NHESS-2014-196, Reply to referee #1 comments

Please find below the line-by-line response to the comments of referee #1 to the paper entitled "Simulating tsunami propagation in fjords with long wave models", by Finn Løvholt, Sylfest Glimsdal, Patrick J. Lynett, and Geir Pedersen. We thank the reviewer for his / her comments, and we find that they have contributed improving the overall quality of the paper.

Referee comment: 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.
Reply: We understand this comment, and agree partly to it, in the sense that it is valuable to link the model simulations to the measurements as close as possible. On the other hand, there are limiting factors, such as scale effects and undocumented circumstances. In the present case, we did not have velocity data immediately available, which makes a direct comparison with data somehow less relevant as the angle of incident of the waves has a direct importance. Moreover, the comparison with the Coulwave simulations with measurement data are tuned to obtain first order agreement with data.
Authors changes: In the introduction, we elaborated our discussion concerning the scope.

Referee comment: 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).
Reply: Although we agree with the reviewer that including additional models would be interesting, we find that the three different models (Coulwave, GloBouss, and MOST) with different parameter combinations are sufficient to demonstrate the main scope of the paper, namely how some key parameters affect the alongshore propagation of tsunamis in fjords
Authors changes: We have clarified the reason for using the different models. A new section has also been added partly for this purpose (as also elaborated below).

Referee comment: I mainly miss a separate section clearly describing the observed data to be used for comparison, perhaps just after the introduction.
Reply: We agree that the description of the work in general could be clearer. A couple of related comments were addressed by the other reviewer. We have tried to take these comments jointly into account.
Authors changes: A new subsection is introduced, linking the data to the different model simulations. The new subsection is put into the first part of section 3, labelled "3.1 Model setup", but is more oriented towards explaining how relevant parts of Lindstrøm et al. (2014) are related to the simulations (some more details concerning the laboratory measurements are added however).
Referee comment: In general, time series figures are too small and difficult to see.
Reply: Agreed. Point also raised by the second reviewer.
Authors changes: We have increased the fonts for most figures. In addition, we have tried to zoom in more on the important areas where possible, to improve readability.

Referee comment: 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.
Reply: This was somehow explained in the manuscript already. Running these kind of models with high grid resolution and large non-linearity often leads to instability (see Løvholt et al. 2013), which prevent high accuracy. We are hence forced to run with relatively coarse resolution. This is most likely the reason for the large discrepancy in Figure 12 (we observe better agreement in Figure 7 with local forcing and higher resolution). Moreover, errors may accumulate over the long propagation distance along the fjord.
Authors changes: We have tried to elaborate the points mentioned above in the revised version of the manuscript.

Referee comment: P4858---L22. 1958 Lituya Bay.
Authors changes: Revised.

Referee comment: P4859---L1---7. A figure showing the location of Storfjorden and Aknes rock---slope would help to locate the scenario.
Authors changes: An inset has been added to Figure 1.

Referee comment: 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.
Reply: Exactly, we employ different numerical schemes (the NLSW is not implemented in FV). We see that a similar statement was missing for the Boussinesq model.
Authors changes: A new sentence has been added, specifying the numerical scheme used.

Referee comment: P4861---L27 Centered formulation does not seem a good choice for hyperbolic systems.
Reply: For Boussinesq type equations, which do not display a purely hyperbolic nature, centered differences generally work well. It is not obvious that this is also the case for special features like inundation. However, we tested several formulations, and it turned out that the centered scheme was most robust. Please note that the present system is not a pure hyperbolic one, and also, that the centered formulation is merely applied for the treatment of the dry-land inundation.
Authors changes: No changes.

Referee comment: P4862---L2 "it's" must be changed to "its"
Authors changes: Done.

Referee comment: P4862---L2 or ------ using a non---dispersive (an "a" has been removed)
Authors changes: Done.

Referee comment: 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.
Reply: Again, we emphasise that the Boussinesq models are fundamentally different from NLSW; the sharp wave fronts that occur in NLSW models are generally smeared out.
Authors changes: No changes.

Referee comment: P4864---L1 Intensive? Or computationally demanding?
Reply: Both.
Authors changes: Text amended.

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

Referee comment: 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.
Reply: We think it would be extremely demanding to resolve this problem which is common for several of these kinds of models. We addressed the subject in Løvholt et al. (2013), but resolving the instabilities are beyond the scope of this paper. The mechanisms of instability most likely involve higher order terms of non-linear or dispersive nature, and are extremely hard to pin down. We have also reason to believe that such instabilities are common for this type of Boussinesq models. Efforts applying analysis in a heuristic manner have not yet produced relevant and systematic insight allowing us to mitigate these instabilities.
Authors changes: No changes.

Referee comment: 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?
Reply: We agree that the first sentence is too imprecise, and have revised it. Regarding the trailing waves, we do not expect the same degree of correspondence due to limitations in the input data (lack of directivity of the waves). Anyway, in modelling of dispersive waves there will always be components that are too short to be well represented, due to either model limitations or resolution.
Authors changes: Wording of the sentence revised.

Referee comment: P4867---L23 A dot is missing: “boundary. In the latter case...”
Authors changes: Done.