

Interactive comment on “The relationship between Arabian Sea upwelling and Indian monsoon revisited” by X. Yi et al.

Anonymous Referee #2

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Comments on “The relationship between Arabian Sea upwelling and Indian monsoon revisited” by Yi et al.

General comments

In general this is a well-written paper. However, the paper has two significant weaknesses which must be thoroughly addressed before this paper can be accepted for publication. (1) Some kinds of validation with observations should be conducted to validate the modeling results. I understand there are limited observational data, however, modeling skill assessment is a necessary component to validate the settings and assumptions of the models. Without validation, the modeling results are less convincing. What if the modeling results are wrong? Misleading conclusions might be derived from the model. (2) Most of the correlation analysis in this paper is conducted without

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mechanism analysis supporting. I suggest the authors focus more on the upwelling dynamical analysis and their dynamical relationship between those variables and upwelling indices, and then do correlations between variables. That will help them to better explain the correlations between variables.

Specific comments:

Abstract: Cannot agree on calling sediment records, sea-surface temperature and wind as “upwelling indices”. They are some variables (parameters) that are related to the upwelling studies in this paper, but not indices specifically for upwelling. I understand those terms are used in other papers too, but I still think it is not proper to calling those variables as “upwelling indices”.

Page 2685, Line 7, what does “ka” stand for?

Page 2685, Line 9, suggest to change western and eastern Arabian Sea to “ western and eastern Arabian Seas”.

Page 2686, last paragraph, “The high spatial resolution. . . contributes to capture the upwelling variability on small scales.”. Do you have evidence to support this statement?

Page 2689, Lines 4-8, I understand higher magnitude is associated with higher variation, should that be obvious in the ocean? What is the purpose of doing this kind of analysis?

Page 2689, Lines 9-12, the first mode of EOF only account for 10% of the variance, which is a small partial of the total variance. 90% variance is excluded in your analysis if only considering the first mode of EOF. I do not think Fig. 2b represents much useful information for us to better understand the variance of the upwelling velocity. As the authors said in the paper “upwelling is affected by various and complex processes in this region”. Quantifying the sources of those variances and responsible processes will be more useful and make the paper more publishable.

Page 2690, first paragraph, because the PC1 only accounts for a small portion of the

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total variance, comparing the PC1 with SST will not give us very conclusive results. Even though the correlation is high, it only tells us the SST and SW wind-stress have some contributions to the first principal component. More variance (90%) is not included, which to me is unacceptable.

Page 2961, last paragraph, "The spatially heterogeneous . . . forcing mechanisms." What kinds of forcing mechanisms? Could the spatially heterogeneous be possibly caused by the spatial difference of one single forcing mechanism?

The authors conducted a series of correlation calculation between different parameters (e.g. those in section 6). However, without dynamical analysis of the forcing mechanisms between those parameters, I feel the correlation analysis is not very convincing. So I suggest the authors to have more discussions on dynamics, not mathematical calculations.

Page 2699, the caption of Figure 1, "The red contour. . .", contour of what?

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