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Discussions

Interactive comment on "Wave spectral shapes in the coastal waters based on measured data off Karwar, west coast of India" *by* M. Anjali Nair and V. Sanil Kumar

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Generally, this work presents an impressive data set spanning 5 years. The data appear largely to be a new contribution to the literature, and if so, should qualify the work for publication as a case study. Having recognized that, there are several concerns with potential to change this assessment.

1) It appears that a significant portion of this data may have been published elsewhere. For example, here: Glejin, J., Sanil Kumar, V., Amrutha, M.M. and Singh J., Characteristics of long-period swells measured in the in the near shore regions of eastern Arabian Sea, Int. J. Naval Architecture and Ocean Engineering, 8, 312-319, 2016. Reply: From the wave data collected for two years period (2011 and 2012) at the

study location, swells of period more than 18 s and significant wave height less than 1 m were separated and used to study the characteristics of low-amplitude long-period swells by Glejin et al. (2016). Glejin et al. (2016) presented the wave characteristics of low-amplitude long-period swells which occur for 1.4 to 3.6% of the time in a year. Statistics presented in Glejin et al. (2016) is different than that presented in this paper. Study on wave spectral shape and the interannual variations over a period of 5 years for this location has not been attempted so far. This information is now added in the revised paper. 2) Why is the broader-context not communicated? It seems peculiar that several existent publications by the authors use verbatim text describing the area, methods and motivation, as well as the same analysis techniques and products, but those works are referenced only narrowly in regard to speciïňĄc details. For example, data collected over nearly the same period of record (March 23, 2010 to November 6, 2014) only a few kilometers up the coast, and with essentially the same analysis is reported here: Anjali Nair, M. & Kumar, V.S., Spectral wave climatology off Ratnagiri, northeast Arabian Sea, Nat Hazards (2016) 82: 1565. doi:10.1007/s11069-016-2257-5

These publications share signiïňĄcant portions of text and techniques: Glejin, J., Sanil Kumar, V., Sajiv, P.C., Singh, J., Pednekar, P., Ashok Kumar, K., Dora, G.U., and Gowthaman, R., Variations in swells along eastern Arabian Sea during the summer monsoon, Open J. Mar. Sci., 2 (2), 43–50, 2012.

Glejin, J., Sanil Kumar, V., Amrutha, M.M. and Singh J., Characteristics of long-period swells measured in the in the near shore regions of eastern Arabian Sea, Int. J. Naval Architecture and Ocean Engineering, 8, 312-319, 2016.

Indeed, there appears to be such a plethora of work in the area that a recent compendium was published:

P. Vethamony, R. Rashmi, S.V. Samiksha, V. M. Aboobacker M, Recent Studies on Wind Seas and Swells in the Indian Ocean: A Review, The International Journal of

Ocean and Climate Systems, Vol 4, Issue 1, pp. 63 - 73. Informing the reader of the broader context in which these measurements exist, and to clarify novel differences between published work and data presented in this paper is warranted.

Reply: Earlier the above publications were referred only at appropriate places in the manuscript. Now as per suggestion of the reviewer, we have added a paragraph in the introduction to provide a broader picture of the studies carried out in the eastern Arabian Sea and to clarify the differences between published work and the data presented in this paper.

3) The expressed objective of the work is: "This study addresses two main questions: (1) How the high-frequency tail of the wave spectrum varies in different months? (2) What are the spectral parameters for the best-ïňĄt theoretical spectra?"

The ïňĄrst objective is questionable since the method to assess slopes of the spectral tails is not revealed. The second objective is questionable since an ad hoc method consisting of a piecewise concatenation of different theoretical spectral functions is used. SpeciïňĄcs of how the concatenated frequency bands are determined is not provided, nor is an assessment of the physical assumptions inherent in concatenation of these different spectra.

The authors have attempted to clarify the data with statistical analysis, but the speciïňĄc methods and algorithms are not provided, a prime deïňĄciency. If there is desire to improve understanding and interpretation of the data, some effort to connect the data/statistics to underlying physical processes could improve the publication value. For example, you have argued that "Understanding of the wave spectral shapes is of primary importance for the design of marine facilities" yet little effort is made to quantify or relate physical parameters/forcings pertaining to the change in spectral slopes/peaks. Reply: We used the statistical curve fitting techniques to assess slopes of the spectral tail. Now we have added the below details.

An exponential curve y = k.fb is fitted for high frequency part of the spectrum and

the exponent (value of b) is estimated for the best fitting curve based on statistical measures such as least square error and bias. The slope of the high-frequency part of the wave spectrum is represented by the exponent of the high-frequency tail.

SpeciïňĄcs of how the concatenated frequency bands are determined is now added. Now we have deleted the representation of double-peaked wave spectrum with the theoretical spectrum.

For the present study JONSWAP spectrum is tested by fitting for the whole frequency range of the measured wave spectrum. It is found out that the JONSWAP spectra do not show good fit for higher frequency range, whereas Donelan spectrum shows better fit for high-frequency range. Hence, JONSWAP spectrum is used for lower frequency range up to spectral peak and Donelan spectrum is used for the higher frequency range from the spectral peak for single-peaked wave spectrum. Theoretical wave spectra is not fitted to the double-peaked wave spectra.

Now we have also added the plots showing the variation of high frequency tail with significant wave height, mean wave period, wind speed and inverse wave age.

4) Please address the lack of consistency in precision of your statistical estimates and the lack of standard error or conïňĄdence limits. For example, wave frequencies are variously reported with precisions of 1, 2 or 3 decimal places, even in the same sentence. Slopes of the wave spectra are reported with 1 or 2 decimal point precision. Kindly adopt a uniform usage for expressing the precision of your estimates, ideally ones that reïňĆect physically pertinent cutoffs. Please add error estimates or conïňĄdence bounds to your estimates, or address quantitatively why they are not germane.

Reply: Consistency in precision is maintained now in the text. The wave spectrum is with a resolution of 0.005 Hz from 0.025 Hz to 0.1 Hz and is 0.01 Hz from 0.1 to 0.58 Hz. It is now added. 5) You quote estimates of the slope of high-frequency portions of the wave spectra, but never define how those estimates are made. Reply: Now it is explained as mentioned under 3.

6) You plot and describe "normalized" spectral densities, but never detail how this normalization is applied. One is then left to question whether the normalization is speciïňĄc to each time period, or normalized across all periods so that direct comparisons of different years is meaningful.

Reply: Now it is added as below. Normalisation of the wave spectrum is done to know the spread of energy in different frequencies. Since the range of maximum spectral energy density in a year is large ($\sim$ 60 m2/Hz), each wave spectrum is normalised through dividing the spectral energy density by the maximum spectral energy density of that spectrum to understand the distribution of energy in different frequencies, specifically in the wind-sea and swell regions.

7) Line92 "The average monthly sea level at Karwar varies from 1.06 m (in September) to 1.3 m (in January)" You are apparently quoting geodetic elevations here, but there is no reference datum speciïňĄed.

Reply: They are with respect to chart datum and is now added.

8) Line 100 "The data for every 30 minutes from the continuous records at 1.28 Hz are processed as one record. From the time series data, the wave spectrum is obtained through fast Fourier transform (FFT)."

My understanding of waverider spectral processing is that motion samples are recorded at a sample rate of fs = 2.56 Hz, not 1.28 Hz. The bandwidth of the spectral estimate is fs/2 = 1.28 Hz. I do not believe that the data over 30 minutes are processed as one record. In standard Datawell processing, records of length 200 seconds are collected (N = 512 samples). 17 records are then FFT'd, windowed, and averaged with a 50% overlap to produce the 30 minute power spectral density estimate. Kindly verify your description, and if it differs from the standard Datawell spectral processing, detail and justify the differences.

Reply: We used DWR-MKIII and the sampling interval is 3.84 Hz for this system. A

digital high-pass filter with a cut off at 30 s is applied to the 3.84 Hz samples. At the same time it converts the sampling rate to 1.28 Hz and stores the time series data at 1.28 Hz (Datawell, 2009). From the time series data for 200s, the wave spectrum is obtained through fast Fourier transform (FFT). During half an hour 8 wave spectra of a 200 s data interval each are collected and averaged to get a representative wave spectrum for half an hour (Datawell, 2009). These are now corrected in the paper. 2.56 Hz is correct for WR-SG. Now we have mentioned the type of buoy also to avoid confusion to the reader.

9) Line 185 - 189 Here you describe dispersion relation impacts characterizing the wave spectrum of your data, you might consider moving this information to the beginning of the section.

Reply: Moved to the beginning of the section

10) Line 268 "Contour plots of spectral energy density (normalized) clearly show the predominance of wind-seas and swells during the non-monsoon period (Fig. 9)."

Here you have apparently applied the unspecified 'normalization' to month long temporal windows of spectral density averaged over years. Is the normalization value specific to each month, or is it over the entire ensemble so that meaningful intercomparisons can be made? The normalization, as well as the averaging must be defined.

Reply: In Figure 9, each wave spectrum at 30 minutes interval is normalised through dividing the spectral energy density at each frequency by the maximum spectral energy density of that spectrum. Each normalised wave spectrum will have a maximum spectral energy density of 1. This is now explained in the paper.

Since the frequency bins over which the wave spectrum estimated is same in all years, the monthly and seasonally averaged wave spectrum is computed by taking the average of the spectral energy density at the respective frequencies of each spectrum over the specified time. Here normalisation is not done.

11) Line 281 "To study the characteristics of different wave systems, average mean wave direction and average wave spectral energy density grouped under different peak frequency bins are plotted in Fig. 10." The meaning of "wave systems" is not clear. The meaning of "average mean" is not clear.

Reply: Here the wave systems refers to wind-seas and swells approaching from different directions. Anyway this paragraph is now removed.

12) Line 281 - 294 It isn't clear to me that this section along with figures 10 and 11 provide meaningful analysis that isn't already discernible from the previous results, rather this seems like part of the exploratory data analysis and seems redundant. My recommendation is to remove this section and figures.

Reply: Figures 10 and 11 removed and the paragraph deleted.

13) Line 296 "The behavior of the high-frequency part of the spectrum is governed by the energy balance of waves generated by the local wind fields. When the wind blows over a long fetch or for a long time, the wave energy for a given frequency reaches the equilibrium range and the energy input from the wind are balanced by energy loss to other frequencies and by wave breaking."

You have argued that the change in slope is indicative of a change from local wind dominated to swell waves, a physical connection. And have recognized that the dynamic equilibrium between generation and dissipation processes are what control this slope... but fail to make any meaningful physical connection between the slope behaviour and the underlying physics. This is not required in a paper that only presents data, but you have invested some effort in quantifying the spectra, it would seem natural to attempt a physical connection to sea state, wave height, wave age, wind speed or some physical parameter.

Reply: We could not find a physical connection between the slope behaviour and the underlying physics. Slope is represented by the exponent of the high frequency tail.

We have added the scatter plot between exponent of the high frequency part of wave spectrum and significant wave height, mean wave period, wave age and wind speed.

14) Line 296 - 313 This section seems to try and convey in detail a very simple observation that more energetic wave spectra have steeper high-frequency tail slopes. If this needs explanation at all, it seems it should be much simpler. Again, how the slopes are computed is not defined.

Why are slopes in table 4 and figure 12 numerically negative, yet in the text are all positive? You refer to slopes as increasing, but are they not becoming more negative?

Reply: The methodology on computation of exponent of the high-frequency tail which represents the slope is now added.

The exponent is negative. As the exponent reduces, the slope of the spectral tail increases. Now it is corrected.

15) Line 307 "It is shown in Fig. 12 that the slope also increases as the mean wave period increases."

Figure 12 presents an interesting association between high frequency wave spectra slope and monthly mean wave height, but there is only one sentence referring to it with no exploration of its physical significance. It suggests a nonlinear saturation of slope as a function of wave height. You may wish to consider the suggestion following the comments.

Reply: Added the suggested analysis at the end.

16) Line 312 "The slope of the high-frequency end of the wave spectrum becomes milder when the wave nonlinearity increases. The study shows that the tail of the spectrum is influenced by the local wind conditions."

An attempt to physically connect your observations/statistics to physical forcing. Good. But this is unsatisfying. You are arguing that by virtue of large Hm0 alone that nonlinearity increases? This seems speculative at best.

Reply: Now we have added a Figure showing the variation of the skewness of the sea surface elevation data with the significant wave height to show that the nonlinearity increases with increase in Hm0. The below sentences are also added.

The most obvious manifestations of nonlinearity is sharpening of the wave crests and the flattening of the wave troughs and these effects are reflected in the skewness of the sea surface elevation (Toffoli, 2006). No skewness indicates linear sea states, positive skewness value indicate that the wave crests are bigger than the troughs. Figure 10 shows that nonlinearity increases with increase in Hm0.

17) Line 315 4.3 Theoretical wave spectra

This section is very unsatisfying. How the fits to the theoretical spectra are determined is not provided, the only clue is: 'values for α and ÏŠ were randomly varied within a range'. That this is not explained at all is a serious deficiency.

Reply: Explanation is given above for Qn. No 3. Equations used and parameters are also explained in Section 'Data and Methods'

18) Line 317 "The monthly average wave spectra for the year 2015, is compared with JONSWAP and Donelan theoretical wave spectra."

Why was the only the year 2015 used in the monthly average wave spectra fits? Is there reason to believe that interannual variability is insignificant and can be ignored, figure 6 suggests not. How then can monthly means from one year be considered representative of the wave climate? Year 2015 is reported to have 14772 observations, while years 2011, 2012 and 2014 have 17300. How do you justify extracting mean statistics on a data set missing 15% of the data?

Reply: There was no reason for selection of 2015. Now we have used 2011 during which 99.98% of data collected.

19) Line 320 - 326 "For these months the first peak is fitted with JONSWAP spectrum and second with Donelan, and the fitted spectrum shows a good match with the measured one. In the monsoon period, the spectrum is single peaked with high spectral energy density and during this period JONSWAP spectrum is fitted up to the peak frequency and after that Donelan spectrum is used. During the months May, October and November, after the peak frequency, the measured spectrum is not smooth and hence for this part, Donelan spectrum is fitted in two parts in order to obtain the best fit."

The ad hoc piecewise fitting of spectra is unsatisfying. A uniform criteria from which spectra are fit to different portions of the data does not seem to exist. How can an adhoc scheme be deemed useful to those engaged in the "design of marine facilities"?

How does one interpret the physical basis of these piecewise spectra? Does it make physical sense to apply an ad hoc piecewise spectral concatenation? Are the essential assumptions inherent in the different piecewise spectra being satisfied?

In absence of answering these questions, an alternative approach would be to fit the different theoretical spectra over the entire frequency band, present the results, and perhaps hypothesize/speculate why the different inherent assumptions in the theoretical spectra result in different fit fidelity over the different frequency bands/environmental conditions.

Reply: We tried fitting the spectra for the entire frequency range. Since it is not matching, we have fitted JONSWAP spectra in the low frequency range upto peak frequency and the Donelan spectrum for high frequency range from peak frequency. Now we deleted the fits of rdouble-peaked spectrum.

20) Figure 3 The color bands need improvement. For example, in a) there are two yellow bands with only slightly different saturation, but widely different amplitudes. It is difficult to discern which amplitude corresponds to the sections in the plot.

Reply: Color bands changed.

21) Figure 8 Instead of scaling the ordinal axis for each plot to maximize the dynamic range of the curves, it would be more informative to have a single uniform axis for all plots with the same angular range.

Reply: Now uniform axis is made for all plots with the same range.

22) Figure 9 This is commonly referred to as a spectrogram. The amplitude scale has not been defined. No units are shown.

Reply: The amplitude scale is 'normalised spectral energy density' and hence no unit.

23) Figure 10 - recommend removing this figure and section To make figure 10 more informative for relative comparison across spectral bands, the ordinal axes should have uniform ranges, both for spectral amplitude and direction. This will allow the reader to immediately discern important differences between bands.

Reply: Figure 10 removed.

24) Figure 11 - recommend removing this figure and section Abcissal axes need labels and units.

Reply: Figure 11 removed.

25) Figure 12 Needs to explicitly label ordinates as slopes.

Reply: Corrected in figure

26) Figure 13 In contradiction to the text describing this figure, I'm unable to see where the Donelan spectrum was applied.

Reply: Now it is explained as replied under 19.

Suggested Analysis

As mentioned above, Figure 12 presents an interesting association between high frequency wave spectra slope and monthly mean wave height suggesting a nonlinear saturation of slope as a function of wave height.

The introduction contains a nice discussion of findings that quantify high-frequency slopes, both theoretically and experimentally, with substantial support that in your oceanographic setting that the expected high-frequency decay would be affine with f -4. You mention in general terms how this decay represents an equilibrium between dissipation and energy input dominated by local winds, it may be useful to think about specific processes. For example Donelan et al. (2012) find that in addition to the k-4 dissipation that swells modulate the equilibrium in breaking waves dependent on the mean surface slope, while Melville (1994) also quantified a relation between wave packet slopes and dissipation rate. These results are specific to breaking waves, but one might expect similar relations between surface dynamics and dissipation rate for non breaking waves. If you do not find existing literature pertinent to non-breaking wave dissipation, then perhaps a functional representation of the data shown in figure 12 might be useful in revealing something about the physical connection, and at the very least would provide a predictive basis relating spectral slopes with mean wave heights as a basis for future research.

To that end, you might wish to fit a function of the form: A * exp(λ Hm0) + s0, with initial parameters of A = 8, λ = -2.4, s0 = -3.7 to the data of figure 12. This is exemplified with a subset of your data below. With optimized parameter values you will then have a functional representation of your spectral slopes based on Hm0. Presumably, Hm0 along with other parameters (wavelength, wind...) may lead you or others to a hypothesis relating your spectral slopes to sea surface physics.

Reply: Thanks for all the suggestions. We have added the above in the paper and revised.

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