

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

Anonymous Referee #2

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General comments

The manuscript by Tim et al. describes an analysis of the relationships between climate forcing (internal and external) and vertical velocities in eastern boundary current regions as simulated by coupled general circulation models. What helps this work stand out from other recent model analyses of eastern-boundary current upwelling published within the last year (e.g., Belmadani et al. 2014, Wang et al. 2015, Rykaczewski et al. 2015) is the focus on a single-model ensemble experiment in which a comparison of the variability among runs should help the authors distinguish responses to external forcing from the internal variability. If conducted in a similar manner, and if the model considered here has a behaviour similar enough to the multi-model CMIP5 ensemble, the results should be comparable to those of Wang et al. (2015) and Rykaczewski et

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al. (2015) and could help clarify some of the disagreement between those two previous manuscripts. The authors also investigate variability in winds during the pre-industrial period.

Here, though, the consideration of broad regions and the lack of careful analyses inhibit the ability to interpret results. The authors consider vertical transport in broad regions that are probably more sensitive to wind-stress curl over the open ocean than to alongshore winds (which I will mention below). There is no clear relationship between wind-stress curl and the hypothesized mechanism posed by Bakun (1990), and so this further clouds the ability of readers to interpret the results mentioned here with respect to the authors' aim of considering the impact of external forcing relative to internal variability. The main objectives of the analyses should also be stated more clearly. It is unclear whether the authors seek to investigate the impact of natural or anthropogenic external forcing on coastal upwelling, curl-driven upwelling, the validity of Bakun's 1990 hypothesis, or some combination of these options.

I appreciated the authors' mathematical description of forced and internal components of variability on p 2905. However, this construction did not appear to be utilised later in the analysis, and it did not incorporate the authors' filtering at various frequencies. Perhaps more significantly, the change in the months considered between the past and future simulations was not discussed or included in the mathematical analysis.

I suggest that the authors take a more meticulous approach if they aim to relate their findings to previous hypotheses regarding the response of upwelling to anthropogenic forcing or to other manuscripts. The key issue that prevents me from interpreting the results clearly is the comparison of regions of vastly different geographic extent and distance from shore. The main metric that the authors use as an index of upwelling is vertical velocity (or vertical transport), and this is estimated for regions that extend from the coast to more than 1700 km (in the case of the California Current) offshore. The authors do note (in the later portions of their manuscript, p 2909, lines 7-9) that both wind-stress curl and alongshore wind stress are important in upwelling. Indeed,

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in most of the regions considered here are further offshore than the one or two Rossby Radii that might be considered sensitive to coastal upwelling. It is likely that wind-stress curl and the resulting Ekman pumping play a larger role in influencing vertical velocities 100s to 1700 km offshore than alongshore winds. I would not expect Ekman pumping in offshore regions to respond to changes in sea-level pressure or wind stress in the same manner as coastal upwelling, and so the consideration here of upwelling in offshore regions seems irrelevant to the authors' objectives. For example, see Figures 4, 12, and 13 of Wunsch 2011 (*Journal of Marine Research*, 69, 417–434, 2011) that illustrates that some of the offshore regions considered in this draft are characterized by climatological downwelling as a result of wind-stress curl (but, superficially, an “upwelling-favourable” wind direction). The authors indicate, in text, that “the link between each upwelling index and wind stress, calculated as the patterns of correlations between the index and wind stress in each model grid-cell (not shown), is also very realistic, displaying in each case a characteristic alongshore wind stress that favours Ekman transport.” It is not clear to me what the realistic expectation would be between wind stress and Ekman pumping. The authors probably should also state that they are interested in “offshore” Ekman transport (rather than just Ekman transport).

The second significant issue that prevents clear interpretation of the results is the muddled investigation of the “imprint of external forcing on the drivers of upwelling” (manuscript section 5, p 2909). Here the authors attempt to explore whether the absence of common trends in upwelling can be explained by a lack of common response of sea-level pressure gradient to external forcing. The authors have skipped a few critical steps in this analysis. First, the relationship between the authors' metric of upwelling and winds was not well described. Second, the regions considered in the analysis of sea-level pressure are vaguely mentioned as “the regions most closely correlated with the upwelling indices.” More explanation is necessary to interpret the analyses performed. Perhaps these regions be indicated on Figure 3 and their choice better justified.

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Section 6 of the manuscript (“Imprint of external forcing on stratification”) was unexpected. If stratification is to be considered as part of the analyses, perhaps it should be introduced earlier in the manuscript. However, the authors do not perform an analysis of stratification; instead, global patterns of SST are considered. These SST patterns are not compared with upwelling. The utility of this section of the manuscript was not apparent to me as a reader.

Both sections 5 and 6 which consider the external forcing appear to be limited to external forcing the past 1000 and historical periods. The response of upwelling drivers and surface temperatures to future external forcing was not considered. This should be made clear, as the future change in external forcing is expected to be much larger than that of the past, but the authors' findings in sections 5 and 6 should not be interpreted as applicable to future periods.

The “Discussion and conclusions” (section 7, pp 2911 and 2912) can be improved with more attention to detail. The current discussion does not consider the distinctions between coastal and curl-driven upwelling, but I think this discussion would be essential to the authors' interpretation. The authors note that the trends noted are consistent “in all three simulations in the ensemble” but “are not always consistent with the expectation of upwelling, with some regions showing an intensification but others showing a weakening.” Neither of these statements appear to be precise, as trends in the Humboldt system do not appear to be consistent in the ensemble, and only in one of the regions (Benguela) are the trends consistent with the authors' expectation of intensification. The authors note that their “results generally agree with the ones obtained by Wang et al. (2015).” I found this statement surprising, as Wang et al. (2015) found support for Bakun's 1990 hypothesis of intensification (as the authors note on P 2901, lines 26-27). Here, the analyses seem to offer superficial support for Bakun's hypothesis only in the Benguela region, and the authors note that their results “are in contrast with the hypothesis of a discernible influence of the external forcing on coastal upwelling intensity” during historical and future periods.

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Finally, I suggest the authors highlight the differences between use of a single model ensemble and the multi-model analysis of Wang et al. (2015). Comparing the future projection of one model to that of multiple models, as is done here, is one option for discussion. However, the real strength of using a single-model ensemble is the ability to more clearly distinguish internal variability from the forced response. Some discussion of these different approaches (single-model ensemble vs analysis of multiple models) and the implications for interpretation of model results seems warranted and could strengthen the manuscript.

In summary, I suggest the authors take a more careful and systematic approach to model analysis. They might also consider whether the focus of the manuscript should be on past variability or future anthropogenic forcing. Currently, the analyses appear to be weighted towards the past1000 and historical simulations, with only one paragraph of the results dedicated to the future time period. However, the bulk of the introduction and discussion appear to be focused on comparing results of the current project with previous efforts that focused on analyses of the future period.

Specific comments

P 2900, lines 5-10: The description of the project in the abstract is a bit incorrect. Here, it is noted that the ensembles included simulations of three time periods: past1000, historical, and future. However, only one ensemble included these three time periods.

P 2900, lines 20-21: I am unsure if intensification of the oceanic high-pressure systems is a significant aspect of Bakun's 1990 hypothesis. Clarify this point.

P 2901, lines 10-11: "Also, other long-term records of upwelling intensity are indirect, and sometimes even based on wind records themselves." I found this statement confusing, as I am unaware of methods to directly observe upwelling (as the rate is much smaller than can be detected by direct measurement). All studies of long-term variability in upwelling intensity are indirect. Those that report vertical velocities typically infer those vertical velocities from some simple Ekman relationship or use a model that

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incorporates wind forcing as a significant influence on circulation.

P 2901, lines 20-21: There are a number of other analyses that have examined the effect of increasing greenhouse-gas concentrations on upwelling that may be worth noting. Some of these include Hsieh and Boer (1992), Mote and Mantua (2002), Diffenbaugh (2005), and Rykaczewski et al. (2015). I am not one to require an exhaustive review of literature in an introduction, but the authors may benefit from awareness of the methods and discussions of these other efforts.

P 2902, lines 5-7: These two sentences appear to be contradictory. The first sentence suggests that verification of Bakun's hypothesis in the recent past is critical if it is to be considered valid for the future period. The second sentence suggests that the hypothesis may be valid regardless of whether or not it can be verified for the past period.

P 2904, lines 17-18: If the authors believe that the resolution of the models they utilize can realistically represent wind-stress curl in these regions, they should provide a reference. The publication that comes to mind that has raised scepticism is Capet et al. (2004) which suggests that models of 1 degree horizontal resolution would not represent wind-stress curl appropriately, but the authors may know of a more applicable publication.

P 2906, lines 1-9: Here, the authors provide a qualitative comparison between the models and observations. The authors should provide, at the least, references for temperature and wind observations against which they are comparing model simulations.

P 2907: The authors discuss the prehistorical evolution of global temperatures as a result of external forcing (as interpreted from proxy reconstructions) and then compare this evolution with the modelled upwelling. A necessary intermediate step here is an assessment of whether the two models considered here simulate the global variation that is described. As mentioned earlier, justification for filtering of time series would aid in understanding.

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P 2908, lines 7-10: The “general” result here is one of trends of opposite sign, but California and Morocco are two “exceptions.” It is not clear to me why the two regions exhibiting opposite signs were considered “the general result” and the two regions exhibiting similar trends were considered “the exception.”

P 2909, lines 1-2: Here, when comparing the results of the current analysis to that of Wang et al. (2015) the authors focus on their consideration of different forcing scenarios and the detectability of upwelling responses. However, nothing is noted about the difference in the sign of the projections. I would expect that a disagreement in the sign of projected trends between Wang et al. (2015) and the current analysis would be worth noting.

P 2911, lines 2-4: The manuscript confuses coastal and curl-driven upwelling, and so I am unsure whether the authors should conclude that their findings are specific to coastal upwelling.

P 2912, lines 1-4: My interpretation of the results of Rykaczewski et al. (2015) is that they found most IPCC AR5 models simulated an increase in upwelling-favourable winds in the Canary and Humboldt systems, but no consistency across models in the Benguela system, and a decrease in winds in the California system.

Technical corrections

P 2908, line 4: I interpret table 8.6 in Chapter 8 of the AR5 (Myhre et al., 2013) to suggest that the increase external forcing over the past 250 years is 2.3 watts per square meter rather than 1.6 watts per square meter.

There are a number of grammatical errors that should be caught in proofreading. Most of these did not inhibit understanding of the authors’ intent. The one exception is the first sentence of section 4 (P 2906). This sentence is indecipherable.

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