Report on "**Laboratory and numerical experiments on stem waves due to monochromatic waves along a vertical wall**", by Yoon, Lee, Kim & Shin

The manuscript at hand presents a detailed experimental analysis of the formation of stem waves, due to monochromatic waves interacting with a vertical wall. The experiments presented here are of excellent quality, and they are compared with a weakly nonlinear numerical code (REF/DIF) and a linear analytical solution.

The data are new, and truly interesting for the community. Besides, the comparisons with numerical and analytical data provide good insights of the phenomenon.

Furthermore, the manuscript is well written, and the presentation of the results is relatively clear. For all these reasons, I consider this manuscript should be published in the "Nonlinear Processes in Geophysics". In the meantime, I have the feeling the data are not fully analysed, and the discussion might support them better. I will detail these concerns in the following:

- Presentation of the two models is not sufficient.
  First, the simplification of equation (6) in Kirby and Dalrymple (2002) to equation (1) of the present manuscript is not straightforward. Extra precision should be given, especially focusing on the assumptions used (the order of nonlinearity, the use of parabolic formulation of mild slope equation, which forbids reflexion in the main direction of propagation, but not in the transverse direction, and the use of Padé approximants related to the kind of angles which might be reached in such conditions, …). Furthermore, the manuscript suffers an important lack of details about the numerical solution (numerical grid, boundary conditions used on two out of four boundaries, …)
  Secondly, the linear analytical solution is interesting, because it is linear, and, by definition, does not allow the formation of stem waves. This point is not clearly enough stated in the discussion. Besides, a few more details on the derivation might be welcome.

- The second point which needs clarification concerns the very definition of stem waves. It is not clearly stated in the manuscript, even if the doodle in figure 2 provides good indication. For this reason, the definition of the stem width and its computation is awkward, even if it probably constitutes a major finding of the manuscript (discussion in page 8, lines 5-15). I have the feeling this discussion should be significantly enlarged. For instance, a map of the wavenumbers can be computed from ref-dif data, providing the area were waves propagate parallelly to the wall. A comparison with these data, and the three definitions suggested here could be interesting, providing a benchmark of each of the three methods. Furthermore, the definition introduced by the authors is very interesting: given their definition of lambda, they provide the location of an imaginary wall, where idealized reflexion would appear. The distance between the wall, and this imaginary reflexion location corresponds to the stem width. This point is not explained in the text, and it would support the discussion. Finally, this new definition could be used to analyse the dependence of this width to the two parameters (nonlinearity and angle of the wall). Besides, it was not obvious to me why a single nonlinear parameter K would be sufficient to describe the phenomenon. Few words about it, and a plot of the stem width versus K could also be enlightening.

- The final point which could be improved concerns the interpretation provided by the authors about stem waves formation. Even if their observations are interesting, I was not convinced by their interpretation. Since the phenomenon is nonlinear, it is probably connected to a resonant interaction among waves. This is rather classical (see for instance three waves interactions). Surely, it is connected to a shift in the wavelength of water waves, but this is probably not the main mechanism responsible for their formation.