Natural Hazards
and Earth System
Sciences

Open Access

Discussions

Interactive comment on "Simulating tsunami propagation in fjords with long wave models" *by* F. Løvholt et al.

Anonymous Referee #1

Received and published: 12 September 2014

Review of the paper NHESS-2014-196

F. Løvholt. S. Glimsdal, P.J. Lynett, and G. Pedersen

Simulating tsunami propagation in fjords with long wave models Submitted for publication in Natural Hazards and Earth System Sciences Special Issue: Progress in tsunami science in light of the 2004 and 2011 tsunamis

1. General comments

This paper deals with the simulation of tsunami propagation and run-up in fjords trying to assess models ability to do so for typical wave characteristics of rock slide induced waves. A 1:500 scale model of Storfjorden fjord system is used and some conclusions

C2083

are drawn about the influence of non-linearity and dispersion for leading and trailing waves. Inundation in two locations, Hellesylt and Geiranger, is also simulated producing unequal results.

Authors stressed that their emphasis is to study differences in model assumptions rather than reproducing measured wave field, which sounds strange. Although they can focus their study on assessing model differences, this should be always done trying to reproduce measurements. Besides, if a model comparison is the main point of this works, it seems quite limited to use just two models (although in different modes).

Nevertheless the work presented is interesting, drawing several interesting conclusions. I found it difficult to read this paper and I have the feeling is not too well organized. I mainly miss a separate section clearly describing the observed data to be used for comparison, perhaps just after the introduction.

In general, time series figures are too small and difficult to see.

2. Specific comments and Technical corrections (page and line)

P4858-L16. Should be good to give more details on why the authors state that errors may come from the inundation model if at Hellesylt a good match with observations is obtained while not so good at Geiranger. What fails in the inundation model and if this could be improved.

P4858-L22. 1958 Lituya Bay.

P4859-L1-7. A figure showing the location of Storfjorden and Aknes rock-slope would help to locate the scenario.

Section 2.1 Finally it is not clear to me which discretization method is used in the version of Coulwave utilized here. In L22 it is said FV for Bousinesq, but in L4-5 next page, finite difference for NLSW.

P4861-L27 Centered formulation does not seem a good choice for hyperbolic systems.

C2084

P4862-L2 "it's" must be changed to "its"

P4862-L2 or – using a non-dispersive (an "a" has been removed) P4863-L10. Courant numbers of 0.1 and 0.2. This is going to result in extremely diffusive results! Is a multistep method? Which order? A higher order multistep method could explain such a low Courant number but at the same time will produce a non-TVD scheme with problems of stability... Perhaps all that should be briefly mention.

P4864-L1 Intensive? Or computationally demanding?

P4864-L8 two "are" appear, one should be removed: "are fed into MOST (-) during ..."

P4865-L16-17 Couldwave instability problems are somehow disappointed. It can not be solved in some way? Sounds strange that nothing can be done to go further in spatial resolution, as reducing at the same time the time step.

P4865-L19-21 This sentence is too imprecise, the "is perhaps sufficiently" does not seem too scientific, and "still remains inaccurate for the trailing waves" may mean that something better should be done. Why it is not?

P4867-L23 A dot is missing: "boundary. In the latter case..."

3. Final comments

I find this paper a good work, but I think it must improve several aspects mainly for clarity. My personal feeling is that is difficult to read and understand. My main regret is that being the aim to compare effects of different parameters and model formulations, only two model are used (although in different modes: linear, non-linear, dispersive) and I can not see which are the parameters that have been studied. Some effort should we demand to the authors to improve this work.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C2083/2014/nhessd-2-C2083-2014-supplement.pdf