Review of JFM 16-s-227-R1
On the resonance hypothesis of coastal runup

Although motivated by tsunami which is the consequence of short-lived forcing, this work is an extension of existing work on resonance induced by persistent incident waves of a single period. On the latter topic extensive linear and nonlinear theories exist which appear unknown to the authors. By extensive citation of what has been done in the tsunami literature they overstate the similarity and understate the differences.

The paper deals with two idealized geometries : Model I is on the 1 D resonant scattering by a sloping shelf at the end of a long channel. This topic is a direct extension of shelf resonance whose physics by linear theory is well known, (see e.g., Longuet-Higgins for a circular shelf and Mei 1983 *Applied Dynamic of Ocean Surface Waves*). Model II is on 2 D scattering by a narrow channel open to the sea. When the channel depth is constant and equal to that of the outer sea, the linear and nonlinear resonance mechanics can be found in Bowers,JFM and Mei (1983). To treat coastal runup the authors choose the channel to be a beach of constant slope. The nonlinear solution of Carrier/Greenspan is then used. Outside the sloping shelf the sea depth is constant. The linearized theory is used. For both Models matching at the shelf break is done by linearizing the Carrier-Greenspan solution in an ad hoc manner without checking whether nonlinearity is locally important.

Throughout the paper the authors made extensive reference to past works on tsunami without considering the difference from their own work of persistent and periodic forcing . Tsunami is strictly transient where a finite number of leading crests are the most important to runup. In contrast resonance by periodic forcing takes a long time to reach quasi steady state which is important only at the end rather than the beginning. Hence the frequent and elaborate citations are mere digressions which only interupts their line of reasoning. Here are a few examples (1) The long paragraph on p 6 starting with "Recently Stephanakis..." (2) The paragraph on p 8 " In an effort to calculate solitary wave runup...), and (3) The paragraph on p 9 starting with " The geometry considered by Stephanakis...". And many more. They can all be shortened or eliminated.

In my last review I suggested the authors to compare their nonlinear theory with a linear theory for physical implications in quantity and quality for both Models. Instead in the new section 4.2 the authors claim first that the importance of nonlinearity can be represented as a function of u^2. Without specifying the function they simply plotted u^2 itself. This is too indirect and very unsatisfactory.

For a physically straightforward topic the mathematical treatment here is very convoluted and can be more systematically presented. For example it would be clearer if the Green function in eq 3.9 is defined by listing the governing equation and boundary/initial conditions. For both Models I and II I failed to see any qualitatively new information different from a linear theory after the elaborate mathematics. Does a linearized theory not reveal the essential features in figures 4,5,6, 12 and 13? The authors never question whether in this nonlinear system chaos can be induced by simple harmonic incident waves. For Model II the discussion on the energy flux and Poynting vectors is not worthwhile since figs 9,10 and 11 are hardly exciting.

In short, in this revision the authors merely defended themselves without materially improving the original version. Instead of streamlining the already lengthy document, the paper has grown from 28 to 32 pages. The objectives and results are hardly related to their motivation. It would have been simpler to just make explicit comparison of runup by linear theories of resonant scattering by periodic incident waves and the final steady state (if any) from the nonlinear theory.

I regret that I am less inclined than last time to recommend the paper for consideration by JFM.