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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 the time commenting on our manuscript. In the following we reply to the main concerns.

My first concern is on the goals of the paper which are not clearly stated in the introduction. A focus seems to be made on the comparisons of results from this study with previous efforts in the introduction and conclusion, which does not help the reader to find the link between the different parts of the paper. For example, Chapter 6 on the imprint of external forcing on stratification does not appear to be connected with the rest of the paper.
This concern is common to the opinion of many of the other reviewers and we will

rewrite and extend the introduction to give more detail on the goal of the paper and the analysis done.

T**he authors claim to focus on coastal upwelling, however in this paper they consider upwelling in offshore regions so that the processes at play may differ.**
The chosen regions include not only the upwelling directly at the coast, this is true. Nevertheless, the upwelling is forced by the trade winds in the whole regions selected here. These areas are still related to the alongshore winds as the jet of coastal parallel winds are slightly offshore and wind stress curl develops due to the relaxation of wind speed towards the coast. Therefore, the strength of upwelling in the whole region should be related to the alongshore wind, to the sea level pressure gradient and, thus, to the Bakun hypothesis (Bakun, 1990).
We checked that upwelling in the regions that were chosen for our study do present a uniform evolution of upwelling. We calculated the correlation between the latitudinal mean of the gridboxes which are closest to the coast with the latitudinal means of all other gridboxes. The correlations decay with the distance from the coast, and vanish at the edges of the chosen regions. Thus the regions used here do cover the area of upwelling and are suitable for representing the upwelling in the Eastern Boundary Upwelling Systems in the simulations.

There is no mention of model validation in this paper, nor reference to observation. Are the models able to realistically represent wind-stress curl in these regions at that resolution? The authors appear to be honest about the presence of large uncertainties in the simulated results, would it be possible to describe the underlying assumptions and the nature of those uncertainties?
Comparing the upwelling itself to observations is very difficult due to the lack of measured vertical velocities. Also, the spatial resolution of the atmospheric submodels may be not enough for a realistic representation of the wind stress forcing. This is a caveat that we have to accept until high-resolution simulations over centennial times scales become available. Here, we validate the drivers of upwelling by analysing the connec-

tion between simulated upwelling and the simulated wind stress. We find that this link is quite compatible with the well-known link derived from observations at least at larger spatial scales. The correlation patterns in Fig. 3 show the realistic representation of the sea level pressure patterns connected to upwelling in the EBUS. Furthermore, the annual cycle of the upwelling is realistically represented in the earth system model. Small scale features of wind variability are surely not resolved in these models but the large-scale pattern and, evidently, its relation to the upwelling is.

The differences with the previous work by Wang et al. (2015) are not very well explained. The authors could clarify why they obtain different results, for example why the signs of the trends differ.

We will include a more detailed discussion of the differences and similarities of our results compared to Wang et al. (2015). In 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).

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