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Interactive comment on "LES Modeling of Tsunami-like Solitary Wave Processes over Fringing Reefs" *by* Yu Yao et al.

Anonymous Referee #1

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1. General Comments Extreme meteo-ocean phenomena are the most destructive in nature. Tsunami waves are a major component of this threat. Therefore, the subject addressed by the authors is of interest to the readers of NHESS. The authors managed to explain relatively well the methodology - 3D numerical modeling, validated with laboratory data - and applications via a sensitive study (not very extensive) of some environmental characteristics. They use an open source numerical model and claim that the filtered Navier-Stokes (so primitive equations), with a LES approach to close the equations, will be better in comparison to vertical integrated ones (e.g. Boussinesq). The title is appropriate, the conclusions are in line with the sensitive numerical experiments (although restricted by the relatively small range of variation of the parameters) and the references are useful. There is no major issue with the English language and

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Discussion paper

so the text is understandable. There is not much novelty in this paper but the contribution to the advancement of the knowledge in the behaviour of the fringing reefs is important since these coastal morphologies are common in areas prone to tsunami activity. 2. Specific Comments Point 3.1 describe the experimental settings. These settings correspond to what Froude number? (See your own remarks about the reef-crest widths in page 21 and the implicit scale). Point 3.2 describe the numerical settings. An estimation of the turbulence scales is missing (or the Reynolds number). This way the choice of the discretization would be better understood and justified. With LES this relationship is crucial. Also with LES the imported turbulence at the boundary is usually need to feed the equations, in particular the coherent macro structures to be directly modeled. Some information is missing. Point 3.3 describe the experimental observations and the simulations. The authors refer that some of the discrepancies could be due to "... air entrainment in measuring ...". Actually, what are you comparing ? LES equations are a space filtered non-stationary approach. What are the statistical tools used to tackle both the experimental data and the numerical simulations? 3. Technical corrections. Page 3 (lines 55 and 57) - I think that "scholars" could be replaced with "researchers". Page 4 (line 70) - This is an incomplete description. Mean flows ′models are not phase-averaged. Page 4 (line 71) - Please include also the vortex-force concept and the references. Page 6 (line 149) - The rho is missing in equation (3). Page 6 (line 150) - The rho and miu should be defined after equation (4) and the numerical values given after (9). Page 6 (line 155) - It is not the SGS stress, it is the residual stress. Page 7 (lines 168 and 169) - You are not solving both phases. There are not equations for the air. VOF is not an hydrodynamical equation for the air. It is an equation for the position of the free surface (the two fluids are decoupled). Page 11 (line 256) - The vertical discretization is not kept constant. A better explanation is needed and should be done by direction. Page 14 (line 327) -The effect of the air-bubbles. Any guess of what is the magnitude of this effect? On the other hand, doesn't this influence also the performance of Roeber (2010)? And at D=61,6 m this one is better (at the transition from subcritical to supercritical). Page 21 (line 449) - It is not clear what is the "growth of the back-beach slope"? 1:12 is larger than 1:2 ? There are two references not in the text: Roeber et al. (2010) and Titov et al. (2005).

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