

Interactive comment on “Instabilities within Rotating mode-2 Internal Waves” by David Deepwell et al.

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We thank the Reviewers for their comments. We have responded to all comments, and where applicable have added in their feedback into the article. Reviewer comments are in bold font, responses are in normal font.

Page 3, Line 11: I do not think the authors conducted DNS as they claim in line 11. For such kind of simulations the grid step should be at the level of the Kolmogorov’s scale, but there are no details on both in the text. And what about numerical viscosity? With quite a coarse grid it can be several order higher than the molecular viscosity, $2 \times 10^{-6} \text{m}^2/\text{sec}$, as claimed in the paper.

The text has been modified to include the following paragraph addressing this issue:

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“We have run a series of direct numerical simulations (DNS) in a setup similar to that of Maxworthy (1983), who employed a gravity intrusion from a lock release in a rotating, rectangular tank to generate mode-2 waves. Since the flow develops from a state of rest the precise definition of the term “Direct Numerical Simulation” from the turbulence literature, namely that grid spacing must be smaller than the Kolmogorov microscale, cannot be directly translated to the present situation. We define DNS in the sense commonly adopted in the stratified flow modeling literature, with Arthur and Fringer (2016) providing a concrete example. These authors state that DNS is a three-dimensional simulation which has a grid spacing which is “within approximately one order of magnitude of the Kolmogorov length scale”. The Kolmogorov scale for transitional flows is defined in an ad hoc manner, usually via the explicit calculation of the viscous dissipation rate. The grid scale of our simulations is comparable to this usage since it is within an order of magnitude of the Kolmogorov scale defined from the maximum local dissipation rate. Moreover, our numerical method is spectral, and hence higher order than that used in Arthur and Fringer (2016). The spectral filter used control aliasing applies only to the largest 30% of wavenumbers and leaves the majority untouched, and no subgrid scale model as in Large Eddy Simulation (LES) is used. In the absence of a better term, DNS will be used throughout.”

Page 4, line 8: The authors take the first-mode phase speed as the velocity scale, although the whole model set-up is for the 2nd mode experiments. Does this make sense?

The presented velocity scale is the correct mode-2 phase speed rather than the mode-1 phase speed. We have clarified this point in the manuscript to remove further confusion.

I’m not sure I understand the meaning of two concepts, c_w and a_w . They are introduced in line 2 on page 4 in a very general way, without clear explanation how do they relate to the model set-up. However, they appear in table 2 as input parameters. What is the link of these values with the tank experiment parameters

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(size scales, stratification parameters, rotation, etc)?

c_w and a_w are characterizations (not initial parameters) of the resultant mode-2 ISW which describe the measured amplitude and wave speed along the focusing wall. a_w is related to the depth of the initial perturbation, H_m , while c_w is amplitude dependent. a_w and c_w have maxima along the focusing wall and have their values decrease along a normal to this surface. The choice of a_w and c_w was made following Maxworthy (1982). We have included a short discussion and included additional references on how these parameters are associated with tank experiment parameters.

And why the wave speed, as it is introduced on page 4, is larger than the fastest mode 1 wave speed c_0 ? It seems to me the authors did not pay much attention how their paper will be accepted by the readers.

The wave speed is faster than the linear, mode-2, long wave speed because these waves are highly non-linear and have a strong amplitude dependence. This is a result for finite amplitude mode-2 waves (see Terez and Knio (1998), Brandt and Shipley (2014), or Salloum et al. (2012)). For clarity, we have added a comment about the amplitude dependence of the wave speed which leads to this larger value.

Relatively minor, but important: The presented on page 5 system is not the NS-system as stated.

We have fixed this.

Please, be careful defining the total water density and its perturbations. Secondly, the temperature, salinity and the EoS are the constituents of the NS-type system, but not the density perturbation (find also a mistake in the first eqn.)

We have explained the set of governing equations used and their relation to the set of equations believed to apply to the oceanic situation: "The equations used differ from the oceanic situation in that we take the density as a variable to be evolved, where as in the ocean it is the salinity and temperature that evolve, with density recovered

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from an equation of state. The nonlinearity of the equation of state leads to a variety of complex phenomena (e.g. salt fingering, cabbeling, the fact that pure water has a density maximum at 4 degrees Centigrade, etc). In the laboratory, density changes are typically imposed by variations in salinity with the temperature held fixed. Our formulation mirrors this situation, though the experimentally observed diffusivity of salt proves too low for inclusion in the numerical simulations."

The set of equations we use is standard throughout the literature, but the distinction with the oceanic situation is worth discussion.

I'm not sure why do the author change the Sc number? They call it the Schmidt number (why not the Prandtl number?, but never mind) and vary it from 1 to 10. This does not make any sense if the authors conduct their experiments for the laminar-size grid. The viscosity and diffusivity coefficients are constant at the Kolmogorov scale level (laminar!!), so why the authors considered their ten times variation (Table 2)? What is the idea behind that?

We have rewritten the introduction so as to have the reason for varying the Schmidt number be clear at the outset. An extract from this paragraph reads: "In terms of the numerical modeling literature, we are interested in exploring how the Schmidt number (or Prandtl number in thermally stratified systems) affects the localized shear instabilities generated near the Kelvin wave crest. This is important since Schmidt numbers representative of salt stratification ($Sc \approx 700$) are presently intractable for numerical simulations on all but the smallest scales, but realistic results may be obtained by choosing a Schmidt number larger than that for a heat stratified system ($Sc \approx 7$) but much smaller than that of salt. It also implies that while field scale simulations like those of Sanchez-Garrido and Vlasenko (2009) may have a similar Rossby number to an experimental study, they cannot have the same viscosity and diffusivity, implying that experimentalists need to carefully assess what aspects of such simulations they may successfully observe in the laboratory."

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Finally, what is the spatial grid resolution after all? Looking at Table 3 I can see it is at the level of 10^{-3} m (i.e. 1mm), which is small, but does not tell me whether this is small enough for replication of the laboratory-scale experiments and the background mixing. Maybe yes, but the text in its present state is not convincing enough for me.

We have added "For the resolution listed in table 3, the strongly stratified region of the background stratification contains approximately, $2h/\Delta z \approx 33$ points, while the entire stratification has approximately 140 points. Small scale features in the transitional flow typically are a couple centimetres in diameter and contain about 20 points. The applicability of the stated resolution was also found by comparing the grid scale to the Kolmogorov scale which we define using the maximum local energy dissipation rate. In all cases the maximum grid resolution is within an order of magnitude of the Kolmogorov scale. Thus, our simulations are well resolved."

No details are provided how the initial ISW was created. Figure 1 does show the initial installation, and I can believe that in the vertically symmetrical case the leading ISW is a second mode wave, but it really takes time to form in the front of the wave field. Is 6.4m tank long enough to form it? When the rotation has been switched on? Right in the beginning of the experiment? What is the idea of all these experiments? I would accept the method of initial wave formation and initiation of the rotation after that to learn the effect of rotation, but all the details must be explained. I'm really confused without the correct setting of the experiment conditions. Lines 15-25 on page 6 do not bring any clearance on this point.

It was assumed that a reader either has a prior knowledge of tank scale mode-2 experiments or is prepared to follow some of the references provided, but obviously this needed to be cleared up. Based on the reviewer comment, we have ensured that some discussion of the lock-release generation method is provided and that sufficient references have now been given which explain in detail the process by which the ISW is

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formed. For the initial conditions of the manuscript, the resultant mode-2 ISW is essentially formed within a meter of the collapse region. This is sufficiently quick and leaves the majority of the tank to be used as the domain for the rotation affected ISW. The rotation is present from the moment the simulation begins (as would be the case in a laboratory realization of our numerical set up). Lines 15-25 on page 6 describe a way to measure the location of the wave front in the x-y plane by tracking the kinetic energy. This is used to show the span-wise variation in the ISW rather than the vertical variation which is normally done.

Interactive comment on Nonlin. Processes Geophys. Discuss., <https://doi.org/10.5194/npg-2017-71>, 2017.

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