Answer to Reviewer # 1:

I am grateful to the reviewer, as his remarks, comments and criticism have helped to improve the manuscript.

The reviewers comments are reproduced in blue and my answers are written in black and the changes added to the manuscript are given in green.

In this paper, the dynamics of dense bottom gravity currents are analyzed with the help of Direct Numerical Simulations (DNS) of the Boussinesq equations. The set-up used by the author is highly idealized (e.g., in ignoring lateral gradients of averaged quantities), and the simulations have been conducted at the unrealistically small Reynolds numbers typical for DNS. However, in spite of the fact that such "laminar and weakly turbulent gravity currents" (see title) do not exist in the real ocean, the manuscript discusses many aspects of the problem that are at least of theoretical interest, and worth being published. The paper is well-written and clearly structured but contains a number of errors and points that need to be improved.

I thank the reviewer for the good judgement of my work.

First of all, the limitations implied by the combination of low Reynolds number and lateral homogeneity should be made much clearer in the introduction and conclusions: (a) the assumption of horizontal homogeneity removes the possibility of large-scale flow instability, contrary to essentially all field and laboratory observations, (b) the low Reynolds number implies that the mixing parameters and coherent structures identified in this study cannot easily be generalized to high-Re flows.

The reviewer is right. Concerning his criticism (a): I had and have stated in the introduction of the manuscript that large scale instability is important process, but that it is not considered here as this paper is on small scale processes only, which are not and will not be ecxplicitely resolved (in the foreseeable futur) by numerical models of the large-scale ocean circulation.

I added in the introduction:

"The quasi 2D meso-scale dynamics is assumed to be well represented in today's high-resolution hydrostatic numerical models of the ocean dynamics and it is not investigated here. The appearence of meso-scale instability and variability is hindered by the homogeneous initial conditions in the direction parallel to the ocean floor and it is excluded in the numerical integrations by the small size of the domaine of integration. The subject of the paper is the second point the small scale turbulent dynamics which is fully 3D, non-hydrostatic and involves scales smaller than a metre in all spatial directions. The influence of this small scale turbulent dynamics on the large scale has to be parametrised in today's and tomorrow's numerical models of the ocean dynamics, as they do not and will not explicitly resolve it."

Please note that in the section "the physical problem considered" I had and have written:

"In the mathematical model employed (see section **??**) and in its numerical implementation (see section **??**) all variability on larger horizontal scales is suppressed by the periodicity L_x and L_y in the x and y-direction, respectively. This is beneficial to our goal of exploring the small scale turbulent fluxes. The Rossby radius L is much larger than the domain size and the appearance of mesoscale structures is thus artificially suppressed and L is not an important parameter in our experiment. The meso-scale dynamics is usually well represented in todays regional high-resolution hydrostatic ocean models."

Concerning the reviewers point (b) the "Discussion and Conclusion" section started and starts with:

"The spatial resolution of our model is around a thousand times coarser than the dissipation scale in the ocean. The explicit viscosity/diffusivity has to be increased by roughly the same factor as compared to the molecular values. In the presented calculations the turbulent fluxes are therefore largely dominated by those due to viscosity and diffusivity."

Two lines later I wrote and write:

"The present work is a first step towards resolving the turbulent fluxes by explicitly resolving the larger eddies responsible for them. Simulations where the resolved scales are fine enough so that the explicit viscosity/diffusivity is smaller than the resolved turbulent fluxes are a challenge for future calculations."

I in the numerics section appears: "But, also note that the flow has not passed the mixing transition to a turbulent flow with an inertial range (see Dimotakis 2000) as the Reynolds numbers are below 10^4 (see table **??**), which means that the route to dissipation of momentum and density gradients can not be extrapolated to higher Reynolds number flows."

Please note, that the majority of lab exp. on gavity currents have also NOT passed the mixing transition at $Re \approx 10^4$.

I now added (taking the reviewers words): The low Reynolds number implies that the mixing parameters and coherent structures identified in this study cannot easily be generalized to high-Reynolds number flows.

Second, I was surprised to see that in spite of the idealized nature of the study, only a single parameter set was investigated with the DNS approach (Tab. 1), and very few with the parameterized one- and zero-dimensional models.

There are essentially two very different types of processes in the system the small scale turbulent dynamics and the large scale inertial oscillations and they interact. These processes have a large scale separation in space and time. The first process asks for a high resolution in time and a very short time step as compared to the period of the inertial oscillations. So it is a highly stiff problem. The fast turbulent dynamics adjusts to the inertial oscillations and acts on it through the turbulent fluxes. To resolve the two types of dynamics and to investigate the interaction is computer-time consuming. The choice of the right parameters needing a few prelimminary numerical experiments and the run presented here consumed over one year of my actual computer resources. Other researchers possibly dispose of better computer resources.

I now added at the end of section 4:

Please note that the large scale dynamics and the small scale turbulent dynamics have a large scale separation in space and time, which asks for substantial computer power in the direct numerical simulations.

Although the role of nearinertial oscillations in gravity currents forms a central part of the manuscript, the reader is left without any information about the region in the parameter space where these motions are important. For shallow gravity curents, Umlauf et al. (2010) have e.g. shown that near-inertial motions are so quickly damped that they are hardly relevant. But what about other classes of gravity currents? I doubt that near-inertial motions in gravity currents have not received a lot of attention only because they are filtered out by the methods used to investigate such flows, as implied by the author on the bottom of page 2003. Maybe in many cases, they are just not relevant?

Integrations of the governing equations show inertial oscillations when the Coriolis force is a few times larger than the frictional forces. So dedicated research to this phenomena is important. These inertial oscillations most probably favour meso-scale instability which then hides them. Other phenomena, as canyons and ridges probaly reduce such oscillations. When a flow has a constant source of dense water these oscvillations might be visible in space rather than in time. But all these are speculation that should, to my view, not appear in the manuscript. I definitely would like to see lab exp and observations dedicated to this phenomena.

In the introduction I added the sentence:

Inertial oscillations of oceanic gravity currents are also discussed in detail by Wang et al. (2003) using a 1D 2-layer model.

Finally, a lot of relevant previous work appears to have been overlooked. This includes at least the following:

· The theoretical modeling study by Wang et al. (2003) focuses on gravity current motions, Ekman effects, and inertial motions, and is therefore very close to the present study. This must be discussed.

I was unaware of the study of Wang et al. (2003). It is now discussed in the paper, in the introduction I added the sentence (already noted above):

Inertial oscillations of oceanic gravity currents are also discussed in detail by Wang et al. (2003) using a 1D 2-layer model.

And in section 3.3 (one dimensional model) I added:

Wang et al. 2003 give an analytic solution of a two layer linear model when diffusion is neglected. Such solution shows a persistent interfacial Ekman layer which is not a realistic feature as can be verified in my numerical solutions of the 3D model below and as it is discussed in Wirth (2011). The broadening of the interfacial dynamics is a key feature of gravity currents already noted by Ellison and Turner (1959).

My personal opinion is that the above shows the power of my approach using a hierarchy of models as in this way solutions from the more complete model (3D) prevent me from using unrealistic assumptions in the construction of the more simplified models, that is a persistent vanishing interface thickness.

· The numerical simulations of gravity currents with very high-resolution models by Ozgokmen et al. (2002,2004,2006) are completely ignored. These studies neglect rotation but from a physical and numerical point of view they are certainly relevant for the present study.

The reviewer is right the series of papers by Özgökmen are a pioneering and important work on fine-resolution numerical simulations of turbulent gravity currents, but they neglect rotation. Rotation is the dominant player in my simulations, so the dynamics is fundamentally different. Furthermore all these publications are cited in Legg et al. 2009 to which I refer in my manuscipt concerning turbulent fluxes. The literature on oceanic gravity currents is so large that it is no-longer possible to include references to all the important papers and I thus choose to cite the review papers.

I now write in the introduction after the citation of Legg et al. 2009:

For numerical studies of turbulent fluxes in non-rotating gravity currents please see Özgökmen et al. 2006.

and add the reference:

Özgökmen, T.M., P.F. Fischer, and W.E. Johns (2006), Product water mass formation by turbulent density currents from a high-order nonhydrostatic spectral element model. *Ocean Modelling* **12**, 237-267.

· The horizontally averaged equations (16)-(18) are identical to the equations used in the study by Arneborg et al. (2007). The latter applied a one-dimensional (slope-normal) turbulence model to study exactly the same type of flows as in this manuscript. The findings of Arneborg et al. (2007) should be discussed and compared to the authors results. This would also widen the parameter space in the direction of shallow flows (see above).

Yes, the equations appear in Arneborg et al. (2007) but they are then averaged in the vertical direction and not used in their entire maunscript. These equations are the classical Ekman layer equations and appear in some form in every text book and article on Ekman layer dynamics. My equations (16) - (18) contain the turbulent fluxes which are then parameterized in eqs. (20) - (21) by an anisotropic eddy viscosity parameterisations, which is new to the best of my knowledge. In Arneborg et al. the vertically averaged equations are then supposed to be subject to a quadratic bottom friction that is "directed oppositely to the main flow direction". I do use the 1D model and do the calculations without this constraint on the forcing direction (which completely neglects the Ekman dynamics) and I do present an analytical solution. I now added the reference to the Arneborg et al 2007 at the end of section 3.3 and write:

Variants of the 1D model and the 0D model in the next section are already discussed in Arneborg et al. (2007)

· As pointed out by Wahlin (2002,2004) and Arneborg et al. (2007), under certain conditions the one-dimensional equations (16)-(18) also apply for gravity currents flowing down a sloping channel or along a ridge. Such flows are much less prone to large-scale instability, which makes the use of the one-dimensional (laterally homogeneous) model derived in the manuscript more justified and more relevant.

Yes, large scale instability can be reduced by topographic features but also are the inertial oscillations. I prefere not to talk about the (important) effect of ridges and other topographic features on the results of my research as it is pure speculation. For example: I think that the persistence of an interfacial Ekman layer in Umlauf et al. (2009a,b) is due to the channel geometry.

· The relation between the solutions in (28) and (29) and the already existing quasistationary solutions provided by Wahlin and Walin (2001) should be dicussed. Particularly, the solution with Ekman friction in Wahlin and Walin (2001) is interesting because it avoids the alignment of stress and flow that the author complains about on the top of page 2015.

The important paper by Wahlin and Walin is aleardy cited in the manuscript. At this specific location I however do not see much in common as in their paper all the dynamics depends on the large scale gradient of the interface h, which is completely flat in my calculations.

Minor points:

1. p2005, l6. Probably "along-slope" is meant here instead of "cross-slope".

I thought that there is down-slope and cross-slope (same as along-slope). I now replaced with along slope and add "along isobaths" to make it clear. It now is:

along-slope (along isobaths)

2. p2006, l14. The Froude number is written with an "e" in the end (needs to be changed throughout the text). What is g'? Same as g'0?

Oups, now corrected everywhere (both).

3. p2006, l25-27. This reference to the Miles-Howard stability criterion applies only if Ri is the local Richardson number. As defined in line 22, however, Ri is a constant scaling parameter not directly related to local shear instability (though, of course, it may be indirectly related to the true local value of the Richardson number).

I added :a local Richardson number

4. p2008, l1. The Ekman number defined at the bottom of page 2007 corresponds to the ratio between friction and Coriolis forces, and not to its square. Please delete the words "square of" in this line.

Now corrected.

5. p2008, l21. Please dicuss the relation between the two Ekman layer thicknesses δ and $\delta_{EK}*$.

In the turbulent case the vertical viscosity is not constant but a (linear) function of the distance from the floor, so no equivalent of δ exists in this case, there are many papers on this subject especially in the atmospheric PBL community, that is why I refere to the paper by Ferrero *et al.* 2005 and the book by McWilliams 2006. There is strictly no equivalent of δ in the turbulent cas as the Ekman spiral is distorted. But I do prefere not to open this pandoras box and just state that the turbulent Ekman dynamis roughly extents to $0.5\delta^*_{Ek}$.

6. p2009, l17-19. I don't agree with the authors statement that interfacial instability "comes from the turbulence generated by the boundary". Many relevant gravity currents are much thicker than the Ekman layer (so bottom turbulence cannot even reach the interface), and mixing in the interface is driven by local shearinstability. Even without rotation, the studies by Ozgokmen (2002,2004,2006) have shown that shear-instability is the primary mixing mechanism in the interface.

Yes, the reviewer is right the phrasing of the sentence is wrong and it does not contain any new information. The critical angle compares two sources of instability, but does not say anything about the local shear instability (my point (i)), but this is already made clear in the previous two sentences. The sentence:

"For slopes steeper than α_{crit}instability of the interface is mostly due to hydraulic jumps, for smaller slopes it comes from the turbulence generated by the boundary."

is now omitted.

7. p2010, Eqs. (6) and (8). Something is wrong here with the buoyancy term. From (14) and the text below this equation I obtain:$\alpha_s = gsin\alpha = -\gamma T sin\alpha$ such that $\alpha_s T$ in (6) becomes $-g\gamma \sin\alpha T^2$. This cannot be correct. Same for (8).

The definitions of α_c and α_s are now corrected in the formulas on p 2011 (last line of section 3.2)

Also, it should be shown by scaling why for this type of problems the vertical Coriolis terms, sometimes called can be ignored ("traditional approximation"). I know that the rotation axis is normal to the sloping plane (so the vertical terms disappear by definition) - but I wonder if this is really justified.

So do I, and I published two papers on the important effects of the non-traditional term in ocean convection

Wirth, A. & B. Barnier (2006), Tilted convective plumes in numerical experiments, *Ocean Mod.* **12**, 101–111.

Wirth, A. & B. Barnier (2008), Mean circulation and structures of tilted ocean deep convection, *J. Phys. Oceanogr.* **38**, 803–816.

So I am well aware of the problem but it is not discussed in the present publication nor is it in any other publication on the dynamics of oceanic gravity currents, to the best of my knowledge. It is an interesting and still open problem.

8. p2011, l14. It is rather unlucky notation to call the thermal expansion coefficient γ. This symbol is normally used for the adiabatic lapse rate, which I found quite confusing (in particular in view of the fact that there is no special need for exotic notation here).

Usually α is used for the thermal expansion coefficient and that is used already for the slope so I used γ.

9. p2012, l13-14. The "geostrophic part", by definition, should not show any Ekman effects. The meaning of this sentence is unclear.

"geostrophic part" is now replaced by non-oscillating part.

10. p2012/2013, Eqs. (20)-(22). In spite their anisotropic nature, it would be helpful for comparision with previous studies to formally compute diffusivities from $\nu_x =< u'w' > /(\partial u/\partial z)$ and $\nu_y =< v'w' > /(\partial v/\partial z)$. This would also be useful for showing how much ν_x and ν_y differ from each other (i.e. how large the expected anisotropy in fact is). Maybe it is very small and can be ignored?

To show the anisotropy I plot the cosine of the angle between the direction of the shear in the z-direction and the direction of the flux of the momentum in the z-direction. I found this to be the best way to make the point of unisotropic viscosity tensor (it also saves one figure in a paper that has many figures). Other choices are clearly possible.

11. p2021, l25. I cannot identify any "upward propagating waves" in Fig. 12. The flux shown in this figure is very patchy anyway, maybe due to the colorscale that appears to have been chosen to emphasize tiny differences near 0.

Yes, I deleted this fig. from the manuscript and its refernce to it as it does not really contribut to the understanding and as there already too many figs.

12. Appendix. In (A1)-(A6), the author appears to use two different variables, v_G and V_G , for the same quantity. In the line above (A6): $lim_{z \rightarrow infty}$ of what?

It is now corrected.

13. Figure. Quality of figure is generally not good.

· Axes labels (what quantitiy is shown? what units?) are missing in essentially all figures.

I added axes labels in the figures.

· Time (x-axes) is given as record index, the z-axis is given as number of numerical grid points. This makes it unecessary hard to interprete and compare figures.

In the engineering community this is common. In fact the resolution is the key parameter in numerical simulations and the trained eye can estimate how well certain features/structures can be trusted. I now added the dimensional values in the axis labels.

· Axes in Fig. 8 are completely unreadable.

Now changed.

· The range of the colobar in Fig. 12 is inappropriate to show what is going on. Why not showing this in a log plot (with a contour line separating postive and negative regions).

Yes, I deleted this fig. from the manuscript and its refernce to it as it does not really contribut to the understanding and as there already too many figs.

14. Reference Legg et al. (2009): Misplaced dots (twice) in Özgökmen.

Now corrected

References:

Arneborg, L., V. Fiekas, L. Umlauf, and H. Burchard. Gravity current dynamics and entrainment A process study based on observations in the Arkona Basin, J. Phys. Oceangr., 37, 2094-2113, 2007.

Now cited.

Ozgokmen, T.M., and E.P. Chassignet, 2002: Dynamics of two-dimensional turbulent bottom gravity currents. J. Phys. Oceanogr., 32/5, 1460-1478. Ozgokmen, T.M., P.F. Fischer, J. Duan and T. Iliescu, 2004: Entrainment in bottom gravity currents over complex topography from three-dimensional nonhydrostatic simulations. Geophys. Res. Letters, 31, L13212, doi:10.1029/2004GL020186.

Now cited.

Ozgokmen, T.M., P.F. Fischer, and W.E. Johns, 2006: Product water mass formation by turbulent density currents from a high-order nonhydrostatic spectral element model. Ocean Modelling, 12, 237-267.

Wahlin, A., 2004: Downward channeling of dense water in topographic corrugations, Deep-Sea Research part I, 51 (4), 577 - 599.

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Wahlin, A., 2002: Topographic steering of dense bottom currents with application to submarine canyons, Deep-Sea Research part I, 49 (2), 305 - 320.

Now cited.

Wang, J., M. Ikeda, F. Saucier. A theoretical, two-layer, reduced-gravity model for descending dense water flow on continental shelves/slopes, J. Geophys. Res., 108(C5), 3161, doi:10.1029/2000JC000517, 2003.

Now cited.