Ocean Sci. Discuss., 8, C788–C793, 2011
www.ocean-sci-discuss.net/8/C788/2011/

Ocean Science Discussions

Interactive comment on "Laminar and weakly turbulent oceanic gravity currents performing inertial oscillations" *by* A. Wirth

Anonymous Referee #1

Received and published: 22 November 2011

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.

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.

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. Although the role of near-inertial 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?

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.
- 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 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 author's results. This would also widen the parameter space in the direction of shallow flows (see above).

- 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.

- The relation between the solutions in (28) and (29) and the already existing quasi-stationary 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.

Minor points:

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

OSD
8, C788–C793, 2011

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.

5. p2008, l21. Please dicuss the relation between the two Ekman layer thicknesses δ and δ^*_{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 shear-instability. Even without rotation, the studies by Ozgokmen (2002,2004,2006) have shown that shear-instability is the primary mixing mechanism in the interface.

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

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).

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

OSD

8, C788–C793, 2011

Interactive Comment

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?
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.
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): $\lim_{z\to\infty}$ of what?
13. Figure. Quality of figure is generally not good.
    - Axes labels (what quantitiy is shown? what units?) are missing in essentially all 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.
    - Axes in Fig. 8 are completely unreadable.
    - 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).
14. Reference Legg et al. (2009): Misplaced dots (twice) in Özgökmen.

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.

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.

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.

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

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.

Interactive comment on Ocean Sci. Discuss., 8, 2001, 2011.