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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 #4

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————— General comments —————

The paper proposes to analyse the fingerprints of external forcing of the climate system on a particular oceanic system: the upwelling of four key regions bordering California, Peru, Morocco and the Benguela. For this purpose, the authors analyse different simulations from MPI-ESM climate model either over the last millennium, including the so-called historical era, as well as for future scenarios with different anthropogenic emissions of greenhouse gases. They found no significant change in all upwelling systems analysed following external forcing over the last millennium. They confirm this result using CESM-CAM5 model simulations. For the projections, they only find statistically significant trends within RCP8.5 emission scenario and no clear trend in scenarios with smaller radiative forcing.

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This is an important topic given that upwelling regions are usually regions with biological blooms, due to nutrients supply, so that it is important to know how they could respond to climate changes. The paper is generally well written. The analyses proposed are interesting and most of the time makes sense. The evidences presented support the main conclusions of the paper.

Nevertheless, at different occasions I have a hard time to understand the logical of the arguments. Clarifying and clearly stating the main hypotheses that are being tested in the paper could easily improve this. Furthermore, you will find below a few key elements that need to be tackled before publication.

————— Main issues —————

1. 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
2. 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.
3. 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?

————— Specific comments —————

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- P. 2900, l. 11-12: you should specify for which period you are talking about. This last sentence could be improved.
- P. 2900, l. 24: please specify “stronger” than what?
- P. 2901, l. 4-5: in the following you refer to Humboldt and Canary by Peru and Morocco. It would be nice to keep the same terminology in the all the paper.
- P. 2901, l. 27: “lack of observations” where and when? I can’t believe there is absolutely no source of instrumental observations in the analysed regions.
- P. 2902, l. 4: replace “external” by “radiative”.
- P. 2902, l. 5: please state clearly what you have in mind by “Bakun’s hypothesis” in a sentence. Is it that “upwelling should answer to external forcing”, or that “external forcing may change land-sea contrast and therefore wind stress and upwelling”. This is important to clearly define here what you meant.
- P. 2902, l. 11-13: “if the upwelling...” this question is a bit different than the one stated l. 6-8. Indeed, the external forcing can act as a pacemaker for internal variability, without leading to variations that goes beyond the internal variability (cf. Ottera et al. 2011, Swingedouw et al. 2015). What is your point here? I believe it is looking at external forcing playing as a pacemaker, but please clarify your main question to be analysed here.
- P. 2902, l. 14-23: I think this paragraph should come before the former one. The paragraph l. 5-14 should end with presentation of the plan of the paper I believe.
- P. 2903, l. 6: In Fig. 1, you only depict the upper panel. You do not use the lower panel, which is misleading, since in the present study, you’re not looking at all CMIP5 models, but just two. Please remove lower panel, or replace it by the models you are looking at.
- P. 2903, l. 15-17: It should be stated how strong is this solar forcing, given the

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debates that exist on the scaling. What is the difference between Maunder Minimum and present day for instance? I assume this is a weak solar forcing, but please clarify this.

- P. 2904: please describe rapidly CESM-CAM5 model.

- P. 2904: l.25 - p. 2905, l. 2: this is depicting a first result and not really “data and methods”. So I think, this should move in section 3, and state Fig. 2 as a support for this result.

- P. 2905, l. 15: “proportional to the external climate forcing”. I disagree with this statement since the response to external forcing could be lagged by a few years. Indeed, you analytical strategy would account for that, so this is mainly an issue with the word “proportional to”. What about “Related with”?

- P. 2905, l. 23-27: Here I think more details should be given. I assume the results proposed assumed that correlation between y_j and y_f is equal to zero, which won't be the case empirically. Please clarify.

- P. 2906, l. 8: “observations”. Which? Can you at least provide a reference?

- P. 2906, l. 9-10: “lower resolution”. Here I'm really confused. What do you mean by lower resolution? Are projections and last millennium simulation not using the same model that you loosely called MPI-ESM? Indeed, different versions of this model exist (MPI-ESM-LR and MPI-ESM-MR). Even though these models do share lots of element, the difference in resolution is paramount, and makes these two models different. I can't find any mention of different version of MPI model used in section 2. This should be clarified by using the CMIP5 name for the models.

- P. 2906, l.23: the first sentence is too general and not necessarily true: you have not proven it and no references substantiate it.

- P. 2907, l. 4-5: I think a better or additional reference to Fernandez-Donado et al. (2013) will be Schurer et al. (2015)

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- P. 2907, l. 6-7: “These high temperatures. . .”. I do not think there is any consensus at the moment on what caused the MWP. Indeed, as stated before, solar activity is now believed to be very small and not large enough to cause by itself the MPW. There have been lots of volcanic activity around the 13th centuries, so that your definition of the MWP as 1000-1300 AD is problematic in this respect. Please clarify and give references to support this strong sentence.

- P. 2907, l. 16: “mostly not statistically no significant”. Please state the level and the test used, and how you compute the degrees of freedom. Such an estimation of significance should be done everywhere.

- P. 2909 l. 22- 24: “These expected trends. . .” This sentence is not clear to me. Please clarify. To which Table or figure are you referring? Table 2 I assume. But why have such expectations. Recent McGregor et a. (2015) paper rather argue that it is the volcanic forcing that leads the decreasing trend over last millennium. And since the trends are not significant in Table 2, what can we say from that? Just that signal (if any) to noise is too small?

- P. 2910, l. 23-24: Indeed, internal variability may have played a very big role over the last millennium, notably to explain MWP (cf. Goosse et al. 2012). This is why you should avoid strong statement as p. 2906, l. 23

- P. 2911, l. 15: please be more specific: Benguela has a positive trend, while California and Morocco a negative one and Peru no significant one.

- P. 2912, l.1-8: following my main issue no 3, you should specify here more clearly the differences in which regions compared to your results.

- P. 2912, l. 23: add “two” before “state-of-the art” to be more specific.

- Table 1,3 and 4: p-values? 10 and 30 should be reversed at the end of the legend I assume.

- Table 2: star are not defined

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- Fig. 2: keep the same scale for b and c. Why are such differences? Two versions of the MPI model? Is it possible to have an idea of observations?
- Fig. 3: remove non-significant regions?
- Fig. 6: Is it the same for the other regions?
- Fig. 7: Why can't we find the same vertical bar as in Fig. 5?
- Fig. 9: legend: What is skin temperature? Why showing only two members? Remove non-significant correlation?

Technical comments

- P. 2900, l. 1: please remove the capital to “eastern boundary upwelling systems”
- P. 2900, l. 6: “volcano” should be “volcanic”
- P. 2900, l. 6: “simulations of ensembles” should be “ensembles of simulations”
- P. 2900, l. 15 “EBUs” why is the final “s” not in capital letter? This should be “EBUSs” according to Want et al. (2015). Correct everywhere in the ms.
- P. 2901, l. 26: add a coma after “EBUs”
- P. 2905, l. 8: remove “the” before “initial conditions”
- P. 2909, l. 2: replace “find” by “found”
- P. 2912, l. 9: add a “be” after “has to”
- P. 2912, l. 23: “state-of-the art” should be “state-of-the-art”

References

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