Clim. Past Discuss.,
doi:10.5194/cp-2016-124-AC3, 2017

Climate of the Past Discussions

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

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Received and published: 1 February 2017

We thank the reviewer for the detailed reading of the manuscript and for pointing out the problems. In the following, we sketch how we plan to eventually revise this manuscript to address these issues.

1. We agree with the reviewer that a more detailed explanation of the differences among the models is missing. We will improve this by adding a more detailed comparison of the models in the revision of the manuscript. As for the different atmosphere used by CESM1 (CAM5) CCSM4 (CAM4), we could check the results of the CESM1 Large Ensemble simulations, which is also with CAM5 atmosphere, however only RCP8.5 is available.

The objective of the work is to study the impact of external forcing on Arabian Sea upwelling. These external forcings are different over the past millennium and in the future, both in character and amplitude of variations. The issue of model validation is not central, since there are very few instrumental or proxy "observations" or indicators of upwelling. This is why we analyze two (or four, as the reviewer indicated) models and look of their identified responses of upwelling to forcing, or lack therefore, can be explained in a physically consistent way. For instance, we address the question if the knowledge of the upwelling response to forcing over the past millennium can give insights about its response in the future. The conclusion from our study is negative. We find that, in the past, the decadally varying forcings, like solar irradiance or volcanism, do not exert a discernible influence on upwelling, according to the model simulations. In contrast, all simulations do show a consistent response to orbital forcing, which is also consistent with Mid-Holocene simulations, and consistent with the trend in wind in both millennium and Holocene simulations. At decadal time-scales, therefore, upwelling variability is almost purely due to internal variations.

The response to strong scenario future forcing is qualitatively different from the response to orbital forcing. Greenhouse gases cause an intensification of the wind, but this intensification is overridden by the stronger stratification of the water column.

2. The external forcings used in the CMIP5 simulations are explained in many other previous papers, but we understand that problems about the forcings might arise as also mentioned by reviewer #1 and by Dr. Sebastian Lüning in the short comment. In the revision of the manuscript we will explain the differences in the forcings and their impacts on the Arabian Sea upwelling.

We are aware that the significance test is missing for Fig.2. However, in fact all the simulations show similar correlation patterns, although not all of them are significant at the 95% level. We will also explore this when revising the manuscript.

3. We thank the reviewer for the suggestion of analyzing the individual forcing exper-

iments of CESM. However, this is already included in the study, albeit indirectly. The simulated upwelling in all three simulations in each ensemble are uncorrelated with each other at decadal timescales, which means the influences of the forcings varying at decadal timescales, solar variability and clustering of volcanic eruptions, are very small. If their impacts were strong, the upwelling simulated in all simulations will tend to be synchronized. This is not seen in the simulations. The analysis suggested by the reviewer, for instance the spectral coherence between upwelling and forcing, would then be inconclusive. It may well happen that both records, forcing and upwelling, show the same spectral peaks, but the coherence would then be inconsistent among the three simulations, since the simulated upwelling is not synchronized in the simulations. The revised version of the manuscript will explain this point in more detail, although it is already mentioned in the initial submission.

Although the p-values do not support significant trends at 95% level in all simulations, the overall picture is highly statistically significant, even if none of the trends would be, taken in isolation, statistically significant. The reason is that all six simulations show negative trends. Given that all six are independent samples, the probability that this happens by chance is (0.5)**6 = 0.01. Therefore, the overall trend analysis, only considering the sign of the trends, is statistically significant at the 0.01. Since the trends are not only all negative, but most are also statistically significant at the individual 0.05 level, the overall significance is much stronger than p=0.01. In other words, it is very unlikely that all six trends are negative, just by chance. We would explain this point in more detail in a revised version.

The reviewer also suggests to use the CESM simulations forced by orbital forcing. In this study we have looked at the orbital forcing in a slightly different way, as the difference between the mid-Holocene and pre-industrial equilibrium simulations, which provide the same circulation trends over Asia as the millennium simulations. In the revised version we will confirm these results by looking at the upwelling trends simulated in three orbital only simulations of the LME with the model CESM.

4. We do not completely understand the reviewer's concern about the missing interpretation for the lack of a significant upwelling trend in RCP2.6 as the compensation between the increasing winds and the water stratification. Quite likely, this is due to our unfortunate formulation of this paragraph. The externally forced trends in wind and upwelling will likely be the same signs as in the strongest scenario RCP8.5, although in the weaker scenario those trends may be smaller and therefore not significant. This does not mean that they do not exist. Our interpretation is that in the weaker scenario, both effects are in a closer position to compensate each other, in contrast to the strongest scenario where the effect of stratification dominates. However, this is a possible (likely) interpretation. If, for instance, the imprint of the external GHG forcing on wind were non-linear and changed sign with the strength of the forcing, this interpretation would be invalid.

We thank the reviewer for the suggestion to analyze the changes in future Monsoon circulation in more CMIP5 models. That analysis has been already conducted in other context with focus on changes in Indian rainfall (Menon et al., 2013, doi:10.5194/esd-4-287-2013). These authors actually found that the simulated changes in the Monsoon circulation are quite robust across the CMIP model ensemble and consistent with the changes simulated by the MPI-ESM and CESM models, i.e. a strengthening of the upwelling favorable winds in the future under RCP8.5 scenario. The revised manuscript would incorporate a more detailed discussion of this point.

5. The change in trend in the G.Bulloides record was reported by Anderson et al (2002). We agree with the reviewer that the flip in the model simulations is not as clear as in the G.Bulloides record and it should be better supported in the manuscript. More statistical evidence will be provided in the revision. A possible cause of the change in trend might be related to the solar irradiance forcing, as there is a multi-centennial trend from about 1700 to present in the solar forcing. If the statistical analysis fails to detect a change in trend in the models, the revised version will document the disagreement with the G.Bulloides record on this point.

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