August 11, 2016

Dear Editor

I am pleased to resubmit for publication the revised version of " Experimental investigation of solitary breaking waves in the swash zone", which no has change title to "Experimental and computational investigation of solitary breaking waves in the swash zone". I appreciate the editors and the reviewers comments and suggestions, and I will address and answer the reviewers concerns and questions in the following.

First of all I would like to point out the main concern .....

Reviewer #1:

This paper reports a combined laboratory and numerical modeling of a set of solitary-wave runup tests with mostly plunging breaking waves during the process. Laboratory measurements were conducted in a small scale tank (water depth 0.2 m) with the velocity field measured using PIV at 4 locations and the runup height measured using a camera as well as acoustic gauges. The experiment seems to be carefully conducted and of good quality. The numerical model was validated with good agreement. Nevertheless, the numerical model, a boundary integral model combined with a viscous boundary layer model, is not new - it has been published in Physics of Fluids in 2013 by the authors.

Yes, the BIM and boundary layer models have been presented and used before, and are thus only sketchily described in the present paper. Still, the smaller inclination angle, in relation to the 2013 article, makes the computations more demanding. Hence, new tests and documentaion on accaracy are required.
Anyway, these models are not at the hearth of the submitted paper. The main point of their application is to demonstrate that the experimental setup do provide data in good agreement with theory when the flow is regular. This supports that the irregularities observed in the other measurements are real.

The experimental data, although some have been used for validation in the POF paper, are mostly new and interesting to researchers in the related area.

No data from the present investigation was used in the 2013 article, or vice versa. The angle of inclination is different and the incident waves (fig.3) have been measured anew, and even in a slightly modified manner. Additions are made in the introduction to clarify the relation between the 2013 paper and the present one (lines .... and ....).

Even though this study adds values to our understanding of solitary wave runup and breaking, I don't see clear new findings from the study. I feel the study may *be published as a Technical Note rather than a Technical Paper due to its limited scope and findings, providing the following comments are adequately addressed.*

We don't quite follow the referee here. In the paper we present a new and rather elaborate set of experiments. Phenomena such as bubble dynamics to to plunging breakers at the shore are examined, the paper has normal length and our scope and findings are no more narrow or unclear than what is common in papers on irregular flows. We believe that the paper should not be reduced to a technical note. 1.The figure with the measured surface elevation (FIG 3, and FIG 4) have been nondimentiolized by the water depth. Also the runup height and the shoreline positions figures FIG 5 and 6 is scaled with the water depth. The figures that show boundary layers are not dimensional, due to non-scaled behaviour layer.

2.The number of repetitions is three (N=3). This number is chosen due to practical reasons. Between each of the run, the water needs settle, which takes approximately 45 minutes depending on wave characteristics. Our goal in this study is not to examine the turbulent structures, but to investigate how the velocities profiles changes due to different wave height and at different location on the beach.

3.The photos in Figs. 2 and 13-14 are difficult to read and understand. Not sure what the causes are (the original images, the image enhancing (gradient) process, and/or the pseudo color?)!

Figure 2 is changed to a raw image from $\alpha = 0.30$, where the contrast has been enhanced instead of using the scaled image from matlab. Hopefully, this black and white image will be easier to understand. The Image is also titled with the same inclination as the beach. More information and interpretation regarding the gradient magnitude images are provided in the manuscript, and these images are also tilted.

4. The maximum runup height for the breaking waves for the BIM model were not defined, since the model breaks down long before maximum runup. Figure 5 is a observed shoreline from the wavetank. The maximum runup height was defined as the highest impingement of water on the beach for both the BIM and the experimental study.

5.

6.

7. The figures are so unsmooth due to the relatively small size of the swash

8. In the paper by Chang and Liu, they see an bias error in PIV algoritm, which result in Psedo turbulence intensities in the non breaking waves. They

sugguest that the error is related to the ration of the particle image to the pixel size in the images. This is often reffered to as peak locking in PIV. However the oscillations in Figure 12, is found by performing PTV on the images. We would expect to get the same type of bias error with the PTV technique, but the error could by minimized by particles.
Reviewer #2

1.One is the effects of deformed beach as discussed in the last paragraph on page 6. If the systematic depression along the centreline is significant enough to cause the pronounced transverse variation of runup, authors may also have to report runup value averaged over the transverse direction as well as the maximum value.

For the non breaking wave $\alpha = 0.10$, an traverse field of view average runup height is added in the manuscript. In the breaking wave cases the shoreline deformation was too large, such that only pieces of the shoreline was captured in the field of views. An average of the captured shoreline would therefore be misleading and are not included.

2. The other is the uncertainty of the data. In many parts of the manuscript, authors implied that the uncertainty of the velocity data are significant, which results in marked variations among different runs in figures 10 & 11, for example.

The uncertainty related to the data, the experimnetal setup, and the PIV algorithm can be related to the deviation between the runs for the non breaking case. The deviation between the runs observed in Figure 10 and 11 is larger than the ones in Figure 9a, and the deviations must therefore be interpreted as physical.

It is understandable given that the challenging situation with bubbles in thin layer with high velocity flow. However, there is no formal discussion on the uncertainty other than the discrepancy between the runs. Authors should carry out formal uncertainty analysis and quantify it.

Yours sincerely,
Lisa Smith

References

Chang, K-A., and PL-F. Liu. "Pseudo turbulence in PIV breaking-wave measurements." Experiments in fluids 29.4 (2000): 331-338.