

Interactive comment on “Temporal variations in the wind and wave climate at a location in the eastern Arabian Sea based on ERA-Interim reanalysis data” by P. R. Shanas and V. Sanil Kumar

Anonymous Referee #1

Received and published: 23 January 2014

General comments

The paper is dedicated to the discussion of annual and seasonal variations of surface wind and significant wave height (SWH) over the Arabian Sea, using Era-interim reanalysis dataset. As a general comment, the quality of the paper is fair in terms statistical analysis. It needs however some changes at least in the abstract and in the conclusions since those, at the present state, do not fully reflect the statistical significance of the findings shown in the present work.

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Specific comments

The abstract and the conclusions of the paper refer to annual maximum SWH (abstract), and annual maximum wind speed (conclusions), while it was mentioned in the paper (section 3.1) that for windspeed the difference in trends between annual maximum wind speed and the mean speed is statistically meaningless since it is due to few extreme events (at least in this context). This should be clearly stated in these sections (or modify these sections altogether) to provide a complete overview of the results.

Introduction: please mention Honnawar at line 18, since it is shown in figure 1, referred to in the same section. Moreover, is Figure 1 presenting the considered area in terms of reanalysis data? Or it is a subsection of the area shown? Or it is just referred to the buoy location? This is unclear from the paper.

Section 2 starts describing the data, but it would be more useful first to provide the reader with a description of the main climatological features of the area (e.g. monsoon variability during the year) to put in a context all the discussion of the results in section 3. Some readers may be unfamiliar with the local phenomenology of the Eastern Arabian Sea.

Section 2.1, lines 5-6: it is inappropriate to talk about “resolution” of the model, what is mentioned in the paper is the grid size. The actual resolution of the model is coarser than the grid size.

Section 2.1: it should be mentioned that ERA-Interim assimilates, among the various kinds of observations, also scatterometer and altimeter data.

Section 2.2 lines 14-16: the description of the methodology used to derive the wave spectrum is unclear.

Section 2.3 line 26: “better performance”: with respect to what? Another model? Please clarify.

Section 2.3: Some validation of the model is provided in this section using buoy data,

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and figure 2 shows a reasonable matching in terms of the SWH. Is there any buoy wind data available to check also the wind fields?

Section 3.1: as said, discussion of results would be eased by a previous description of the local phenomenology (e.g. south west monsoon season).

Section 3.2: discussion is referring also to Table 1, but it is never mentioned in the text.

Section 3.2: a comment on the statistical significance of different trends of annual maximum SWH and mean SHW (as in section 3.1 for winds) would be useful when commenting Figure 8 (lines 22-24).

Section 3.3: discussion is referring to Figure 10, but it is never mentioned in the text. There is also reference in the text to non-MKS units (line 18, mph). However this is what was a bit puzzling as mentioned earlier: there is a discussion of few single events. But as figures 5, 8 and 10 show, these affect mainly the maximum values, which give a general "impression" of what can happen in the area. However, as clearly said in section 3.1, those few events drive the statistics of the maximum values in a statistically meaningless way.

Section 3.4: Focus is on the outliers, trying to put them in relationship with the modulation from other phenomena (e.g. ENSO). This appears unbalanced considering the limited statistical significance of these outliers and the weak (if any) link to climatological large scale forcing.

Section 3.5: It is unclear if you are referring to/using annual means or monthly means in these tests. A bit more discussion should be spent in commenting the results and the structure of this section should be improved to improve readability. Initially it should be mentioned that the t and Z tests are performed to check if the slope of the regression lines is 0. The t test should follow a t-distribution with $n-2$ degrees of freedom if the null hypothesis is true. Please check.

Section 3.5: it is mentioned that statistically significant trend is found for windspeed

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and wave period mean trends. However Table 4 indicates significant results (SG in the table) for the windspeed mean and windspeed 90th percentile. Please clarify. By the way, "man" is "mean" in table 4?

Technical corrections

Section 2.3 line 21: instead of "are done": "have been done".

Section 3.1: many "show" should be "shows" (e.g. "the monthly mean wind speed shows...")

Please spell acronyms out also when well known the first time they are mentioned in the text (e.g. ENSO, NOAA, JAMSTEC, etc.).

Section 3.5, line 11: it is "degrees of freedom", not "° of freedom". Please correct.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 7239, 2013.

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