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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.

Anonymous Referee #5

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——— General comments ———-

This paper proposes to analyze the relationships between climate forcing and vertical velocities in regions of eastern boundary upwelling systems bordering California, Peru, Morocco and Benguela using coupled general circulation models. This work is relevant in the framework of the impact of climate change on ecosystems, as upwelling areas are known to be highly productive areas exhibiting biological blooms.

The strategy is based on the use of single model ensembles from two different earth system models for the past, historical and future (2006-2100) periods. Future scenarios correspond to different anthropogenic emissions of greenhouse gases. The authors found no significant change in upwelling systems associated with external forcing over the last millennium. Concerning the future development of upwelling in the regions of

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interest, only the RCP8.5 emission scenario gives a statistically significant trend.

This paper is well written and shows interesting results. However the logical developments are sometimes hard to follow, especially for a non-specialist, and the reader is left with the feeling that more could be made with the analysis. I would thus recommend publication if some efforts are put on the analysis and if the following points are addressed.

———— Main issues ————————-

- 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.

- The 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.

- 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?

- 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.

———— Specific comments ————————-

- P. 2904, l.24 : The upwelling indices used in this paper should be better described. Also, the relationship between the metric of upwelling and winds could be better de-

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scribed.

- Table 1: the meaning of (r1, r2, r3) has to be better defined.

- p. 2902, l. 5: an explanation of “Bakun’s hypothesis” would be appreciated.

- Fig. 5: why is there a different filter between past1000 and historical simulations?

- Fig. 9: the “skin temperature” is referred to without any mention on how this was computed. Please explain.

———— Technical comments ————————-

- P. 2900, l. 6: “volcano” should be “volcanic”

- P. 2900, l.15: why does the final “s” in “EBUs” is not capital letter?

- P. 2904, l.5: remove reference to “Otto-Bliesner et al., 2015”

- P. 2905, l.8: “with different THE initial conditions”, remove “the”

- P. 2911, l.28: “not the evolution of upwelling” should be “nor the evolution of upwelling”

- Fig. 1, last sentence of legend: “how many model are”, “model” should be “models”

Interactive comment on Ocean Sci. Discuss., 12, 2899, 2015.