Responses to comments from Reviewer #2

I appreciate the editor and reviewers for the re-assessment of the manuscript. I revised the manuscript according to the minor comments from Reviewer #2.

Minor Comments

1. Following the suggestion by the Referee # 1, the author added the following statements (lines 77-80): "*However, it is well known that the wave amplitude is not significantly a ected by non-linearity unless the non-linear wave distortion leads to wave breaking (Carrier and Greenspan, 1958; Tuck and Hwang, 1972; Synolakis, 1991). The nonlinear shoreline motion can be readily derived from the linear solution via the hodograph transform, and the run-up height is unchanged from the linear case (e.g. Pelinovsky and Mazova, 1992).*

The last sentence is inexact and has to be modified. The linear and nonlinear theory give the same extrema at he shoreline when the boundary assignment in the hodograph space is linearized. In this case the hodograph transformation essentially reduces to a map that deforms the linear solution (without affecting the extrema). These are, in fact, the hypotheses under which the largest number of analytical works available in the literature are obtained.

On the contrary, if we consider the whole boundary assignment (that is, we include the nonlinear contributions), the wave height predicted by the nonlinear theory is larger than the linear theory. An evidence of this is given in Antuono and Brocchini (2007) where the nonlinear contributions are accounted for (at least at the first order of a perturbation approach). Please add some comments about this point.

2. Again about the difference between linear and nonlinear solutions, it is worth noting that the inclusion of nonlinear contributions substantially modifies the conditions for wave breaking (see, for example, Antuono & Brocchini 2008, Antuono & Brocchini 2010). Since the analytical solution proposed in the paper holds true for nonbreaking waves, I think that some comments about the range of validity (namely, the range for the occurrence of non-breaking waves) should be added in the revised manuscript.

Response to Comments 1 and 2 : I agree with the reviewer. The newly added sentence in response to Reviewer #1 was not exact. In order to correct the sentence while keeping the input from Reviewer #1, I modified the paragraph as follows. I clarified that the statement is valid under the linearlized boundary value assignment, but stated that the nonlinear modification is small when the boundary is placed in deep water. I also added some sentences about wave breaking in response to Comment 2 citing the suggested references. However, it is not possible to provide the range of the occurrence of non-breaking waves in general, without specifying a wave type. Therefore, I just stated that the wave breaking criterion can be given as a breakdown point of the hodograph transform given the specific wave condition.

However, it is well known that the wave amplitude is not significantly affected by non-linearity unless the non-linear wave distortion leads to wave breaking (Carrier and Greenspan, 1958; Tuck and Hwang, 1972; Synolakis, 1991). The nonlinear shoreline motion can be readily derived from the linear solution via the hodograph transform, and the run-up height is unchanged from the linear case, if the boundary-value assignment is linearized (e.g. Pelinovsky and Mazova, 1992). Furthermore, the ocurrence of wave breaking, which limits the applicable range of the present approach, can be predicted as a breakdown point of the hodograph transform under the same condition. While the linearized boundary-value assignment potentially affects the run-up height and the wave breaking condition, nonlinear modifications are minor as long as the ratio of wave amplitude to water depth is small at the boundary (Antuono and Brocchini, 2007, 2008). Therefore, the main process of practical interest can be described by the linear equations when we place the boundary in deep water.

3. Section 3.2. I appreciated the reply by the author. Specifically, he pointed out how the mixed data assignment(initial/boundarydata) is substantially different from a boundary data assignment: "*Therefore, this case supports my previous statement above that the formulation is not mitigated by the presence of dissipation. (The reviewer's concern is true if we formulate the kernel for waves in the infinite time domain using Fourier transform.)*" I think that a brief comment about this point should be added in the revised manuscript.

Response to Comments 3: I added a brief comment on this point in the end of 3.2 as follows.

It is worth emphasising that the present kernel works for such a problem because it is constructed for the intial-boundary value problem. Without the initial condition, we could not derive an incident wave signal from offshore wave data when a full node is formed at the boundary.