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Ocean Science
Discussions

Interactive comment on "Imprint of external climate forcing on coastal upwelling in past and future climate" *by* N. Tim et al.

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We thank the reviewer for commenting on our manuscript. In the following we will respond to the main critical points mentioned in the review.

There is no observation at all to refer with in this paper. I think the author should at least find ocean reanalysis data for Fig. 2, in order to properly evaluate their simulations.

Of course it would be a great benefit having observations to evaluate our model results. Unfortunately, it is very difficult to measure upwelling itself due to much higher horizontal than vertical velocities. Observational data sets regarding upwelling

are proxies of the upwelling itself like sea surface temperature and wind stress. Furthermore, these data sets are not covering large spatial and, even more important, temporal scales. When analysing the last hundreds or thousand years, sediment cores are used as proxies. These are locally restricted and the derived variable is not the upwelling itself but e.g. temperature, nutrients or species which could be influenced by other factors, too. Thus, it is very difficult to evaluate earth system model results with observations when analysing upwelling of the last thousand years.

The reviewer suggests to compare with ocean reanalysis, but this comparison would not be between model versus observations, but rather model versus model. The ocean model used in ocean reanalysis is also driven by atmospheric data sets that do not have a very fine resolution, also that the caveat always remains whether the wind forcing would be adequate to realistically represent coastal upwelling. The amount of ocean observations in the EBUS is also quite limited, with the exception of the California Upwelling system. All in all, the upwelling simulated in ocean reanalysis would rather be a model product, only very weakly constrained by ocean observations, if at all .

The reviewer is right that it is difficult to validate the realism of the simulated upwelling, other than comparing the sea surface temperature fields with observations. It is, however, well known that all climate models display a temperature bias in the EBUS, the origin of which is not well known. This is an open question that cannot be resolved here.

The statistical analysis is not very clean. The statistical significance of the different correlation computed should be systematically given with a threshold clearly stated, and with an appropriate way to account for the number of degrees of freedom including auto-correlation in the time series. This is true for Fig. 3, 5, 6, 7 and 9. Also the "statistical model" from page 2905 should be better depicted with assumptions clearly explained.

We calculate correlations in different settings. In one setting we calculate correlation patterns across simulations. In other settings we calculate correlations between individual time series. The requirements to show the statistical significance are different. When we show correlation patterns, for instance the correlation pattern between a upwelling index and the SLP field, it is the physical plausibility and interpret ability of this pattern what is more relevant. There will be correlations with gridpoints that are statistically significant, but for others this correlation will be non-significant. This will be necessarily so. For instance, upwelling will be more strongly correlated with the coast-parallel winds, which means that the correlation to the SLP field will be zero on those locations (where the SLP gradient is highest). This illustrates that the interpretation of a correlation pattern is more based on physical reasoning.

In other settings we do calculate the correlation between individual time series, for instance between the upwelling indices in the different simulations (Fig. 4 and Fig. 8) . As we claim that there is no connection between these indices, we have to put care in this case in establishing the statistical significance. For this we do not rely on the estimation of an effective number of degrees of freedom, but rather on Monte Carlo simulations, in which we produce synthetic time series that have the same serial correlation structure as the original series, but that are otherwise uncorrelated in time (see e.g. Ebisuzaki et al. J. of Climate, 1997, A Method to Estimate the Statistical Significance of a Correlation When the Data Are Serially Correlated). Since we calculate the correlations after different degrees of time series smoothing, this method takes care. The threshold of significance in this calculation is 0.95 for all correlations and trends.

The mathematical description is the base of the statistical analysis used here. Correlating the three simulations of an ensemble shows us if the external or internal forcing drives the temporal variations. The construction is used in the manuscript

whenever correlation simulations of the same ensemble. We will explain this point more clearly in the revised version.

Why are your results so different than Wang et al. (2015)? Do you use the same boxes? Is MPI an outlier in the CMIP5 database? According to his Extended Data Table 2, this does not seem to be the case. Please clarify this point. More specifically, the sign of the trends were the same in the different regions in Wang et al. (2015), while here for your RCP8.5 you find either positive or negative trends. Why do you limit your table 3 and 4 to past1000 and historical and not to RCP8.5 to confirm mechanisms proposed by Wang et al. (2015) within your framework?

We will include a more detailed discussion of the differences and similarities of our results compared to Wang et al. (2015). However, we are surprised by the comment of the reviewer. In the paper of Wang et al. (2015), extended data figure 3, the long-term trends in the period 1950-2100 in the EBUS are shown. For all regions there are positive and negative trends (blue and red squares), so that clearly not all models agree in the sign of the trend. Wang et al. (2015) define a level of consensus among models (if 80% of the models agree) to claim robust trends. In our study, we calculate the trends in the period 2006-2100 (in contrast to 1950-2100 in Wang et al. (2015)). We additionally show that for some regions, e.g. Humboldt, this discrepancy in the trend may be due not the different model structure, but also to internal variability, as we find in the MPI-ESM model. It is not easy to identify the trends simulated in individual models, as their figure 2 show only the ensemble-mean together with confidence intervals, specifically for California, the long term trend seems to be mainly not robust across the models. We agree with the reviewer that our manuscript should explain more clearly the differences between the Wang et al. (2015) study and ours and also how our conclusions complement the conclusions reached by Wang et al. (2015).

References:

Ebisuzaki, W. (1997). A Method to Estimate the Statistical Significance of a Correlation When the Data Are Serially Correlated. J. Climate, 10:2147–2153, doi:10.1175/1520-0442(1997)010<2147:AMTETS>2.0.CO;2.

Wang, D., Gouhier, T. C., Menge, B. A., Ganguly, A. R.: Intensification and spatial homogenization of coastal upwelling under climate change, Nature, 518, 390–394, doi:10.1038/nature14235, 2015.

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