

Interactive comment on “Indian Ocean Dipole modulated wave climate of eastern Arabian Sea” by T. R. Anoop et al.

Anonymous Referee #2

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This submission is concerned with the relation of wave statistics (“climate”; off the west coast of India) to the fields of sea temperature and winds, especially as related to the Indian Ocean Dipole IOD (intensity). A conclusion is that “The signature of IOD on the wave climate mainly depends on the modification of wind field induced by the phase of IOD events.” To some extent this is obvious as waves are generated by winds. The question arises of how much beyond this is found in this manuscript. There are findings about which wind fields are effective for waves in some areas. However, the relation to IOD as distinct from the wind field is not so clear and there is much discussion of the temperature as an indicator of the IOD without much clarity about why it should be considered in addition to the wind field (except that it provides the index DMI for the IOD). I wonder whether the attempt to relate the waves to the IOD, as distinct from the

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wind directly, is actually helpful. At least, it should be better motivated.

The study is carried out mainly by modelling with validation using deep-water wave data for a month (only). There are years of (near-) coastal data at Ratnagiri and Honnavar which are used to find wave statistics to compare with DMI – why not also to validate the model with the same sort of statistical comparison as for the deep-water waves?

There is a curious focus on October (only). Why? Apparently wind anomalies are greatest then, but (start of section 3) “The months of October and November are the calm period for the AS” so one might think that other months would be more interesting for analysing influences on waves.

More detailed points.

Introduction. This could better start with some motivation for the study. If it is to understand the wave “climate” better then the motivation should be about the importance of the wave climate. The waves are driven by the winds. However the description of IOD curiously (over-) emphasises the temperature field even though it is said (page 2474 lines 17-18) “The monsoon wind patterns in the north IO affects the spatial distribution of sea surface temperature (SST) in tropical IO”. There is some overlap between page 2474 lines 15-18 and page 2475 lines 23-26. I think the main seasonal cycle should all be described in one place, probably before discussion about the changes associated with the IOD.

Page 2475 line 2. Please define what “positive” IOD means. If this means positive DMI then DMI should be defined first.

Page 2477 line 17. Please clarify “the coupled system” (WW3 with nested SWAN) sooner than page 2478 line 7.

Page 2478 lines 22-26. The WW3 comparison with observations is only for October 2009. Why not for the whole 3 months of observations? Is the comparison in November and December about the same as in October?

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Page 2479 line 26. Better “northerly direction and going southwards again change their direction to north-westerly before . .”

Page 2481 line 22. “greater” than what? A comparison is implied. Likewise “higher” than what. I guess in both cases the comparison is with a negative value of zonal wind component.

Page 2482 line 3. “Influences of IOD on the temperature variability of the north IO are shown in Fig. 6.” What is the relevance of this? If relevant, should it come earlier in the text?

Page 2483 Line 23. “The decrease of wave height” – what decrease, i.e. in what conditions? Line 26. As before, why only October? What about other months?

Interactive comment on Ocean Sci. Discuss., 12, 2473, 2015.