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Interactive comment on "Note on the directional properties of meter-scale gravity waves" *by* Charles Peureux et al.

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The authors would like to thank the reviewers for their comments, which helped improving this manuscript. The questions asked by the reviewers are rewritten in bold and answers follow.

1) The manuscript seems to conclude that bound waves do not play a role on high wavenumber bimodality and this is consistent with a numerical investigation in Toffoli, A., M. Onorato, E. M. Bitner-Gregersen, and J. Monbaliu (2010), Development of a bimodal structure in ocean wave spectra, J. Geophys. Res., 115, C03006, doi:10.1029/2009JC005495, where they showed that free wave nonlinearity is causing the bimodal lobes to form. However, the Authors mention in

the abstract (line 2,page 1) that "distribution can be obscured by the presence of bound waves". Just looking at figure 4, this statement does not seem to be very relevant. What do the Authors actually mean with "obscured by the presence of bound waves"?

The role attributed to bound waves in the manuscript has been highlighted by both reviewers. This paragraph takes both remarks into account. The extraction of bound waves is one of the interesting features allowed by the stereo-video technique. Without anticipating any role played by bound waves in the origination of bimodality, the sentence "distribution can be obscured by the presence of bound waves", is probably inaccurate. It is indeed true from figure 4 that bound waves do not "obscur" bimodal directional distributions, i.e. make bimodal directional distributions look unimodal. However, looking at the same figure, the parameters of bimodality presented on figure 5 are strongly influenced by the presence of bound waves, particularly the lobe ratios, equation (20). In addition, the bound waves depend on the full spectrum of free waves (Hasselmann, 1962; Janssen, 2009). Removing these bound waves allows to get rid of the potential variability of the spectrum of free waves, especially the long waves part, which can be quite different from the short waves part. For these reasons, the extraction of bound waves is important for quantifying the lobe ratio and other parameters that define the spectral shape. We have thus changed the abstract, with the new sentence

"The later indeed tend to reduce the contrast between the two peaks and the background".

2) In general, the Introduction is a bit weak. There is an extensive literature on the high frequency bimodality of the wave spectrum, which could be discussed in mored details. In addition to field observations, there are a number of numerical investigations that shows and tries to explain the formation of this high frequency bimodality. Besides the cited Banner and Young (1994) and Gagnaire-Renou et al (2010), the Authors should refer to Alves, J. H. G. M., and M. L. Banner (2003), Performance of a saturation-based dissipation-rate source term in model-

ing the fetch-limited evolution of wind waves, J. Phys. Oceanogr., 33, 1274–1298; Dysthe, K. B., K. Trulsen, H. Krogstad, and H. Socquet-Juglard (2003), Evolution of a narrow-band spectrum of random surface gravity waves, J. Fluid Mech., 478, 1–10; and Toffoli, A., M. Onorato, E. M. Bitner-Gregersen, and J. Monbaliu (2010), Development of a bimodal structure in ocean wave spectra, J. Geophys. Res., 115, C03006, doi:10.1029/2009JC005495, among others.

References to the aforementioned articles have been added to the introduction :

'"Bimodality is also found after having solved for the nonlinear evolution equation of the surface elevation field, whether computing it for gaussian wave packets according to a Nonlinear Schrödinger equation (Dysthe et al., 2013) or for unimodal wave spectra from the Euler equations (Toffoli et al., 2010)"

3) Reference to Munk (2009) is not well discussed, I think, I do not quite understand what the "challenge" mentioned at line 17 of page 1 is.

The challenge here presented has to do with the reflectance measurements presented in Bréon and Henriot (2006). These measurements are integrated over wave scales and present puzzling simple relationships between wind speed and cross-wind or down-wind slope variance. However, this data gives no information on the underlying distribution among frequencies and wave numbers. The challenge here was to obtain frequency/wave number resolved data, to help understand the integrated relationship.

4) At line 4, page 2, there is reference to the "directional distribution of backscatter". What does the backscatter refer to?

The backscatter refers to the radar backscatter as a function of azimuth. This has been corrected in the introduction :

"The distribution of radar backscatter as a function of azimuth clearly shows that the directional wave spectrum is unimodal above $6\ \mathrm{cm}$ *wavelength in the gravity-capillary range (see the review in Elfouhaily et al., 1997)"*

5) It seems that the main contribution of the manuscript is an extension of the work in Lecker et al (2005), but no specific details about this referred study are provided. Lecker et al (2005) should be discussed in more details to better highlight the novelty of the present manuscript.

The discussion on Leckler et al. (2015) has been expanded in the introduction :

"As shown by Leckler et al. (2015), stereo-video imagery is capable of resolving these waves and provide information on the time and space scales needed to interpret integrated wave parameters such as surface slope. In this paper, a record from a young wind waves field taken from a platform in Crimea have been analyzed. In particular, the presence of harmonics, the shift induced by the current on the short surface waves dispersion relation and the wave field bimodality were part of the conclusions. Here we focus on the short waves field bimodality and extend their analysis to the whole range of frequencies. The characteristics of bimodality are here quantified and the consequences on physical variables detailed."

6) It is mentioned that a Fourier analysis is conducted over a physical domain of dimension 25.6 X 25.6 mËĘ2. This seems quite small to me, considering the the dominant wavelength is of about 45m. It means that physical domain does not contain one full wave form, creating uncertainties in the Fourier analysis. It is indeed a well known problem that the frequency domain is not well resolved if a whole number of periods is not present in the physical space. The resulting wave spectrum is therefore questionable. The Authors should make sure that their domain contains at least one full dominant wave period for the Fourier analysis.

The limitation of having a small analysis window compared to the dominant wave spatial scales reflects on our inability to resolve properly wave numbers around the spectral peak (this fact is quite clear in Figure 3, with slices taken at constant frequencies). This problem does not present for wave periods, since the record duration is longer enough to contain hundreds wave periods. However, smaller scales are well resolved. Moreover, we have reduced the influence of aliasing errors accounting for a limited portion of the 3-D spectrum (Eqs. 11 trough 15).

This is a well-know issue in array processing for ocean waves (e.g. Kinsman, 1965; Donelan et al., 1985) or seismic waves. For wave components with wavelengths larger than the array size, the usual technique is that of a "slope array" (e.g. Graber et al., 2000) that gives a robust estimate of mean direction and spread (and at least first 5 moments of the directional distribution as given by a buoy). In Leckler et al. (2015) this array processing was combined with the direct FFT to give a full spectrum from the peak to the short waves because they computed the second order spectrum from the dominant waves. Here we focus on the shorter components.

We have thus added : *"Wavelengths longer than 25 m can be resolved using standard slope array techniques (e.g. Graber et al., 2000) as done by Leckler et al. (2015) and Benetazzo (2006). These longer components are not the focus of the present paper. "*

7) At line 5, page 6, the difference interaction should be k = k1 - k2, right?

The reviewer is correct, but (k1+k2,omega1-omega2) is the same as (k1-k2, omega1+omega2). We have modified the text to make it more intuitive:

"The difference interaction gives $\vec{k} = \vec{k}_1 - \vec{k}_2$ *and* $\omega = \left|\omega(\vec{k}_1) + \omega(\vec{k}_2)\right|$ *,* E_{diff}*."*

Moreover, the interaction kernel from Sharma and Dean (1979) uses the minus sign between the phases.

8) Not sure I understand the meaning of "background spectrum" at line 10 of page 10.

This sentence is probably misleading. The background spectrum is not another kind of spectrum. This name is probably inappropriate. The directional distribution at a given wave scale does not always fall down to zero. In fact, there is always a bit of energy remaining, especially towards smaller scales. The origin of this energy may be either

an actual surface waves signal or noise. This would require further investigations. This background is similar to the term α introduced by Tyler et al. (1974) in their fitting function to account for a non-zero energy level for waves propagating in opposite directions. This has been corrected in the manuscript :

"We can note that they are particularly sensitive to the background energy level. This level is given by the constant term C^{st} *of the fitting function (17), without knowning whether this level is actual surface waves signal or noise."*

9) I don't think I understand the reasoning at the beginning of page 12 (around line 5). Also, what "their" refer to in " . . . the short wave bimodality substantially reduces their contribution to Stokes drift"?

Indeed "their" refers to the contribution of short waves. This has been corrected in the manuscript :

" In particular, the short waves bimodality substantially reduces the contribution of those waves to the Stokes drift."

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