

August 15, 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 now has changed 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:

We appreciate the positive assessment of the referee and feel that his points has helped us to clarify and improve the paper significantly.

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 documentation on accuracy are required.

Anyway, these models are not at the heart 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 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. *To make the results useful to other researchers, I suggest that the authors nondimensionalize their measurement results, similar to what they did in Table 2.*
2. *For example, the measured velocity may be normalized by the phase speed, and, similarly, the vertical quantities by the wave height and the horizontal quantities by the approximate wavelength (similar to what Grilli did).*

It is not clear to us exactly what paper of Grilli the referee has in mind, but we take it that he suggests a scaling with the waterdepth H as length scale and \sqrt{gH} as velocity scale. This may be appropriate for some quantities, but not at all for boundary thicknesses, for instance. The figure with the measured surface elevation (FIG 3, and FIG 4) have been made dimensionless 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 still given with units.

To do: Lisa; check axis legends in changed plots

2. *Line 38. Based on Figs. 8-11, $N = 3$ was used in the study? The information is not provided in the text. If so, it is too small if the flow is turbulent.*

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. More experiments than is reported in the article were also performed. Moreover, the processing of PIV/PTV velocities for densely resolved time spans is time consuming etc. Hence, it was beyond the resources available to increase N to 20, say. We do not attempt to quantify turbulence. Instead we investigate the evolution of overall velocity profiles 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 rotated 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 rotated.

4. How was the maximum runup height defined in Fig. 5 for breaking waves? Was it defined only up to the impingement (after that the model cannot handle)? If so, that is not a typical definition of wave runup height.

4. The maximum runup heights for the breaking waves for the BIM model were not defined, since the model breaks down long before maximum runup. This is explicitly stated in the start of sections 2.3 (line 99-100) and 3.2. The text is also amended in these locations. Figure 5 displays the observed shoreline from the wavetank. The maximum runup height is defined as the highest shoreline elevation for both the BIM model (available only for the smallest amplitude) and the experiments.

5. The average deviation, sigma bar, in Table 3 varies from 0.01 to 0.1 if normalized by $u = 0.4$ m/s. This magnitude is too small to resemble the turbulent level in a turbulent flow, but too large to resemble the fluctuation level in a laminar flow. Even though the authors mentioned that the flow was laminar and transitional, the results in the table need more elaboration to interpret.

Our point is simply to quantify the apparent poorer repeatability for case 50 higher up on the beach. This poorer repeatability then points to a transitional flow due to the breaking or local instabilities. We are at present not able to distinguish firmly.

To do: Check point on particle distribution.

6. The flow is unlikely in the turbulent regime due to the small physical scale. It is perfectly fine to conduct such laboratory-scale experiment so advanced techniques such as PIV can be employed for detailed flow measurements. However, the authors should also address the limitation of using such a small scale setting and its results in practical applications.

We definitely agree with the referee that this should be emphasized. Possible limitations due to the small scale is now outlined in the discussion section.

7. Figs. 10-11. Why are the mean velocities so unsmooth? If the flow is turbulent or transitional, presenting the mean velocity based on 3 repetitions is too meaningful.

We are not certain. However, previous investigations may indicate that it is due to instabilities. We are now discussing this (lines), but we cannot make firm conclusions.

To do: Is it the seeding or what ?

8. Fig. 12. Are these "oscillations," as the authors described, realistic? I would think such oscillations may represent the encounter of eddies at and beyond the breaking. Nonetheless, the measurements were taken prior to the breaking while the occurrence and sequence of eddies seem to be too regular and repeatable. That seems to be not quite physically possible. Could the oscillations be the pseudo turbulence reported by Chang and Liu (2000, Experiments in Fluids 29, 331-338)? The authors should be able to tell if the figure is replotted with the units of the vertical axis replaced by pixels.

Figure 12 shows results 120 in-beach, meaning that it is after breaking. However, such oscillation are conceivable also without breaking; they may be due to infection point instability in the retarded boundary layer (see Pedersen et al 2013), but one would then expect a growth. The discussion of this around 203 is now elaborated. In the paper by Chang and Liu, they see an bias error in PIV algorithm, which result in Psedo turbulence intensities in the non breaking waves. They suggest that the error is related to the ratio (1:6 in their case) of the particle image to the pixel size in the images. This is often referred to as peak locking in PIV. However the oscillations in Figure 12, is found by performing PTV on the images. Moreover, our particles are around 3 pixels in size and our results are thus not prone to peak-locking. This is now explicitly stated at line 72??

We are grateful to reviewer for his positive attitude valuable suggestions.

Reviewer #2

First of all, the title is somewhat misleading. As mentioned in the begging of this report, it deals also with numerical aspect and non-breaking waves. Could authors consider finding a more suitable title?

Agreed. The new title is "Investigation of breaking and non-breaking solitary

waves and measurements of swash zone dynamics on a 5° beach.”

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 experimental 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.

An analysis of the errors and the uncertainty associated with measurement

technique PIV are added to the manuscript.

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.