1998), this is the first example quantifying the resolution necessary in terms of the frictional bottom boundary layer dynamics. However, it does not help us much with solving the problem of dense gravity current representation in ocean climate models, for which sigma coordinates are ill-suited. It does serve as a useful benchmark for future dense gravity current modeling, including perhaps future hybrid terrain-pressure coordinate models.

We thank the reviewer for his positive judgement of our work and for his comment: "[...] this is the first example quantifying the resolution necessary in terms of the frictional bottom boundary layer dynamics". Which is true to the best of our knowledge and which was the purpose of doing this research and writing this paper. We also agree, when the reviewer proceeds with: "However, it does not help us much with solving the problem of dense gravity current representation in ocean climate models, for which sigma coordinates are ill-suited". But, for todays high resolution regional models sigma models are (for a large part of our colleagues) the better choice. And, with computer power increasing with time, sigma coordinates might be the better choice even for climate models in a foreseeable future, possibly in a hybrid coordinate model, as pointed out by the referee.

Specific comments

1. Reference to earlier work on the influence of vertical/horizontal resolution

and verti- cal coordinate on gravity current modeling. I was surprised that the authors did not refer to earlier studies which for example com- pare sigma and z-coordinate simulations (e.g. Ezer and Mellor, 2004; Ezer, 2005) or examine resolution criteria for gravity currents in different coordinates (e.g. Winton et al, 1998). The resolution criteria examined in this study are focused on the frictional boundary layer - it would be nice to know if this criteria is always more stringent than the earlier criteria, or if in some cases the earlier criteria would supersede the bound- ary layer criteria. The effort to reconcile the benefits of sigma coordinates with the use of z-coordinate models for large scale simulations lead to the hybrid formulations such as Killworth and Edwards, 1998. Nonetheless those formulations have not been widely implemented, due to the inherent difficulties of matching sigma coordinates and z-coordinates. What information can your resolution criteria provide to guide future hybrid schemes?

The papers are now referenced as the referee suggested as they represent research where the effect of the resolution and grid geometry on gravity current dynamics is explored. We like to mention, however, that in Winton et al, 1998 the first grid point is 50m above the bottom, they increased the vertical viscosity up to $.5\mathrm{m}^2\mathrm{s}^{-1}$ a value that can not be justified physically. With such high values all the gravity current is in the Ekman layer dominated by friction, no vein dynamics is present. The distinction between the dynamics of the vein and the friction layer was found to be key in Wirth (2009). Similar arguments can be made for the other publications. We see that only three levels in the Ekman layer are enough, but they lead to a minimal resolution of around one metre for the first grid point. For z-coordinate models this requirement is so stringent that such coordinate system is excluded. We have thought about using hybrid coordinates, but we have no experience so far and prefer not to comment on it.

2. The effect of convective adjustment. While I agree with the authors that increasing the diffusivity to a large value in a convec- tively unstable region while leaving the viscosity unchanged is not perhaps physically justified, it would still be useful to demonstrate that the peculiar effects seen in the con- vective adjustment simulation are in fact due to the viscosity changes, rather than due to the tracer mixing. I have seen other gravity current simulations where convective adjustment seemed to have no effect - this could either be because the viscosity is not being modified during convective adjustment or perhaps because the frictional layer was not properly resolved, so little inversion in stratification occurred.

Yes, if the entire gravity current is in one or two levels only, convective adjustment will not have a large effect, as these calculations do not represent the frictional layer. In our calculation the dynamics of the frictional layer is destroyed by the convective adjustment, but if the frictional layer is not resolved anyway there might not be too much difference.

3. 3D simulations The 3D simulations are useful, in order to evaluate how the boundary layer constraints carry over to 3 dimensions. However, I didn't find the description of the 3D results to be very informative. A better quantitative comparison with the 2.5D calculation is needed. For that purpose, it would be useful to have a 3D calculation at identical resolution to one of the "successful"

2.5D simulations, yet the resolution of G03 and G11 is slightly different - why?

At the end of the same section we wrote: "The higher resolution in the zone Z3 as compared to the 2.5D experiments is required for the numerical stability of the calculation. Indeed, the eddy dynamics due to large scale instability of the flow, which are suppressed in the 2.5D calculations, ask for a higher resolution in Z3 (see section **??**)." This part is now replaced right after G11 is introduced and the wording is changed to emphasise that the resolutions agree in Z1 and Z2 but not in Z3.

I suggest carrying out a 2.5D and 3D calculation at identical nx and nz, and showing the results on the same plot. There is some qualitative comparison between the 2.5D and 3D calculations in the text, but I find it a bit confusing. For example, you say there is a laminar phase which lasts for 5-7 days, followed by a phase dominated by eddies. Firstly, the plots shown in figure 7 and 8 only go up to 7 days, so no evidence is therefore given of impact of the second phase on the transports.

The 3D part clearly suffers from the fact that a reference experiment is not possible. We wrote in the manuscript (beginning of section 4.2):

"In the 3D experiments a reference experiment, as performed for the 2.5D case, is prohibited by the size of the calculation."

At the end of subsection 5.2 we wrote:

"In the experiments some of the gravity current water reached the boundary on the lower side of the domain after only a little more than five days and the dynamics of the downslope and along slope dynamics is altered. Furthermore, our resolution in the zone Z3 is too sparse in all experiments to allow for a detailed evaluation of the eddy dynamics. These two reasons prevent an analysis of the eddying regime."

For these reasons we did not like to consider the eddying regime after 5 to 7 days as our resolution in Z3 is clearly insufficient to represent correctly its evolution.

Secondly, you say that during the first phase, the downslope transport is increased by 30% compared to the 2.5D case, yet a glance at the transports at 5 days shows about 1800 for G11 and 1000 for G03, so the increase is more like 80%, not 30%.

The referee is right, more precision is needed (the 30% are compared to the 2D reference exp. (G01)) in the description the passage now reads:

The three dimensionality increases the downslope movement by about 30% when compare to G01 and by 70% when compare to G11

The 3 fold increase in downslope transport in G11 at later times compared to the 2.5D case described in the text is not seen in the plot (because it does not extend long enough).

We added:

(not shown)

I would also like to see the cross-current structure for the 3D case to compare with figure 2: Is the friction layer still found on the downslope side in 3D, both in the initial laminar regime and in the later eddying regime? It is very important to show that the conclusions for the necessary resolution of the frictional layer carry over to 3-dimensions, since most practical dense gravity current simulations are indeed 3D.

Yes, the reviewer is right such figure should be included, this is now done. We added a fig. and the following sentence in subsection 5.2.

When the temperature in the 3D cases is averaged in the along slope direction the picture qualitatively resembles the 2.5D results as can be seen by comparing fig. **??** to fig. **??**.

Technical corrections

p4, l12: "neither of the conditions are met" - what conditions? Please clarify.

It now reads:

the Ekman layer is neither linear nor homogeneous nor stationary.

p11 l18-22 and p11 l26-p12 l1 contain identical text. Please delete one. p14 l2: "descents" should be "descends".

Sorry, I do not have the referee's version of the manuscript, so I do not know which repetition the referee referes to.

Done

p14, l17-18: "with only 3 levels in the zone Z3 a correct Ekman veering is observed". Do you mean zone Z1 or zone Z3? I thought Z1 is at the bottom. Why would resolution in Z3, far from the bottom, control the simulation of Ekman veering?

Oups, thank you! (We corrected the error)

References: Ezer and Mellor, 2004: Ocean Modelling v6 Ezer 2005: Ocean Modelling v9 Winton et al, 1998, JPO v28.

Now included.

Answer to referee # 2:

Comments on 'On the numerical resolution of the bottom layer in simulation of gravity currents' by Laanaia et al. This paper describes numerical simulations to investigate effects of the vertical res- olution in the bottom boundary layer on gravity currents at a slope. Authors used a numerical model with both z-level and sigma co-ordinate. Most experiments were car- ried out in a two-dimensional form (the authors called it 2.5 D) and a small number of 3D experiments were also carried out. The conclusion seems no surprise - sufficient vertical resolution should be used in the bottom boundary in order to get descent and transport of the gravity current and the Ekman dynamics right. The interesting results are that the refined vertical resolution does not have to be very costly, sometimes, a few more level is sufficient. From this point of view, the paper is worth reading.

We thank to referee for his positive remarks.

Major comments: My main concern of the paper is if the results and conclusions, under highly idealized conditions, can be useful to many more realistic environments, namely, is the param- eter space explored enough. For example, the slope of 1 % is quite gentle; what will happen if the slope is much greater; the temperature difference of the gravity current being only 0.2 K. Minor comments:

Yes, the reviewer is right, we did change neither the slope angle nor the temperature difference. Our findings are, however, explained based on solid dynamical arguments, as resolution of the Ekman layer, the vein. There is no reason to think that these results change when the slope angle or the density difference is increased.

We added the following sentence to the discussion section:

The results are explained based on solid dynamical arguments (dynamics and resolution of the Ekman layer and the vein) and can be extrapolated to gravity currents with different slopes and density (temperature) anomalies.

P418, line21: 'Oceanic gravity currents are small scale processes, only about 100km . . .' In oceanography sense, I do not think that 100 km is a 'small scale'. The model: there is not sufficient information about the model. For example, how does it calculate horizontal pressure gradient terms in a sigma coordinate (which is an important issue)? The sentence is now changed to:

Oceanic gravity currents are small compared to the basin scales in the horizontal and vertical directions, only about 100km wide and a few hundred metres thick.

Yes, the referee is right, we added in the subsection explaining the numerical model: The standard Jacobian formulation is used for the calculation of the horizontal pressure gradient (see Madec 2008, page 90).

2.5 D model: I think this is really a 2D vertical section model in oceanography sense. Again, this is a terminological issue, but since this journal is 'ocean science', not fluid mechanics, I think that '2D model' is more appropriate.

The 'X.5D' notation was introduced (to the best of our knowledge) to 'ocean science' by P. Killworth (Killworth, P. D. and J. M. Smith. “A one-and-a-half dimensional model for the Arctic halocline”. Deep-Sea Research, 31, 271-294). We think that in the present context it is very useful as it informs the reader that although the figs are 2D the dynamics is not 2D but that the geostrophic velocity is in the direction transverse to the plane shown. So it full fills an important pedagogical purpose.

p424: Constant vertical viscosity and diffusivity is not consistent with a no-slip bottom boundary condition. This maybe an important issue here since it must be a factor considering vertical resolution in the bottom boundary layer. Convection adjustment: I found the results with convection adjustment puzzling. In a hydrostatic model, density inversion can occur, but has no physical meaning and contradicts the hydrostatic approximation. It, therefore, should be suppressed. From Fig.2, one cannot visually see the density inversion happening. However, there is a significant difference between fig.2 and fig.4. I personally do not think that the high vertical diffusivity is the correct method to do this. However, it is a practice often being used.

Constant vertical viscosity and diffusivity is consistent with a no-slip bottom boundary condition.

Yes, the results with convective adjustment also puzzled us and it took us some time to understand why the simulation showed these “steep walls” and we included the fig. and a short paragraph to warn the reader, that convective adjustment might be very beneficial in the surface layer, it’s a disastor at the bottom.

Yes, we agree with the referee: increasing the vertical viscosity and diffusivity has many deficiencies, also because it does not include an advective transport (known as non-local transport) (please see Wirth & Barnier JPO 38, 803–816.