

Interactive
Comment

Interactive comment on “Influence of natural surfactants on short wind waves in the coastal Peruvian waters” by D. Kieffer et al.

Anonymous Referee #2

Received and published: 18 September 2015

General comments

Kieffer et al. present measurements of wave slope statistics obtained during a cruise in the upwelling regions close to the Peruvian Coast. The approach used is, in essence, a modern take on the approach used by Cox and Munk (1954) - the major differences are that an LED array is used as a light source instead of the sun and that the measurements are made from a ship rather than an aircraft adding the complication that motion correction must be made in order to interpret the data correctly. That the light source used in this new iteration of the technique is a finite distance from the water surface means that the authors are able to better examine spatial variability than Cox and Munk (1954) - their measurements were made from an aircraft using sun

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

[Interactive
Comment](#)

glitter, thus they will have been averaging over much larger ocean surfaces. This is an important distinction if the effect of “slicks” that result from enrichment of organic matter at the water surface are to be assessed for their effect on air-sea interaction.

In general I think the authors do a reasonable job of introducing the topic - measurements of wave slope are certainly useful to the air-sea interaction community. Given that the approach used can be seen as an iteration of the Cox and Munk (1954) work I think that the detail given in the methods is OK. However, the authors should bear in mind that all details required to replicate the approach should be included - e.g. how do the authors define a “speckle” and locate it in their image? I am not an expert in image analysis, rather more a potential end-user of the instrument so all technical steps should be clearly signposted for readers who may wish to use the approach. I think going back to the original paper by Cox and Munk (1954) to see how they present their approach would be useful for the authors in this regard.

Where my major concern lies is the authors interpretation of the results. They use the term “natural surfactants” in their title implying that they have directly measured the influence of these on wave slopes. However, they have not - there are no coincident measurements of surfactants at the water surface presented in the manuscript. What the authors in fact do is plot the wave slope as a function of water temperature with the reasoning that this can be used as a proxy for upwelling in the region and therefore increased productivity. This is a very tenuous assumption - the link between upwelling and primary productivity is not straightforward, nor is the link between primary productivity and the enrichment of organic matter at water surfaces.

If the authors remove the unwarranted references to “natural surfactants” from their manuscript and use a title that reflects the manuscripts contents accurately; improve upon the technical description of the device; and include some more detail on the laboratory measurements used to calibrate the instrument then they are well justified to use this cruise data as a case study in the same manuscript. Figure 5 in the manuscript comparing their data to those of Cox and Munk (1954) is certainly of interest. As for

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

where to submit this technical note, I would suggest this very journal - members of the air-sea interaction community may not find it if goes to a more technical journal.

To summarise, unfortunately, I cannot recommend publication of the manuscript in its current form. However, given the improvements outlined above I see no reason why it cannot form the basis of a technical note in Ocean Sciences.

Specific comments

- P1293 Line 4 - I would add the parameterisation of Nightingale et al. (2000) here.
- P1293 Line 5 - It is unjustified to state “any recent deta collection shows significant deviations of the measured transfer velocities from a simple $k(U_{10})$ -relationship” since this is not the view of the field in its entirety. Better to say “*some* recent. . .”
- P1293 Line 7 - Again the authors use language that suggests there is no doubt when that is not the case. “This variability is obviously caused. . .” should read “This variability *might* be caused. . .”
- P1301 Line 5 - The authors mention that the error in the wave slope measurements in the lab was less than 4%. I would like to see a discussion on whether this error has increased by going from the lab to the field e.g. due to motion correction.
- P1302 Line 22 - The authors mention that they encountered rather mature seas but no data on this is presented in the manuscript. Significant wave heights etc. are important background information that should be included in the case study.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Technical corrections

- P1294 Line 2 - should read “systems are capable *of delivering*”.
- P1294 Line 26 - should read “This allows direct comparison with local measuring techniques. . .”.
- P1302 Line 20 - would read better as: “...less than 4% of the mean *and* solid circles represent measurements *where* variability was greater.”
- Figure 5 - the dashed line is missing from the legend.

References

Nightingale, P.D., Malin, G., Law, C.S., Watson, A.J., Liss, P.S., Liddicoat, M.I., Boutin, J. and Upstill-Goddard, R.C. (2000). In situ evaluation of air-sea gas exchange parameterizations using novel conservative and volatile tracers. *Global Biogeochemical Cycles* 14: doi: 10.1029/1999GB900091.

Interactive comment on *Ocean Sci. Discuss.*, 12, 1291, 2015.

OSD

12, C719–C722, 2015

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

