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Discussions

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

Anonymous Referee #1

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The manuscript describes an interesting analysis showing spectral bimodality at high wavenumbers (frequencies). This topic has been studies extensively, both numerically and with the aid of field observations, over the past years. An interesting and original conclusion of the present manuscript is the effect that current exerts on the asymmetry of the lobes. In my opinion, this manuscript is suitable for the Journal of Ocean Science. However, there are a number of comments and concerns that should be addressed by the Authors.

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"?

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 modeling 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.

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.

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

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.

6) It is mentioned that a Fourier analysis is conducted over a physical domain of di-

mension 25.6 X 25.6 mˆ2. 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.

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

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

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”?

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