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Ocean Science Discussions

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

Anonymous Referee #1

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General Comments This paper describes the analysis of some nearshore observations of infragravity (IG) waves taken at 4 widely separated locations: southern California, at Martha's Vineyard and the Field Research Facility (FRF) at Duck, on the east coast of USA, and at Lagos in Nigeria. Emphasis is placed on the statement that 'Infragravity wave directional properties outside of the surf zone are seldom studied'. The authors refer to conventional and 'new' directional analysis methods. The objective of the analysis was to obtain improved insight into the IG wave directional properties. This has been done by a correlation analysis of IG wave height and wind wave height, against wave steeness, relative depth and spectral width. It was found qualitatively that when infragravity wave energy increased, their directional distributions become predominantly bimodal, having peaks in both incident (shore-normal) and reflected di-

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rections.

Finally the IDSB model of Reniers et al. (2002) was applied, showing some skill in reproducing the observations.

The topic of IG waves is interesting and receiving more attention recently since free IG waves in the deep ocean are thought likely to need taking into account for interpretation of altimeter data e.g. Aucan and Ardhuin (2013). The present paper, however, is rather superficial and poorly written, and needs major revision. It omits many significant references, makes many inaccurate and unsupported statements and the analysis is not very strong, while it also lacks convincing evidence of new results.

Specific Comments 1. Remove the first sentence from the Abstract, it is not helpful or accurate and the definition of IG waves is discussed in the Introduction, it is not part of the original work in this paper. Note that infragravity waves could refer to all gravity waves with longer periods than about 30s, which would include tides, tsunamis, Rossby waves, but is usually taken to refer to those gravity waves between 30-300s period, linked to wind-waves. 2. Section 1: The Introduction needs a through revision. A cursory search of the literature on IG waves reveals quite a number of papers not referred to here. There are some inaccuracies and inconsistencies. For example, contrary to the authors' statement that the physics of free wave generation is not comprehensively understood, Aucan and Ardhuin (2013) state that the liberation of bound IG into free IG waves at the shoreline is now relatively well understood. They refer further to Henderson and Bowen (2002) but they acknowledge it remains a difficult modelling problem (mainly due to difficulty of resolving nearshore length scales). Given their long wavelength, most of the outbound free IG energy is trapped by refraction as edge waves on the shelf, propagating alongshore, and only a small fraction of the IG energy leaks into the open ocean as free waves (Webb et al., 1991). Please carry out a more careful review of the literature, then state the motivation for the present work. What are IG waves important? State what methods are used and what is the aim of the paper. Clarify what is original in the analysis and results. 3. Section 2: Why were

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the specific datasets chosen? Most of the analysis (as acknowledged) focusses on the FRF data. While the Lagos data were referred to, the Martha's Vineyard and Baja data are not presented. What dates were selected (not shown in Table 1)? 4. Section 3 is about frequency analysis and section 4 about directional analysis: There is a lot of detail about fairly standard wave spectral analysis but no clarification as to what is the 'new' analysis method. The methods are not clear and the results are presented under the same section. It would be better to separate the sections. Why is no attempt made to separate free and forced IG waves (these are clearly separated in wavenumber space)? Is there evidence of edge waves other than the statements about complex multi-modal seas? 5. The numerical modelling is wrapped into section 4 where it might be better to have a separate section. The results of the model show the same disparity between model and observations as already identified in Reniers et al. (2010). Technical corrections Please take care with the English, in many places there are superfluous or missing words and unnecessary repetition.

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