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 modelling 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 Pedersen et al. 2013 article, makes the computations more demanding. Hence, new tests and documentation on accuracy 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 verse. The angle of inclination is different and the incident waves (Fig.3) have been measured a new, 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 (page 2, lines 19 and 37).

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. 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. 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. However, we take it that he suggests a scaling with the water depth $z_c = A$ as vertical length scale, $x_c = KH\sqrt{H/A}$ (with some suitable value for the constant K) as horizontal length scale and $u_c = \sqrt{gH}$ as velocity scale. Then, t_c becomes x_c/u_c. Firstly, z_c will then be different from case to case. Secondly, x_c is cumbersome and dependent on K. However, we felt that a modified scaling with $x_c = z_c = H$ etc. could be useful, even though it is still not appropriate for important quantities like the boundary layer thicknesses. Unfortunately, in our view the scaling obscured rather than clarified the figures and, even more, the discussions in the text. Hence, we removed the normalization again, except in Figure 4 where the normalization is given in the axis labels.

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), and is provided in the text in page 4, line 89. This number is chosen due to practical reasons. Between each of the run, the water needs to settle, which takes approximately 45 minutes depending on wave characteristics. More experiments than what 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. 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 now 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 (page 14 line 238), 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 wave tank. 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.

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 last paragraph 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 at page 13 line 211, but we cannot make firm conclusions.

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 re-plotted 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 is now elaborated in line 229 on page 14. In the paper by Chang and Liu, they see an bias error in PIV algorithm, which result in Pseudo 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 on page 4 line 73 .

References

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

Pedersen, Geir K., et al. "Runup and boundary layers on sloping beaches." Physics of Fluids (1994-present) 25.1 (2013): 012102.