



A discussion on “Numerical computations of resonant sloshing using the modified isoAdvector method and the buoyancy-modified turbulence closure model” [Appl. Ocean Res. (2019), 93, article no. 101829, doi:10.1016.j.apor.2019.05.014]

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A discussion on “Numerical computations of resonant sloshing using the modified isoAdvector method and the buoyancy-modified turbulence closure model” [Appl. Ocean Res. (2019), 93, article no. 101829, doi:10.1016.j.apor.2019.05.014]

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1 First and foremost, the discussors wish to compliment the authors, Li et al. (2019), on their impressive results  
2 involving computational fluid dynamics (CFD) simulation of resonant sloshing. We have written this discussion  
3 merely to clarify what we feel are some potential mis-characterizations of our recent work (Larsen and Fuhrman,  
4 2018) found within the paper. While we recognize that these have not been the main emphasis of the discussed paper,  
5 we raise the issues below with the simple hope of preventing their further propagation in the literature. In Li et al.  
6 (2019) it is stated that “Larsen and Fuhrman (2018) proved that classical two-equation RANS closure models are  
7 unstable in the two-phase flow. Those closure models were originally developed for single-phase flow, and those  
8 turbulence models can be applied to the two-phase flow which is treated as a single continuum mixture. However,  
9 those turbulence models can lead to the overestimation of the turbulence level in the transition region at the [air-water]  
10 interface.” We fear that the statements above mis-characterizes the nature of our work regarding two important issues,  
11 as detailed below.

12 First, the statement above that we proved the (unconditional) instability of several classical two-equation mod-  
13 els for two-phase flow is not correct. Building on the prior analysis of Mayer and Madsen (2000), the analysis of  
14 Larsen and Fuhrman (2018), as stated clearly on their p. 424, assumes constant density and hence pertains specifically  
15 to the most canonical case of single-phase (and not two-phase) flow. This restriction in the analysis was merely for  
16 the sake of simplicity i.e. the number of phases being modelled is not of central importance in diagnosing the funda-  
17 mental cause of the instability (hence over-production) of the turbulent kinetic energy density  $k$  and eddy viscosity  
18  $\nu_T$  in two-equation turbulence models beneath (non-breaking) surface waves. The statement above leaves the impres-  
19 sion that the instability is somehow linked to models originally designed for single phase conditions being applied  
20 in two-phase situations. This is not the case: While our analysis has been made formally assuming constant density,  
21 it holds reasonably for the bulk fluid region beneath non-breaking surface waves in one- or two-phase models, since

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22 this region (in either case) is effectively comprised of a single (water) phase. This assertion was confirmed with two-  
 23 phase simulations of propagating waves by Larsen and Fuhrman (2018), e.g. their Figure 3, which still demonstrated  
 24 the instability (exponential growth) of turbulent kinetic energy density (and hence the eddy viscosity ) essentially as  
 25 predicted by their single-phase analysis. Again, the instability proved in this context is inherent in the basic traditional  
 26 turbulence model equations, and does not somehow depend on the number of phases being considered.

27 Second, the statements above gives the impression that the over-production of turbulence is a problem specific to  
 28 the air-water interface. The perception that the over-production of turbulence in two equation turbulence models be-  
 29 neath surface waves is necessarily linked to the air-water interface (or near surface) region seems to persist in (at least  
 30 some of) the recent literature (similar characterizations can be found in e.g. Devolder et al., 2017; Ahmad et al., 2019;  
 31 Kamath et al., 2019; Ouda and Toorman, 2019). We wish to emphasize that, while such characterizations are not nec-  
 32 essarily incorrect, they are certainly incomplete. Over-production of turbulence can certainly occur due to problems  
 33 specific to the interface region. At the same time, characterizing the analysis of this problem by Larsen and Fuhrman  
 34 (2018) as somehow being specific to the interface or near-surface region, as done in the discussed paper and in several  
 35 of the references above, is not accurate. On this issue we therefore offer the following clarifying points:

- 36 1. As shown (conditionally) by Mayer and Madsen (2000) and (unconditionally) by Larsen and Fuhrman (2018),  
 37 most (all that have thus far been analyzed) traditional two-equation turbulence closure models are unstable *in*  
 38 *the entirety of the nearly-potential flow region beneath (non-breaking) surface waves where there is finite strain.*  
 39 This includes, but is certainly not limited to, the near surface region.
2. Relatedly, it is indeed near the surface where the unstable growth rates will be largest, see e.g. Eq. (2.12) of  
 Larsen and Fuhrman (2018), which was derived from linear wave theory and reads:

$$\langle p_0 \rangle = \frac{k_w^2 H^2 \sigma_w^2}{2} \frac{\cosh(2k_w z)}{\sinh^2(k_w h)}, \quad p_0 = 2S_{ij}S_{ij} \quad (1)$$

where  $\langle \cdot \rangle$  represents period-averaging,  $k_w$  is the wave number,  $H$  is the wave height,  $\sigma_w$  is the wave angular frequency,  $h$  is the water depth,  $z$  is the vertical distance from the sea bed, and

$$S_{ij} = \frac{1}{2} \left( \frac{\partial u_i}{\partial x_j} + \frac{\partial u_j}{\partial x_i} \right) \quad (2)$$

40 is the strain-rate tensor, where  $u_i$  are the mean velocities in the Cartesian  $x_i$  directions. This shows that  $\langle p_0 \rangle$ ,  
 41 which largely governs the production of turbulent kinetic energy, grows with  $z$ , a point which has been similarly  
 42 made by Kim et al. (2019). It is thus likewise near the surface where this problem will typically first become  
 43 evident in CFD simulations of surface waves, to be quickly followed by regions further below. When viewed

44 in animations (see e.g. the wave train animations available at: <https://doi.org/10.11583/DTU.8180708>),  
45 this may appear as turbulence originating from or near the surface spreading throughout the water column, but  
46 in actuality it is most likely turbulent kinetic energy in the potential flow core region growing exponentially at  
47 a rate which varies vertically.

- 48 3. Adding a buoyancy production term (so-called buoyancy modification) to the  $k$ -equation, as done in the dis-  
49 cussed paper, creates a local sink in the turbulence and eddy viscosity near the air-water interface. This may  
50 remedy issues local to the interface and likewise remove a local “triggering” mechanism further below, thus de-  
51 laying the onset of instability (as effectively shown by Devolder et al., 2017). This term alone does not result in  
52 a formally stable turbulence closure, however. This is clear from the work of Larsen and Fuhrman (2018) (their  
53 analysis, as well as their Figures 3, 4a, 6a,b, 11, and 12a,b), all of which demonstrate severe over-prediction of  
54 turbulence beneath waves throughout the nearly-potential flow core region, or consequential un-physical wave  
55 decay, even with the buoyancy production term active. This is likewise clearly demonstrated in the animations  
56 referenced above. As pointed out by Larsen and Fuhrman (2018), this is even clear from the simulations of  
57 Devolder et al. (2017), their Figures 5b and 7b, both of which show kinematic eddy viscosities which have  
58 evolved to be 100–1000 times larger than the kinematic viscosity of water, after wave train simulations lasting  
59 approximately 20 wave periods. This problem is likewise seemingly evident in the discussed paper, specifically  
60 in the uniformly high eddy viscosity field shown in Figure 34a of Li et al. (2019). Achieving formal stability  
61 requires e.g. further modification of the eddy viscosity, as suggested by Larsen and Fuhrman (2018). We would  
62 encourage the authors to consider making a repeat of their simulation with a formally stabilized closure model,  
63 for comparison.

64 We again congratulate the authors on their paper, and sincerely hope the discussion above helps provide clarity on  
65 the specific issues raised.

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