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Reply on RC1

Chuan Li et al.

Author comment on "Observations of extreme wave runup events on the U.S. Pacific Northwest coast" by Chuan Li et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-425-AC1, 2021

Response to general comments

We appreciate the reviewer's assessment and the thorough review. Regarding the limitations of the analsys, we will clearly state that that only nearshore measurements available to us during this 'experiment of opportunity' are from the tide gages along the coast. Regarding the methodology, we will make the clarifications as the reviewer suggested. Please see our response to each specific comment below.

Response to specific comments

- (Omited as specific comments starts with #2)
- This is a valid point. In our revision, we will use 'bound infragravity waves' to refer to incoming infragravity waves prior to incident wave breaking. We will use the terms 'infragravity waves' and 'infragravity motions' to refer to onshore movements of these low frequency waves after incident wave breaking.
- We agree with this assessment. In our revision, we will refer to this work as an example of large runup from energetic infragravity waves generated by abrupt breaking of incident waves.
- By low frequency runup we meant runup associated with infragravity wave frequencies. We will clarify this in our revision. This particular statement is not related to the previous statements but is a key finding from the previous work in discussion.
- We agree that this did not read very clearly in the original manuscript. In the revision, we will make it clear that the data is sampled at 1 Hz but is averaged and recorded every minute.
- We appreciate this comment. We will use a more quantitative measure to identify the "onset" of large water level fluctuations. We think the X*std(eta) suggestion is a good one. In the revised manuscript we will also identify the onset at each station rather

than the approximate onset over all stations. This will provide more detailed analysis for each station.

- We agree here as well. Here, in the revision, we will not write "offshore directed" when we are describing the data itself. We will use care to differentiate what the data shows and what we are hypothesizing based on other observations and theory.

- We realize that the spectra indicate that at some gages there were strong water level motions in the longer, 13-22min periods. However, video footage (https://youtu.be/JMYLvSsWR_g) shows that the extreme runup events were much closer to the 5 min period. The tide gages are also well inside the inlets. As some of the videos show, the heights of the bores associated with the extreme runup events were considerably reduced as they traveled further into the inlets. Furthermore, the two gages that show the largest fluctuations – La Push and Charleston (we do not include Crescent City here because it is known to amplify shelf resonance from its harbor) – show a clearly stronger signal in the 5 min period (taking into account the log scale). We will clarify this point in our revision.

- We thank the reviewer for making us aware of these new works on meteotsunamis. We will cite these papers and add their results into our discussion. We note that in Shi et al., the large wave trains have periods on the order of hours (compared to our 5 min signal). We also note that the pressure disturbances in this work (<1HPa) is still on the low side of the pressures found to generate meteotsunamis in the study of Anarde et al. (~1-3HPa). The runup period (~5 min) in this work is also lower than the meteotsunami period (~20 min) in Anarde et al. However, we do think it is important to include these new papers in our discussion.

- We agree with this assessment and per the reviewer's suggestion conducted a wave group period analysis. To do this, we used Kimura's (1980) method of computing the mean wave group period directly from the energy spectra (which does not require generation of time series). We note that there is an inherent uncertainty with any method of wave group period calculations, which arises from how the wave group is defined. A wave group is usually defined as consecutive waves with wave heights above some threshold wave height. We used the significant wave height as this threshold wave height. The significant wave height is a common choice of threshold wave height (e.g., Kimura 1980, Battjes and van Vledder 1984, and Rodriguez et al. 2000), but the resulting group period can vary if a different threshold is used. Given this, we computed a mean group period of roughly 6 minutes for the low frequency swells during the period of observed large runup events. We feel that this agrees reasonably with the roughly 5-minute peaks of the tide gage water level signals.

- The typical period of waves measured by DART during the high temporal resolution recording was about 3.5 min. It is a bit lower than the 5 min peaks of the tide gage spectra. In the revised manuscript we will provide this information and comment that it is still a plausible period for wave groups based on our analysis from the previous comment.

- We thank the reviewer for this comment. We wish to note that van Dongeren et al. 2007 write that the *forcing* of the incident waves was off-resonant. However, the alpha parameter was measured for the entirety of the shoaling region which includes the resonant region. This is supported in section 5 of van Dongeren et al.: "… amplitude of the incoming long waves can be evaluated by fitting a function of local depth with an unknown power alpha to the observed amplitude as … in the shoaling region between x = 8 m and x = 25 m." If we refer to van Noorloos 2003, which is the source of the physical experiment that van Dongeren uses, we see that x = 25 m (x is measured from the paddle) refers to the location of infragravity wave breaking. This shows that growth throughout the resonant conditions, all the way up to infragravity wave breaking, is considered. This is also consistent with Battjes et al. 2004 (Figure 5) which shows that they compare the amplitude growth of the infragravity waves against h^(-1/4) and h^(-5/2) all the way up to infragravity wave breaking.

- We appreciate this feedback from the reviewer and agree that the wording in the model portion should be improved. We will include a figure of the model result in our revised manuscript (see reply to reviewer #2). Specific points are addressed below.

  - As we stated in our reply two comments above, the growth parameter alpha was considered in van Dongeren et al. 2007 up until infragravity wave breaking. In fact, Figure A1 in van Dongeren et al. shows that infragravity wave amplitude growth persists past incident wave breaking.
  - The reviewer's statement is correct: the short-wave height is shoaled using linear wave theory until H=gamma*h. We will state this explicitly in our revision.
  - We thank the reviewer for this comment and agree that the wording can be improved. Indeed, as the reviewer wrote, we simply use dispersion to determine the depth at which 25 s waves enter shallow water. We will make it clear that this isn't a result of our simple model, but rather just from dispersion. The reviewer is correct in that equation 2 is used until the incident waves are in shallow water, and that the van Dongeren et al. relation is used shoreward of it. We show in our response to #11 (regarding lines 276-279) that the empirical relation *was* derived from shallow water data, as the term non-resonant was used to refer to the *forcing* of incident waves. This was likely written as such in order to show that the entirety of resonant growth is captured.
  - Beta is calculated using equation (3), i.e $\beta = h_x / \omega \sqrt{g/h}$ where ω is the IG wave radial frequency. We assume 12 waves per wave group in both 10s and 25s short-wave cases. Hence, ω is also changing. We will make this assumption clear in the revision.
  - We thank the reviewer for this comment. First, we've gone back to the model and improved its formulation. Instead of using a deep-water formulation for infragravity waves from offshore to shallow water, we are now using a formulation (also from Longuet-Higgins and Stewart 1962, equation 3.26) that is valid for all water depths. We now compute maximum infragravity wave heights of roughly 6.5 m and 1.1 m respectively for 25 s and 10 s incident waves. There are no published field data of infragravity waves for 25 s incident waves of 5 m wave height on very low sloping beaches that we know of. However, there are field data for smaller waves on similar beaches. For example, Fiedler et al. 2018 (Fig. 2) show a maximum a maximum cross-shore infragravity wave height of about 0.9 m at roughly 5 m water depth. This is for a case (refer to their Fig. 1) with approximately 10 s period, 3 m wave height offshore incident waves. When we ran their case, we computed maximum infragravity wave height of about 1.1 m at roughly 6.3 m water depth. We feel that

our model compared reasonably here.

  - This is indeed a typo. The breaking depth the incident waves for this case is about 9.9 m. 1.8 m was the breaking depth for the infragravity wave (2.4 m in the revised model). We will correct this in the revision.

- We thank the reviewer for this comment, and address them below for each specific point.

  - We thank the reviewer for catching this as well. As we described in a previous comment, these calculations are based on omega (infragravity wave frequency). We calculated this by assuming 12 waves per group. We omitted this in the original manuscript and will add this information in the revision.
  - The reviewer is correct that a larger IG wave height does indeed lead to greater dissipation. We currently do not mention this in the paper and will state this in the revision. However, the reason why we end up with less dissipation with 25 s waves compared to 10 s waves is that omega (infragravity wave frequency) is lower for the 25 s waves.

- The point that we are trying to make is that infragravity waves associated with 25 s incident waves are much larger, but also dissipates less, than that associated with 10 s incident waves. We will make this more clear in our revision.

- We agree that this is not well-worded. We meant to write that the lack of strong signals prior to the observations of extreme runup events onshore imply that the incoming waves were not detectable at the far offshore sensors. The strong signals that were detected at the far offshore sensors were detected after the extreme runup events were first observed onshore, which implies that these were the returning waves.

- We agree with this assessment and will include this description in our revision.

Response to technical corrections

We agree on the confusion caused by the term “very low frequency” and will rephrase as “low frequency swell.” The reviewer is also correct on figures 8 and 9 versus 8 and 10. The reviewer is also correct on sqrt(m_{-3}). These will be corrected in the revision.