# Response to RC1

Dear Anonymous Referee #1,

Thank you for reviewing this manuscript. Regarding the general comments, it is true that infragravity (IG) waves are increasingly gaining attention in deep ocean research. Notwithstanding, IG waves are formed and most dynamic in the coastal region, so we believe field data analysis of this area is of interest to the oceanographic community. Furthermore, IG waves in the nearshore has been suggested to play an important role in sediment transport, e.g., formation of longshore bar and beach cusps (Baldock and Huntley, 2002), and are of interest to the industry for loading and offloading of carriers, port development, etc. The below text details our response to the specific comments, and attempts to demonstrate that statements in the manuscript are not inaccurate and unsupported, and outcomes of the analyses are relevant.

1. We have removed the sentence in the revised version.

2. We understand and acknowledge there are a number of studies regarding the liberation of bound IG waves, e.g., Henderson and Bowen (2002) and Janssen et al. (2003), and the process is reasonably understood through theoretical and experimental studies. Therefore, we should have tempered the language, and this has been revised. However, we would like to show that the physics of this process is still a very complicated issue.

   A thorough and comprehensive review of IG wave generation is provided in Baldock (2012), which is referenced in the original manuscript. While noting that the progressive release of bound waves from wind-wave group has been clearly observed in some experiments, e.g., Janssen et al. (2003), he debates evidence of the widely adopted assumption that bound IG waves are released during wind-wave breaking where reference is often made to Longuet-Higgins and Stewart (1962). Through a review of existing experiments and papers, he describes the implications of IG wave dissipation to the release of bound waves and also reveals that Longuet-Higgins and Stewart (1962) themselves state that bound waves could be reflected before wind-wave breaking, which implies that the bound wave release is not related to the onset of wind-wave breaking. He confirms that there are two different modes of free wave generation, and the dominant mode of generation is understood to be dependent on relative beach slope and wave steepness. We agree that horizontal resolution to simulate IG wave generation is a problem for oceanic scale spectral models (like WAVEWATCH III®) but not so for phase-resolving models. Currently, we still lack an established numerical IG wave model that can accurately simulate IG wave processes in any wave and geographical conditions, although, we note that the first application of the sophisticated SWASH model to field scales with irregular bathymetry is reported in Rijnsdorp et al. (2015). Furthermore, estimates of bound waves become unreliable for large waves as the assumption of a slowly varying wave group is no longer valid (Reniers et al., 2002, 2010). Even sophisticated models such as Herbers and Burton (1997) and Henderson et al. (2006) do not explain large wave events with strong nonlinearity.

   Regarding why IG waves are important; Webb et al. (1991) did find that only a small fraction of IG energy leaks into the deep ocean, but Aucan and Ardhuin (2013) state that little is known about typical energy levels and variability of free IG waves in the open ocean. They further point out the IG wave relevance to the open ocean citing references therein, and their investigation led to the development of an IG module in WAVEWATCH III® (Ardhuin et al., 2014). Since IG waves are generated and most dynamic in the nearshore, we believe IG wave field observations in this region is important. We have included the above literature review in more detail, clearly stated the IG wave relevance, and provide with clarity the original and new methods, analysis, and results in the revision. The revision has also been amended to clearly state that there are two analysis components: the frequency analysis and directional analysis components.

3. The manuscript structure has been revised and include a section to provide clear details regarding the selection of data sets and description. The Martha's Vineyard and Baja data are included in the frequency spectrum analysis, but the results were not strong in the directional analysis. This has been clarified in the revision.

4. The manuscript structure has been amended to make these points clear for readers. Regarding separating the free and forced IG waves, we refer to Point 1 that the estimates become unreliable for extreme waves, and hence, we focus on total wave energy. We acknowledge that wavenumber spectra could be used to detect edge waves, and that may provide insights into the complex directionality; this has been explored in the revision.

5. The revised paper has a separate modelling section. The modelling was undertaken to validate the observations as there is no previous work to show that IG wave directions can be derived from the instruments used in this study. The same disparity between observations and the model raises an interesting point whether the current understanding and model formulation are accurately reproducing reflected wave magnitudes. This point has been clarified and discussed in the revision.

Lastly on the technical comments, the revised manuscript attempts to take care of the superfluous use of English and missing words, which will be submitted in due course. Thank you kindly for your review.

Kind regards,

Takehiko Nose on behalf of all co-authors

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