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Interactive
Comment

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

Anonymous Referee #1

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General comments

The paper by Tim et al. investigates a potential imprint of external forcing on the four EBUS by use of climate model simulations. EBUS are important regions for ecosystem productivity (totaling about 20% of global fish catch) and hence their sensitivity to climate changes is of interest.

That being said, I think this paper here is not very valuable for the discussion of EBUS sensitivity to climate change, as there are just too many uncertainties associated with past forcing, too many potential caveats in model simulations, and too simplistic of an analysis provided here.

The paper is honest about all of these caveats, which I appreciate. But given what

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we know about EBUS in GCMs (which the authors seem knowledgeable of), I wonder why the authors set out to write this paper in the first place. Given the evidence we have from other paleoclimate modeling studies, it seems naive at best to expect to find robust signals in models, let alone signals that can be backed up with proxy evidence (hence the lack of proxy evidence in this paper). So in that sense this paper is a model sensitivity study, and for that it provides not enough details about model mechanisms to be a significant contribution. More so, the future part (where you could actually hope to find significant changes due to the strong rcp85 forcing) is covered more comprehensively by Wang et al. (2015). To put it provocatively, one could have reached the same conclusions as this paper (probably even with the same confidence) by studying the existing literature.

So I am a bit at a loss as to what to recommend. I generally support publishing papers that test a null hypothesis and fail to reject it (not every study needs to be a "game changer"), but this particular example strikes me as poorly formulated. I recommend major revisions that should focus on providing more in-depth analysis of the mechanisms governing the variability and sensitivity of the EBUS in the models, since this would be a valuable contribution to the field.

The paper is written very well, the figures are mostly very clear, and the statistical methods are sound. Given the expertise of some of the authors I believe this paper can be turned into a valuable contribution given some efforts, but currently it leaves the taste of a prematurely submitted version.

Specific comments

P2904L25ff: how do these upwelling regions and seasons compare to observations? Maps and time series of observations are needed to put the model performance into perspective. Also, why does the upwelling season change for Benguela in the future?

P2905L3f: how were the timeseries detrended? Linearly? Or with a trend estimated from the control run? How is the drift in the simulations?

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P2905, the equations: where is this scheme applied later in the paper?

P2906L9ff: besides big differences in the mean upwelling in the different resolutions, there are also relative changes between the different regions visible in Fig. 2. Not all the regions increase by a factor of two. Why could that be? Is there a difference in variability associated with the changing resolution? These are all questions that I would hope to be answered in a model-only study. Currently, the discussion of the importance of differing model resolution is not very insightful. Along the same lines, I have doubts as to the usefulness of global models in studying EBUS. The winds that actually cause the upwelling can be very narrow (narrower than the model resolution) and coastal topography might play a role (which is not resolved well in global models). So I do not know how we can validate the models on something that they do not simulate well (strong narrow winds). It reminds me of the cloud feedback that gets studied a lot with GCMs, although they do not resolve most of the processes. One might be able to learn something, but one has to be extremely careful as to not over-interpret the model results. I think the authors here lack a little bit of this carefulness in their model validation and I would encourage them to expand that part – especially since there is no forced response to talk about anyway, they could spend more time on the model mechanisms and variability.

P2906L17ff: what the authors write here does not seem to hold for California (Fig. 3b). The correlation does not switch sign at the coastline as in the other three regions. In fact, the California system appears to act quite differently from the other systems. There is lots of literature on this (see Wang et al., 2015 and others). I think this would be interesting to explore.

P2909, first paragraph: I think this could go into the Introduction rather than the Results.

P2907L22ff: I do not think it is straightforward to understand the link between this paragraph and the one before. Are the authors saying that the PNA is internal variability, unaffected by external forcing, and therefore the California upwelling system

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will not/cannot be affected by external forcing? These would be a bit too far-reaching conclusions. I do not believe we understand the natural variability of the PNA very well. Pacific observations are worse than Atlantic ones and there are no credible PNA reconstructions back in time. But maybe I misunderstand this paragraph. In any case, some clarification would be helpful.

P2909L1ff: this is another example of the authors not making a huge effort to support their claims. They compare Wang et al. (2015), which used 22 models, to one model here.

P2909L24ff: there is some literature on past1000 simulations, showing that there is no discernible effect of external forcing on SLP variability (e.g., Yiou et al., 2012). I believe even under strong anthropogenic forcing people do not expect to easily see a forced response in SLP (Deser et al., 2012 and some of her following papers). So in summary, we know that you will probably not find a forced response in local SLP over the last millennium. And since the authors themselves state that SLP gradients (or wind stress) drives the upwelling, I am not sure whether we need a paper presenting a conclusion that can be derived from the literature. To be fair, there is always the chance for non-linearities, so it is good to thoroughly investigate this with coupled models. But I do not think the authors provide much more information than was already out there on EBUS in coupled models.

P2910L18ff: "...has been so far overlooked but that has been found in previous analysis..." Has it been overlooked or not? Please clarify. In fact, I think besides Tett et al. (2007), Lehner et al. (2015) also found a similar result, correlating CESM and MPI simulations.

P2912L17: what is meant with "climate clouds"?

Fig. 1, bottom panel: please plot your own simulations and focus the figure on the time period covered in this paper. This panel has really not much to do with the paper here: it is not about global temperature, it does not go until 2300 but goes further back

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than 1850, and it does not use the same models. Like a number of other things in this manuscript, this leaves the taste of a rather carelessly put together paper.

Fig. 2: please provide observations and indicate the upwelling regions that you average over on the map.

Fig. 5: please provide reasoning for changing the filtering between past1000 and historical.

Fig. 6 and 7: please provide indication of significant correlations.

Fig. 9: this is an interesting figure! In addition you could answer: how is the correlation between the two different models (CESM and MPI)? I.e., how much of the correlation is due to structural similarities/differences between models and how much is due to common forcing?

Technical comments

General: I believe the “Morocco” system is called “Canary”, for example in Wang et al. (2015). I recommend using “Canary”, it seems more common.

I would be happy to provide more typo and wording corrections on a more advanced draft later on. However, the language is generally good and pleasant to read.

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