

## ***Interactive comment on “Arabian Sea upwelling over the last millennium and in the 21st century as simulated by Earth System Models” by Xing Yi and Eduardo Zorita***

**Anonymous Referee #2**

Received and published: 20 January 2017

The study by Yi and Zorita analyzes the temporal evolution of Arabian Sea upwelling over the last millennium and in future climate scenarios using available coupled GCM simulations. Although the topic is potentially of interest to the climate research community, there are several major problems with this study which prevent me from recommending publication of this manuscript.

1) Design of the study: The authors claim that they analyze "the outputs of two earth system models" to study the last millennium and the future: MPI-ESM and CESM. This is not true. In fact they used "MPI-ESM-P" and CESM1 (with CAM5 atmosphere) for the last millennium and "MPI-ESM-LR" and CCSM4 (with CAM4 atmosphere) for the future scenarios. I don't know how much "MPI-ESM-P" differs from "MPI-ESM-

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LR" and unfortunately the authors do not explain the difference between these two models. However, it is very clear that CCSM4 and CESM1 are in fact quite different models producing different climates and different climate variability. In other words, the authors used FOUR different models for this study, but a detailed explanation on how these models differ (MPI-ESM-P vs MPI-ESM-LR and CESM1 vs CCSM4) is missing. With that said, I do not understand the intention of analyzing past and future changes in one paper. What is the link between the last millennium and the future projections in this study? Is the intention to validate the models used for projections by means of the last millennium simulations? Given the differences of the models used for the paleo-simulations and the projections, this wouldn't make sense however. In addition, more proxy records than just the *G.bulloides* record would be needed for a credible validation. Or is the intention to elucidate differences in the forcings between past and future upwelling changes? If this would be the case, then the forcings should be analyzed in much more detail (see my next point).

2) Forcings: The paper does not describe which forcings were included in the last millennium simulations. Much more explanation of the experimental design would be required in the methods section to clarify what drives the externally forced climate variability in the models. Moreover, the study does not properly analyze how the models respond to these forcings. In other words, what causes the correlation between modelled upwelling and *G.bulloides*-derived upwelling which the authors claim to see in Figure 2. Is it the long-term (orbital-forced) trend or higher frequency fluctuations associated with solar and/or volcanic forcing? Statistical analyzes would be required to examine e.g. spectral coherence (and the *G.bulloides* record should be shown anyway). Last but not least, are the calculated correlations (Fig. 2) statistically significant? A significance test is missing (which takes autocorrelation of the time series into account).

3) Interpretation of the LME results: The authors state that the "identified upwelling trends can presumably be attributed to the orbital forcing". The LME project, the CESM

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results have been taken from, also provides individual forcing experiments (e.g. orbital only). Why didn't the authors use these simulations to test their hypothesis? However, first the authors would need to show that the trends are statistically significant. The p-values shown in Figure 1 do not support significant trends in the CESM simulations. So what are we talking about?

4) Interpretation of the RCP2.6 results: The authors "explain" the lack of a significant upwelling trend in the RCP2.6 simulations by "a compensation between the opposing effects of the increase in upwelling favourable winds and the stratification of the water column". However, p-values shown in Figure 8 do not suggest significant trends in wind stress. What is wrong here? Moreover, it is well known that the monsoon circulation behaves very differently in response to future global warming in different models. The authors should use a much bigger number of models from the CMIP5 pool to come to robust conclusions.

5) "Flip" around year 1550: I don't see a "flip". No appropriate statistics is provided to corroborate a flip. And what should be the forcing of such a flip?

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Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-124, 2016.