

Review of the manuscript “Head-on collisions with trapped cores”
submitted to *Nonlinear Processes in Geophysics*
by V. Maderich, K.T. Jung, K. Terletska, and K.O. Kim

GENERAL

This paper uses primarily 2-D simulations to study the collision of internal solitary waves with trapped cores of different amplitudes. The motivation is observed collisions of Morning Glory clouds in Australia. Results focus on the phase shift, amplitude change and kinematic mechanisms underlying the actual collision.

I find this paper to be an interesting read which, nevertheless, leaves several questions. Numerous questions exist about how the simulations sweep parameter space, how the initial trapped core waves are set up and the physical mechanisms behind the actual collision. In terms of the latter, I am greatly concerned about the adequacy of the 2-D and 3-D resolution of the simulations, particularly in light of the use of a Schmidt number of $O(10^3)$??? How well do these simulations resolve the finer features one expects, even in 2-D, due to the wind-up of the isopycnals by the K-H billows and how can we truly speak of turbulence and mixing at the resolutions used ? How much are the computed fields smeared at the finest-resolved scale by numerical diffusion ? Finally, there are a few points where the English needs polishing.

One general grammatical comment: When describing the results, the authors often shift between past and present tense. Please keep the verb tenses consistent throughout the text.

I list my specific comments below. If the authors address them I will gladly consider re-reading the paper to recommend it for publication.

SPECIFIC

Abstract

Line 12: Change “monotonous” to monotonic.

Introduction

Page 2, Line 2: The English feels awkward here. I would change to “... experiments and numerical solutions of both the DJL equation and the actual Navier-Stokes equations.

Section 2

1. Use of a Schmidt number of $Sc = \nu \approx 1,000$ is highly perplexing. Such a value of Sc should allow the formation of very fine scale patterns in the density field: 2-D runs can support very sharp gradients, either due to the straining of the pycnocline during collision or due to the roll-up of isopycnal lines by K-H instabilities, which are most likely below grid resolution. In 3-D, one would expect a Batchelor

scale (presuming the K-H billows can attain some level of turbulence) which is equal to $1000^{1/2}$ times smaller than the Kolmogorov scale. Are the simulations resolving this scale ?

The authors need to clarify the following points:

- a. Have they conducted grid independence studies at least for their 2-D higher-amplitude ISW collision runs, where we expect the finest-scale patterns to form in the density field ?
- b. How many grid points span the actual pycnocline ? My back-of-the-envelope calculations show that the pycnocline is very coarsely resolved. Upon wave collision, it'll even be further strained and less resolved. Numerical diffusion of the low-order method underlying the authors' model can artificially smooth out things.
- c. In a 2-D run, how many grid points does one have across a K-H billow associated with instabilities along the wave ? One would need at least 30 grid points to guarantee that the resultant transverse instabilities are properly resolved in 3-D.
- d. When 3-D runs are conducted, what is the local Reynolds number (based on local value of shear and B-V frequency along the wave-strained pycnocline) in the regions where K-H billows are observed, prior to K-H billow formation ? Is this Reynolds number high enough for actual turbulence to form within these billows or do they simply form, possibly pair and support some weak transverse instability ? How do we know that there are not scales smaller than the transverse instability that form ? Again, numerical diffusion can drive some very spurious results here.
- e. **MOST IMPORTANTLY:** In 2-D, the authors should conduct a comparison of one simulation of high amplitude ISW collision at $Sc = 1$ and 1000, where I would hope/assume $Sc = 1$ is well-resolved by the authors' choice of grid. How do the results compare ? The $Sc=1$ case is presumably more relevant to the atmospheric Morning Glory case which motivates this study.

The authors need to answer all the above questions. If they cannot they should at least be honest that their results are highly contingent on the degree of pycnocline resolution and the degree of numerical diffusion in their low-order numerical method.

2. *Page 3, Line 10:* The authors discuss at this point the various scaling parameters they use. Later on in the paper, in page 7, there's a discussion as to how such a scaling does not work for the Euler equations. To this end, it would help greatly if the scaled Navier-Stokes eqns. were written out explicitly here and a warning was given to the reader about potential inapplicability of this finding to the Euler eqns.
3. *Same page, line 19:* Correct to "The simulations of interacting ISWs". Now, when one turns to table 1, there is an exhaustive list of simulations, organized in 4 groups, A through D. This is not an easy table to read. Please separate groups A, B, C and D by a space. Also, both in the text of page 3 but also in the figure caption, help the reader out by clearly stating what A, B, C and D represent. Finally, in the caption define what the first 5 parameters are so that the reader doesn't have to flip back and forth to the actual text.
4. *Same page, line 23:* Apparently, the authors are using these runs to double up for both simulations of mode-1 waves with trapped cores, for a near-surface stratification, and mode-2 waves in a two layer

stratification. The latter assumes perfectly symmetry of the solution around the middle of the pycnocline. Is this a realistic assumption and could it lead to misrepresentation of the actual physics ? How do the authors contrast this approach to that used by Stastna and Deepwell who examine the full domain.

5. *Same page, line 26:* Is the no-flux condition applied to salinity or density ? The authors should clarify what active scalar they actually examine and what type of equation of state they use, if it is salinity they are actually working with.
6. How are the initial actual waves generated ? Are they produced by solving the DJL equation and then inserted into the Navier-Stokes solver to allow for the trapped core to actually evolve dynamically ? Alternatively, is some higher-density fluid released at the pycnocline as done by Stastna and Deepwell ?
7. See Comment 1 above: How do we know that the resolution used by the authors is sufficient ? Have grid-independence tests been conducted ? What is the resolution of various critical lengthscales of the problem ? I seriously question the utility of the 3-D runs, at least until the authors are honest about their limitations.

Results

8. *Page 4, Line 16:* The reference to fluid having escaped both trapped cores and then subject to a buoyancy-driven collapse, countered by viscosity and diffusion of mass, raises the question: Are the trapped cores of the original waves subject to any leakage of mass in the first place ?
9. *Page 4, line 29:* What is a “small offset pycnocline” ?
10. *Page 5, Line 14 and onward:* We suddenly are told that the numerical simulations include runs with internal waves with trapped cores reflecting off a side boundary. See my comment (3) above. Nowhere in section 2 are we told that reflecting internal waves are studied. Pre-dispose the reader please !
11. *Same page, line 30:* Beyond K-H instabilities, are the other mechanisms through which fluid can escape the trapped core ? Consulting Kevin Lamb’s two JFM papers (2002 and 2003) might provide some useful insights in this regard.
12. *Same page, line 33:* Can one truly speak of mixing in a 2-D context, when the actual process is turbulent but not resolved in 3-D ? At least qualify the statement by saying that “mixing, as represent in a 2-D context”.
13. *Figure 9 and relevant discussion in text:* The top four panels need to be magnified by at least a factor of two. Any smaller-scale feature is barely visible and any transverse structure cannot be seen at all. This begs the question once again, how well-resolved are these transverse instabilities ? The authors

use 45 spanwise grid-points and it seems that the domain is wide enough to capture about 4 (??) wavelengths thereof. Again, taking into account the numerical diffusion of their method, can we really speak of resolving anything below the scale of the transverse instability ? Please see my comment (1). As such, any mention of turbulence and mixing in this section should be made with great caution.

14. *Page 7, line 11*: More detail is needed as to how ΔE_{dis} is defined. Does one conduct a run of a single wave and measure the energy at the beginning at end of the run, with any losses driven by viscous decay (and apparently numerical diffusion) and shear instability ?
15. *Same page, line 22 and onward*: This is a very interesting discussion. However, please see my comment (2) above. Including the actual scaled Navier-Stokes in the text would help the reader understand why this scaling won't apply to the Euler equations.

Moreover, the remaining discussion is confusing. Please clarify what is meant by “complete” and “incomplete” similarity. As always, my concern of use of a Schmidt number close to 1,000 arises.

Conclusions

16. *Page 8, line 10*: This study also examines mode-1 waves, simply with a near-surface stratification. Clarify that this contrast is made to mode-1 waves in a “two-layer stratification”.
17. *Same page, line 15*: Again, I doubt that this study resolves any turbulence. What we're seeing is the product of numerical diffusion.

Also, correct “monotonous” to “monotonic”.

18. Trapped cores in internal solitary waves are efficient mechanisms for transporting particulate matter, not just mass (see the work of Lamb). Can the authors at least offer some comment here as to how much collision impacts the capacity for an ISW to transport mass ?
19. It is clear to me that this study examines trapped core waves where the core forms due to near-surface stratification, i.e. one is looking at surface cores. However, the work of *Lien et al.* clearly observed subsurface cores in the South China Sea ; the localization of the cores in the subsurface originates from the presence of a background current and the specifics of its vertical structure. Although I see an investigation of ISWs with subsurface cores to be outside of the scope of the particular study, it would help if the authors referenced such phenomena as a topic of future investigation.