Review of Imprint of external climate forcing on coastal upwelling in past and future climate, by Tim et al.

In this paper, the authors examine the effect of external atmospheric forcing (GHG concentration, volcanic emissions, solar activity) and internal variability on EBUS (eastern Boundary Upwelling Systems) variability at different time scales. For that they use 7 ensembles of 3 simulations performed with 2 earth system models (MPI-ESM and CESM-CAM5) over the 900-1850 (past1000), 1850-2005 (historical) and 2006-2100 (future) periods.
Decomposing the variation of a climate record into the sum of a component whose variability is externally forced, and a component whose variability is internally driven, they perform across ensemble analysis and show that the variability over the past periods is mainly driven internally. For the future period, variability is also mainly of internal origin for scenarios rcp2.6 and 4.5 and long-term trends are statistically not significant for most of the cases. For scenario rcp8.5, California and Morocco EBUS show statistically significant negative long-term trends and Benguela EBUS shows a statistically significant positive long-term trend.
The objective of the paper, which is to determine the impact of external forcing of EBUS and their response to climate change, is an interesting scientific questions. However, the paper needs some improvement, concerning both the methodology and the writing, before to be published.

General comments

1 - The main and big problem of the paper is that the authors do not go enough into details in their explanations and reasoning. The reader needs to make a strong effort to understand the logical reasoning and the conclusion induced by a given explanation. A big effort of explanation and clarity is necessary, requiring to explain clearly the objective of each section, to provide more details, to go deeper in the analysis and to improve the link between sentences, paragraphs and sections. This is true throughout the whole paper.

2 - The results found by the authors for the evolution of EBUS in the rcp8.5 scenario are not in agreement with previous studies made with other models (Wang et al., 2015, Rykaczewski et al., 2015). First, it is necessary that the authors develop their discussion about these differences, trying to understand them. Second, this suggests that the choice of the model is a source of uncertainty resulting in a range of variability that can be as large as the one associated to the variability of external or internal origins. The authors should therefore examine the possibility to extend their study to other models of CMIP5, that gathers 20 modeling groups (as seen on Fig. 1, 42 models were used to perform historical simulations, and 25 to 42 to perform scenarios simulations). This does not require to run additional simulations, but to gather 2D fields of SST, mean SLP, velocity / transport at 50m depth and wind stress. This would greatly increase the robustness of their conclusions. It seems possible to use other ensemble to estimate effect of external forcing (for example by comparing trends) even if initial conditions differ across models. The authors could examine the correlation between simulations performed with different models, as they do here for simulations performed with the same model. Note that the authors mainly use here results from MPI-ESM, and do not discuss a lot results from CESM-CAM5, though they say at the beginning that they will examine the effect of the model.

3 - The authors show that the variability of the upwelling over the past periods in mainly internally driven. They should explain more clearly what are the mechanisms involved (some of the explanation is already present in Sections 5 and 6, but needs to be developed).

Specific comments

Abstract

P 2900

L 12 : Conclusions of the type "except for" or "only for" are too strong since there are only 4 cases (of upwelling or scenario). Moreover scenario rcp 8.5 may be the most realistic one in terms of GHG emissions …

1- Introduction

L24 : "stronger external climate forcing". Please be more precise (stronger trade winds ?). In general in the paper the authors mention the increase or evolution of external forcing, they should be more precise.

P2901

L1 : What about the conclusions ? "Solar irradiance and volcanism": please briefly explain how.
L 3: Could you please give some details about the « metadata analysis ». What it is based on ?
L 4: Same comment as p 2900, I would remove "only" since it concerns 3/4 of the cases.
L 5-10: This part should be developed, giving more details about upwelling intensity indicators used (wind?). L8-9 does indeed make sense only if this is based on wind indicators.
L 14-19: The effect of El Niño is not clearly explained . Please explain more precisely the relationships (El Nino -humidity- radiative forcing- land ocean thermal contrast- wind ?).
L 17-19, what is the "changing frequency of ENSO"?
L 25 : rcp 8.5 is the scenario that results in the strongest warming. The result of Wang et al. (2015) is in contradiction with the result of the present study, this should be investigated deeper in this paper (did they use the same indicators ? What was the analysis that support their conclusions ? What were the differences between the configuration of the models that they used and the one used in the present study ?…)
L 28-4 : This should go after L 2 , since it deals with uncertainty associated to upwelling evolution estimations in the past.

P 2902

L14-23: this part should not be here but in the part about models configuration, and should be clarified. What is the criteria for high resolution of atmospheric model, and is it met in the present study ?
L 22-23: what does that imply ?

End of part 1: in the introduction, the authors presented the context of their study. At the end of this introduction they should clearly state the specific scientific questions they are addressing, and how (L 10-13 only gives the reader a hint), and present the structure of the paper.

2- Data and methods

For the sake of clarity, I would add subsections (2.1 Models and simulations, 2.2 Upwelling index, 2.3 Methodology).

P 2902

L 25: The acronym of CESM-CAM5 are explained and not MPI-ESM. This is not consistent .

P 2903

The description of the model and simulations should be much developed : what are the atmospheric and oceanic models, resolution (that is detailed on page 2904), etc ...
L 5: a reference is missing for the CMIP5 scenarios description, while Fig 1 is not very useful. It would be more informative to briefly explain that it is a part of the CMIP5 project, to explain what is rcp (useful for l 22), to provide a reference and to just indicate the average delta T by 2100 and 2300 for each rcp. Adding something about the current level of GHG compared to those scenarios could also be informative …

L 7 : please check English, "contributing" is strange
L 16 : « include ». Does that mean that there are other forcings not mentioned here ?
L 17 : A reference for tropospheric aerosols is missing.
L 19 : please check
L 22 : please provide a reference for GHG concentration sources, as well as for each forcing, that should be clearly stated : it is not clear if the forcings taken into account for past1000 and historical simulations are also included in future simulations.
L 23 : how do initial conditions differ ?
L 28 : the authors used the result of simulations performed with a second model to assess the impact of the model on the results, however in the rest of the paper nothing is said about this. Is it really sufficient to use those simulations (who do not include future simulations) ?

P 2904:
L 6: A «) » is missing
Again it is necessary to describe better the model configuration (resolution of ocean and atmosphere models, etc…) and the characteristics of the CEMS-CAM5 simulations. The description of the forcing is very vague and similarities and differences between both models should be clearly stated. For example are "All forcing" the same as the ones used for MPI-ESM?

L 10-18 : this paragraph about the model configuration should go on the previous page with the 1st paragraph.
On p 2902 the authors cite Small et al (2015) about the necessity to use a high resolution atmospheric model. Does the 2 degree resolution used here corresponds to this criteria ?
L 15-18. This needs to be justified.
L 19: Please provide some reference and justification for the choice of upwelling indicator, which is a key point of the paper since it is used to base the results and conclusions of this paper. Why are 2 different indicators used (mass transport and velocity). It would help show the spatial boxes on a map for example on Fig 1 and 9.
L 25 - L 2 p 2905 : What about the realism of those upwelling seasons ?

P 2905: How is the detrending made?
L 5-7: I don't understand this sentence.
L 7-9 : If initial conditions and models are different but forcings are similar, comparing the simulations should also show the effect of internal forcing and model choice no?

From L10, it should be clearly stated here that the authors are presenting the equation on which all their reasoning is based in the following sections.
L 24-26 and Formula 4: This should be explained more in details, in particular the assumptions made. (yf and y'1 and y'2 are uncorrelated, hence their covariance is 0, <x,y> is the covariance of variables with mean equal to 0, and it is assumed that the internal and external variability do not interact (the decomposition is linear)). The conclusion of this formula should be explained : if the correlation y1,y2 is weak, the variance of yf is much weaker than the variance of y1, meaning that the effect of internal variability is much stronger than the effect of external forcing. This formulae is never referred to hereafter in the paper.

P 2906
3- Representation of upwelling and its drivers in the model
This part about the ability of the paper to represent correctly the EBUS is much too succinct.
L 6-9: Those affirmation need to be justified through references, and possibly figures of observations.
L 10: I would add « than in the historical simulation ».
L 10-12: Not only the resolution changes between 2 simulations. When referring to Wang et al. (2014) the resolution of the models used in this study should be compared to the resolution of the model used here.
L 13: It would however help to show the figure
L 17-20: This needs to be better explained and justified.

4 Imprint of external forcing on coastal upwelling
4.1 Past1000 and historical simulations

P 2907:
First paragraph until l 7. It would help to present the evolution of mean temperature during this long period, which would support l 14-15 « do not show a centennial evolution comparable to the global mean temperatures or the global mean external forcing as just described. »
L 12 : regional upwelling INDEX.
L 19 : Here it should clearly stated that this results from Formula 4.
L 22-26: The sentence above does not necessarily lead exactly to this conclusion, I would rather say that results are consistent with the previous study rather than they support it…

P 2908:
L 1-4: to what variable is the « external forcing » of 0.3 and 1.6 W.m-2 referring to ?
L 9-12 : Again I think that one can not really speak about « exceptions », but rather about « cases »: California is the only case over 4 where there is a significant trend in historical that can be attributed to external forcing.

4.2 Scenarios
L 23 : remove « only »
L 23-24 : « This support our results » : please explain better why
The discussion about the disagreement with Wang et al. (2013) results should be developed.

5 Imprint of external forcing on the drivers of upwelling
P 2909: Please provide some details about the SLP computation (area...). Otherwise it will not be reproducible.
Since authors compute both wind stress and SLP, they should check that these 2 variables are indeed correlated, to support l 8-9.
L 8: explain briefly
L 17: low and not significant
L 19-24 (« Regarding … forcing . »). This should be explained more precisely
The main conclusion of part 5 seems to be that the impact over the last centuries of external forcing on the atmospheric drivers of upwelling (wind stress and SLP) is not significative, therefore that no matter the quality of the upwelling representation in the coupled model, one could not detect any impact of external forcing on the upwelling.

6 Imprint of external forcing on stratification
Parts 5 and 6 should be merged in a section about the imprint of external forcing on atmospheric and oceanic factors implied in upwelling, with a first part about atmospheric drivers and the second about stratification. It should be explained at the beginning of this section that the authors want to determine why the imprint of the external forcing on coastal upwelling is weak over the past periods, looking at the imprint of the external forcing on processes involved in the functioning of upwelling.

P 2910
L 15-17 : references are needed
Once again, this part needs to be developed and explained more clearly. It would be interesting, together with the value of the correlation, to indicate the level of significance.

7 Discussion and conclusions
P 2911:
The context, objective and methodology of the study should be clearly reminded. For example, it is not completely clear if L 1-10 concerns past or present study (or both).
The authors need to develop the discussion about the obtained trends for the future scenarios. In particular, they should explain why these trends are obtained, by examining the evolution of the

atmospheric and oceanic drivers of the upwelling. They began to do that for the past periods in parts 5 and 6, but not at all for the future period.
More generally the authors should do a strong effort to develop this part and improve its structure (for example I don't think that the paper can finish on a last sentence like the one on L 22-23).

L 8-10 : this is not true for California.
L 14-15 : « these trends are not always consistent with the expected intensification » : why is intensification expected ?
L 19-20 : again show these boxes on Fig. 9
L 24-26 : the intensity of the upwelling decreases in rcp 8.5 for California and Morocco upwelling in the present study, so the authors can not reasonably conclude that their « results generally agree with the ones obtained by Wang et al. (2015) on the influence of a strongly increased future greenhouse gas forcing on upwelling intensity in the EBUs. »

P2912
L 5: Again, the conclusion can not be that « These results agree partly with our findings ».
L 9: be kept in mind

Figures and Tables

Fig 1: I don't think it is really necessary to include this figure.

Fig 2 : Could it be possible to indicate the mean over the ensemble and, for b and c, the enveloppe ?

Fig 3: What is the level of significance of correlation ? Is the map for one simulation representative of all the situations ? Could the authors try to produce a figure that uses all the simulations to make this map more robust ?

Fig 4 : I suggest to merge this figure with Fig. 8 (same plot but for scenarios instead of past1000 and historical), and to remove Tab. 2 that does not provide any additional information.

Fig 5 and 7, and Tab 1.
Tab. 1 and Fig. 5 show the same information, so I suggest removing Fig. 5 (values outside the 5-95% significance range could be put in italics in Tab. 1) and add the same information for CESM-CAM5 in Tab. 1.

Fig. 6 : it would be nice to write the name of the variable on the graphs title.