Nonlinear Processes
in Geophysics
Discussions

NPGD

**Interactive
comment**

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

Anonymous Referee #2

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General Comments

The paper shows exciting numerical results of the evolution of a second-mode internal solitary-type wave in a rotating and stratified channel. The manuscript describes well the degeneration of a leading internal solitary wave both in rotating and non-rotating environments. In the case of rotating environments, the authors find that the initial internal solitary-type wave evolves into a Kelvin-like wave which degenerates in a train of secondary Kelvin waves and Poincaré waves. The authors examine the emergence of K-H shear instabilities as a function of two controlling parameters, the Rossby number, and the Schmidt number, finding that smaller Rossby numbers are associated with an intensification of the shear instabilities, while small Schmidt numbers are associated

with weak shear instabilities.

I think the manuscript fits within the journal scopes, it is well structured, concise and it has clear figures. However, I think the article could examine more in-depth the topics addressed in, especially in the matter of the transition to turbulence, and have a clear message and scopes for the readers. Before the paper is recommended for publication, the authors should address the following comments:

Mayor Comments

- There is no comparison between the numerical results and field observations, or attempts to suggests how and where the processes discussed in this manuscripts could be observed in nature. I think is relevant to motivate the readers with some realistic applications of the paper's outcomes. For instance, the authors might compare their numerical experiments in the absence of rotation with the laboratory experiments performed by Carr et al. (2015), since they have collaborated on recent work (Deepwell et al. 2017).

- The authors do not discuss the implications of the free-slip boundary conditions adopted in their numerical experiments; this is the case of Maxworthy (1983). For instance, how does the growth of shear instabilities near no-slip walls would change the results?

- The authors explore only one background stratification aspect ratio h_1/h_2, h, with different wave amplitudes. I wonder why they did not explore other background stratification. I would expect that in nature an upper layer thinner than a deep layer, or vice-versa. It might be interesting to explore how the asymmetry in layer thicknesses would change the growth and structure of K-H like instabilities.

- The title of the article suggests that the focus of the manuscript is the transition

to turbulence driven by second vertical mode internal waves in a rotating environment, but the paper is more general than this title. The paper examines the macro- and micro-scale processes driven by the degeneration of second vertical mode solitary waves in a rotating and bounded stratified flow. I would suggest the authors think of a different title.

Specific Comments

1. In the introduction, one would also cite the work by Moum et al. (2003) or more recent observations by Zhang & Alford (2015), and for motivations, for instance, Cuypers et al. (2010).

2. On page 2, between 20 and 25, I would mention the results obtained by Melville et al. (1989).

3. How did you estimate that a single turbulent patch has a vertical extent of about 40 cm, where are the 25 points coming from? Vertical grid points? Are the authors making a reference to Ulloa et al. (2015)'s paper?

4. Subsections 1.1 and 1.2: Why equations are not enumerated? Please, enumerate them.

5. Page 3, paragraph 20: It seems obvious, but the authors could write that the domain is $L_x \times L_y \times L_z = 6.4\,\text{m} \times 0.4\,\text{m} \times 0.3\,\text{m}$, or something like that.

6. Table 1: Missing units of z_0 and h; both variable should be defined and schematised in Figure 1.

7. Page 4, paragraph 10: ' ... we *attempt* to carry out a Direct Numerical Simulation (DNS) ... '. Concerning this sentence, what is the Kolmogorov scale of the numerical experiments? What is the Ozmidov scale? If authors are solving the Kolmogorov scale, please take the word 'attempt' out of the sentence, otherwise explain further.

8. Page 4, paragraph 10: Did Sanchez-Garrido & Vlasenko (2009) define in the same way the Rossby number? It seems that their Rossby number is almost twice smaller than the smaller Ro considered in this work.

9. Table 2 is referenced in page 4; so far there is no clear explanation about how c_w and a_w were estimated. The authors explained these parameters were parametrised on the wall, $y = 0$ m, and the amplitude was defined before the formation of instabilities. Was the emergence of interfacial instabilities defined by visual inspection of the isopycnals? How was then estimated the phase speed, c_w? This matter requires a bit of further explanation.

10. Subsection 1.2: I would suggest rewriting the description of the governing equations. I would suggest something like 'our numerical model solve the Boussinesq equations of motion on an f-plane ...'. 'stratified Navier-Stokes equations' sounds a bit unusual.

11. Page 5, paragraph 15: Is SPINS the acronym given to the numerical solver or is a general pseudo-spectral method?

12. Page 6, paragraph 5: What type of computational resources were used to perform the numerical experiments (machine, number of cores, computational time)?

13. Page 6, paragraph 5: How many vertical grid points does the numerical experiment use to solve the peaks of the square of the buoyancy frequency, N^2, on the initial solitary wave? I would include this information to show that the density

transitions are well solved. For instance, the vertical length-scale of the pycno-cline thickness in the wavefront, $\sim h$, is solved by around 17 and 36 grid points each vertical grid resolution, respectively.

14. Section 2. The authors start refereeing the work of Maxworthy (1983). Similarly to ?, I would compute the relationship between wave amplitude, A_w, phase celerity, c_w, and the dependence with the controlling parameter, the Rossby number (and later with the Schmid number). These results can be discussed along with Figure 3.

15. Equation 1. The definition of ξ does not allow a quantitative comparison between the numerical experiments. I would recommend using a scaling that allows comparison.

16. Page 6, paragraph 25. Where does the shear reach its maximum? at $\tilde{y} = 0$?

17. Figure 3: I would include the Rossby number for each experiment to show the background rotation environment along with the resulting wave dynamics.

18. Section 2 (Page 7, paragraph 5) How did the authors compute the energy partition between Kelvin and Poincaré waves? The authors may compute spatial spectra as a function of time to quantify the energy contained in the spanwise and streamwise axes.

19. Page 8, paragraph 10. How is this Kelvin-Poincaré wave resonance compared with the one studied by Melville et al. (1989), and the observed by Renouard et al. (1993). It seems that the train of solitary waves obtained by Ulloa et al. (2015) also converges to azimuthal secondary Kelvin waves.

20. Page 9, paragraph 10: Is there any experimental evidence that a secondary Kelvin wave becomes more energetic and overtakes the leading Kelvin wave? What is the relevance of this phenomenon? This might be possible to show, for

instance, by using a circular basin such as those used by Wake et al. (2005) and Ulloa et al. (2014). However, this would be a different problem. I think the authors should emphasise the relevance and implications of having secondary Kelvin waves in the system.

21. Page 12, paragraph 5: Is there any relationship between the internal Rossby radius of deformation and the length-scale where shear instabilities were found in the spanwise direction? The authors state that this region is confined to a quarter of the transversal length-scale, L_z. However, did they observe any change in terms of the Rossby radii?

22. Page 12, paragraph 10: Why did the oscillatory K-H like billows disappear once the vertical resolution was increased? Any further thought? What was the most critical wavenumber in both cases, when $n_z = 256$, and $n_z = 512$. The authors could perform a stability analysis to understand the nature of the instabilities and the growth rate.

23. Page 12, paragraph 10: Regarding the von-Karman like vortices. I would think that this kind of instabilities is possible to have a physical sense. There is a localised solitary wave propagating through a mid-layer in an initially quiescent stratified fluid. If we move on the leading solitary wave on the near-wall zone, the solitary wave feels there is a streamwise flow in the opposite direction. This scenario leads to a shear flow that could be similar than the one observed in the generation of von-Karman street vortices. Could you please give a look at the vertical velocity profile along the core of the leading wave?

24. Page 13, paragraph 5: Is there any clue of baroclinic-like instabilities? Have the authors observed the vorticity field in the $x - y$ plane?

25. Page 14, paragraph 10: Did the authors compute the local gradient Richardson numbers on the leading solitary wave zone?

26. Page 16, paragraph 10: Do the authors think that the channel width plays any role in the train of solitary-type waves structure and the shear instabilities growth? If so, please comment.

27. Page 17, paragraph 10: What grid resolution would be required to solve the Batchelor scale in a numerical experiment with $\mathrm{Sc} \approx 700$? How far are we to solve this problem?

28. Page 18, paragraph 10: 'Since the stratification has broadened in this case, the Richardson number has increased leaving the unstable region for shear instabilities to form.' Please, explain better this sentence.

29. Results: Schmidt number dependence. What is the message from this section? In a thermally stratified fluid ($\mathrm{Pr} \approx 7$) we could expect dynamics such as those shown in Figure 14(a,b).

30. There is a problem with the references, the doi link is duplicated.

31. Abstract: I do not understand what does the last sentence mean: '*Comparisons of equivalent cases with different Schmidt numbers indicate that while low Schmidt number results in the correct general characteristics of the modified ISWs, it does not correctly predict the trailing Poincaré wave field or the intensity and duration of the K-H instabilities*'. What is not possible to predict at low Schmidt number (guessing the authors refer to Sc = 1)? How are the authors predicting the trailing Poincaré wave field and the intensity and duration of K-H instabilities?