First we would like to thank both reviewers for taking their time to assess our manuscript and to provide their helpful comments. Below we address the comments from reviewer #1. We thank reviewer #2 for their reading of the paper which appears to have resulted in no further comments.

Overall the manuscript has significantly improved. Most of my comments have been appropriately addressed in the revised manuscript. The remaining comments I have are all related to Section 6.2 in which the 'simple' IG model is presented and used as an attempt to explain the large run-up events. The description of the wave model itself is now much clearer, but I still have issues with the last part that links the model output (H_ig at the breakpoint) to beta_H and ultimately to the run-up.
We thank the reviewer for their thorough assessment of Section 6.2 and their excellent suggestions. With the help of their comments, we were able to spot written mistakes in our values of beta_H and our descriptions of beta_H calculations in the previous manuscript, which we have now corrected in the revision (details in below responses to specific comments). We have also followed the other suggestions. For response to each specific comment, please see below.

• First of all, as also recognized by the authors, Beta_H is defined in van Dongeren et al. using the IG wave height "near the shoreline". Although I recognize it is quite a vague definition, calculating beta_H from IG wave characteristics at a depth of 9-13 m depth as done in the present manuscript does not seem appropriate, so I am not sure that it is a good idea to use it as such for a quantitave
This was a written mistake and we thank the reviewer for bringing it to our attention. We in fact calculate the H (IG wave height near the shoreline) in beta_H using an iterative method, starting with the maximum IG wave height H_max (at breaking point, 9-13 m depth), but eventually arriving at a H such that H = RH_max, where R is the reflection coefficient via van Dongeren et al. Please see lines 351-354 in the revised manuscript for a detailed, corrected description. This is how we have always calculated it, so the results haven't changed (except for the written mistakes where we wrote values of R in place of beta_H, see below).

• Defining the IG wave period as 12 times the incoming wave period as a general rule of thumb (applicable for different wave periods) seems rather arbitrary. I realize that the authors need to make a choice here but I think they should comment a little bit more on it (or recognize explicitly the limitations of this choice/possible implications).
We agree with this assessment and have clarified our choices and their limitations in the revised manuscript. Please see lines 321-323.

• I could not find back the values of beta_H given by the authors based on the info given in the text. Actually, based on the definition of beta_H and the fact that the IG period is defined as 12 times the incident wave period, I would expect that [beta_H for T=25s]/[beta_H for T=10s]=2.5 (as the IG wave height is approx. the same in both cases), which does not appear to be the case.
We have a written mistake here. The values of 0.49 and 0.89 are reflection coefficients, not beta_H as we have written in the previous manuscript. We have corrected this in the revised manuscript (see lines 354-356). To further clarify, [beta_H for T=25s]/[beta_H for T=10s] is actually not 2.5 as the values of H (infragravity wave height) in the equation are also different for the two cases (see response to the first specific comment, above). Apologies for the written mistake on the description of the beta_H calculations previously.

• Assuming I made a mistake and that the values of beta_H are indeed 0.49 and 0.89 as stated in the manuscript, the associated reflection coefficients according to van Dongeren would then be 0.15 and 0.5 (not given in the manuscript), which means that in both cases quite a lot of

dissipation takes place. So I do not think the authors can claim lines 349-350 that the IG waves for a 25s carrier wave show a “low dissipation” for instance.
As we stated above, the reflection coefficients (R) are actually 0.49 and 0.89 for the 10 s and 25 s carrier waves cases, respectively. We also no longer use the word ‘low dissipation’ and now simply state the R values.

• I do not understand the reasoning lines 347-349 leading to the conclusion that the energy available for run-up is 3.5 times larger when the incident waves are 25 s instead of 10 s, so I cannot judge of its validity.
Actually, I am wondering if the authors are not trying to push too far their simple modelling approach (i.e. not only to estimate Hig but also the runup induced by these long waves). An alternative would be to use a numerical wave-resolving model which could be used over the same simplified bathymetry for waves of same Hs but different peak periods (assuming a given spectral shape). Anyway, if the authors decide to stick to their simplified approach the points raised above should be addressed.
We agree that making inferences on runup based on our simple model is somewhat reaching. In the revised manuscript, we now simply state the wave height and wave energy comparisons close to shore between the 10 s and 25 s carrier wave periods. Please see lines 354-357 in the revised manuscript.

• Lines 349-352: Overall I am not sure we can really support the statement of dissipation based on the videos. In particular I do not think that the fact that the run-up front slows down (or does not slow down) on the video recordings is a good indication of wave dissipation (or absence thereof). I would say that it depends mostly on the timing with respect to the run-up/run-down cycle.
We agree with the reviewer and removed our statements on dissipation based on the videos.

Other comment:
Line 262: the authors need to give some more details on the “simple shelf geometry” they use in their calculations to make their results reproducible.
We have added a detailed description of the simple shelf geometry we have used in lines 254-257 of the revised manuscript.