Answer to Reviewer # 2:

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

Review of Article "Laminar and weakly turbulent oceanic gravity currents performing inertia oscillations"

The paper by Achim Wirth uses a range of 1D and 3D modelling to describe how the bulk structure of oceanic gravity currents is influenced by inertial oscillations and the role of inertial . Many simple models of gravity currents have not studied these processes, so this theoretical paper is helpful in describing this process. Eventually models may be able to include the effects of tides, uneven topography, internal waves, high Reynolds number turbulence and other processes, but at this stage there is still value in idealized process studies such as this paper.

I thank the reviewer for the good judgement of my paper

I have a number of concerns though about the range of validity of this paper to the ocean. In the actual simulations only one set of parameters is discussed, rather than a range of simulations looking at the role of dimensionless parameters. For instance in a paper about the role of Ekman boundary layers, I would have expected the Ekman number to be introduced and a range of simulations conducted. The simulations choose a specific dimensional quantities of a gravity current 200m deep with an interface 20m thick. There are a wide range of oceanic density currents, so the authors need to give references as to why this choice is representative. Also it would be useful to have horizontally averaged vertical profiles of density and velocity, these could be compared with typical field observations.

There are essentially two types of processes in the system the small scale turbulent dynamics and the large scale inertial oscillations and they interact. 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. For the justification of the thickness of the gravity current I now refere to the paper by Price and Baringer (1994). The Ekman number was and is introduced on page 2007 (last line)

There have been previous studies that have noted unsteadiness in flow dynamics of gravity currents - due to a variety of processes beyond inertial oscillations. For instance Ilker Fer's work in Lake Geneva, Gordon Arnold's work in the Antarctic and recent theoretical work by Paul Holland "Oscillating Dense Plumes" in JPO 2010. By reviewing these article the author could give a better context to why oscillations in the velocity are important.

I. Fer writes in his work (2002) on Lake Geneva that: "However, it appears unlikely that rotational control will be complete or that the flow will be in quasi-geostrophic balance since the duration of the slugs is much less than the local inertial period, 17.4 hours." The source of time dependence is the initial condition and not inertial oscillations.

Arnold Gordons work (see "Energetic plumes over the western Ross Sea continental slope" JGR 31, L21302, 2004) concentrates on sporadic events of downward cascading plumes inertial oscillations are not mentioned.

Hollands paper on "Oscillating dense plumes" discusses a 1D 1-layer model subject to periodic forcing (by periodic injection or a periodically varying pressure gradient). The Coriolis parameter does not appear and turbulent fluxes are parameterised.

In the excellent work of these three researchers I do not see much overlap with my work.

Other comments.

All the graphs need to be redrawn as currently axis are not labelled.

Usually Froude number is spelt with an e at the end, not as Froud.

Oups, now corrected everywhere.

If this 3D is about an oceanic current, then it doesn't make sense to have density only a function of T. This would only be the case for a freshwater system, but even then $T > 0$ (not $T < 0$ as on line 17 page 2005). It would be simpler if the author just used density rho or density anomaly in all equations, rather than T.

The water in the gravity current is denser than the surrounding so its temperature is lower. Using a linear equation of state and a density that is only a function of temperature there is strictly no difference in using temperature or density, I choose to use temperature, other choices are clearly possible but do not change the scientific results.

The reduced gravity definition on line 1 of page 2006, in eq 14, and on line 26 page 2016 implicitly assumes that this is a two layer system, despite using a smooth profile for density. Consider using the integral definition of g' as in the paper of

The configuration used is very close to a two layer system, with a thin interfacial layer. I do not see how a different definition of the initial reduced gravity could modify the results in the present experiment, so I used the simplest one. Other choices are possible.

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.

On lines 27-29 page 2005 the authors mention that dilution can occur in a gravity current, but don't discuss any observations of entrainment. See the two recent articles by Claudia Cenedese in JPO for detailed discussion on entrainment in real gravity currents. I particular entrainment can occur for $Fr < 1$ and low Re, so the main reason it doesn't occur in the present model is due to resolution and low Reynolds numbers. In light of the two papers below would also query the statement on page 2009 line 10-19 that only turbulence at the bottom boundary causes interfacial entrainment, as this is clearly not the case for many oceanic density currents.

On line 10 I first mention that local instability leads to to turbulent fluxes before mentioning the influence of the bottom boundary at second place. The formulation was however ambiguous the paragraph is now rephrased.

There is, however one important point that I want to make here. J.S. Turner (Buoyancy Effects in Fluids p170-171, refering to Batchelor 1954) writes, that entrainment "[...] implies that there is a mean inflowacross the interface boundary [...]" so it is a phenomena which is asymmetric about the boundary. Local shear instability in a Boussinesq fluid is by definition symmetric so it can not by itslef lead to entrainment but only to mixing. Entrainment occures when fluid of a less turbulent fluid is incorporated in a fluid that is more turbulent. Such difference in the turbulence levels, can in a gravity current only come from the bottom, as all the rest is symmetric about the interface (via a Gallilean transformation). This facts are not reflected in most definitions of entrainment used by researchers in the field of oceanic gravity currents. Clear definitions are however important. It is not my purpose to finger point ill defined definitions but to come up with a clear definition myself.

Turbulent fluxes at the interface include mixing, entrainment and detrainment. They can be induced by: (i) local instability of the interface due to a low Richardson number (Kelvin-Helmholtz instability), or (ii) hydraulic jumps for Froude numbers exceeding unity, or (iii) by turbulence from the bottom boundary layer reaching the interface. Turbulent fluxes due to (i) are symmetric about the interface (in a Boussinesq fluid) whereas (ii) and (iii) can lead to asymmetric fluxes such as entrainment. The relative importance of (ii) versus (iii) can be expressed by $0.5\delta^*_{Ek}/H_{crit} = 0.5\sqrt{c_G}/\sin\alpha$ (with $H_{crit} = v_G^2/g_0'$). This leads to critical angles $\alpha_{crit} = \arcsin(\sqrt{c_G}/2)$ which lies between one and two degrees, slopes typical for oceanic gravity currents.

I also added in the conclusion section:

Turbulent fluxes of higher Reynolds number gravity currents can be estimated in dedicated laboratory experiments as done by Cenedese et al. 2010 and Wells et al. 2010.

Cenedese, Claudia, Claudia Adduce, 2010: A New Parameterization for Entrainment in Overflows. J. Phys. Oceanogr., 40, 1835-1850. doi: http://dx.doi.org/10.1175/2010JPO4374.1

Now cited

Mathew Wells, Claudia Cenedese, C. P. Caulfield. (2010) The Relationship between Flux Coefficient and Entrainment Ratio in Density Currents. Journal of Physical Oceanography 40:12, 2713-2727

Now cited.

I several places in this paper (page 2003, 2006, 2017 and elsewhere) the authors use the word "suppose" when they should use the stronger word "Assume".

Thank you, suppose is now replaced everywhere by assume (8 locations).

The Richardson number on line 23 page 2006 is not strictly a gradient richardson number, as this would be a function of depth. Rather it is still a bulk parameter.

When considering the instability criterium I now added : a local Richardson number

Page 2013 line 25. If the author plotted the velocity profile it would be clearer what the gradient is. This could be compared with such canonical profiles as in Ellison and Turner 1959 JFM, which show a broad velocity profile at the upper interface and sharp profile at the base.

Yes, the presence and persistence of strong vertical gradients of the horizontal velocity at the bottom is shown in Fig. 7, as is the dissaperance of the interfacial gradients. Fig 5 where the horizontal velocity just above the bottom is plotted is dedicated to the gradient in the 1D and 3D model. Please note that the 0D model is constructed in such a way that the interfacial gradient (strong or not) does not matter. This is the case as we vertically integrate from the bottom (no slip) to the surface (free slip !!). I thus prefer not to talk about the interfacial gradient at this point as it could lead to the wrong impression that there is an approximation in eqs (23) - (25), whereas it is only averaging.. I now added:

The gradients for the 1D and 3D model can be obtained from Fig. 5, which shows the velocity 0.5m above the ground.

Page 2015. In the 3D I would be much happier if dimensionless variables are used throughout. Rather than refering to particular heights above the base, a dimensionless height of z/H would make the simulation comparable to other oceanic density currents. Similarly all times should be a fraction of To.

Page 2015 line 13. At low Froude number flow is likely to be highly anistropic I would argue, as stratification in very strong.

I start the sentence with "At small scales the turbulent structures are almost isotropic [...]" Near the bottom the stratification is vanishing and so the structures approach isotropy.

Furthermore in a stratified shear flow scales below the Ozmidov and the Corsin scale approach isotropy. The isotropisation at small scales is a key idea of universal turbulence theory (se: Frisch, "turbulence: the Legacy of A.N. Kolmogorov). The important point I want to make here is that strongly anisotropic grids are justified when only the large scale dynamics is considered explicitely, but when small scales are considered the grid has to approach isotropy.

I now added:

Strongly anisotropic grids are justified when only the large scale dynamics of oceanic gravity currents is considered explicitely, as at such scales the dynamics is clearly anisotropic. At small scales, however,...

Page 2018 section 5.1. I found Fig 8 very hard to interpret as it is not clear what direction the flow is moving in the different frames. Labeling the figure axis would be a good start, but I really want to see some indication of the direction of the mean flow. For instance do the roles become unstable becaase the flow changes direction, or is it just due to the time the take to develop?

Figures are now labeled. To determine the velocity in and above the Ekman layer I refer, the reviewer to the other figures. I cite the instability analysis of Dubos which shows similar instability behaviour for stationary rolls. To my understanding instability calculations for evolving rolls are beyond actual instability analysis. So, I do not know the influence of changing flow direction on the stability of the rolls. The similartity to the instability of stationary rolls seems strong. This is due to the fact that the appearence of the rolls and their instability evolces on a time scale faster than the inertial period.

Page 2020. Near bed stresses could also be estimated by Reynolds stress profiles. Rather than use (34) and (35) the authour could directly calculate $< u'v' >$

The reviewer is right in a fully turbulent flow with a well developped log-layer one could calculate $\langle u'w' \rangle$ and $\langle u'w' \rangle$. This, is how the bed stress is obtained in observations and lab experiments, where measurements in the viscous sublayer are impossible due to its thinness.

In my "laminar and weakly turbulent" (see title of my paper) the dominant part of the bed friction is still transmitted by viscous friction and no clearly developped log-layer exists. At the bed $w = 0$ and I thus obyain the real bed-stress with zero approximation.

Page 2022. Eq (36) The author would be better off using the algorithm describes by Winters and D'Asaro "Diascalar flux and the rate of fluid mixing" 1996 JFM. This method would get around the apparent fluxes that are caused by waves, that result in positive and negative fluxes seen in figure 12

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.

Page 2024. Line 20 - An interfacial Ekman layer IS seen in field observations of the Baltic by Arneborg and Umlauf

(I suppose the reviewer refers to page 2023) Yes, an interfacial Ekman layer is clearly visible in the observations of Arneborg and Umlauf but it might be caused by the continuous inflow condition and dissapear further downstream. Also in the channel geometry water that flows one way in the bottom Ekman layer has to return somehow and does it in the interface (it can not happen in the geostrophic interior). In my geometry no return flow is necessarry. I did not discuss this point in the paper because all this is speculation, I can only see that in my geometry it does not happen. I remember having discussed this point with L. Umlauf at the EGU meeting in Vienna 2010. I now added:

An interfacial Ekman layer has been observed by Umlauf and Arneborg (2009a,b) in a continuously forced gravity current in a canyon at one current section.