Reviewer A:

Dear authors,

I think the observation presented here is very interesting, and despite the conclusions are very speculative, and should have been supported by some modeling effort, I believe the article can ba accepted for this journal.

I think that the conclusion relating the exceptional duration of the dispersive signal to a local interaction between the free oceanic tsunami and the arrival of the atmospheric pressure wave is quite solid, as it is consistent with the Omira et al. 2022 model. I agree with the reviewer on the point that the second point, the hypothesis stating that the secondary, short-duration dispersive signal observed at some seismic stations is related to local gravity wave, generated at distances of approximately 100-200 km from the recording site by the oceanic/atmospheric interaction is more speculative, but I still think that it is worthy to propose some ideas. I have now modified the manuscript to state that: "However, developing a detailed physical model of the proposed interaction is needed before accepting or not this tentative hypothesis"

I have just very minor comments which are in the annoated ms i am returning. After these corrections the article should be acctepd.

Annotated manuscript not received **Recommendation: Revisions Required**

Reviewer B:

To Whom It May Concern:

The author presents a novel and amazing observation of gravity waves following the 2022 Hunga-Tonga eruption. The signal consists of a 1-40 mHz gliding spectral band that gradually increases over a period of a few days following the eruption.

This observation reflects a well known oceanographic gravity wave observation. Even though there's a big literature on this topic, I don't think this at all undermines the novelty of the observation presented in this manuscript. In fact, I think this might be the best observation ever made of this well-known phenomenon.

Walter Munk first noticed in 1950's that ocean surface gravity waves (SGW) dispersion means that incomming swell from distance storms gradually shifts to higher and higher frequencies (Munk and Snodgrass, 1957; Haubrich et al., 1963; Snodgrass et al., 1966). More recently, this phenomenon has seen a resurgence of interested because it is well documented on the Antarctic ice shelves, where broadband seismometers can directly observe ocean swell (MacAyeal et al., 2006; Lipovsky, 2018; Hell et al., 2019; Aster et al., 2021).

I have now reinforced the information provided in the manuscript on the previous observations of oceanographic gravity wave, following the Reviewer excellent recommendations'. Specifically, I've changes a couple of sentences in the Introduction (lines 96-100), included a new paragraph in section 2 (lines 159-170) and modified slightly the Conclusions (line 327-330).

I think that this review enhances the exceptional character of the gravity wave observed after Hunga-Tonga eruption, as its exceptional character arises from the fact of having been recorded at sites distributed all around the world.

Munk and Snodgrass (1957) showed that one can estimate the distance to the source of the SGWs using the formula, R = g/ [4 pi (df/dt)], [eq 1] where R is the epicentral distance, g is gravitational acceperation, and df/dt is the observed slope of the gliding spectral peak. For SGWs from storms, the frequency range is much narrower (~ 30-50 mHz) and so df/dt is approximately constant, i.e., the frequency glide follows a linear increase with time. The case presented here is a little bit more interesting because df/dt is not constant.

For the case in the present paper, we would need to go back to the idea that [eq 1] essentially derives from a group velocity calculation, R/T = g / 4 pi f, [eq 2] where T is the time since the event time.

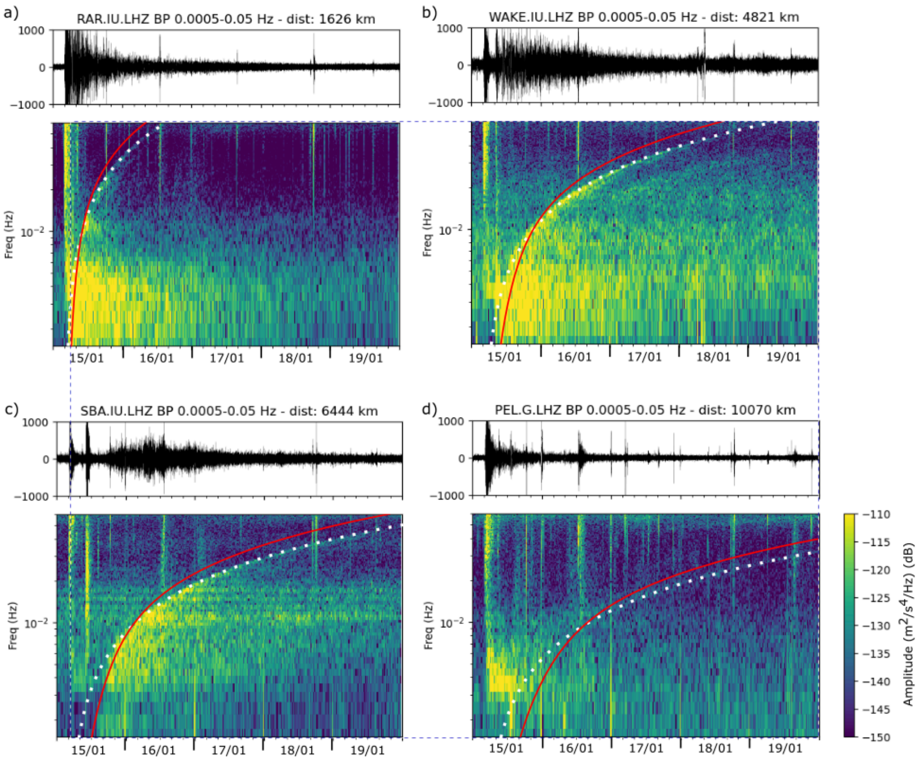
Is there anything interesting from this previous work that can benefit the current study? Maybe. First of all, I think that equation 1 in the text is probably not quite right. Following previous work, I think it's more relevant to use the group velocity rather than the phase velocity. This might explain the systematic shift between the modeled and predicted values. (Model seems to over estimate frequency in Figure 1 and Figure 2).

My modeled dispersion curves uses indeed the group velocity, defining $k = g/v_p$, $v_p = 2*v_g$ and $v_g = d/t$. I have now stated this clearly in the text (lines 130-140).

This model matches nicely the observation. As commented in the Figure caption, the dispersion curve were slightly shifted upwards to do not obscure the signal. However, as this can be misunderstanding, I've now reworked Figures 1, 2 and 6 eliminating the shift and use a different dotted line to keep the spectrogram visible

One minor comment is that the author indicates that they believe some model-data mismatch can be attributed to the deep water hypothesis. If that's true, then I see no reason to limit the analysis to the deep water hypothesis given that the general case is only minimally more complicated; it's omega^2 = g k tanh(k H).

In fact the general case provide dispersion curves that fit worse the data, as shown in the figure below, where the curves calculated using the general case formula are shown in red.



I don't have an explanation justifying why the deep water hypothesis provides better adjustments; as I think this is not essential for the objective of my contribution, I would prefer do not enter in details in the manuscript

All the best,

Brad Lipovsky

Recommendation: Revisions Required