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Interactive comment on "In situ observations of infragravity wave directionality at nearshore coastal sites" *by* Takehiko Nose et al.

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We thank Referee #2 for the review and comments.

Since we have been notified by the Topic Editor both referees do not want to review the revision, we only provide responses to major comments of RC2.

On the point of missing references 1). All references mentioned in RC2 are interesting and some should be included, but they are all deep ocean measurements inferring information of the IG source or characteristics. Our paper actually concerns direct measurements of the source region, as the title itself suggests, and is unaffected by deep ocean IG waves (unless local IG waves are benign). 2). RC2 mentioned papers are all array measurements, only one of our data sets is an array, the rest are point

measurements. The main findings of IG directionality are derived from a point measurement. 3). On the point of references, it is briefly mentioned that all so-called major references in RC1 come from a single paragraph of Aucan and Ardhuin (2013).

On comment 2 – 1). Pearson's correlation is mentioned first time the values is reported, but we agree this should be better clarified. 2). We analysed a correlation between wave height ratios and relative depths that include a wavenumber to consider the wave period. 3). We actually discuss why wavenumbers of mean periods have better correlation than Tp and show this is due to Tp being noisy in the observations (noisiness of Tp is also mentioned in Ardhuin et al. (2014)). We can easily analyse Tm0,-2, Tm0,-1 or other mean periods. 4). Of course Ardhuin et al. (2014) parameterisation is highly regarded and appropriate for many studies. It does however require a coefficient that is site specific and has dimensions. Non-dimensional parameterisation that is not site specific may indicate closer connection to the physical mechanism, and provided the selection of parameters is appropriate, it does not require retuning. Given IG waves are an extremely complex process, we may be a bit lofty but our approach could potentially be the first step.

Comment 3 - The referee says we should use IMLE, an iterative form of MLM, which we did with 10 iterations.

Comment 4 - We can add Kuik et al. (1988) as a reference of conventional spreading as well but the reason the A spreading is used is already discussed. This can also be easily elaborated.

Lastly, Herbers' papers are very comprehensive, but after 20 years, is there no progress? And why are IG wave measurements in the field so scarce and IG models remain far inferior to wind wave counterparts?

Comments RC1 and RC2 are greatly appreciated and will improve the paper, but we do raise a point that these major comments do not seem to critique the essence of the paper.

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