

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/nhess-2020-425-RC2>, 2021
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Comment on nhess-2020-425

Anonymous Referee #2

Referee comment on "Observations of extreme wave runup events on the US Pacific Northwest coast" by Chuan Li et al., Nat. Hazards Earth Syst. Sci. Discuss.,
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he paper here does a lot of work to bring together all of the data for some extreme wave runup events, and then performs some analysis. The work with the data is very good, but the analysis is not.

The best work done by the authors is represented in Figure 14 where they show how low-frequency weighted moments of offshore waves relate to onshore RMS fluctuations of onshore tide gauges. This is useful, but not entirely clear as it would have been better to compare results using the $m-3$ moment to results using other spectral moments. $\sqrt{m-3}$ is linearly proportional to wave height. The authors suggest results but don't actually show anything definitively. The proportionality is also dimensional, which is a real problem. If the mechanism proposed here is true, then it should be able to be reduced to a dimensionless fit, but the authors state that this does not work. Why? This question must be answered.

A second issue, not addressed here, is that all tide gauges have stilling wells that deliberately filter out higher frequency water level fluctuations. I am not sure of the frequency response, but am certain that higher frequency components will be more damped than lower frequency components. For this reason the lower frequency components will be disproportionally represented in the signals. How do the authors account for this?

I have harsh words for the model presented here. The authors claim it is simple, but do not give anywhere near enough information for a reviewer or reader to be able to evaluate it or reproduce it. I can't evaluate the details of what this model is or how it was produced, so can't evaluate its appropriateness or accuracy or even all assumptions included in the model. I could not reproduce it even though I am familiar with this area. It also makes impossible predictions. A carrier wave amplitude of 2.5m at 25s is stated to reach an infragravity amplitude of 2.3m by 5.8m depth. This is wrong. We know that it doesn't happen because it would be observed instantly and would be the overwhelming feature. If the phenomenon were correct, it would also have to occur for low amplitude

shorter period waves that occur all the time and it doesn't. As one final note, the authors refer to the wave amplitude, but don't even say whether this refers to the amplitude of the RMS wave, the significant wave, or something else. This model needs work.