**Ocean Science
Discussions**

Interactive
Comment

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

Anonymous Referee #2

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 modell=ing 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 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

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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 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.

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.

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 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

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.

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.

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

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

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".

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.

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.

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.

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?

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'>

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

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

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