Ocean Science
Discussions

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

Anonymous Referee #2

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General comments: This paper claims to provide some new analysis of directional IG waves and reports on some preliminary numerical modelling results. In general the paper lacks focus, misses important relevant references and is very fuzzy it is presentations so that it is impossible to understand the figures and the data that is analyzed, and the general feeling is that the paper contains nothing new compared to Hebers et al. (1994, 1995). Based on the following 3 major concerns I suggest that the paper is rejected and the authors take the time to sort out their ideas and clarify them rather than wasting the time of reviewers.

1) Missing references: there are quite a few recent IG array analysis performed by groups in the UK and the US: Harmon et al. (GRL 2012), Neale et al. (JGR 2015), Godin et al. (JGR 2014) that looked at the directional distribution of IG wave energy,

and Rawat et al. (2014) performed the first trans-oceaning tracking of IG wave events.

2) It is very frustrating that the there are no details on the regression analysis mentioned several times by the authors. What parameters are used? What is the correlation coefficient? If it is Pearson's r, then r=0.93 ... is typically less than reported by Ardhuin et al. (2014). In general page 8 is particularly disappointing. The authors discuss correlation with mean periods $Tm02$... but why use $Tm02$ when it is well known that it is the peak which matters? Why not use $Tm0,-1$ or even the more exotic $Tm0,-2$ of Ardhuin et al. (2014)? That previous paper provided already a parametric relationship for both the total IG energy and its frequency distribution ... why not use that relation? Is the one proposed here any better?

3. The directional analysis result is not clear al all. For example, on figure 10: which method is used? IMLE usally performs well on large arrays, why not use that one for the FRF (and by the way the directional spectra are already computed by C. Long or K. Hathaway at the FRF...

4. Why chose a non-conventional "A" spreading parameter that cannot be easily measured direcly, unlike Kuik et al. (JPO 1988) sigma which is simply given from $a1$ and $b1$? this "A" is fairly sensitive to reflections...

Other minor details:

Page 2 line 27: "more skewed toward unity" -> "closer to unity"

Page 3: other datasets could be cited : e.g. Harmon et al. (GRL 2012)

Page 8, line 13: why use $Tm02$ instead of $Tm0,-1$ or other ??

Line 19: "we aimed to develop a simple parametric relationship": which one?

Page 10, line 30: Is it the MEM of Lygre and Korgstad (1986) or another estimator?

Page 11: Eq. 14 is not necessary ... and variables $a1$, $b1$ are not defined.

Page 19: line 22: "sufficiently resolved" by what?